PILLARS OF THE PAST

Volume IV
By Charles Ginenthal

CHRONOLOGY OF THE AGE OF STONEHENGE AND THE MEGALITHIC WORLD

Including:

Supplement: The “Land of Punt” Redux
By Lewis M. Greenberg

With A Linguistic Note on the “Land of Punt,”
Addendum (2012)
By Ralph E. Juergens and Lewis M. Greenberg

and

Appendix: Astronomy and the Short Chronology
By Lynn E. Rose
THE VELIKOVSKIAN

A Journal of Myth, History and Science
Quota pars operis tanti nobis committitur?

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PREFACE

This book is the final volume of *Pillars of the Past* and a continuation and exploration of how scientific, technological and other forms of evidence developed and employed in the previous volumes can be utilized to place Stonehenge, the megaliths of Europe and the people associated with these into the short chronology and specifically into the chronology of Immanuel Velikovsky. Prehistorians, archaeologists, as well as historians presently hold that these monuments and artifacts attributed to them were created/raised during the Late Neolithic and down to the Middle Bronze Age—ca. 4000–1500 B.C. If this chronology is correct, it contradicts Immanuel Velikovsky’s thesis that global catastrophes occurred after these times. One cannot realistically expect that these great stone monuments in Britain, Ireland, Scotland, Brittany, Malta, Gozo, Spain, etc., would still be erect and more or less intact after the Earth had experienced global extraterrestrial cataclysms that shook the very foundations of the globe. In terms of Velikovsky’s hypothesis the period to which these monuments are assigned, 4000–1500 B.C., was the age of the mammoths and other mega-fauna and thus it would hardly be possible that the primitive people of this time would have undertaken the enormous effort to move great stones to form circles or other massive architectural constructions. They clearly were still in their technological infancy.

Furthermore, several critics of Velikovsky have come forth to point out that these megaliths are oriented to specific places in the sky such as the solstices which when retrocalculated indicate that the orientation of the Earth’s axis had not changed catastrophically but was then in the same location as today, something impossible had the axis moved as Velikovsky claimed.

Nevertheless, all these criticisms are based on the collective view of the authorities that the chronology of the megalithic age is valid. If the scientific technological and other forms of evidence contradict the established chronology and move the age of these monuments into much later historic times, which is the thesis of this book, then the criticisms of Velikovsky can have no standing. As with the prior three volumes of *Pillars of the Past* which employed forensic historical evidence and proved the chronology of the ancient Near East is about 2000 years too long, this book will attempt to prove that Stonehenge and the megalithic age is thousands of years too old. This hypothesis is not original with us as a
chronological concept; it was presented by Algernon Herbert and James Fergusson and others in the 19th century based on the evidence then available to them. Today of course, after 150 years of additional research and archaeological excavations there exists a great deal of new evidence that can and will be brought to bear on the chronology of Stonehenge and the megalithic age that fully supports Herbert, Fergusson and thus also Velikovsky’s placement. Like Herbert and Fergusson, we place this megalithic epoch in the post-Roman times—the age of the Anglo-Saxons.

This author suspects that many readers reading the last paragraph, imbued with the viewpoint that the chronology of the Megalithic Age is set in stone, and seeing the world through that uniformitarian filter, will believe no such Velikovskian cataclysms happened, will put down this work as utter nonsense and read no further. Since they have most probably not read or availed themselves of the previous volumes of this series, they cannot fathom nor understand how well the various and numerous forms of interdisciplinary scientific, technological and other evidence have so clearly shown that the chronology of the ancient Near East is deeply in error. Not having read these volumes, their conceit that such matters are settled will give them leave to ignore that evidence as well as the interdisciplinary forensic historical evidence that will be presented here. We suggest that any critics of these works who fail to read dispassionately and fully all the material evidence presented are not serious and therefore their criticisms cannot be taken seriously. All too often we have shown, almost as a rule, critics of the short chronology have ignored facts that were presented to raise other non-scientific criticisms.

The character and nature of this interdisciplinary evidence to be presented below will show that the prehistoric chronology of the megalithic world is built on foundations that not only do not give support to that chronology but are often in contradiction to it and with one another. Rather than having a well-defined, ordered chronology connected to the various forms of acceptable evidence called upon as support, what actually exists is a plethora of unresolved problems, enigmas and contradictions that have little connection with that chronological reality as that reality is seen through the lenses of science and technology. Not surprisingly, the greatest authorities in these particular fields of research admit that not only have the scientific foundations presented to uphold that chronology failed, but that the secondary dating mechanisms—pottery dating, metallurgical dating, artifact dating, etc.—have also failed, again and again in every case. These failures have not sunk in and thus must be exposed because they are still presented as sound support for the established chronology of the megalithic age.
This volume, like the earlier ones, calls for a further revolution and revision of the megalithic age, and we recite the words of Colin Renfrew, who gave voice to the dating revolution that moved Stonehenge, etc., back into the Neolithic/Bronze Ages, as a preamble to this dissertation.

“Archaeologists all over the world have realized that much of prehistory, as written in the existing textbooks, is inadequate: some of it quite simply wrong… But what has come as a considerable shock, a development hardly foreseeable just a few years ago, is that prehistory as we have learnt it is based upon several assumptions which can no longer be accepted as valid… Several commentators have spoken recently of a ‘revolution’ in prehistory of the same fundamental nature as a revolution in scientific thinking. It has been suggested, indeed, that the changes now at work in prehistory herald the shift to a ‘new paradigm’, an entire new framework of thought, made necessary by the collapse of the ‘first paradigm.’”

Renfrew was, in this instance, discussing the radio carbon revolution that made European prehistory vis-à-vis Herbert and Fergusson’s historical placement of Stonehenge, et al., in post-Roman times go back into the Neolithic and Bronze Ages. The earlier theory held that the megalithic age grew out of its contact with the more advanced civilizations of the Near East.

The revolution we envisage attempts to reinstate that earlier paradigm not only because the radiocarbon revolution failed, as we will show and cite a major authority to that effect, but because the positive forensic interdisciplinary historical evidence requires just that, namely Stonehenge and the megalithic world must be placed in the post-Roman epoch and into Medieval/Anglo-Saxon times.

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CHAPTER 1
BUILDING THEORIES BASED ON ASSUMPTIONS

“No one has yet been able so to classify the contents of cognate monuments as to construct a chronometric scale which is applicable for the elucidation of their dates; and no à priori reasoning has been hit upon that is of the smallest use in explaining either their age or their peculiarities.”


“In 1866 T.G. Bonney confirmed the existence of such chronological confusion in the popular mind. ‘In England, everything of unknown origin is instinctively assigned to one of four—Julius Caesar, King Arthur, the Druids, or the Devil,’ adding that Stonehenge had variously been considered the handiwork of the [Breton Queen] Boadicea, the Phoenicians as well as ‘the later Britons, the Saxons or the Danes’… the circles were Roman, they were Danish, and---Anglo-Saxon monuments.”


There are to this day innumerable problems with understanding Stonehenge and the megalithic age; this was admitted by Christopher Chippindale as late as 1983.

“Stonehenge sets a puzzle that has never been solved….

“Imagination has made Stonehenge a field of conjecture ‘where the mazes of wild opinions are more complex and intricate than the ruin’...

“But there have been surprises in recent years. By the 1930s professional archaeologists thought they had at last got the measure of Stonehenge. All those amateurish speculations, Druidical fantasies and the rest of the ‘pow pow nonsense’ were to be swept away by the objective facts contained in a few excavation reports. Not so. The past few decades have been much odder than any that went before. First those sober, cool and objective scholars went chasing after
a vision of ancient Greece. When they recovered their composure they found that
they had lost their hold on Stonehenge; it had passed metaphorically into the
realms of astronomers and alternative archaeologists, and, literally, into the hands
of swarms of people who seemed to have no legitimate claim on it—not just
modern-day Druids, but...weirdoes,...and worse.”1

Jacquetta Hawkes has encapsulated Stonehenge with this pithy bon mot: “Every
age has the Stonehenge it deserves—or desires.”2

The crucial dilemma regarding Stonehenge and the megalithic age is that the
chronology on which it now stands does not have scientific and technological
pillars to support it. As with the established chronology of the ancient Near East
which is riddled with problems, enigmas, and contradictions, because it is not
upheld by scientific and technological pillars, so too, Stonehenge and the
megalithic age suffer from identical problems, enigmas and contradictions because
of this lack of forensic historical foundations. As Anthony A. Aveni explains:

“...Jacquetta Hawkes’s epigram has become almost a cliché to describe the way
we shackle ourselves to the present, the way we deny the diversity of past peoples
compared with ourselves by garbing our ancestors in our scientific clothing and
pushing our ideas and motives into their empty heads.”3

The problem echoes the words of Alan Gardiner that our understanding of the
history of the ancient Near East is in chains because of the chronology laid down
by Manetho and adhered to by legions of authorities: “No Egyptologist has yet
been able to free himself from the shackles imposed by the native annalist’s
[Manetho’s] thirty Dynasties, and these are likely always to remain the essential
framework of our modern expositions.”4

Rather than being shackled to a Stonehenge imprisoned in the present-day
conventional chronology bereft of connections to historical reality, we shall in the
later chapters of this book get to a Stonehenge and megalithic world connected to
historical reality because it is unshackled from that conventionally accepted
chronology and underpinned by forensic historical evidence. Before examining the
latest theoretical expositions of the megalithic age we are going to examine the
earlier theories to show not only how unrelated to reality and irrelevant they were,
but to show in later stages of the book that modern researchers with their vast knowledge have trodden down these self-same paths to explain these various monuments and force them to fit their “desires.”

The first theory was that of Inigo Jones whose research, if one can call it that, presents several approaches to Stonehenge, which were in the 1900s applied to it and the hundreds of other megalithic structures that dot Britain, Ireland, Scotland, and Brittany. Jones held that Stonehenge was constructed by the Romans during their 360-year reign of Britain. Lee Morrissey nicely captures the essence of Jones’s theory and chronology:

“By claiming that Stonehenge was classical [i.e., Roman] Jones initiated the seventeenth and eighteenth century’s Stonehenge controversy, by making Stonehenge more important that [sic] it had previously seemed. Working on a commission from James I...Inigo Jones assembled The Most Notable Antiquity of Great Britain, Vulgarly called Stone-Heng, on Salisbury Plain, Restored, published posthumously in 1655 by his student John Webb. In this work, Jones claims, for two related reasons, that ‘Stone-Heng...was a work built by the Romans.’ To begin with, Jones decided that Stonehenge was a dilapidated ruin. Then in his drawings, that is, what architects call a ‘plan’, he provided the parts of Stonehenge he thought were missing. On the basis of these plans he argued that Stonehenge represented a Roman temple. Moreover, the scale of the work, according to Jones, was beyond the capability of the [Neolithic] British. Regardless of whether or not Stonehenge is Roman, the Britons were incapable of building it because as ‘a savage and barbarous people,’ they were ‘destitute of the Knowledge, even to clothe themselves, much less any Knowledge had they to erect stately Structures, or such remarkable Works as Stone-Heng.’

“Through a principle of aesthetic noncontradiction, Jones argues that ‘where Art is not,’ i.e., among those British savages, ‘nothing can be performed by Art.’ Because Jones sees ‘Art’ in this monument, art of which the British either are or were incapable, it must then have been built by the Romans: where art is, then can something be performed by art. However, considering Stonehenge as it stands, most interesting is Jones’s assumption that the builders of the monument had art; for by ‘Art’ he specifies that he meant ‘order,’ ‘symmetry,’ and ‘decorum,’ of which these Britains ‘neither had...in them.’”

5 Lee Morrissey, From the Temple to the Castle: An Architectural history of British Literature 1660-1760 (Charlottesville VA 1999), p. 85
What Jones had done with his architectural plan of Stonehenge was literally to invent structures there that did not exist except in his theory which were all of his own invention.

Morrissey explains:

“...Jones came to this conclusion by comparing his own stylized rendering, his own plan of an imagined complete, circular Stonehenge, with Palladio’s plan of an ancient Roman theater in Barbaro’s 1556 edition of Vitruvius...Although the surviving columns and lintels of the [pentagon-like central horseshoe-shaped] wall suggest an ellipse, Jones disregarded the physical evidence [and] invented [an extra trilithon making] hexagonal ‘walls,’ and drew the outer [nearby] wall of Stonehenge as a true circle. Jones then inscribed, within his Stonehenge plan [between the hexagon and the nearby circular sarsen stages], ‘four intersecting equilateral triangles [forming an eight-pointed star] corresponding to the ancient theater.’ The ease with which these triangles fit inside Jones’s version of the monument cinched his argument; the triangles showed that Stonehenge had ‘Art,’ that it had order and symmetry. And because the triangles fit into Jones’s drawing just as they had in Palladio’s, Jones concluded that the Romans built Stonehenge, just as they had supposedly built the theater that Palladio’s illustration represents.

“As Christopher Chippindale notes, ‘The best thing about the analysis is its nerve.’

This type of mathematical invention was to be employed by others in the 20th century to make Stonehenge and the other megalithic monuments conform to theory, not to fact.

“Inigo Jones drew his conclusions on the basis of highly stylized drawings of the monument as it might have stood, given his reading of its remnants, i.e., on inaccurate drawings.”

We will encounter this approach in the chapter that deals with astro-archaeology, by which researchers imagined particular alignments between and among the various megaliths and their surrounding landscapes to provide “order” and “symmetry” which accorded with their own theoretical constructions, even employing triangles of a unique form that fit a plan and could be construed as definitive evidence that the theory had merit. Others, based on reconstruction or construction of these monuments and the surrounding landscape, claimed these

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6 Ibid., pp. 86-88
7 Ibid., p. 88
gave clear evidence of unique astronomical alignments, while various other possible alignments were completely ignored or dismissed; even one which we will show gave an alignment at Stonehenge itself of the summer solstice sunrise for the time frame which was completely overlooked. In all these other cases the evidence was either so ambiguous or erroneous there was no way to prove what was being propounded.

In Inigo Jones’s case conflict over his vision ultimately led to its collapse.

Dr. Walter Charleton, physician to Charles II in 1663 wrote *Chorea Gigantum* which attempted to refute Jones and presented his own theory, arguing that when “‘diligently compared’...with monuments in Denmark...Stonehenge was ‘erected by the DANES...and principally designed to be a Court Royal, or Place for the Election and Inauguration of their Kings’...”8

Jones’s student, John Webb, in *Vindication of Stone-heng Restored* in 1665,

“defended Jones’s Roman interpretation, contending that the Danish could not have built Stonehenge because after the fall of the Roman Empire, they, like Jones’s British before the Roman Empire, were [also] not capable of constructing such a building...Not only was Stonehenge not built by the Danes, but in fact it fell into its present state of disrepair under their political leadership.”9

A major figure in the history and chronology of Stonehenge and therefore of the other megalithic monuments in northwestern Europe was John Aubrey who effectively answered Jones and Webb as well as Charleton:

“In the 1690s John Aubrey...permanently altered the Stonehenge debate by changing the method of research. Calling his method ‘comparative antiquity’ and describing it as ‘a kind of algebraic’ method..., to make the stones give evidence of themselves,’ Aubrey compiled and calculated in manuscript (but did not publish) the results of research he had undertaken on a commission from the Duke of York. Aubrey considered the stone circles of Wiltshire [the region of Stonehenge] in light of the other remaining circles spread unevenly throughout the British Isles, most of them in the north and west of England...

“Aubrey even found a stone circle in Ireland, at which point he remembered that ‘the Romans had no dominion in Ireland, or (at least not far) in Scotland: therefore those temples are not supposed to be built by them: nor had the Danes

Aubrey claimed that these monuments preceded written history and therefore Roman times in that part of Europe.

Peter Lancaster Brown discusses Aubrey’s *Monumenta Britannica*, a manuscript residing in the Bodleian Library at Oxford:

“Aubrey tells us in his manuscript: ‘There have been several books writt by learned men concerning Stoneheng, much differing from one another, some offering one thing, some another…’ Aubrey submitted that Stonehenge and other circle monuments he had inspected ‘were Temples of the Druids.’ Exploiting an early usage of the comparative method in archaeology, he wrote: ‘When a traveller rides along the Ruines of a Monastery he knows by the manner of buildings, Chapell, Cloysters and etc, that it was a Convent, but of what Order, Benedictine, Dominican and etc, it was, he cannot tell by the bare View. So it is clear that all the Monuments, which I have recounted were Temples. Now my presumption is, That the Druids, being the most eminent Priests or order…‘tis odds, but that these ancient monuments, etc...are as ancient as those times...’

“Aubrey admits that his theory is conjectural, and with a nice turn of phrase concludes: ‘...but although I have not brought it into clear light; yet I can affirm that I have brought it from an utter darkness to a thin mist, and have gonne further in this Essay than anyone before me.’ Aubrey justifies his general speculations, noting: ‘These Antiquities are so exceedingly old that no Bookes doe reach them, so there is no Way to retrieve them but by comparative antiquitie...’”

Brian M. Fagan tells us that Aubrey:

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11 Peter Lancaster Brown, *Megaliths, Myths and Men: An Introduction to Astro-Archaeology* (Mineola NY 2000), pp. 54-55 [original spelling left in citation from Aubrey]
“...considered the builders of the stone circles ‘Ancient Britons,’ ‘savages’ who lived in a shady dismal wood.” 12

He compares them to Native Americans:

“They were two or three degrees I suppose less savage than the Americans.” 13

In essence the builders of the megalithic monuments were primitives living in the prehistoric dawn of time. This conclusion was so strong in its appeal, especially to modern researchers, that few have questioned the reality of the chronology that Aubrey presented. Since these monuments were neither Roman nor Danish, they had to be older than both cultures. What had not been considered except by a few 19th century heretics was that these monuments could have been built in post-Roman and pre-Danish times, when England was still largely illiterate so that “no Bookes doe reach them...,” 14 and that the people of that age were still comparatively backward in historical development of the “arts.” Those who later challenged Aubrey’s chronology were regarded as cranks at worst or misguided at best.

As Morrissey further states:

“It is difficult to overestimate the influence of this unpublished argument [by Aubrey]. When Daniel Defoe’s Tour arrives at Stonehenge, for example, he refers to the varied attributions put forward by Inigo Jones and John Aubrey; clearly Defoe was familiar with this argument, circulated rather than published. Moreover Defoe calls Stonehenge ‘that celebrated piece of antiquity; the wonderful Stoneheng.’ One hundred years earlier Stonehenge had been described in terms of its ‘weight and worthlessness;’ by the 1720s Defoe is calling it ‘a reverend piece of antiquity.’ In appreciating [the great age of] Stonehenge Defoe positions himself as someone familiar with several authors who have written on this relatively obscure subject...

“As a result of Aubrey’s archaeology Stonehenge was no longer conceivably Roman, yet its importance grew, as Defoe’s Tour suggests. ‘The elegant Roman and Danish Stonehenges were in decline, and the rude British was rising’, writes Chippindale. This [chronological] change left eighteenth-century observers (and today’s scholarship) with a problem: how could the importance of Stonehenge be

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12 Brian M. Fagan, From Black Land to Fifth Sun: The Science of Sacred Sites (Reading MA 1999), p. 117
13 Ibid.
14 Ibid.
accredited at the same time that England was going through a significant period of classicism? As Howard Weinbrot points out, “the danger or irrelevance of classical culture in a putative neoclassical age is not one of the more attractive paradoxes of literary history.”

The problem is that primitive societies do not build large stone temples. These constructions are generally made by highly developed, well-organized societies.

“That is, if Stonehenge represented an archaeological remnant of an ancient Britain, then it must have been one part of a well-organized now-forgotten culture. The predicament that this archaeology consequently presented was how to describe the lost culture, symbolized by the scale of Stonehenge.”

While it became accepted that prehistoric Britons had built Stonehenge and the many other megalithic monuments, the problem then as now is to explain how a Neolithic culture of subsistence farmers and herders, as well as hunter-gatherers, found the time to build Stonehenge, had the organizational structure to plan, organize, and carry out these building feats, and why they did so when the usual pressing need to obtain sustenance, at this early period, must have been of paramount importance for their survival. We shall return to this question below.

The answer to this problem, then as now, was to create a British, Irish, Scottish culture prior to all other historical ones that was unique, in that it was able to do, prior to these more advanced societies, feats of construction that took these others (based on the established chronology) perhaps a thousand years and more to produce. In the words of Sir Leonard Woolley:

“If hitherto change had been painfully slow, it was because Neolithic man was hard put to it to live. All his efforts were necessarily devoted to getting sufficient food for his family out of an unpromising and thankless soil; only when the struggle for existence ceased to absorb all his time and energy could he find leisure for the amenities of life [and for large building projects]. The first requisite for civilization was a wide extent of rich soil easily worked.

“...To get the best out of life man requires not only good soil but also a climate that permits and encourages him to work out of doors, in moderation, all year round...Only where soil and climate alike were favourable could man produce in

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15 Morrissey, *op.cit.*, pp. 91-92
excess of his actual needs and yet have leisure to enjoy the surplus; and hitherto he
had found no region which satisfied both conditions.”

The researcher who elevated these primitive early people to the level of a fairly
advanced society was William Stukeley in the 18th century who created an entire
society. According to Burton Feldman and Robert D. Richardson:

“William Stukeley, a pioneer of modern archaeology and a well known writer
on Druids... was a somewhat more colourful figure than most students of myth... he
traveled about England observing and studying ancient remains; he became a
cleric, dabbled in many fields..., was a friend and correspondent of [Isaac] Newton
and [William] Warburton, and was described by the latter as an honest and a
learned man who was a mixture of ‘simplicity, drollery, absurdity, ingenuity,
superstition, and antiquarianism’... Later he laid out a garden with a Druid temple
and an old apple tree, covered with mistletoe, in the center. Stukeley appears to
have been genially crazed on the subject of Druids, sometimes signing himself [as]
‘Chyndonax Archdruid’...

“According to [A.L.] Owen, Stukeley had originally thought that Stonehenge
was a ‘huge ideogram,’ a great stone hieroglyph of Druid doctrine... and Stukeley
thought Druidism to have been Egyptian in origin. Why he thought this and why
he changed his mind are not known (Stukeley’s interest in Stonehenge and
Newton’s in Solomon’s Temple are intriguingly similar), but Stuckley’s [sic] new
theory about Stonehenge made it only a part of a much grander conception.”

What was Stukeley’s “grand conception” or better yet “grand illusion”?

“According to this theory, the earliest religion was that of the patriarchs before
the Mosaic dispensation. This primitive [patriarchal] religion, Stukeley argued
(and in this part of his scheme he was not alone), was in fact Christianity, and the
Mosaic dispensation, when it came, was only a period of darkness, a ‘veil
intervening’ between early natural Christianity and the later religion brought by
Christ. Stukeley contends that the descendents of the patriarchs came to Britain,
where they were called Druids, and where they carried on the practice of their
early and pure Christianity in temples such as Stonehenge, uncorrupted by the
Mosaic dispensation... The Druidical...doctrines were transmitted from generation
to generation, eventually ending up as the doctrine of the modern Church of

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17 Sir Leonard Woolley, Jacquetta Hawkes, *Prehistory and the Beginnings of Civilization*, vol. 1,
(NY 1963), p. 364
18 Burton Feldman, Robert D. Richardson, *The Rise of Modern Mythology 1680-1860*
(Bloomington IN 1972), pp. 124-125
England. Thus for Stukeley the Druids were the means by which early Christianity was preserved from corruption and kept alive and pure in England itself.

“As Stukely saw the Druids, they were venerable, learned, and pious; they were to be admired, studied, even emulated; they were the link connecting modern England with earliest times; they were England’s guarantee of spiritual primacy....

“His patriotic quest for English origins and British greatness is related [to his overall thesis]...”19

Stukeley was a religious and nationalistic fanatic. But he was also dishonest as pointed out in Aubrey Burl and Neil Mortimer’s publication of “Stukeley’s Stonehenge: an unpublished manuscript”:

“It was exactly seventy years after Aubrey’s recognition of Avebury [the largest ring in Europe with two smaller inner ones] that Stukeley went there in 1719, being intrigued by [having read] Aubrey’s description of the earthwork enclosure and stone circles. He did not acknowledge his debt. On the contrary he claimed the discovery for himself[, writing:] ‘In 1718 Mr. Roger and Sam. Gale and I took a journey, through my eager desire, to view Abury, an antiquity altogether unknown.’ This was not only a lie. It was a preposterous lie. The year was wrong. The visit was in 1719 [not 1718] by which time Stukeley had already copied the ‘Monumenta Britannica’. Aubrey’s account of Avebury, moreover, had been published over twenty years earlier...It was ungracious to ignore such evidence...”20

They add, however, that Stukeley’s

“...field observations were good but they were written in a historical vacuum. There was not one piece of information about the period in prehistoric Britain when Stonehenge was built....

“‘These Antiquities’, wrote John Aubrey, ‘are so old that no Bookes doe reach them’. They were like the fragments of a shipwreck ‘so that the retrieving of these forgotten things from oblivion in some sort resembles the Art of a Conjurer.’

“Even earlier than Aubrey, William Camden had been sceptical about attempts to recover information about the unwritten years of prehistoric Britain from the writings of classical authors: ‘I feare me greatly, that no man is able to fetch out

19 Ibid., p. 125

the truth, so deeply plunged within the winding revolutions of so many ages…they lie so hidden in the utmost nooke and secretest closet of Antiquitie, as it were in a most thicke wood, where no pathwaies are to be seene…oblivion hath so long removed out of sight of our ancestors’.

“Fifty years later Stukeley remained in the same chronological wilderness.”

The ultimate fantasy of Stukeley is well outlined by James Fergusson; after calling Dr. Stukeley “one of the most imaginative men and one of the wildest theorists” he goes on to show:

“Dr. Stukeley concluded that the Druids were serpent-worshippers, and consequently that Stonehenge, Avebury, etc., were serpent temples—Dracontia, as he calls them, daringly assuming that a word, which in the singular was only the name of a plant, was actually applied by the ancients to serpent temples, of the form of which, however, they were as ignorant as the Doctor himself. Having advanced so far, it only remained to adapt the English circles to this newly discovered form of worship, and Avebury was chosen as the principal illustration. There was a small circle on Hakpen Hill, which had a stone avenue formed by six or eight stones running east and west; between West Kennet and Avebury there was another avenue leading to the circles, but trending north and south. By introducing a curved piece between these fragments, Hakpen became the head of the snake, the avenue its body; Avebury a convoluted part of it, and then a tail was added, a mile long, on the authority of two stones in the village, and a dolmen, called Long Stone Cove, about half-way between Avebury and the end of the tail! Stanton Drew and other circles were treated in the same way; curved avenues, for which there is not a shadow of authority, except in the Doctor’s imagination, were added wherever required, and serpents manufactured wherever wanted. It never seems even to have occurred to the Doctor or his contemporaries to ask whether, in any time or place, any temple was ever built in the form of the gods to be worshipped therein or thereat, or how any human being could discover the form of a serpent in [nearly straight] rows of stones stretching over hills and valleys, crossing streams, and hid occasionally by mounds and earthworks. On a map with the missing parts supplied, this is easy enough; but there were no maps in those days, and in the open country it would puzzle even the most experienced surveyors to detect the serpent’s form.

“Had so silly a fabrication been put forward in the present day, it probably would have been met with the contempt it deserves; but the strangest part of the whole is that it was then accepted as a revelation. Even so steady and so well informed an antiquary as Sir Richard Colt Hoare adopts Dr. Stukeley’s views with——

21 Ibid., p. 9-10 [original spelling preserved]
out enquiry. His magnificent works on ‘Ancient and Modern Wiltshire,’ which are not only the most splendid, but the most valuable works of their class…are throughout disfigured by this one great blemish. He sees Druids and their Dragons everywhere, and never thinks of enquiring on what authority their existence rests.”

Those who followed well after in the 20th century also invented nonexistent religious, educated engineers who would construct these many rings, burial mounds, rows of stones etc., to fit their own peculiarly unique concoctions of how, why, and when these megaliths were constructed, based mostly on their own authority.

The last researcher we will deal with is Sir Norman Lockyer, the founder and editor of the premier scientific journal in Britain, *Nature*. He attempted to date the construction of Stonehenge, etc., by astronomy.

Richard J.C. Atkinson, one of the greatest archaeological authorities on Stonehenge, in a caustic analysis of Lockyer’s dating of the monument iterates what is wrong with his work; first setting down Lockyer’s observations and then commenting on their validity:

“1. *The axis of Stonehenge was aligned accurately by its builders upon the point of the midsummer sunrise at the date of its construction.*

“This is an assumption which is entirely unverifiable. The alignment can have been accurate only within limits of what can be done by eye, without instruments. In any case the *true* line of the axis cannot now be determined.

“2. *The Avenue [used to set the axis] is contemporary with the [circle of the] Sarsen stones and has exactly the same axis, which may be used as a substitute for the axis of the stones themselves.*

“[Based on the chronology of when these elements were constructed, t]he first statement is now known to be false, and the second is probably false as well, and in any case is unverifiable:

“3. *The azimuth bearing [compass direction] of the axis is 49º 34’ 18”* [49 degrees, 34 minutes, 18 seconds] *east of true north.*

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“This is an arbitrary axis chosen for reasons which are totally irrelevant to the question. Lockyer took two points on the estimated midline of the Avenue close to Stonehenge, and four more towards the far end of the first straight alignment of the Avenue...None of these, however, was chosen as the axis. Instead Lockyer observed that a line drawn through the center of Stonehenge approximately on the mean axis passed close to or through an earthwork on Silbury Hill...and another earthwork at Grovely...to the south-west, neither of which he finally selected for the axis...[but] is that of an Ordinance Survey bench mark erected in the nineteenth century A.D., near the former earthwork [of Silbury].

“The extraordinary proceeding seems all the more remarkable in that the two earthworks are now known to be of the Early Iron Age, and to have been built not earlier than the fourth century B.C....The chosen line has just as much, but no more, significance than the fact that the same line, if prolonged, passes through Copenhagen.

“4. For the midsummer [solstice] sunrise to have taken place on this axis the value of the obliquity of the ecliptic is 23° 54’ 30’’.

“5. This value [means Stonehenge was built] about the year 1680 B.C....”

Atkinson adds to this:

“The objections put forward to the first three arguments above are sufficient in themselves to show the unsoundness of the theory. Three others of a more fundamental kind may be stated briefly. The first is that we have no means of telling what the original builders regarded as ‘sunrise’. Was it the first gleam [of the rising sun’s top edge] of light (as Lockyer assumed); or when the sun’s [full] disc was just visible, with the horizon as tangent to its lower margin? The date computed using the first of these positions differs by nearly 4,000 years from that of using the last.

“Secondly, it should be observed that the assumed line of sight along the axis [at the present time] is not marked positively in any way, as are the [precise] sights of a rifle. The axis is determined [broadly] by the mid-points of a number of empty spaces between pairs of upright stones ([stones numbered] 1 and 30, 15 and 16, 55 and 56) and the largest of these spaces (between stones 1 and 30) is five feet wide at eye height. An error of only one inch [right or left] at this point makes a difference of over two centuries in the calculated date, and for any given position of the head of the observer on the axis, the use of the left eye instead of the right, or vice versa, makes a difference of 500 years [either earlier or later in time].

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23 Richard J.C. Atkinson, Stonehenge (NY 1963), pp. 87-88
“Thirdly, it must be remembered that Lockyer’s observations were made with [telescopic and other] instruments of the highest precision, whereas the instruments used by the original builders were confined to their own naked eyes, at the most, a number of straight sticks cut from the nearest hazel-thicket [and set along a straight line give far more accurately] the permissible limits of accuracy of the whole problem are necessarily fixed by the degree of precision of which the original builders were capable [of finding a more precise, direct summer solstice alignment], and any conclusions [using telescopes] based upon the use of narrower limits of accuracy are therefore wholly unreliable.”24

Based on these criticisms, Atkinson concluded:

“In view of these objections it is indeed surprising that the date arrived at from astronomical observations should be [dated by Lockyer] so close to that given by purely archaeological evidence. Nonetheless, neither method of approach can be taken to confirm the correctness of the other. The only conclusion to be drawn from [Lockyer’s] astronomical considerations is that the structure of Stonehenge [phase] II and Stonehenge [phase] IIIa were aligned roughly and approximately on the midsummer [solstice] sunrise. This is a fact; but does not tell us either why or [most importantly] when it was done.”25

For a short in-depth review of Lockyer’s analysis see Peter Lancaster Brown’s *Megaliths, Myths and Men*...(reprint Mineola NY 2000) pages 64-81. The astro-archaeologists who followed Lockyer in the 20th century, to be discussed below, in many instances, even with hindsight into these problems and more diligence, nevertheless were guilty of creating alignments based on assumptions and then dating Stonehenge and the other megalithic monuments with these assumptions to prehistoric times. But there was no way to *scientifically* date these, and that was the crux of the problem. Until an unimpeachable dating method existed to place these in a chronological order, all and any elaboration of their construction and purpose or purposes was not scientific but historic or archaeological in nature and therefore questionable.

The last analysis that failed to date these monuments prior to radiocarbon dating—considered a reliable scientific method—was that of Richard J.C. Atkinson. Peter James *et al.*, point out how this occurred:

“In July 1953 Richard Atkinson, director of excavations at Stonehenge, was photographing a 17th-century graffito on one of the massive sarsen stones when he

24 Ibid., pp. 88-89
25 Ibid., p. 89
noticed the faintly carved ‘outline of a hilted dagger, point downwards’. In Atkinson’s opinion, this fortuitous discovery gave ‘the first direct evidence of the date of the erection of the sarsen stones [circle] at Stonehenge’.

“Despite the fascination it has long held for antiquarians and archaeologists, the unique monument of Stonehenge has always been difficult to date...Identifying his dagger [stylistically] as a 16th-century BC specimen from the historically dated Mycenaean civilization of Greece, Atkinson argued that since no actual examples of this kind of dagger ‘are known from Britain, or indeed from N.W. Europe, it is reasonable to suppose that this carving was executed at Stonehenge within the lifetime of someone who was personally familiar with this type of weapon in its homeland; in other words, not later, say, than 1470 B.C.’

“This precise date was welcomed by British prehistorians. Already they had deduced that the final arrangement of Stonehenge was built for the Bronze Age chieftains of the Wessex Culture, whose rich burials in barrows [mounds both long and circular], dotting the landscape around the monument, had attracted excavators since the earliest days of archaeology. As well as local pottery and bronzework, these aristocratic graves contained amber and faience jewellery, goldwork and bone carvings—exotic and unusual items thought to be proof of trade between Britain and the early Mycenaean world. Atkinson was inspired to speculate further, transforming his new link between Wessex and the Aegean into a connection with profound repercussions. For him the dagger provided evidence that ‘the architect of the monument was himself a Mycenaean’. Although it was pointed out that the techniques [of construction] used at Stonehenge were not found in Mycenaean architecture, Atkinson still felt it was ‘surely more fitting to see them as the product of the relatively sophisticated civilization of Mycenae, rather than of the essentially barbarous, even if commercially successful, aristocracy of our [British] native Wessex culture’.”

Having discovered what clearly seemed to be the carving of a Mycenaean-style dagger, Atkinson, following the usual rules of historical analysis, concluded it had to have been known at the time Stonehenge was built and thus dated to Mycenaean times. This despite the fact that the stone construction techniques at Stonehenge were entirely different than those at Mycenae. James et al. describe what ensued at that time to overthrow Atkinson’s thesis, namely the radiocarbon revolution.

“In any case, Atkinson’s comparison between Mycenaean daggers and the carvings at Stonehenge hardly warrants its description as a Mycenaean monument.

26 Peter James et al., *Centuries of Darkness* (New Brunswick NJ 1993), pp. 1-2
Anthony Harding, author of the standard work on *The Mycenaean and Europe*, is far more realistic:

“...it seems extraordinary that weathered carvings of such ambiguity should be compared with a rather rare type [of dagger] thousands of kilometers away...All in all, the eye of faith is needed to detect any genuine similarity to Mycenaean daggers.’

“Finally, even if an accurate identification of the style, origin (and therefore date) of the Stonehenge daggers were possible, we would still be little the wiser. It could only tell us that the stone circle already existed before the carvings [which were added later]. The gap in time between the two could be vast. The point, ironically enough, is driven home by the very inscription of the 17th century AD [above the engraved dagger and axes]...Adding graffiti to existing monuments is a time-honoured practice, something which he unfortunately forgot in his excitement at finding a ‘Mycenaean link’ with Stonehenge.”

The *coup de grace* was radiocarbon dating which nearly all authorities accept as a fairly reliable method. James *et al.* point out:

“We now know that, far from being a decisive breakthrough, Atkinson’s discovery actually led him to make an error in archaeological dating with drastic consequences—one which seriously distorted our understanding of Stonehenge for many years. Radiocarbon dating...has shown that the Stonehenge sarsen [stone] circle was set up around 2000 BC.”

We shall return to this 17th-century graffito and the engraved dagger and axes below, because the archaeologists have overlooked an important form of evidence which casts serious doubt on the radiocarbon dates ascribed to the various phases of Stonehenge’s construction and all the other monuments of the entire megalithic age. Kevin Greene has rather adroitly captured the essence of the situation in two pithy sentences: “Everything that is currently known, or believed, about Stonehenge is the result of almost nine centuries of speculation, observation and excavation, guided by changing fashions and approaches. Since neither the original builders nor the purpose of the structure have any direct link to the present, its popular attraction stems more from ignorance than knowledge.”

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27 Ibid., p. 3
28 Ibid., p. 2
CHAPTER 2

THE RADIOCARBON REVOLUTION

Who then were these megalith builders and when did they live? The concept of their origins and chronology is well outlined by Philip Coppens.

“The Search for the identity of the Megalithic People has a long history, but it is safe to say that until the 19th century, the question remained largely open-ended. The megaliths were deemed to be there from Roman times, and hence attributed to the Celtic druids. In the 20th century, and to a large extent still today amongst the general populace, the megalithic culture as evidenced by the stones scattered all over Western Europe was considered to be inferior to the civilizations of the Middle East, especially Egypt…”

“There are always two components of one’s past, the ‘hard’ evidence of documents, artifacts, buildings, etc., and the soft evidence of ideas, beliefs, traditions, legends unsupported by ‘scientific evidence,’ that which is dug up, catalogued and either displayed [in museums] or locked away [in storage rooms]. In Ireland, the megaliths are the subject of archaeological investigations, yet are accompanied by a tradition of heroic and mythic builders. And it has to be said that even though archaeology has been able to recover enormous amounts of superb information regarding the megaliths, at its core, it has still not been able to identify who the Megalithic People really were.”

In essence, there is no knowledge of who the people who built the megaliths are, how they arrived in these lands, nor when they arrived. This dead end, we maintain, was created by a flawed chronology. The question of chronology, which is our main concern, has an interesting history, as Coppens continues:

“In 1939, eminent archaeologist Gordon Childe compared Europe with the Orient, and actually put down in writing that prehistoric culture in Europe was inferior to prehistoric culture in the Middle East. Furthermore, he said that the builders had been a community that had originated in the eastern Mediterranean

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1 Philip Coppens, *Land of the Gods: How a Scottish Landscape was Sanctified to Become Arthur’s Camelot* (Amsterdam, the Netherlands 2007), p. 163
and it was thought that their ideas had spread westward along two routes [from the East]: one via Spain to Ireland and the north, the other [from Spain] across France to Scandinavia. England, lying in the middle, was influenced by both streams. The thinking followed the ‘logic’ of the time, which was that farming originated in the Middle East and had spread westward, eventually arriving in Britain. Each novelty that was found in Western Europe was classified as a new stream of immigrants bringing with them new discoveries. As such, one migration was labeled the Grooved Ware People, followed by the Beaker People, followed by the Celtic migration, the Roman invasion, the Saxon immigration, etc. As some archaeologists have stated ‘If all this to-ing and fro-ing really did take place, then prehistoric Europe must have been in a permanent state of turmoil—for which there is no archaeological evidence whatsoever.’

“With Willard Libby’s discovery of radio carbon dating in 1955, this statement was suddenly turned upside down: Suddenly, the pyramids of Egypt were younger than the megaliths of Europe. The diffusion of knowledge from east to west was suddenly proved to be totally wrong. To quote Colin Renfrew ‘Suddenly and decisively the impressive megalith tombs of Western Europe are set earlier than any comparable monument anywhere approaching them in antiquity.’…Renfrew questioned if not ridiculed the old notion that the Megalith people were ‘barbarians’ who used no metal, yet built structures which 5,000 years later remained perfectly intact. Above all he wondered who they were, as the notion that they were colonists from the Near East could no longer be considered valid.

“As a result of recent [radiocarbon] research the time difference [separating the megalithic people from those of the Near Eastern civilization] has now grown to dramatic proportions. The building of some megaliths in Brittany and Carrowmore in Ireland has been shown to have taken place 2000 years before the building of the pyramids. Generally thought to be 3500 BC to 3000 BC, the latter date still pre-dates the pyramids by several centuries. Nevertheless, stones were still being added to Stonehenge until approximately 1200 BC, a date which seems to mark the close of the ‘Megalithic Era’. To quote archaeologist Euan MacKie: ‘If the European megaliths and even the Maltese temples are older than the oldest towns then it is difficult to see how urban society [in the Near East] could have played any significant part in the great social processes which were underway in Atlantic Europe between 4500 and 2500 BC [and] there must have been some specialized proto-urban or urban stratified societies in existence before the earliest megaliths appeared.’
“The concept of migrations of [earlier advanced] peoples has now been abandoned.”

The evidence that upholds this chronology must therefore be examined. There are several ways that have been utilized by archaeologists of the megalithic world to do just that. However, as we will show in the following, each and every one of the foundations/pillars upon which the chronology of the Megalithic Age was built has come to naught. These include radiocarbon dating, pottery sequence dating, typological sequence dating, metallurgy and stone tool dating, artifact dating, the shapes of skulls and barrows, and finally astro-archaeology. Each of these methods was hailed as proving that the chronology of the Megalithic Age was built on a sound basis. Then, as the investigators examined these in greater detail and depth, they all came apart.

Therefore, we will expose all of them so that the reader will understand how the hopes and expectations of the archaeologists were brought down from their lofty summits back to earth where the cold facts of evidence dissolved these modalities into emptiness. And we will further support this assertion by citing the authorities in each of these relevant fields who say directly that the evidence evoked as support for the established chronology of the Megalithic Age has failed in each and every case.

To overcome all the assumptions surrounding Stonehenge, prehistorians and archaeologists required a chronology based on solid scientific grounds. It was thus with the development of radiocarbon dating in the 1950s that the revolution in the chronology of Stonehenge and the Megalithic Age became established. In the minds of the authorities of this epoch, science had finally made the great chronological breakthrough needed to assure and secure for prehistorians and archaeologists that they were standing on a firm foundation based on reality—Scientific reality with a capital “S”. Because it appeared to be a true scientific tool, radiocarbon dating became the backbone—the sine qua non to which researchers could now add flesh.

In the words of Renfrew:

“The second half of the twentieth century saw major changes in the nature of Prehistory. In the first place the development of radiometric dating methods including radiocarbon allowed the construction of a chronology for prehistory in every part of the world. It was, moreover, a chronology free of any assumptions…

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2 Ibid., pp. 163-164
and it could be applied as well to non-literate societies as to those with written records. To be prehistoric no longer meant to be ahistoric in a chronological sense.

“As a direct consequence, a new kind of world prehistory became possible. It was feasible to date, quite independently of one another, all the ancient civilizations of the world…The antiquity of the aborigines of Australia could be compared with that of the Mound Builders of North America or the Neolithic lake dwellings of Switzerland.”

However, those who have read the three previous volumes of Pillars of the Past will know that radiocarbon failed on a colossal scale to date the chronology of the ancient Near East because it was scientifically contradicted by astronomical dating that is accurate to the day. Dendrochronology, which is used to calibrate radiocarbon dates and is supposedly accurate to a particular year, was also analyzed in our work and was shown to be overburdened with problems, such as to exclude it and radiocarbon in tandem with it as a useful dating tool. Since our evidence has shown that radiocarbon dating failed with respect to the chronology of the ancient Near East, how can it be taken to be accurate for the megalithic prehistoric age? Nevertheless, it is incumbent upon us to present the evidence here as well that invalidates the radiocarbon chronology for Stonehenge and the megalithic world. Let us therefore examine this dating evidence yet again as it applies to this region and epoch.

One of the most authoritative books on the dating of this period is that presented by the English Heritage Society in 1995. In Appendix 2 Michael J. Allen and Alex Bayliss point out:

“…until the present work, only a relatively small number of radiocarbon determinations have been obtained from excavation itself and from the Avenue [leading to Stonehenge]…Because of the perceived importance of Stonehenge, great weight has been placed on these dates…and they have become enshrined in [the] literature. The contextual integrity of some of these samples, however, is not

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4 Charles Ginethal, Pillars of the Past, vol. II, Mesopotamian, Anatolian, Mycenaean, Minoan and Harappan Chronology (Forest Hills, NY 2008), pp. 353-434 and beyond
necessarily secure and most were obtained early in the history of radiocarbon dating. They present a number of problems in both archaeological and radiocarbon terms.”

Note that the dates enshrined at Stonehenge were made in the early days of radiocarbon dating before its accuracy was supposedly improved. Several dates came from contexts whose integrity is not necessarily secure and “present problems in both archaeological and radiocarbon terms.” Does this statement suggest in any way that radiocarbon dating at Stonehenge is a precise enough tool to fix its chronology? Yet when one reads the general and other literature about the radiocarbon dating of this monument none of these qualifying/negating statements regarding radiocarbon’s accuracy is presented. Anyone who reads that literature will come away with the false impression that the monument’s date is unquestionably known.

But one must ask: what is the scientific evidence that corroborates these radiocarbon dates? The corroboration of radiocarbon dates is circular reasoning: radiocarbon dates are corroborated by radiocarbon dates. Now throughout the above-cited Appendix great care is taken with these dates, and the possible accuracy of them is cautiously discussed and analyzed. However, at no time is there ever presented another scientific dating method to corroborate radiocarbon dates. All we have are “enshrined,” “not necessarily secure,” radiocarbon findings having “a number of problems in archaeological terms” as support.

Beyond this is the matter of the way in which radiocarbon dates are reported or rather misreported. J. Terasmae offers this provocative statement of how this is done:

“The user of radiocarbon dates…has a responsibility in helping to avoid possible confusion in respect to published dates…laboratories normally [but not always] provide adequate background information (sample preparation [data], age calculations [spreads], corrections [that are made for these age calculations] for radiocarbon dates reported to the user, particularly when special procedures have been involved. However, many users [who then publish these dates] commonly omit the rather essential supporting [qualifying] data, provided by the laboratory. When they publish radiocarbon dates, a ‘third generation’ user [who writes about

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these same radiocarbon dates in other publications]...has no way of knowing what qualifying statements were made by the laboratory initially.”

A further example of the way in which radiocarbon dates are misreported is related to the standard deviations used. Peter James et al. explain what these standard deviations are:

“...it is not possible in practice to give precise radiocarbon dates because of uncertainties involved in the measurement of samples. Therefore Carbon-14 dates are always quoted with a ‘Standard Deviation’ which represents their degree of accuracy. For example, a date of 1000 bp (before present) with a Standard Deviation of fifty years has a 68.3 percent chance (one Standard Deviation) of lying between 950 and 1050 bp, 95.3 percent chance (two Standard Deviations) of lying between 900 and 1100 bp, and a 99.8 percent chance (three Standard Deviations) of lying between 850 and 1150 bp.”

Alasdair Whittle, in his chapter on chronology for prehistory titled: “Fighting the Clock,” points out:

“By far the greatest majority of dates have been quoted, used and interpreted at one standard deviation....It remains, however, a simple truth that if the error [range] is quoted only at one standard deviation, one in three dates will not lie in the time span thus expressed...Everyone knows this but rather few people discuss it...One needs, however, either to express optimism about a two-thirds chance of being correct or pessimism about the one-third chance of being wrong. If pessimism wins, then determinations must be expressed at two standard deviations. With routine precision (say ± 70 to 100 years), this will usually involve a span of 280 to 400 years for a conventional [radiocarbon dating process] or an accelerator [AMS] determination [which is more accurate], at the 95 per cent level of confidence.”

The reader who sees a date, say, of 2500 B.P. (± 100 years) is given the impression that radiocarbon has dated that sample to either 2400 or 2600 B.P. which looks very impressive since the variation one way or the other for our 1000 B.P. date spans only 100 years. But what is kept hidden from the reader who comes across this date is that there is a one-third chance or one out of three chances of that date being incorrect. Would an investor buy stock in a company that had a

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7 James et al., *op. cit.*, p. 322
one-third chance of never paying him interest and even having a one-third chance of falling in price? That is the probability of published radiocarbon dates and yet these dates are presented as if they were secure.

A further aspect of radiocarbon dating rarely discussed in the literature is that the researcher submitting a sample to a laboratory will be asked by that laboratory what is the date he expects to find. This contradicts Renfrew’s assertion, above, that the chronology of prehistory and even history is “free of any assumption”. Once one assumes a date to be found, the whole concept becomes flawed by this assumption! Sheridan Bowman fully admits to this:

“A radiocarbon laboratory will also ask what is the expected age of the sample. This is not cheating! There are two reasons for asking. The primary one is to ensure no ‘memory effect’ [of other earlier samples that might contaminate the outcome] in the processing of a sample: laboratories endeavour to avoid [this kind of] cross-contamination, but any small effect will be negligible if samples of similar age are processed in a sequence; in particular, samples of substantial age…must not follow modern ones. The second reason is to avoid dating samples where radiocarbon will be of little help [because they are so old as to be inappropriate for radiocarbon finding a date] unless the age is a complete unknown.”

But if there is a “memory” effect” from one sample that contaminates the next and this is taken into account, then that same “memory effect” will make the several tests on the new sample affect all the subsequent dates of that new sample. How does one know that these subsequent dates are accurate and not an artifact of that same “memory effect”? One, of course, doesn’t know this. The very method of having the submitter of a sample give the date expected for it contaminates the entire process and is cheating.

The specific answer to Bowman’s position is given by Terasmae who pointedly states that deciding between radiocarbon dates that are unacceptable from those that

“have been adopted as ‘acceptable’ by users means that there is a need for a discussion of problems concerning the radiocarbon dating method and that the users should be encouraged to explain why some radiocarbon dates are rejected whereas others are considered acceptable. One might well wonder whether the

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9 Sheridan Bowman, Radiocarbon Dating (Berkeley CA 1990), p. 55
saying [that applies to this issue means] ‘my mind is made up, please do not confuse me with the facts.’”

For example L.S. Klein, who reviewed the work of Vladimir Miloicic on radiocarbon dating, writes:

“Miloicic suggests to cease the tendentious “critical” editing of the radiocarbon datings, which is constantly done by the physicists, and calls upon their patrons the archaeologists to do away with the “critical” censorship that axes the publication of the complete result [if it contradicts the established chronology]. He appeals to both physicists and archaeologists to publish all of the results of their research without filtering out the dates that strike them as improbable. He also tries to convince the archaeologists to stop the practice of familiarizing the physicists with the age of the finding, and not giving them any figures until they publish theirs! Otherwise, after such editing, which reflects the private viewpoints of the researchers themselves, the dating is bound to be subjective…”

Lynn E. Rose has constantly called for the publication of all radiocarbon dates found, but those who employ this methodology have “their minds made up,” their “assumed” date must be “correct”, therefore, it is permissible to ignore any “qualifications” to their dates found by radiocarbon laboratories. They then publish only those dates with only one standard deviation to make them appear relatively secure, and they withhold from the public all these culling procedures and especially the dates that contradict their assumptions. The entire scheme under these conditions becomes a form of scientific fraud; they are indeed rigging the game!

This is what the issue comes down to: when the user submits a specimen to a radiocarbon laboratory and tells the laboratory what date is expected, the laboratory may nevertheless find dates that disagree with those expected. These, however, the user, on his or her own, can and does reject! But he/she publishes the “acceptable” dates and withholds the evidence that other dates were “unacceptable” and “rejected”. Failure to give the reasons in the published reports for making these determinations and then not allowing the reader to understand, let alone know these determinations were made, is incompatible with good open science and good scholarship. Withholding contradictory evidence, just as in a court of law, is manipulative. The accusation we made that this behaviour is “cheating”, no matter what lamentable reasons/excuses are presented to deny it, stands!

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10 Terasmae, op.cit., p. 2
Let us for the moment assume that the sample dated by radiocarbon might be fairly accurate. What is the corroboration for that date by other scientific dating processes? One answer is dendrochronology which can both corroborate and calibrate such a date. Whittle, however, denies this for prehistory:

“Chronology is now very mixed in quality across our area. Dendrochronology [supposedly] offers astonishingly good detail, but it is not without sampling problems, is restricted geographically mainly to the Alpine foreland, and is likely to remain restricted to settlement sites on lakes and bogs. Routine radiocarbon dating can be seen by comparison with dendrochronology and high-precision radiocarbon dating to be less exact than it had been hoped, though it is still essential [for producing an accurate chronology]. More could be made of the possibilities of typology [the shapes of pottery, axes and other artefacts to be used to organize the chronology by their shapes]. No single chronological perspective on the past is therefore possible.”

We will deal with pottery and artifact typology in the next chapter. A further important point is made by Ralph Ellis respecting radiocarbon dating and stratigraphy:

“One of the major hazards in radiocarbon dating is establishing that the artefact is contemporary to the site that is being excavated…This is relatively easy if the site has layer upon layer of occupation, as this stratigraphy can positively separate each artefact into a known sequence and thus into a rough chronology [with the older specimen toward the bottom, the more recent toward the top]. Any rogue C14 dates would then be easy to spot…

“Stonehenge [and most prehistoric sites across Europe] is not that sort of an archaeological site; there is no stratigraphy. Indeed, it would almost appear as if these monuments were designed so that nothing would directly give away the date of construction.”

Whittle sums up the situation with the following statement as to the efficacy of radiocarbon dating by itself of evidence dealing with the chronology of the prehistoric world:

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12 Whittle, op.cit., p. 194
13 Ralph Ellis, Thoth: Architect of the Universe (Cheshire, UK 2001), pp. 193-194
“The ability of radiocarbon dating on its own to solve [chronological] problems is now doubtful, as the preceding discussion has sought to show, and it is no use pretending, like the emperor, to be clad in sumptuous clothes that we do not possess.”\(^\text{14}\)

What is Whittle saying? He is emphatically admitting that radiocarbon dates “on their own” are “doubtful and it is no use pretending” otherwise. Not only is the radiocarbon dating process riddled with assumptions and problems but the corroboration from other dating methods for prehistory such as dendrochronology and stratigraphy cannot be used to do just that. And yet, once again, has the reading public been made aware of this? Can one find this admission/confession in all or even most of the general literature? No! In fact when one goes through that literature one will instead find statements that the radiocarbon methodology has dated this site or that site and appears trustworthy. The radiocarbon “revolution” as we have repeatedly shown in the previous volumes for the Ancient Near East, as in this for dating the Megalithic Age, is extraordinarily doubtful.

What is of utmost importance in radiocarbon dating Stonehenge and the other megalithic sites is the very material itself that was used to date them. Mark Kidger reports:

“…by measuring the amount of carbon-14 in a pick made of deer antler that was used to dig one of the ditches at Stonehenge, we can arrive at a reasonably precise estimate of when the ditch was dug. Of course, what we are really measuring is how long it has been since the deer died, but we can assume that the antler was recovered from the deer fairly promptly and then fashioned into a pick—and after [being buried] five thousand years [radiocarbon dated], a few years one way or another are all but meaningless.”\(^\text{15}\)

This analysis contains a gaping flaw in logic. If an antler can survive for 5000 years, the antler need not date as closely as assumed, with the digging of that ditch. The deer may have died 5000 years ago but the ditch from which it was retrieved could have, as our Saxon age date for Stonehenge requires, been dug 1500 years ago. Kidger significantly adds:

“…even though the stones themselves cannot be dated…the tools that were used to shape and put them in place sometimes can. In dating Stonehenge [and

\(^{14}\) Whittle, *op.cit.*, p. 22
other Megalithic Age sites, researchers tested a variety of samples of bone and antler taken from different places around the site…

“…it must be stressed that the dating of Stonehenge necessarily entails a great deal of speculation, with the result that several rather disparate chronologies have been proposed. Even [the] English Heritage [Society] refers to two completely different sets of dates in the material they provide about the site…

“Unfortunately, efforts at radiocarbon dating are not always successful. In some cases, no organic material is available for analysis. In others, the results may be inconsistent and discordant, either owing to contamination of the sample or because materials dating from different periods have intermingled. Or there may simply not be enough material to produce a definitive result. Not all samples yield meaningful conclusions when analyzed, and dates are regarded [but not proved] as reliable only when several samples from the same location agree among themselves.”  

In spite of these cautionary considerations, prehistorians and archaeologists generally accept these dates for the various construction phases of Stonehenge. Chippindale points to this overall agreement:

“The Stonehenge chronology at 1993 depended on sixteen radiocarbon determinations of which six now appear unreliable for technical reasons. So a new carbon-dating programme determined the ages of materials, mostly antler and bone, whose contexts at Stonehenge were reliably known, to make a corpus of sixty-four trusty determinations mostly of exceptionally high standards of precision.

“By this work the [original] four-phase division of the [building] sequence, Stonehenge I-IV is reduced to three.”  

Not only is bone and antler employed at the various sites to radiocarbon date them, but so too are the shells of clams, snails and in certain instances whalebone. These can be seriously affected by the marine environment and/or rain water, as Bowman explains.

“…carved whalebone plaque…purported to be of medieval Spanish origin…was radiocarbon dated to 1480 ± 80 BP [well after the medieval Spanish period]. However, the marine effect [caused by ocean water contamination] means that at death the whale itself would have had an apparent age of several centuries. The true

16 Ibid., pp. 14-15
17 Chippindale, op.cit., pp. 220-221
age of the object cannot therefore be accurately assessed, though the radiocarbon result is sufficient to demonstrate that it is of some antiquity, rather than modern.”\(^{18}\)

This “marine effect” will not only affect whalebone, dating it several centuries or more other than it actually is; it will do the same to marine fish bones, clams and other shelled creatures. With respect to the bones of animals on land buried in soil, and fresh water fish, clams and snails, we encounter a similar problem. Vast areas of southern England, such as that at Stonehenge, are underlain by chalk and are referred to as chalklands. The high calcium carbonate [chalk] content in the soil produces another problem for radiocarbon dating known as the ‘hard-water effect’. Calcium carbonate is absorbed by water percolating in the soil, which creates this hard-water condition. People who live in hard-water environments usually resort to buying a mechanism to soften the water by employing salt to remove it. Those who fail to do so experience prematurely corroded metal pipes around taps, baths and showers that at the very least become coated and clogged with a layer of calcium carbonate which is difficult to remove. Any soil laden with percolating chalk which rises in that soil during significant rainfall will affect the radiocarbon date of bones, antlers, shells, etc., lying in it. Bowman outlines the hard-water effect this way:

“Although freshwater shells [as well as bones, antlers] escape the ocean reservoir effects experienced by marine shells, they can suffer another effect: that of hard water…The hard-water effect is so called because it is often associated with the presence of calcium ions resulting from dissolution of infinite-age calcium carbonate [chalk]. However, there can be sources of carbon other than calcium carbonate, such as soil humic [organic] material, soil carbon dioxide and atmospheric carbon dioxide. Furthermore, the activity of the \(^{14}\)C will depend not only on the source of carbon, but on the time elapsed between the carbon uptake by the water and its [subsequent] uptake by a plant or animal. Thus the presence of hardness (calcium ions) coincides with depleted \(^{14}\)C concentration, but the size of the reservoir effect is not directly correlated with the amount of hardness. It affects living organisms such as molluscs and aquatic plants, can account for discrepancies of several centuries and is observed…in fresh water…The term is also applied to the age offset observed for terrestrial shells, for example snail shell, where the organism has been feeding in carbonate-rich areas such as chalkland…

“The hard-water effect is not quantifiable since it is dependent on local factors…The approach taken [to correct radiocarbon dates on such specimens] is similar to that for dating of marine carbonate: assume no change [in the carbon

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\(^{18}\) Bowman, *op.cit.*, p. 24
environment] with time, and evaluate the age offset using recent specimens of the same species from the same locality”.

Bowman and colleagues “assume” the same condition of the environment exists today as it did in the ancient past, and that by seeing what effect the hard water had on the radiocarbon date of a living, modern species one can subtract that date from the ancient one and thus achieve a fairly accurate radiocarbon date. The problem is that the environment has not, as presumed, remained constant. Prior to the invention of radiocarbon dating there were years in which atomic and hydrogen bombs were tested in the atmosphere and ocean. This produced radioactive products, carbon among them, that contaminated the atmosphere and ocean, falling over the land masses with the rain. This amount of radioactive contamination was so great that it affected the radiocarbon taken up by modern living species, making any accurate gauge between them and ancient ones extremely problematic. Bowman readily admits: “Naturally, it is not always possible to locate appropriate pre-bomb specimens.”

What is not discussed is that human beings as well as animals will eat these animals and plants and drink the hard water which will contaminate their bones and teeth as well. Humans and animals through their lives, from the time they suckled their mother’s milk, and then into adulthood, would be taking in and assimilating more dead carbon than can be known, which would give them much greater radiocarbon ages. At death their bones would then be deposited in soils rich in calcium carbonate or other forms of carbon, as Bowman has shown, that would enhance their great age. On the other hand, because many of the bones were cremated prior to burial it can be argued that burnt bones can also be used since these have been found in pits at Stonehenge and elsewhere. Yet Bowman on these points shows:

“By contrast with wood and wood charcoal, the dating of burnt bone is not necessarily more straightforward than the dating of bone. If fact, only in unusual circumstances is burnt bone datable at all by radiocarbon. Collagen [the datable part of the bone] degrades on heating, and in most circumstances of burning of bone, whether accidental in cooking or deliberate in a cremation pyre, the protein fraction of the bone is lost.”

19 Ibid., pp. 25-26
20 Ibid., p. 26
21 Ibid., p. 29
Even if there is still sufficient collagen left to radiocarbon-date a bone, it still is encumbered by the same problems of bone dating outlined above.

When radiocarbon dating of Stonehenge was employed to criticize Velikovsky’s thesis that Stonehenge was built well after prehistory, after 787 B.C., he responded as long ago as April 1967 in the *Yale Scientific Magazine* stating:

“A criterion was offered for determining the age of Stonehenge [via radiocarbon dating]: an antler of a red deer was found under one of the stones and more antlers in the fill holes. But as the Lamont Geological Observatory of Columbia University answered (January 4, 1967) to an inquiry: ‘Antlers and bones are, in general, unreliable for radiocarbon dating.’ Also the Radiocarbon Laboratory of the University of Pennsylvania, in answer to a similar inquiry, let it be known that experience in polar regions proves that antlers are easily contaminated and made to yield invalid dates.”22

The fact of the matter is that the literature is replete with statements that antler and bone are extraordinarily unreliable for providing valid radiocarbon dates, and, importantly, numerous authorities say they should not be used as such. As late as 1999 Harry E. Gove owned up to that fact:

“The radiocarbon dating of bones…raises the question of the credibility of radiocarbon measurement…After all, is it not possible that bones buried in the ground for thousands of years can be contaminated with extraneous carbonaceous material? In his article in the [Willard] Libby retrospective Haynes discusses the carbon dating of bones…He expressed some reservations with the results. The problem with bones, as opposed to charcoal, is to be sure that the carbonaceous material was actually present in the bone when the animal or human died and not from organic or inorganic carbon-containing compounds incorporated into the bone at much later times. Haynes was not alone among archaeologists and anthropologists in his distrust of bone dates…”

“The vexing question of how to date old bones has received renewed attention with the advent of AMS [Accelerator Mass Spectrometry which allows the radiocarbon atoms in the sample to be counted]. Human and animal bone is complex. One of the experts in the radiocarbon dating of bones, R.E. Taylor…has studied the intricacies of dating old bone for many years. He notes that carbon-containing compounds in bone tissue exist in two forms: organic and inorganic.

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The principal indigenous carbon-containing inorganic constituent exists in the form of a carbonate, Apatite. In the case of fossil bones the latter can be mixed with calcium carbonate from ground water sources deposited in the bone matrix and the original organic collagen can be complexed [united] with various fulvic and humic organic [carbon] compounds from the soil in which it was buried.

“The advent of AMS has made it possible to separately radiocarbon date various organic products contained in fossil bone. Taylor states that there now appears to be consensus [but not proof] among investigators concerning the reliability of bone radiocarbon values. Where appropriate biochemical purification procedures are employed, accurate radiocarbon ages can be obtained on bone collagen in cases where the bones retain a significant amount of collagen…Taylor…concluded ‘All investigators agree that bone can be a very difficult sample type with which we work—requiring great attention to detail in sample pre-treatment and preparation…An important goal of such studies will be to develop a preparative methodology [which does not presently exist] which would be able to identify organics indigenous to the bone irrespective of its diagenetic [burial] history.’”

Because the preparatory methods necessary for removing all contaminants from bone collagen is not fully developed or understood one cannot know if the bone or antler in question still contains significant contamination. This is the essence of Taylor’s remarks. In fact, as late as 1987 Taylor had stated that “bone and antler were to be avoided” for radiocarbon dating. Zvi Goffer in 2007 shows:

“Under particular conditions the diagenesis of bone, teeth, or ivory interred for extremely long periods of time [can] be entirely altered chemically or totally dissolved.”

Chippindale’s contention that he has 64 radiocarbon dates for Stonehenge “of exceptionally high standards of precision” “mostly from bone and antler” is not proof that these are valid dates and he never claims this is “proof,” but is an “exceptionally high standard…of precision” for them. He cannot give more than that as evidence. He is only saying these dates are “trust[ed]”, not proved.

Various approaches to get around this problem have been suggested, particularly for bone, antlers, etc., found in arid conditions. Andrea Parmeggiani and Christoph F. Schmidt report:

23 Harry E. Gove, *From Hiroshima to the Iceman* (London 1999), pp. 95-96
“Bone and Antler: Bones were considered as problematic material for 14C dating for a fairly long time…[Because of these problems] age determination is unreliable on the organic fraction. However under favourable dry conditions carbonate hydroxyapatite may yield reliable 14C ages…”

Notice that under “favourable dry conditions” a compound of bone “MAY yield reliable 14C ages.” They do not yield reliable 14C ages but “may yield”. But we do know that the British Isles, Ireland and most of Europe do not enjoy “favorable dry conditions.” These regions experience rainfall regularly which soaks into the soil enhancing the movement of carbonate ions and allowing for the contamination of the bones, antlers etc. John M. Coles has written about the suitability of various materials for gathering radiocarbon ages in the descending order of their reliability and maintains bone and antler are the least reliable:

“Material and quality: Wood, charcoal, peat, leather and hair, marine shells and [last of all] bone and antler, are suitable for [radiocarbon] dating in decreasing order of reliability.”

D.P. Agrawal and Sheela Kusumgar echo this statement, claiming that because of “ground water carbonates…” bone and antler, “are therefore least reliable for dating.” But in discussions of the chronology of Stonehenge and the Megalithic Age in the general literature there is no clarion calling out to the public that the major materials for establishing that chronology are the least reliable.

In an article in *Science* by Christopher Bronk Ramsey *et al.* who employed “short-lived plant remains from museum collections (e.g., seeds, basketry, plant-based textiles, plant stems, fruits) that were associated with particular reigns or short sections of the historical chronology” to date Egyptian chronology, the authors admit “the possibility remains that some cases of contamination may have escaped detection.” After careful analysis they maintain that the established chronology is generally sound. The problem is that they still date the Middle Kingdom to the end of the second millennium B.C.; this is completely contradicted

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28 D.P. Agrawal and Sheela Kusumgar, *Prehistoric Chronology and radiocarbon dating in India* (New Delhi, India 1972) p. 12
29 Christopher Bronk Ramsey *et al.*, “Radiocarbon-Based Chronology for Dynastic Egypt,” *Science* (June 18, 2010), pp. 1554-1555
by the astronomical work of Lynn E. Rose, described in volumes I and III of this series, which is accurate to this day. Astronomical dating is a dating mechanism and no one, we repeat, no one has refuted Rose’s dates for the 12th Dynasty which move it closer to the present by 1477 years. Until the Queen of the Sciences is refuted, the evidence these researchers presented proves nothing, particularly in view of all the other scientific and technological evidence that correlates, corroborates and is congruent with Rose’s dates especially relative to agronomy in the Fayum Basin where salinization makes irrigation agriculture impossible over 1500 years.30

But how accurate is the dating of short-lived plants in the Nile Valley? What is not mentioned by these researchers is that the Nile River contains lots of hard water compounds with carbonates as well as carbon-dioxide. The carbonates are described by Neil Cumberlidge, speaking of the Nile’s sources:

“…in southwestern Sudan between the Albert Nile and the White Nile where the river spreads out to form the great open wetland of the Sudd, comprising standing waters and flooded land dominated by papyrus and grass swamps…the lentic waters of this vast swamp have high rates of organic decomposition and persistently low oxygen levels [and] high carbon dioxide levels…31

In the Geological Survey (US) Bulletin Issues 330-331 (Washington, D.C., 1908), page 82, a survey was published that had been made along three points, namely: the White Nile near Khartoum, the Blue Nile, and the lower Nile below Cairo, to measure carbon dioxide levels in the water. It found carbon dioxide to be the largest dissolved gas running from 49.97 to 36.02 percent of all other gases. On the same page, the article points out: “All such water contained dissolved gases, especially oxygen, nitrogen and carbon dioxide. These, of course, enter the roots during the flood stage in the water.” The Science Reporter, vol. 23 (Delhi, India 1986), p. 167 points out:

“Keeley et al. have recently recorded (Nature 310:694) such a strategy of CO2 absorption by roots… Apparently the plants which take up very little CO2 through their leaves could absorb remarkable amounts of carbon via roots. This dead carbon is absorbed and causes these short-lived plants to date much older than their actual age. But this problem is tacitly admitted when these researchers say “the possibility remains that some cases of contamination may have escaped detection.”

How much is some? This we are not told because it is simply not known. “Some” could have been any number of their samples, but with these problems they have supposedly proved radiocarbon dating of ancient Egyptian civilization is valid. Furthermore, we have shown elsewhere in volume I that whenever short-lived materials from ancient Egypt contradict their chronology, these contradictions are brushed aside with ad hoc excuses.

The fact of the matter is that the only radiocarbon dates acceptable to the establishment historians and archaeologists are those that agree or generally agree with the chronology that has guided their research. What would they do if radiocarbon dating was shown to contradict a chronology that had already been accepted by the entire profession? Would they accept it or reject it? David Henige presents just this situation:

“Historians have availed themselves of the results of radiocarbon dating for more than half a century. The greatest appeal lies in its ability to provide ever narrower ranges of probability for the relicts of events that might otherwise be undatable. Radiocarbon dating is popular in efforts to confirm, or make more precise, dates already reasonably [not precisely in a scientific sense] well documented in the historical record.…

“This happened often enough that eventually it was concluded that there were localized variations in the half-life of carbon, as well as variations over time. While this has helped reduce some contradictions, it remains to be seen where the next soft spot turns up.”

But that soft spot or contradiction turned up when Lynn E. Rose astronomically dated the 12th Dynasty 1477 years closer to the present. Henige then shows how historians respond to radiocarbon dates that contradict the established chronology:

“The agitation that can result when physical and written evidence clash is particularly evident in the case of early Iceland. According to such reputedly unimpeachable sources as the Islendingabók and the Landnámabók, the first permanent settlers arrived in Iceland from Norway in 870/74 [AD], a date which for Icelanders is as important as 1066 to the English, 1492 to (some) Americans, or 753 BCE to the Romans. In 1966 Kristján Eldjárn, the state antiquarian [historian] (and later President) of Iceland, decried efforts to ascribe certain archaeological remains to Celts as ‘weird,’ arguing that archaeological evidence validated the story of a relatively sudden and quick settlement of the island by the

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32 David Henige, *Historical Evidence and Argument* (Madison WI 2005), p. 130
Norse beginning ca. 870 [A.D.]. Archaeology and literature were in harmony. Eldjárn’s views summed up well the prevailing feeling about the relationship of the two forms of evidence in this case; the first was expected to support the second.

“There was some consternation then when a series of radiocarbon dates was derived in the 1970s that suggested that parts of Iceland had been settled as early as ca. 700 [A.D.]. It is no surprise that this dating was strongly resented and resisted. One line of defence was to postulate a ‘local depression…in atmospheric carbon dioxide over Iceland,’ which would imply that ‘all samples from Iceland would give apparently too high an age.’ Ingrid Olsson, its proponent, had done work on early dynastic Egypt, where this actually was the case and so the theory, if a bit circular, was not entirely implausible. Unlike Egypt though, it was not required by a mass of interconnected historical evidence.

“Other evidence soon undercut Olsson’s expedient, but this had no effect on national [and historical] sentiment, which rates written sources too highly to be overthrown by a set of geochronological data. The reaction in Icelandic scientific circles was to forgo using $^{14}C$ dating at all, while the excavator of the site in question even declined to include the disputed results in the final report [just as Velikovsky’s dates reported by the British Museum]. Margrét Hermanns-Audardóttir, however, accepted the apparent evidence of the $^{14}C$ dating. Her published argument was accompanied by a number of critical comments, most of which faulted her for arrogating science above literature, but also criticized her use of non-Icelandic written sources.”

Here it is obvious that both the historians and even the scientists could not stomach the findings of radiocarbon which undercut a major thesis of their chronology. The irony is that some of them claimed literary documentation was superior to scientific evidence—in this case radiocarbon dating. But that was the end of this controversy, as Henige shows:

“V.O. Vilhjálmssson was critical of early efforts to correlate archaeology with written documentation, and was dubious of Olsson’s expedient, but preferred to reject the 700 CE date on various grounds, including the possibility of contaminated samples [such as those not dismissed by the researchers of the Science article on Egyptian chronology]. Adolph Fridriksson agreed that Hermanns-Audardóttir’s conclusions ‘must be dismissed entirely’ on the grounds that she had not met the standards required for overthrowing an existing orthodoxy—which apparently are loftier than those required to establish it in the first place.

33 Ibid., pp. 131-132 (emphasis added)
“This controversy prompted Páll Theodórsson to undertake a thorough investigation of the issues. He looked at three possible reasons for rejecting the dates—laboratory bias, initial age of samples, and locally concentrated depressions—and concluded that there are insufficient grounds for rejecting the testimony of the dated samples, and no grounds at all for rejecting them out of hand. In summing up, Theodórsson cast doubt on the evidence of the *Islendingabók*, pointing out that it was written only in the twelfth century and was based entirely on ‘folklore.’ He laments that Icelandic archaeology is ‘in deadlock,’ but he does not foresee a quick or painless resolution.

“Why this strong preference for a problematical written source against a growing body of scientific evidence? One reason is that geochronological dating is less secure for Iceland than for most other places: the lack of old forest growth rules out dendrochronology; Iceland’s low-maintenance society left fewer artefacts behind; and constant volcanic activity introduces troublesome variables. A second reason relates to the national [historical] ethos. The new dating strikes at the very core of this almost reverential attitude, with the result that…Icelandic society [historians and scientists included] firmly rejects the opportunity to be of greater antiquity than it had believed it was.”

The unpleasant truth is that radiocarbon dating only plays a crucial role for historians and archaeologists when it supports their revered and long held chronology. When scientific, technological, linguistic and other forms of evidence sweep away that pillar of support they behave just like any other insular group who cannot face such cold, clear-cut evidence. We do not agree or disagree with this Icelandic evidence since, as we have shown repeatedly in these volumes, radiocarbon dating is not used as a scientific tool but only as a support for a preferred chronology.

—Ibid., p. 132
CHAPTER 3

TYPOLOGY AND POTTERY DATING PREHISTORY: SUCCESS OR FAILURE

Whittle has told us above that “the ability of radiocarbon dating on its own to solve [the chronological question] is now doubtful…” Therefore, some other form of evidence should correlate with and corroborate the radiocarbon process to validate it. The primary technique employed by prehistorians to do this is to organize the various pottery styles, forms, designs, etc., into a sequential order just as was done in ordering the chronology of the ancient Near East. Although this is not scientific corroboration, it is nevertheless accepted and assumed that it can do just that. In the established chronology these pottery finds date from around 4000 to 1500 B.C. covering roughly 3000 to 2500 years. In the short chronology, we propose these pottery finds date from around A.D. 450/500 to 850/900, covering roughly 400 to 450 years. If the established chronology is correct, there will be very little overlay of styles spanning this long period, and certainly this would never allow for nearly all the different pottery styles, forms and designs to be found at one site. If the short chronology we present is correct, there will be a great deal of overlap of many of these different pottery styles, forms and designs spanning this relatively short period, and it would be expected that nearly all of these, or many of these, could be excavated at one site.

This type of approach to prehistoric chronology was outlined long ago by Oscar Montelius in 1899 in a paper titled “Prehistoric Chronology.” He explains that:

“The determination of a chronology is of the greatest importance for prehistoric research. Chronology may be (a) relative determining only the succession of several periods; or (b) absolute giving a date B.C. or A.D. for each period of a series.

“A relative chronology is possible for every age and every country, if only we have a sufficient number of good finds. A ‘good find’ is composed of antiquities found together in such a way that they can have been placed there at the same moment…”
“When a great number of good finds have been made in different countries, it is possible to divide the course of prehistoric civilization of each country into many periods. The succession of these periods is proved either by the relative position of graves or by the typological evolution of their contents. Typological series were illustrated...showing [a sequence of] (1) axes of stone, copper, bronze and iron from Italy and from Central and Northern Europe; (2) fibulae from Italy and Scandinavia; (3) sword hilts in Scandinavia; (4) bronze vessels in Scandinavia, etc. Other diagrams exhibited the most characteristic type for the Copper Age and for each period of the Bronze Age and Iron Age in Central and Northern Italy, in Central Europe and France, in Great Britain and Ireland, in Northern Germany and in Scandinavia.

“The various types which are characteristic of any one period are very often met within the same find; but types belonging to different periods are seldom found together. If however the types of any two periods are occasionally found together these two periods are immediately successive in the series. Exceptions to this rule are remarkably rare, if not altogether absent. This proves that each period must represent a considerable length of time; for if the time had been short, the remains from the different periods ought to have been much more confused than is found to be the case.”

This is precisely what we found regarding pottery dating in the ancient Near East in volume I of Pillars of the Past, Chapters 5 and 6, wherein we contended that the chronology was shorter by about 2000 years. At one supposed Pre-Dynastic Egyptian site John Dayton described that “beads [were found] from the 6th Dynasty, dating from 2345-2181 B.C., from the 12th Dynasty, supposedly dating from 1991-1782 B.C., from the 18th Dynasty, dating from 1570-1293 B.C., and from the 26th Dynasty, dating from 664-525 B.C.”

“Beads, like pottery [or other artefacts], can be identified by their size, shape, style, etc., as organized by the archaeologists. Yet here we have, in supposedly pre-dynastic to around early dynastic times, sites with artefacts that came from four different dynasties of the Old, Middle and New Kingdoms.”

We further went on to show of Dayton in Minerals, Metals, Glazing and Man, that:

“In Chapter 16 he presents similar evidence related to “Egyptian Ist Dynasty Pottery.” In Chapter 17, he does the same for “The Egyptian Middle Kingdom;”

As Montelius explained, when the time period is “short, the remains from different periods ought to [be] much more confused,” that is, mixed. This is exactly what Dayton found and what we will present in this chapter with regard to prehistoric pottery, namely that the pottery dating of prehistoric Europe also indicates it is extremely short compared with that of the lengthy established chronology. Also, as in the Near East, the established chronology created Dark Ages, in which whole peoples and their buildings, graves and artifacts disappeared. In some instances these Dark Ages were totally or almost completely lacking in people, buildings, graves, and artifacts as well as documents, such as the “Greek Dark Age,” ca. 1250-650 B.C., and were shown to be “phantom centuries” created by historians and archaeologists. In other instances, these Dark Ages were totally or almost completely lacking in people, buildings, graves, and artifacts as well as documents, not because they were “phantom centuries,” but because all the evidence of a well-known people was given to a totally unknown people. For example, the well-known Chaldean period became a Dark Age when all the materials in it were given to the unknown and historically unmentioned Sumerians; the Medish period became a “Dark Age,” called a “Black Hole,” when all its materials were given to the unknown and unmentioned Mitanni. In the later chapters of this book we will show that these prehistoric Megalithic Age peoples were, in fact, the post-Roman peoples of the Anglo-Saxon period, ca. A.D. 450-900. That is, when all the materials of the Anglo-Saxons from A.D. 450 to 900 were given to the prehistoric Britons of the Late Neolithic, Bronze and Early Iron Ages, the Anglo-Saxons vanished from the earth and could not be found, or barely found, in the archaeological record.

In the same way that the historians and archaeologists organized the pottery shapes, styles, designs, etc., to fit the chronology of the ancient Near-East, prehistorians have done just that for pottery shapes, styles, designs, etc., for the Late Neolithic Bronze and early Iron Ages. Rodney Castleden states:

5 Ginenthal, *op. cit.*, p. 191
“Archaeologists [of Stonehenge] have traditionally been preoccupied with stones and potsherds, the solid finds [of these] have to be the starting point and…all other [chronological] inferences are ultimately based upon them.”

Andrew Jones elucidates this sequential pottery dating chronology:

“The typological classification of pottery remains the primary tool for archaeologists seeking to understand the chronology of a given site. Pots have consistently been employed by archaeologists as a fine-grained indicator of the presence of specific cultural groups. Archaeologically pots appear to equal people.”

The problems related to pottery dating of prehistoric Europe arose after Montelius wrote in 1899 as more and more sites were excavated and the pottery found undermined that chronology. According to John Hunter and Ian Ralston:

“Ever since Lord Abercromby’s encyclopaedic study of Bronze Age urns early [in the 20th] century, pottery analysis has dominated archaeological research into this period. Subsequent compendia of [prehistoric] Beaker pots…, Collared Urns…, northern Food Vessels and a mass of regional studies have investigated issues of [pottery] typology, chronology, decorative variation, regionality, production, distribution, status and deposition. In many respects, the aim of establishing a finely tuned ceramic chronology…for the Earlier Bronze Age has not been realized. Even the seven-step sequence [of Peterborough Ware to Grooved Ware to Beakers to Food Vessels to Collared Urns to Trexister Ware to Biconical Urns to Deverel-Rimbury] proposed in the early 1970s for Beaker decoration, and adopted by some archaeologists, has been undermined…”

While we will concentrate attention on the British Isles, the broad spread of these pottery forms is remarkable. According to Richard Bradley:

“Bell Beakers…distribution is truly international and extends from Denmark to North Africa. They are found as far east as Hungary and as far west as Portugal.”

The various forms of Beaker pottery found on the Continent are also found in different sites in Britain and there are problems related to how these different styles

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8 John Hunter and Ian Raiston, *The Archaeology of Britain: an Introduction from the Upper Paleolithic to the Industrial Revolution* (London 1999), pp. 80-81
came to specific areas of Britain but apparently not to others nearby or between sites where similar types of Beakers exist. After discussing these problems and how to organize Beaker pottery styles into the conventional chronology, Bradley shows that:

“...there are both empirical and theoretical problems to address. A practical difficulty concerns Beaker chronology in Britain and Ireland. David Clarke (1970) had postulated a series of migrations linking specific parts of Continental Europe to particular parts of these islands, identifying such links [mainly] on the basis of pottery types and...associations [with other artifacts]...Continental scholars disputed some of the long-distance connections on which his interpretation was based and proposed a simpler scheme. The British and Irish sequence [compared to that of continental Europe] had apparently moved in parallel with well-documented [Beaker] developments in The Netherlands…

In order to evaluate these connections of Britain and Ireland chronologically with Europe, a radiocarbon survey was undertaken. The astonishing finding to come out of this program was that it found that radiocarbon dating did not correlate with the pottery dating. Bradley informs us of this:

“It remained to test these ideas by a programme of radiocarbon dating. This was undertaken by the British Museum in the late 1980s (Kinnes et al. 1991). The results were unexpected, for they did not support any of the existing [chronological] schemes. They suggested that there was little evidence for a succession [over time] of different [Beaker pottery] types, even when it was indicated by artefact associations. Some of the dates were exceptionally late [that is far closer to the present than the established chronology allowed]. This left students of the period in a quandary. Most executed the difficult [mental] manoeuvre of rejecting the validity of any Beaker chronology yet [at the same time] ascribing their material to styles which presuppose such [an unchanged chronological] sequence.”

This crucial test that was undertaken to correlate and corroborate the established chronology, wherein radiocarbon dating and pottery sequence dating would confirm one another, failed utterly. The pottery sequences could not be correlated with the radiocarbon ages for these sites on the Continent, in Britain and Ireland. The radiocarbon ages of these sites on the Continent, in Britain and Ireland could not be correlated by their pottery styles. The contention of the professionals that there were direct linkages and close interconnections between pottery sequence dating and radiocarbon dating was a failure. In spite of this failure the researchers

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10 Ibid., p. 144
11 Ibid.
were unwilling to accept the verdict of their own evidence. While rejecting Beakers as a methodology by which prehistoric times could be organized, they still maintained that the same conventional chronology was to be employed for understanding and organizing that chronology. They rejected the pottery evidence that was one of the foundations of their chronological edifice in order to maintain the same edifice. In fact that timeframe had no correlation with the evidence that was supposed to be one of its main pillars. Radiocarbon still had no corroborating, correlation or connection with anything except itself.

In certain instances the prehistorians either accept or reject one of the other forms of dating as the correct approach to chronology. Here Whittle describes that dichotomy:

“One may contrast two recent pronouncements. On the one hand there is the admission that in some cases, as in the Dutch Late Neolithic, traditional [pottery and other artifactual] typochnology has been refined to a greater degree of accuracy than radiocarbon dating, ‘to the degree of detail where the statistical error of the [radiocarbon] measurements starts to interfere’ with the complications of the sequence (Waterbolk, 1983:64). On the other hand it has been boldly stated (Bradley and Gardiner, 1984:2) that ‘radiocarbon dates are always more useful than those derived from [pottery and artifact] studies’…”

With respect to the typology [shape, size, etc.,] used to delineate chronology from pottery and other artifacts, Whittle reports that “there are many who feel that there are lies, damned lies and typology.”

Gavin Lucas asks:

“Why is it that the British Beaker (and indeed other prehistoric pottery) has proven intractable to typological analysis, unlike pottery of later periods?”

If one accepts the European pottery chronology laid down in the early 20th century then the British and Irish pottery sequences are out of step with it as well as are other pottery sequences. If one accepts the pottery chronology of Britain and Ireland as valid then the pottery sequence laid down earlier is out of step. This Lucas maintains has been an “intractable” problem. Martyn Barber shows the entire concept, that typology is an independent methodology for analyzing and

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12 Whittle, *Problems in Neolithic Archaeology*, op.cit., p. 22
13 Ibid., p. 31
14 Gavin Lucas, *Critical Approaches to Fieldwork: Contemporary and Historical Archaeological Practice* (London 2001), pp. 103-104; see also Whittle, op.cit., pp. 31-32
organizing pottery and bronze artifacts into a valid chronological sequence, is based on circular reasoning and is clearly subjective:

“For many involved in such metalwork studies the detailed classification of Bronzes was often regarded as an objective process which provided the foundations for broader social and economic interpretations: ‘…it is the artefacts which are the “stuff,” the “facts” of our discipline and…everything else is inference.’ There seems to be a widespread belief among today’s young (and not so young) prehistorians that methods are an end in themselves, that one can somehow ‘experiment endlessly with methods without [in terms of metallurgy] having a good grounding in basic raw materials, the artefacts’ (Burgess and Coombs, 1979a, 1). The fact that typological classification was itself inference, a subjective method of arranging artefacts according to certain assumptions about what was important in, say, spearhead design and its relationship to technological ability seems not to have been recognized [by the profession]. Interpretation was being seen as something that arose from the results of classification, when in fact an interpretation of the material was itself central to the classification process.”

A.F. Harding writes that, although the materials of the Neolithic Age exhibit “a certain sameness,”

“Where this is not the case, the opposite is often true: the remains are so bafflingly enigmatic that it is hard to see how one can make much progress with understanding them, other than through post-processual approaches. A good example of this would be the study of megalithic tombs, where detailed typological study is a quick route to insanity.”

Post-processualism allows for a wide range of interpretations of archaeological materials as developed by Ian Hodder in reaction to processualism which kept to a strict methodology. This allows for a way out of the confusion of “baffling enigmas” that exist when one sets up a cohesively tight method. It is an attempt to escape the confusion that arises from a rigid set of rules by which one must analyze and interpret prehistoric materials. But this confusion is exactly what Montelius warned his colleagues about in terms of typological studies. If there are few “[e]xceptions to this rule…[t]his proves that each period must represent a considerable length of time; for if the time had been short, the remains from the

15 Martyn Barber, *Bronze and the Bronze Age: Metalwork and Society in Britain c. 2500-800 BC* (Stroud UK 2003), p. 19
16 A.F. Harding, *European Societies in the Bronze Age* (Cambridge UK 2000), pp. 5-6
different periods [would be] much more confused…” And that is precisely what we have.

One aspect of the typological dating method for organizing chronology is not only the connections of artifacts from prehistoric periods with each other but that these can also be verified when there are connections of these to similar materials in ancient Near Eastern cultures. Since it is assumed these civilizations are properly dated, any similarity between late prehistoric Bronze Age artifacts or pots with those of historic societies not only proves the connection but validates the chronology. It is undreamt of that both chronologies could be in error, but this is what was proven based on forensic historical grounds for the ancient civilizations of the Near East in volumes I, II, and III of this series. Nevertheless, if indeed the established chronology for prehistoric Britain, say, was correct, it would be impossible or extraordinarily improbable to find several different types of pottery and artifacts from very different ages and stages of prehistory at one and the same site. We have cited Flinders Petrie in his 1904 book *Methods and Aims in Archaeology* in volume I of this series, page 188 thus:

“Let us suppose some old country mansion, where it had been the habit to close permanently any room in which the owner had died and leave everything in it undisturbed. If we went through such a series of rooms we could not doubt their order of date, if we looked at their contents. The William IV room could not be put to the middle of George III’s reign; the George II room could not be supposed to go between those of James II and Anne. Each room full of furniture would have some links of style with that of the generation before, and of the generation after it, and no doubt could exist as to the sequence of the whole series. What is true of a room full of furniture is equally true of a grave full of pottery.”

Even with a certain amount of overlapping of pottery styles and a few out-of-place pots found together, over a period of thousands of years of prehistory one should not find most of these stylistically differentiated pottery types and other artifacts at one site. However, over a period of 400 to 450 years, which we maintain is the length of the megalithic age ca 450–900 AD, this could be the case and in fact is! Julian Thomas reports this remarkable fact:

“Mount Pleasant, on the outskirts of modern Dorchester…was one of a number of extremely large embanked enclosures or ‘henges’ built at the end of the Neolithic period in Wessex (c. 2000 bc/2500 BC)...While the scale of the excavations

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undertaken at Mount Pleasant was somewhat more modest than at Durrington [Walls], the results were remarkable for a number of reasons. First, although the bank and ditch of the henge were still vaguely discernible on the surface, excavation demonstrated the additional presence of a massive timber palisade, roughly concentric to and located within the earthworks. This palisade enclosure was subsequently dated to the years immediately after 1700 bc (c. 2000 BC), and its presence is related to the second unusual feature of the site, a RICH POTTERY SEQUENCE RUNNING FROM THE NEOLITHIC INTO THE EARLY BRONZE AGE. This in turn gave rise to some debate since THE CERAMIC ASSEMBLAGE APPEARED TO DEMONSTRATE THE CONTEMPORANEITY OF A NUMBER OF DIFFERENT STYLES OF BEAKER POTTERY, AND INDEED OF OTHER TRADITIONS OF BRONZE ARTEFACTS, WHOSE RELATIONSHIP HAD OFTEN BEEN CONSIDERED IN EXCLUSIVELY DEVELOPMENTAL TERMS. This prefigured more recent concern over the [radiocarbon] dating of Beaker ceramics (Kinnes et al. 1991), and requires that some other relationship than a simple chest-of-drawers [or mansion with rooms in which these pottery and artefact types occur] sequence be hypothesised to explain the coexistence of these styles. As Simpson (1968, 202) notes, it is easy to gain an impression of orderly succession in the funerary context: Beakers followed by Food Vessels, followed by Collared Urns and so on. The significance of the Mount Pleasant [pottery and artifact] sequence is that it necessitates a reconsideration of a number of structures and patterns [of chronological development] in the archaeological evidence which have hitherto been assumed to be chronological or developmental in character.\textsuperscript{18}

Of course, Thomas had to get around this obvious contradiction to the established chronology and thus presents a complete revamping of the evidence to make it fit that chronology. That is, since the pottery found at Mount Pleasant contradicts that chronology, the methodology of interpretation applied to it must be wrong and a new one that maintains that chronology must be applied which is assumed to be correct. In his conclusion Thomas gives us the new interpretation that will salvage the established chronology: “…we argued [not scientifically or technologically proved] that the complex interconnections between people, places, substances and artifacts in later Neolithic Britain formed a mobile economy [with widespread trade connections across Europe] which allowed a plurality of identities [of pottery forms] to be sustained without declining into chaos…”\textsuperscript{19}

\textsuperscript{18} Julian Thomas, \textit{Time, Culture and Identity: An Interpretative Archaeology} (London 1996), p. 183 (capitalization added)
\textsuperscript{19} \textit{Ibid.}, p. 232
Note that Thomas suggests that without this reinterpretation of the pottery sequence dating their paradigm would descend into “chaos.” He had to conclude that, supposedly unlike pottery from other times and later ages, prehistoric pottery remained unchanged over as much as a thousand years. That is, people who made pots never changed their style or advanced in their production techniques for about 1000 years and perhaps more.

In an earlier book Thomas presents this hypothesis to escape the contradiction in which prehistorians found themselves:

“Reviewing the radiocarbon evidence for the Beaker pottery which [it is assumed] began to be used in Britain after 2700 BC, Kinnes et al. suggest that ‘the consolidated evidence casts doubt on stylistic succession as the determinant of internal chronology for British Beakers’ (1991, 38;…). It has long been accepted [but not proved] that throughout the Beaker episode, individual decorative motifs survived for long periods eventually being combined with new elements to create new styles of vessel…However, it now seems that particular classes of Beaker, such as Clarke’s (1970) All-Over-Corded [type] and European Bell Beakers continued to be made [in the same style and the same way] over hundreds of years….The most that can be said is that some ‘early’ style Beakers (such as the Wessex [types]) may be associated with early radiocarbon dates, but [because they were produced over several hundred years or a thousand years] might equally be later, while ‘late’ Beakers will tend to be genuinely late.”

How does anyone know if a form of pottery that was made of the same size, style, designs etc., for about, say, 700 to 1000 years dates to the early, middle or later period of that time span? Thomas tells that “‘early’ style Beakers…may be associated with early radiocarbon dates,…but might equally be later, while ‘late’ Beakers will tend to be genuinely late.” Here it is admitted that radiocarbon dates and pottery styles that are ‘early’ might equally be ‘later’ and that later ones TEND to be ‘later.’ Rather than face up to the morass into which they have ensconced themselves with this form of logic, prehistorians continue to assume their chronology has a valid ceramic basis. John C. Barrett more honestly puts the situation thus:

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20 Julian Thomas, Understanding the Neolithic (London 1991), pp. 120-121
“Too much cannot be made of the few radiocarbon dates we have from Durrington, Woodhenge and Stonehenge; we are obviously dealing with a high degree of chronological uncertainty.”

Thomas has cautioned his readers concerning the basic flaw inherent in interpreting pottery styles to determine chronology which clearly even applies to his own interpretation:

“While the study of Neolithic pottery in Britain has always relied upon exhaustive empirical observation, this investigation has inevitably taken place in the context of prevailing modes of interpretation. What we choose to consider as a relevant feature of a pottery vessel is determined to a great extent by our broader conceptions of the [chronological] events and processes which might be illuminated by the artefact. Thus something of the tone of Neolithic ceramic studies was set by Reginald Smith when he studied assemblage from the pits at Peterborough. Smith (1910, 346) compared the impressed cord decoration [placed on the surface of the pottery] and oval indentations on vessels from Peterborough and Mortlake with Neolithic pottery from Finland and Sweden. Moreover, he examined the similarities between these pots and the Food Vessels of the Bronze Age in terms of continuity of tradition amongst a native population driven into the west of Britain, and Ireland, by invaders from [Scandinavia from] across the North Sea (1910, 351). Evidently, Smith’s priorities for analysis must be understood within the emerging understanding that artefact styles might be correlated with ethnic entities in the past [and others], and that the distributions of objects could provide an indication of folk movements in prehistory…The point here is not to berate Smith for presenting an account of Neolithic pottery which was driven by naïve conceptions of population movements in the past. Rather, it is to suggest that the study of this material has always been guided by implicit or explicit theoretical concerns. Changing the theoretical framework, we achieve a different and perhaps fresh interpretation, although this may be no more impartial or objective.”

But that fresh, different interpretation respecting this material vis-à-vis its chronological place or even the length of time it was used in the past has simply not dawned upon him or his colleagues. In a sense, prehistorians are trapped in a chronological morass of their own making devoid of a scientific foundation. Whittle, citing Cahen and Gilot (1983), describes the prehistorians’ paradigmatic paralysis in the starkest of terms:

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22 Thomas, *op. cit.*, pp. 89-90
“On the contrary, so-called typological chronology is imprisoned in tautological reasoning since it is the very characteristics of the objects which are being classified which serve to determine their succession through time...In the absence of...(independent) criteria...the chronological ordering of material rests on assumptions, indeed on intuitions, whose scientific [radiocarbon] foundations remain unverifiable.”

Whittle has told us that organization of the chronology of prehistoric times employing radiocarbon dating, allied with pottery sequence and artifact sequence dating, is built on a methodology that uses “tautological reasoning” that “the chronological ordering of material rests on assumptions, indeed on intuitions whose scientific foundations remain unverifiable.” He is saying that this applies to the entire field, not just some areas, because every researcher that employs pottery sequence and artifact sequence dating is imprisoned in “tautological reasoning” based on “assumptions, indeed intuitions whose scientific foundations remain unverifiable.” How much of the general literature presented to the public and media has allowed them to be cognizant of this? When one goes through the general literature one will find all of this is ignored. In this instance radiocarbon scientific dating (A) should be independently correlated and corroborated by independently organized pottery sequence and artifact sequence. (B) A is correct because it is supported by B and vice-versa. But this connection has failed. Neither A nor B are independently verified nor connected to one another even internally. Chippindale, nevertheless, presents us with this ex cathedra pronouncement that things appear to be just fine:

“Dating provides the clearest example of a whole area of archaeological work that has been transformed by technical [radiocarbon] advances. Only a few decades ago, the major aim and achievement of the study of a single site, like Stonehenge...was to place scattered [archaeological] material evidence into a coherent chronological framework, and to give the artefactual record [of pottery, axes, beads, and others] a chronological order as a ‘culture history’...”

“At Stonehenge...direct and indirect techniques of dating now provide a good account of the [chronological] sequence in which structures were built at the site, of phases of abandonment and re-modelling, and so on, within a well-dated context...”

The evidence Chippindale offers provides “a good account,” not a “scientifically absolute account.” Neither Chippindale nor any of his colleagues

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23 Whittle, *op.cit.*, p. 16
can point to a single dating methodology that is scientifically correlated with, and corroborated by, another scientific method. It is an archaeological/chronological edifice lacking foundation below and without pillars of support for its upper structure—a house of “tautological reasoning” resting on “assumptions, indeed on intuitions whose scientific foundations remain unverifiable”—a house of cards.

Roger White discusses what prehistorians have refused to consider, namely that their chronology is *Ages in Chaos* because of these anachronistic findings:

“The process of [archaeological] analysis, like that of excavation, is mundane to a degree, and certainly has to be methodical or nothing sensible will emerge…It is often at this point that one finds that the interpretation one placed on a site [where/when] some particular evidence is found to be incorrect. The cherished hypothesis about the association of certain features to form a building [or date a group of pots], for example, may be undone by the realization [of] finds…the features are [found to be] of different dates and thus cannot belong together. If so, then one must have the courage to accept that one is wrong and look again at the evidence to recast it. To do otherwise would be a form of intellectual dishonesty: you would have to falsify or ignore evidence to produce the result you wanted, and this cannot be countenanced (Webster 1991: 121-123). However, accepting the evidence can often lead one to another and more interesting hypothesis. It is here that the excavation record is tested to the limit, since if one postulates an alternative hypothesis, then it may be that you can interpret other parts of that record to fit the new pattern.”

This is just what we will attempt to do in the rest of this book, namely connect pre-Roman interpretations of artifacts to the post-Roman period.

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CHAPTER 4

NEOLITHIC AND BRONZE AGES: BONES, BARROWS AND CHRONOLOGY

Not only have prehistorians constructed their Megalithic Age chronology on radiocarbon and pottery and artifact sequence dating; they have determined that nothing has separated the Late Neolithic and Bronze Ages from the post-Roman age. Since the Stone Age came before the Chalcolithic [Copper and Stone] Age which preceded the Bronze Age that preceded the Iron Age, there was no reasonable explanation for placing the Neolithic and Bronze Ages into the post-Roman epoch. Sally Exon et al., employing radiocarbon and pottery sequence dating methods, have outlined the chronology of prehistoric Europe:

“I Mesolithic to Early Neolithic… c. 3300 cal BC
II Middle Neolithic: Stonehenge phase 1… c. 2900 cal BC
III Late Neolithic: Stonehenge phase 2… c. 2500 cal BC
IV Late Neolithic: Stonehenge phase 3… c. 1800 cal BC
V Early Bronze Age: Stonehenge phase 3… c. 1600 cal BC”¹

Wherever these distinctive materials are found, such as bronzes, they are dated to the Bronze Age; stone tools, etc., to the Stone Age; and iron tools, etc., to the Iron Age. Although there is a good deal of overlap, the chronology seemed airtight—but only at first glance. With respect to barrows or burial mounds, these too are dated by the artifacts in them being either stone, copper, bronze, or iron as well as radiocarbon dating and pottery sequence dating. But how much credence can one put in this chronology, especially if it is also riddled with problems? What is most striking is that the same problems that we have identified in volumes I, II, and III regarding the Copper, Bronze and Iron Ages in the ancient Near East plague the chronology of prehistoric Europe, and have been well known for over a century. In 1872 James Fergusson pointed out that:

¹ Sally Exon et al., Stonehenge Landscapes: Journeys Through Real and Imagined Worlds (Oxford UK 2000), p. 19
“This system seemed so reasonable and philosophical [i.e. scientific]…that it was instantly adopted both in the country of its birth and in England and France; and the succession of the three ages [of human technical development]—stone, bronze, and iron—was generally looked upon as firmly established as any fact in chronology. Gradually, however, it has been perceived that the hard and fast line at first drawn between them cannot be maintained. At the last meeting of the International Archaeological Congress, held at Copenhagen in the autumn of 1869, it was admitted on all hands that there was a considerable overlap between each of the three ages. Men did not immediately cease to use stone implements when bronze was introduced; and bronze continued to be employed for many purposes after the use of iron was well known.”

To enlarge on this fact Fergusson presents a litany of stone, bronze and iron objects found in barrows that supposedly date to prehistoric times. Among these artifacts are found Roman coins which Fergusson and we claim were still available after the fall of Rome to the post-Roman-pagan population who we suggest built these mounds:

“In his ‘Vestiges of the antiquities of Derbyshire,’ published in 1848 by Thomas Bateman, we find the following among other interesting facts…

“On Winster Moor (p. 20) a gold Greek cross—undoubtedly Christian, with a fibula of the same metal richly ornamented, and a quantity of glass and metal ornaments [were excavated].

“[In prehistoric] Pegges Barrow (p. 24). Several Anglo-Saxon ornaments [were found], most probably of the seventh or eighth century [A.D.].

“In a [prehistoric] barrow at Long Roods (p. 28) were found two urns, with calcined bones and a brass coin of Constantine, of the type ‘Gloria exercitus.’

“In Haddon Field Barrow (p. 30) were found 82 brass coins: among them [of emperors] Constantine 9, Constans 17, Constantius II. 9, family of Constantine 3, Urbs Roma 1, Constantinopolis 2, Valentinian 5, Valens 12, Gratian 3. The remainder [are] illegible.

“At [prehistoric] Gib Hill, near Arbor Low (p. 31), of which more hereafter, there were found a flint arrow-head 2½ inches long, and a fragment of a basaltic celt [or stone axe]; also a small iron fibula, and another piece of iron of indeterminable form.

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2 James Fergusson, Rude Stone Monuments in All Countries: Their Age and Uses (London 1872), p. 10
“On [prehistoric] Cross Flatts (p. 35) the weapons found with the skeleton were an iron knife, the blade 5 inches long; a piece of roughly chipped flint, probably a spear-head; and a natural piece of stone of remarkable form.…

“In the great barrow at Minning Lowe (p. 39) were found [several Roman] coins…

“In a smaller barrow close by were found fragments of a coarse, dark-coloured urn, a flint arrow-head, a small piece of iron, part of a bridle-bit, and several horses’ teeth; lower down, a [burial stone] cist with an iron knife, with an iron sheath; and on the outer edge another interment, accompanied by a highly ornamented drinking-cup, a small brass or copper pin, and a rude spear or arrow-head of dark grey flint.…

“In a barrow on Ashford Moor (p. 57) were found, scattered in different parts, a small iron arrow-head and five instruments of flint.

“In [prehistoric] Carder Lowe (p. 63) was found several instruments of flint… and lower down, with the primary interment a splendid brass or bronze dagger; a few inches lower down a beautiful axe hammer-head of basalt. In another part of the barrow another interment was discovered, accompanied by an iron knife and three hones [for sharpening tools or weapons] of sandstone.”

In terms of bronze in these barrows along with iron, Fergusson shows:

“In [prehistoric] Stand Lowe (p. 74), on digging towards the centre, numerous flint chippings and six rude instruments were found, and above the same place a broken whetstone. The centre being gained, an iron knife was found of the kind generally attributed to the [post-Roman] Saxons. This was immediately followed by a bronze box and a number of buckles, fibulae, and articles of iron, silver, and glass, all showing the principal interment to have been of very late date. Mr. Bateman adds—‘the finding of instruments of flint with an interment of this comparatively modern description is rather remarkable, but by no means unprecedented.’…”

“The other tumuli examined by this indefatigable explorer either contained objects generally of the same class or nothing that was of interest as marking their age. If his other works, or those of others, were abstracted in the same way, numerous examples of the same sort might be adduced. The above, however, are probably sufficient to show how little reliance can be placed on the hard and fast distinction between flint, bronze, [brass,] iron [and glass] ages which have hitherto

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3 Ibid., pp. 11-12
been supposed to govern every determination of age in this science. If in a
hundred short pages of one man’s work so many instances of overlapping, and,
indeed, of reversal of the usual order of things [such as iron implements found
beneath those of bronze or stone, or bronzes beneath stone artifacts], can be found,
it is easy to understand how many might be added if other works were also
examined. All, however, that is wanted here is to show that the Danish
[Stone/Bronze/Iron Age] system is neither perfect nor final, and that we must look
for some other means of ascertaining the age of these monuments if we are to
come to a satisfactory conclusion regarding them.

“The fact is that, though a tomb containing only stone and bone implements may
be 10,000 or 20,000 years old, unless it can also be shown that stone and bone were
no longer used after the [post-Roman or] Christian era, it may also be as modern, or
more so, than that [more ancient] epoch. Unless, also, it can be proved that stone
implements were never used after iron was introduced, or that bronze was never
employed down to a late period, this system is of no avail; and after the examples
just quoted from the Bateman diggings, it seems the merest empiricism to assume
that the use of each class of implements ceased on the introduction of another; and
till it can be shown at what date their use did really cease, any argument based on
their presence is of very little value. This, however, is a task to which no antiquary
has yet applied himself; all have been content to fix the age of the monuments from
the assumed age of their contents, empirically determined.”

Sir John Lubbock points to the following that supports Fergusson’s reports:

“The long barrow at West Kennet…was opened by Dr Thurman [and] found
perfectly undisturbed. In the chamber were several skeletons…and among other
things were found specimens of pottery formed on the wheel, and looking very
like Roman in pattern and design,…just outside the chamber, in the undisturbed
earth [were] other specimens of undoubted Roman or post-Roman manufacture.
Though perplexed by his discovery, Dr. Thurman offers no suggestion to account
for their presence, but subsequently Albert Way suggested that the tomb might
have been subsequently used [without disturbing the soil], and that the more
modern occupants may have introduced these objects. No proof whatever was
offered of this theory, it is merely put forward to get over a difficulty which
otherwise seemed insuperable to its author. It is not, however, the only time that
such difficulties have occurred.

4 Ibid., pp. 13-14
“Some years ago Dr. de Closmadeuc...opened a perfectly virgin tumulus at Crubelz. After penetrating through three perfectly distinct but undisturbed strata, he reached the roof of the enclosed dolmen. In it he found the usual products of cremation and the inevitable flint arrowheads...he refers...to the ‘absence de toute trace des métaux...’ [absence of any trace of metal] but he did find that ‘fragments de tuiles qui dénoncent l’industrie Gallo-Romaine ont accidentellement pénétré dans l’intérieur’ [Gallo-Roman tiles got into the interior by accident].

“Will anyone pause for a moment to consider what is involved in this supposition? These tiles, which it is admitted are scattered in quantities over the [surrounding] plains, must somehow have got to the top of the tumulus some fifteen feet high, have penetrated through the undisturbed strata of the mound, inserted themselves between the close-fitting stones of the roof, and finally lodged themselves in the interior of the chamber! We have heard some strange stories of what pottery can do; but this is certainly endowing it with more intelligence and activity than it is usual to ascribe to even Roman tiles; but any hypothesis, however absurd, is preferable with some minds to the heresy of admitting that any dolmen or tumulus can be subsequent to Roman times. So in like manner the Baron de Bonstetten opened another tumulus in the same neighbourhood. At one foot below the undisturbed surface the usual deposit of flint instruments was found, and, two feet below them, two statuettes of Latona and a coin of Constantine II, but this without the least shaking his faith in the prehistoric character of the monument he was exploring. When the French savans come to apply to these monuments the same keen, clear logic with which they are in the habit of dissecting other questions, they will probably find that they have a vast gulf to bridge over between the departure of the Romans and the erection of the dolmen at Confolens, and they will require not only these two, but a great many more to fill up the gap; but, when it is filled up, it will be among the most interesting chapters of their monumental history.”

As we will see below, there are other strong antics and acrobatics that Roman pottery tiles and coins can perform insinuating themselves beneath megaliths.

Given that radiocarbon dating, pottery sequence and artifact typological dating has failed, one wonders if the concept of Stone Age to Bronze Age to Iron Age to delineate the established chronology of the prehistoric megalithic world can be employed to supplement these. We do not disagree with the concept but question whether it can furnish a clear direct chronology for the post-Roman/Medieval epoch. We maintain that with the fall of the Roman empire—to be discussed

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below—the Roman world’s economic, trade, and industrial system collapsed and the population that survived the events that brought about that collapse could no longer maintain that system; it was forced to return to the land and had to relearn how to make all that had earlier been made and achieved. In this sense the post-Roman world had in large measure returned to the Stone Age. Therefore, we should find that post-Roman Britain and Europe were using stone tools and weapons through much of the post-Roman/Medieval period. Along these lines Arthur Hadrian Allcroft points out that:

“The use of stone for various purposes continued long after the introduction of bronze and [after the introduction of] iron, but in ever-decreasing proportions as the metals supplanted the cheaper but less tractable [stone] material. There were parts of Scotland, for example, whose inhabitants remained to a great extent men of the Stone Age until the Middle Ages were elderly.

“The introduction of bronze into northern Europe is thought to have occurred about 1800 B.C., that of iron about 500 B.C. The use of bronze or iron was certainly familiar to some tribes of southern England long before it became common to those in the interior, not to speak of the remote parts of the island of Ireland; so that even within the same area of Great Britain there existed contemporaneously communities of all three ages and there is no definite date at which any one of the three can be said to begin or end.”6

Madison Grant further remarks:

“…Neolithic polished stone implements which ultimately became both varied and effective weapons and tools continued in use long after metallurgy developed. In the Bronze Period, of course, metal armor and weapons were for all ages of the greatest value. So they were necessarily in the possession of the military ruling classes only, while the unfortunate serf or common soldier who followed his master to war did the best he could with leather shield and stone weapons. In the ring [of warriors] that clusters around [King] Harold for the last stand on Senlak Hill [in 1066] many of the English thanes died with their Saxon king armed solely with stone battle-axes of their [Neolithic] ancestors.”7

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A. de Mortillet speaks of three phases of the survival of the use of stone: “Persistence (the more or less prolonged real employment), habit and atavism, tradition; the survival of stone weapons (e.g., axes among vikings, Saxons, etc.), stone tools.”

Gerard Fowke, citing Knight, succinctly reports:

“The English used stone weapons at the battle of Hastings in 1066, and the Scots lead by [William] Wallace did the same as late as 1288…while stone axes were used by the Germans in as late a period as the Thirty Years’ War.”

There is evidence of this multi-age condition in the linguistic studies carried out as reported by Samuel Hibbert for the Saxons and Scandinavians:

“Yet if any doubt can still exist that stone axes were either Scandinavian or Saxon implements of war, it is removed by an extract from an ancient Teutonic romance of the eighth century to be found in Eccard’s Commentaries ‘de Rebus Franciae Orientalis’…The passage to which I allude is thus translated: ‘Then they let ashen spears fly with rapid force, that then stuck in the [enemies’] shields; then they thrust together resounding stone axes.’ The expression stone axes is in the original staim bort—a term compounded of such words of later orthography as stein, a stone, and barte, an axe.”

England, Scotland and Scandinavia came very late into the Iron Age, even after the Norman conquest, as outlined by Thomas Turner:

“Little is known regarding the production of iron in this country under the Saxons. The chaotic condition of the government during the centuries immediately succeeding the Roman departure was unfavourable to the progress of the industrial arts, and it was not until the close of the Saxon period that iron trade began once more to flourish…But under the Normans the iron trade again declined and the metal became a comparatively rare and costly material, so that the Scots in a predatory expedition in the tenth year of Edward II ‘met with no iron worth their notice until they came to Farness in Lancashire, where they seized all the manufactured iron they could find and carried it off with the greatest joy, though so heavy of carriage, and preferred it to any other plunder.’ In the reign of Edward

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9 Gerard Fowke, Archaeological History of Ohio: The Mound Builders and Later Indians (Columbus OH 1902), p. 522
III, the [iron] pots, spits and frying pans of the royal household were classed among the royal jewels.”

Here we have a picture of post-Roman Britain, Germany, and Scandinavia which in terms of metallurgy looks like the Neolithic and Bronze Ages. Prehistorians, however, have failed to consider that this post-Roman world is in reality the time of the Megalithic Ages. On the other hand the picture of pre-Roman Britain, which should have exhibited the culmination of the prior metallurgical tradition of some 3000 years and was a time of high metallurgical development, is nothing of the kind. In Britain the people had not mastered this art and their supposed production output and development was practically non-existent. Even when we reach the time of Caesar’s brief invasion, as explained by R.G. Collingwood and J.N.L. Myres:

“(...) Devonshire, Wales, and the north-west were still occupied by a backward and poverty-stricken Bronze Age population, living in hut-farms and hut-villages and owning hardly any implements except of wood and stone.”

What appears self-evident is that bronze and iron not only came late to Britain, Scotland, Scandinavia, and Germany, but that even after these peoples had contact with Rome, a highly advanced metallurgical culture, they were unable to emulate it after Rome fell. They clearly were using stone weapons and tools with some of iron and bronze here and there, but were still immersed in Stone Age culture before and long after their contact with the Romans. Had they actually been capable of producing metal tools after Rome abandoned these regions, why were they still using stone axes in Britain in the times of William the Conqueror, in 1066, or with William Wallace at the end of the 13th century, or during the predatory raid of the Scots in the time of Edward II (1264-1327)? Why were iron pots, spits and frying pans in the reign of Edward III (1312-1377) classed among the royal jewels? Why were stone axes still in use during the Thirty Years’ War in Germany (1618-1648)? Why were stone axes common enough in Scandinavia and Saxon Germany when they were directly referred to in Medieval and Teutonic romance stories? The answer seems clearly to be that in post-Roman Europe people were still living in Stone, Bronze and Iron Age cultures where very little metal was available to the vast masses of the population. To suggest that the various peoples of post-Roman

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Europe were not living in Neolithic, Bronze or Iron Age conditions is warrantless. Yet in spite of the fact that the stone/metallurgical situation was largely the same in both prehistoric and post-Roman Europe, the interpretation has always been that the stone/metallurgical condition only applies to prehistoric Europe. But this has not been proven. It has been “guardedly” accepted as the most reasonable explanation as discussed by Sir Norman Lockyer.

“Prof. Gowland guardedly writes:

“...The occurrence of stone tools does not alone prove with absolute certainty that Stonehenge belongs to the Neolithic age, although it affords a strong presumption in favour of that view. But, and this is important, had bronze been in general or even moderately extensive use when the stones were set up, it is in the highest degree probable that some implement of that metal would have been lost within the area of the excavations, and if so lost, it would certainly have been found together with the stone tools. Further, the employment of deer’s horn picks for the extensive excavations made in the chalk around the base of the monoliths also tends to support the view that bronze implements cannot have been in common use. If they had it would seem not unreasonable to assume that they would have been employed, as they would have been so much more effective for such work than the picks of deer’s horns.

“Again, the chippings of the stones of Stonehenge in two of the Bronze age barrows in its neighbourhood show that it is of earlier date than they.”

Note that Gowland omits from the entire discussion the evidence that Ferguson cited regarding iron, bronze, stone and even coins that were dated to Roman or post-Roman times as if these do not exist. He “assumes” that bronze tools were common when they may not have been common. This, too, is the assumption that persists today. If, as we claim, with the fall of Rome metallurgical development collapsed and only a small number of such bronze and iron artifacts existed—as we have shown up until the time of Harold of Hastings whose many thanes fought and were buried with their stone axes, etc.,—then of course deer’s horn picks would have been used instead of the more effective bronze or iron. The reason it was guardedly assumed that these constructions were prehistoric is that it was well known in the early 20th century that the post-Roman peoples of Britain and the Continent were still using stone weapons and tools, that there was no scientific method of proving the chronology accurate.

13 Sir Norman Lockyer, *Stonehenge and Other British Monuments Astronomically Considered* (London 1906), pp. 76-78
and that it was assumed such megalithic structures had to be derived from contacts with the ancient civilizations of the Near East. Prior to the radiocarbon dating revolution that prehistoric megalithic chronology was still disputed.

There were other more cogent reasons for having a guarded position regarding that chronology. There was evidence that metal tools were being employed in the Megalithic Age because evidence from the stone structures themselves indicated that the operations being performed could not have been accomplished with stone tools but required hard metallic ones. For example, it was discovered that a grave had been cut into a bedrock layer of flint, an extremely hard stone. While flint does fracture to form a conchoidal shaped chip which is rounded and is excellent material for stone tools, the depth of the fracture tells us fairly well whether the grave in question was hacked out with either a metal or stone tool. Hard stone striking hard flint to leave these fractured pieces found in the grave requires enormous energy to shatter the flint deeply. Because flint is so hard the stone tool under the impact of many blows will itself either break or shatter and thus there would be broken pieces of those, too, scattered about. R.J. Mortimer discussed the finding of just such a grave:

“During its construction several thick beds and large blocks of flint had been cut vertically through showing that tools of no mean kind had been used. These broken flints (which along with the excavated chalk had been put back into the grave) were found to be splintered in a manner which seemed to indicate the use of metal tools. More than twenty of these pieces of flint have been preserved and clearly show conchoidal fracture. This kind of fracture is the result of a sharp blow given with a hard and somewhat pointed tool. It might have been produced by a stone axe, but it scarcely seems possible that so deep a grave could have been formed by stone tools alone. Had only the latter been used, some would surely have been broken and splintered [by the powerful concussions against the solid wall of flint]...Not one was discovered, however, while three picks of stag’s horn and several others were taken from the rough chalk filling.”

To paraphrase Gowland:

“the lack of the occurrence of metal tools does not alone prove with absolute certainty that that grave belongs to the post-Roman Saxon epoch, although it affords a strong presumption in favour of that view. But this is important: had there been only stone tools available when the flint bedrock grave was dug, it is in the highest

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degree probable that some implement of stone would have been broken and lost within the gravesite, and, if so lost, it would certainly have been found. Further, the employment of stag’s horn picks there as well tends to support the view that metallic implements had been in use alongside antler picks because that grave could be effectively dug with such metallic tools, especially iron.”

In this respect one can actually test which type of tool—stone, bronze or iron—was used to cut that grave in the flint bed. All that is required is to go to a nearby bed of the same flint layer and with axes of either stone, bronze or iron attempt to achieve the same results, namely vertical walls and large conchoidal splinters. If the stone tools break in the process, it is apparent that stone was not used; the infill of that grave was made by debris and the broken stone tools which were cast aside would have been found there. The prehistorians and archaeologists have failed to determine the chronology by testing it publicly and fully executing the procedure that created that grave. Rather than scientifically replicate that operation and determine the truth with respect to chronology, they have not carried out that test. Until that crucial test of replication—the true scientific method to discover truth—is carried out with stone tools one can reasonably and logically maintain that iron or bronze tools were employed and that this grave was not dug in the Bronze or Iron Ages (B.C. era).

Although this is the first form of evidence that undermines the established chronology, it is by no means the last. For example in the Cornish tin mines of Britain the miners did not use bronze tools to extract the ore; instead they used antler pick axes. Martyn Barber reports:

“[Richard] Carew wrote that the Cornish miners believed the streamworks they encountered to be ‘very ancient and first wrought by Jews with pick axes of holm, box and harts horn,’ such items being ‘found daily amongst the rubble’ in contrast to the less frequent metal objects..., though it is the metal objects which have tended to attract attention. This is somewhat ironic, as it is the wooden and antler implements which probably represented the mining tools.”15

This shows that even when the Cornish tin mines were being exploited (in post-Roman times, as we will see below) the miners were working with wooden and antler implements. This, too, indicates that antler pick axes were used well into the medieval period, and again challenges and contradicts the concept that the finding of such tools places them in the Neolithic Age. Another clear contradiction to the concept that the Megalithic Age belongs in Neolithic time is the finding of stone

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15 Barber, *op. cit.*, p. 98 (emphasis added)
balls in the Orkney Islands, Scotland, at Skara Brae, radiocarbon dated via bone specimens to that prehistoric period. The enigmatic stone balls exhibit various complex designs cut deeply into them and, as discussed by Anna Ritchie:

“…there are several ornamental stone objects from Skara Brae, the purpose of which is far from obvious and which are therefore assumed to be ceremonial. Amongst them are two balls, one 66 mm (2½ in) in diameter and incised with geometric decoration and the other 77 mm (3 in) in diameter and painstakingly carved into knobs and grooves. There is also an oval ball, 92 mm (3½ in) long, with four knobs at either end and panels of grooves in a band around the middle, and a spiked or T-shaped object with a central band of incised decoration, again in panels…These two objects are very similar to those from [the site of] Quoyness, though more highly decorated, and they appear to be unique to Orkney. Whatever the concept that lay behind the carved balls, they were widespread beyond Orkney. Almost 400 have been found, mostly in northeast Scotland…Some of the balls bear very elaborate and finely carved ornamentation. Although some have been found in datable contexts, they are normally assigned to the third and second millennium B.C.

“Some doubt has been cast recently on the date of some of the carved stone balls by James Macauley, an engineer who has carried out practical experiments in making such balls. In some cases the angle created in the decoration of prehistoric balls has proved impossible to achieve with stone tools and can only have been carved with strong metal tools…Macauley feels that while some balls were made and used in Neolithic times, others may have been reworked or made entirely at a later period, particularly in Pictish [post-Roman Medieval] times when similar decorative designs [similar to the Neolithic designs] were fashionable. The T-shaped object from Skara Brae was made from a very hard stone [assumed to be wrought] by pecking and grinding [with stone tools], and Gordon Childe [who excavated Skara Brae] described it as ‘one of the most astounding monuments of human skill and patience known’.”

Knight and Lomas further elucidate:

“…the more elaborate balls of Skara Brae…are difficult to explain. Attempts to recreate them using Stone Age tools by engineer James Macauley failed, as it proved impossible to carve the difficult angles without using strong metal tools. For those other balls it had been suggested that the more intricate patterns had been carved during the Bronze and Iron ages. However, the stone balls of Skara Brae were found beneath the layer of wind-blown sand which was laid down when the village was abandoned in 2655 BC—long before any metal objects reached

16 Anna Ritchie, Prehistoric Orkney (London 1997), pp. 70-71
this part of the world. This poses the question, did the engineers of Skara Brae know something about the technology of working stone that we have forgotten?”

We are further told: “There is no real evidence of [these balls] having continued in use to the Iron Age.”

Now not only is Skara Brae radiocarbon dated via bone specimens but its pottery is Grooved Ware, also pottery sequence dated to the Late Neolithic. But it is evident that the stone balls at Skara Brae had angles that stone tools will not carve but that iron will. Furthermore these stone balls have designs on them which are evidently Pictish. Niall M. Sharples, Alison Sheridan and Audrey S. Henshall fully admit of these “carved stone balls [at Skara Brae] he [Gordon Childe] observes that they share the same unusual distribution as objects with Pictish symbols.”

In terms of designs on artifacts and pottery, historians and archaeologists would ordinarily date objects with clearly similar designs to the same period. But this would place not only these stone balls and Skara Brae in the medieval period but indicates that the rest of the Megalithic Age must be placed there as well. Rather than follow their own methodology of using highly similar designs or symbols to date objects to the same epoch and, knowing strong metal tools carved the angles on the balls at Skara Brae which dates them to about Pictish times, prehistorians and archaeologists are pecking, shaping and grinding the evidence to make it fit their chronology. They have assumed that two totally different peoples separated in time by thousands of years, but living in the same region just by coincidence created two sets of stone balls of about the same size, but amazingly with highly unique similar symbols and designs. Just as with the grave attributed to the Neolithic Age which implies that metal tools were used to create conchoidal fractured shards, they are reshaping reality.

In volume I we showed that there was no known source for tin to make bronze in the ancient Near East until long after 1100 B.C. In the established chronology of the megalithic world, the Early Bronze Age supposedly begins around 1600 BC. In essence, the peoples of prehistoric Europe were making bronze well before the civilizations of the Near East, and surely had to have a source of tin to manufacture

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this metal. But here we encounter the identical problem we encountered in volumes I and II of *Pillars of the Past*, namely that there was no source of tin to make bronze during the prehistoric Bronze Age in that part of Europe. We had assumed that Herodotus was correct when he claimed “that tin and amber do come to us from what one might call the ends of the earth.”

“It seems apparent that Herodotus is discussing the tin mines of Cornwall, England.” That was before we had examined the problem of the source or sources of prehistoric tin. Nevertheless, Trevor Palmer has claimed that Cornwall tin was being mined around 2000 B.C. in criticizing this author who denied Cornwall was being mined to supply the ancient world with tin:

“According to Ginenthal, this scenario [for the sources of tin in the Ancient Near East] cannot be sustained, even on its own terms. The manufacture of bronze requires a supply of tin to add to copper [and the first two possible sources of this metal are Cornwall and Spain].

“…possible sources were Bohemia… and Cornwall… Cornish tin was mined from around 2200 BC onwards, and from this time was traded and used for bronze manufacture down the Atlantic coast of Brittany and France, towards Spain. Ginenthal, however, states that: ‘Surely it is inconceivable that in 1500 BC Egypt was trading with England, or even Spain, or with Bohemia, to obtain tin.’

Palmer further cites several authorities that say the same thing and this would suggest to the general reader that the evidence surely supports Palmer’s claim. But because neither he nor they have clear-cut evidence to back up that thesis, Palmer adds the caveat that this trade “may or may not have been so.” Nevertheless, Kaj Birket-Smith contradicts Palmer and his sources regarding tin trade in early historic times between the ancient world with Cornwall, Spain and Bohemia:

“In Europe, tin is to be found—or was to be found—along with copper in Spain, Cornwall, and central Europe [Bohemia]; but although Cornwall early on became a great source of supply and was the goal of the Phoenicians during voyages in the Atlantic, for reasons of chronology alone, neither of these places [Cornwall and Spain] can be considered the home of Bronze in the Old World.”

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22 Trevor Palmer, “Science, Technology and the Chronology of the Ancient World,” *AEON*, vol. VI, no. 6 (October, 2005), p. 47
24 Kaj Birket-Smith, *The Paths of Culture* (Madison WI 1965), p. 81
Lionel Casson, one of the major authorities of trade in the ancient world, also contradicts Palmer’s claim:

“The Greeks and the Romans got their supplies [of tin] from the rich deposits in Portugal, Spain and England, and there are those who argue that these same places were exploited almost from the beginning. They conjure up voyages by intrepid Minoan and Mycenaean sailors out through the Strait of Gibraltar into the Atlantic to bring back the precious substance…

“…archaeologists were long convinced that Aegean seamen…having gotten that far [Spain], could they not have made it the rest of the way to the tin lands [of Portugal and Britain]? Today…people are coming around to the view supported by archaeological evidence, that British tin became important only after the seventh century B.C. and Spanish and Portuguese only during the days of the Roman Empire.

“The tin [for the ancient Near East] then did not come from the west.”

Although it is admitted by the authorities that Cornwall tin was not traded to the ancient Near East, they, nevertheless, all maintain that tin was undoubtedly being mined in Cornwall, Britain, in pre-Roman or even prehistoric times. Palmer stated directly “Cornish tin was mined from around 2000 BC onwards.” Birket-Smith echoes this: “Cornwall early on became a great source of [tin] supply.” Casson adds that “British tin became important only after the seventh century [B.C.]” From these statements one could draw the erroneous conclusion that the Britons had mined tin at Cornwall as early as 2000 B.C. for their own use, and thus there is no “Tin Problem” for their Bronze Age. However, none of these authoritative statements are backed up by anything resembling archaeological evidence. The concept that Cornwall was producing tin in pre-Roman or even prehistoric times is simply stated as an ex cathedra fact. Tin was mined at Cornwall in early times because tin was mined at Cornwall in early times. Nevertheless as late as 2009 Jane McIntosh specifically informs us:

“Tin mines in Spain were worked before Roman times, and tin may also have been mined in Cornwall, though no ancient mines have been located there.”

Since as late as 2009 no one has been able to find archaeological evidence of mining operations in Cornwall, this means there is no such evidence. No evidence

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26 Jane McIntosh, Handbook of Life in Prehistoric Europe (Oxford UK 2009), p. 216
means no evidence, despite the *ex cathedra* statement of authorities to the contrary. To get around the fact that no ancient tin mines have been located there, Sabine Baring-Gould offers this ad hoc explanation:

“Though tin was exported from Cornwall, bronze was not manufactured there till a comparatively late period. Bronze came from the East, and the great centre whence radiated the trade in bronze weapons was the basin of the Po [river in Italy].

“What seems to be abundantly clear is that the export of tin from Britain had come to an end by the first century of the present era. Caesar, on invading Britain, heard nothing about it, and when Britain was finally conquered, the Romans who worked the lead mines in the Mendips, and the gold and copper in Wales, totally neglected Cornwall, held it to be worthless. They never settled there…”

Here we are told that tin was indeed mined up to the Roman era and mining then suddenly stopped completely for no given reason. Not only were the Cornwall mines closed but the miners filled them in and allowed vegetation to grow over them that disguised the fact that they existed, so well, that even today archaeologists cannot find anything to prove they existed. But no evidence is actually given that these pre-Roman prehistoric mines existed, other than the statements that they did. This, in spite of the fact that one year prior to Baring-Gould’s statement McIntosh admitted “no ancient [tin] mines have been located in Cornwall.” The ancient tin mines are simply not there! They only exist in the minds of historians who want them to exist. When, then, was Cornwall tin actually mined? According to L.F. Salzman there is no evidence that Cornwall was exploited for its tin in pre-Roman or prehistoric times, but was only well into the common era several centuries after Rome abandoned Britain:

“There is no reference to these mines in the literature of the period of Roman occupation, nor are there traces of anything approaching an occupation of Cornwall by the Romans, who appear to have ignored this corner of Britain completely. After the departure of the Romans, and before the Saxons conquered this district which did not happen till the middle of the tenth century [A.D.], there is some evidence of tin being worked here, as Cornish tin is said to have been carried over to France in the seventh century [A.D.], and in the life of St. John of Alexandria, who died in 616, there is a story of an Alexandrian galley coming to Britain for tin. That the Saxons had tin [in the 7th century] seems probable from the discovery of Saxon remains in the St. Austell tin grounds and elsewhere, but the

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industry can hardly have been of any great importance at the time of the Norman conquest [in A.D. 1066] as there is no reference to it in the Domesday Survey.

“While the history of tin mining in Britain prior to the middle of the twelfth century is problematical, there is from that time onwards an immense mass of material bearing upon the subject.”

The fact of the matter is that all the statements purported to suggest Cornwall’s tin mines were worked and its tin exported to the ancient Near East, or that the tin was utilized in prehistoric Britain itself, are supported by nothing but the desire to make this theory into fact. The “Tin Problem” is as lethal to the established chronology of the Megalithic Age as it is to that of the ancient Near East. Typical of the many statements regarding this desire is that of George Robert Rapp and Christopher L. Hill:

“Perhaps the greatest unsolved problem is Old World Bronze Age metallurgy is where the tin came from. The plentiful placer cassiterite [tin ore] from Cornwall was certainly mined for British Isles Bronze Age metallurgy but we do not know how widely that it was traded.”

Robin George Collingwood and John Howard Linton Myres encapsulate the problem: “The early British tin-trade is so controversial a subject that some hardiness is needed in order even to mention it.” They go on to show:

“The chronology of this period is very vague. The Late Bronze Age in southern Britain is regarded [not proven] as beginning about 1000 B.C. and lasting until about 400 B.C.; elsewhere it is thought to last to about 200 [B.C.], and in Scotland to as late as the beginning of our era. These dates, however, are not only mere approximations, but the events which they are designed to fix are only the beginnings of slow and gradual processes. The peasant-civilization described above may have taken shape after 1000 [B.C.], but it did not cease to exist at the date when the Bronze Age is said to have closed.”

The same applies to the Iron Age, where they add:

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28 L.F. Salzman, English Industries of the Middle Ages, Being an Introduction to the Industrial History of Medieval England (1923, reprint Charleston SC 2009), pp. 63-64
30 Collingwood and Myres, op.cit., p. 70
31 Ibid., p. 21
“When we speak of the Iron Age as beginning at a certain date...it must be remembered that this does not imply a general abandonment of Bronze Age fashions, nor even the general beginning of such abandonment; and hundreds of years after such a date the majority of Bronze Age villages were still carrying on their old life with little change.”\textsuperscript{32}

In terms of the short chronology the British people at the time of the Roman invasion and occupation would have been still employing Stone Age tools along with those of the Bronze Age. Collingwood and Myres describe Britain at the time of the Roman invasion, showing Britons were still living in the late Bronze Age and not at all in the Iron Age:

“The general [nature] of British civilization at this time may be described as having a Late Bronze Age character..."

“The Late Bronze Age was a period when Britain as a whole was a backward country by comparison with the Continent; primitive in its civilization, stagnant and passive in its life, and receiving most of what progress it enjoyed through invasion [by the Romans] and importation from overseas...

“They made rude pottery without a wheel, and still used flint [not bronze or iron] for such things as arrowheads; but they were [it is assumed] visited by itinerant bronze-founders able to make swords, spears, socketed axes [etc.]. Judging by the absence of towns and the scarcity of anything like true fortification...[i]n particular, towards the north and west, it was strongly tinged with Neolithic survivals, sometimes so strongly as to give it an ambiguous character as between Neolithic and Bronze Age types; in these parts inhumation, the Neolithic burial rite, sometimes lingered on.”\textsuperscript{33}

The picture of Britain drawn by Collingwood and Myres is that of a people still living in Neolithic Age conditions with here and there a smattering of Bronze Age materials. The identical problem that exists in the Near East regarding the source of tin also exists for that of megalithic Bronze Age Britain. Peter Northover admits:

“The problems of the origins of tin supplies are well known. The very small number of Bronze Age tin [artifact] finds has always been seen as one of the largest of these difficulties. However, it is now clear that for much of the Bronze Age, especially in lowland Britain, tin was generally transported already alloyed

\textsuperscript{32} Ibid.
\textsuperscript{33} Ibid., pp. 20-21
as bronze and that most of that bronze originated on the Continent. In the Highland Zone and Ireland conditions were different but, even so, the only traffic in tin to be expected is from the tin sources direct to those primary copper production centres without tin sources of their own, e.g. North Wales. As we have not found such centres it is not surprising that tin does not appear in the archaeological record. When considering the origins of the tin used in the manufacture of swords it is readily apparent that the source of the tin is closely bound up with the ultimate source of the copper used in the bronze. A useful clue to tin origins may lie in the very small inclusions of tin associated with chalcocite [copper] inclusions in a range of Late Bronze Age metalwork.”

As in the ancient Near East, we encounter explanations for the sources of tin based on the established chronology but without a factual or scientific basis. The bronze implements were made in Europe and traded with the British, we are told. But we are never told just where on the Continent the tools were made, nor where on the Continent the tin mines actually existed, nor given any evidence that these mines were actually operating at that prehistoric epoch. Just as historians have invented tin mines in the Caucasus Mountains to supply tin to the ancient Near East that cannot be found or, if found, are shown to have been exploited from 2200 to 1200 B.C., so, too, archaeologists of megalithic Bronze Age Britain, Ireland and Scotland have conjured up copper mixed with tin from mines in continental Europe that cannot be found or, if found, are shown to have been exploited from 2200 to 1200 B.C. They have also conjured up itinerant bronze trading families, back then, who crossed Europe to England, across the English Channel, that carried these metals or were peddlers, but this, too, cannot be shown to have existed except in theory!

Of this theory Barber writes:

“The image of the itinerant smith is frequently encountered in the archaeological literature concerning the Bronze Age. Among earlier generations of prehistorians Gordon Childe was a notable advocate on their behalf and was probably more responsible than anyone else in establishing them as a ‘fact’ of Bronze Age life.”

Barber then goes on for several pages to discuss these itinerant smiths but does not substantiate their existence. D. Blair Gibson makes it clear that Childe’s itinerant craftsmen are a fiction in an article provocatively titled “Death of a Salesman: Childe’s Itinerant Craftsman in the Light of Present Knowledge of Late

34 Peter Northover, “The Analysis and Metallurgy of British Bronze Age Swords,” Ian Colquhoun and Colin Burgess, eds. The Swords of Britain (Munich Germany 1988), p. 130
35 Barber, op.cit., p. 129
Prehistoric European Craft Production,” stating “Wells [following Childe] attributes the movement of metal and techniques between regions to itinerant bronze smiths. There is no ostensible proof of itinerant smiths as the mechanism of metal exchange, or, for that matter, for their existence.”36

Barber explains the “Tin Problem” by first citing two major authorities:

“All the vital elements of modern material culture are immediately rooted in the Bronze Age…Modern science and industry not only go back to the period when bronze was the dominant industrial metal, their beginnings were in a very real sense conditioned and inspired by the mere fact of the general employment of bronze and copper’ (Gordon Childe, 1930, 2-3).

“The introduction of bronze was one of the greatest non-events of all time’ (Andrew Selkirk, 1971, 113). [Barber then comments]:

“These two brief statements neatly capture the central dilemma faced today when trying to evaluate the significance of the earliest metal objects both to archaeologists and the people who made and used them. Just how important is bronze to our understanding of the Bronze Age? Once the back bone of the period, the core around which its chronology and cultural history were constructed, since the 1970s it has become increasingly difficult to justify retaining that central role for metalwork studies.”37

The reason for this “central dilemma” is that research has utterly failed to support the chronology upon which this research is based. Barber continues:

“No single [fact] accounts for this change in fortunes. Instead several developments have combined to relegate bronze to a position of lesser importance. First of all, the impact of the numerous sites excavated from the 1960s onwards has been considerable. Previously, the only type of Bronze Age site excavated in any quantity was the round barrow—the principal funeral monument of the [British] Early and Middle Bronze Age…Consequently little is known about the everyday people who made and used bronze and who are buried in the barrows. As Colin Burgess acknowledged as late as the mid-1970s, ‘Most of our knowledge of the period is derived from burial and ritual sites and unassociated artefacts, which give a biased picture, too concerned with matters spiritual, religious, and technological,

37 Barber, op.cit., p. 17
and not enough with more mundane aspects’ [of people actually using bronze items] (Burgess, 1974, 165). As a result the major artefact types [found] have essentially come to represent the people who used them, cultural history being written from studies of their geographical spread and chronological development. Indeed, Burgess’ own comprehensive account of the period was still rooted firmly in detailed study of metalwork and pottery [types associated with it]…

“Secondly came the dawning realization that bronze is, in fact, quite scarce on excavated sites. Few settlements yield more than the odd scrap, and it occurs only with a minority of burials, the attention focused on the ‘wealthier’ grave assemblages [with bronze items] perhaps helping to obscure this fact. This lack of direct association between bronzes and both the living and the dead had long been implicit in metalwork studies, but it is only with the growing quantity and variety of excavated sites from the 1960s onward that we have been able to appreciate how little we really know about the uses and meanings of bronze in the Bronze Age. Consequently the grand chronological and cultural scheme for the Bronze Age, constructed largely via close study of metalwork, effectively became little more than what it had really been all along—a sequential arrangement of the metalwork which had few clear links with the other forms of evidence from the period, and which lacked any reliable form of independent verification.”

As with radiocarbon and pottery dating “which lacked any reliable form of independent verification” by which to decipher the chronology of Bronze Age Europe, bronze artifacts themselves “lack…any reliable form of independent verification.” The point that Barber also makes is that: “Few settlements yield more than the odd scrap [of bronze], and it occurs only with a minority of burials.” But if Bronze Age Europe could produce bronze artifacts, it would be rather more common than the odd scrap occurring only in a minority of burials. In fact, it well describes Britain of the Anglo-Saxon/post-Roman era when, as we have shown above, people were still using stone tools and weapons well into the Middle Ages. It also fits the fact that these post-Roman peoples were not mining tin in Cornwall until quite late, making these bronze objects a rarity for all but the rich, and they were, as Barber stated, “in fact, quite scarce on excavated sites” of the “‘wealthier’ grave assemblages.” Isaac Taylor describes the condition:

“As bronze has been found in round barrows [dated to the Bronze Age], it is frequently asserted that the Celts were armed with bronze weapons when they invaded Britain. This conclusion is not borne out by the evidence, which indicates that the Celts arrived in the [pre-Bronze] Neolithic period, and obtained bronze by

38 Ibid., pp. 17-19
commerce with Gaul at a later time. Canon Greenwell tabulates 485 interments in round barrows; in 201 cases these were associated with pottery, in 150 cases with implements of stone, bone, or horn, and in only twenty-three with bronze [artifacts]. Of these twenty-three cases only five were primary [central] interments, fifteen were [later than the Bronze Age] secondary interments, and the rest doubtful.

“Mr. Mortimer, who has opened 241 round barrows in the East Riding, containing 629 bodies, found pottery in 203 cases, stone implements in 150, and bronze in twenty six. These facts make it probable that when round barrows [dated to the Bronze Age] were first erected bronze was either unknown or extremely rare, but that it had to some extent come into use when secondary interments took place in barrows.”

In only four percent of these supposed Bronze Age burials both Greenwell and Mortimer admit that bronze was found. This is not a description of a fully developed Bronze Age society. It is a description of a society that possessed practically nothing in the way of bronze. And we repeat, it in fact well describes Britain in the Anglo-Saxon/post-Roman era when people were still using stone tools and weapons well into the Middle Ages and were not mining tin at Cornwall. Furthermore, the bronze objects employed by archaeologists to organize these artifacts into a chronologically ordered sequence through time inevitably also collapsed.

Barber shows that “though our knowledge remains far from perfect, the ever-increasing number of excavated sites is failing to demonstrate any significant developments that could be blamed on, or attributed to, a switch from stone to bronze, or from bronze to iron.”

As for the sources of tin, Barber deals with this in his unit titled “The Tin Problem,” (citing R.D. Penhallurick, *Tin in Antiquity*, 1986): “Searching for field evidence of prehistoric [tin] mining nowadays seems to me rather like looking in the proverbial haystack for needles which formerly existed but which may no longer do so.” Here Barber calls the evidence for Bronze Age tin mining “disappointingly elusive”:

“Tin presents something of a problem when it comes to understanding the exploitation and circulation of metals in Bronze Age Britain and, indeed, in

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40 Barber, *op. cit.*, p. 24
41 *Ibid.* p. 97
Europe. Tin deposits across the continent as a whole are far less common than copper, and one of the largest tin fields in Europe occurs in Cornwall and West Devon, yet physical evidence for its [ancient] exploitation is remarkably limited. For the south-west, of course, this is not just a problem for tin—the region is also notable for the marked absence of evidence for prehistoric exploitation of its copper sources. As Sharpe (1992, 35) noted, ‘not one securely-dated prehistoric mining site has yet been found in the south-west, despite considerable efforts to locate them. This is not to suggest that they did not exist, nor that they will not be found in the future, nor, more importantly, is it to say that there is no evidence whatsoever that prehistoric exploitation of ore did take place.”

The old historical/academic adage is therefore that the absence of evidence of tin mining in ancient Bronze Age Britain and Europe is not evidence of absence of tin mining. What historians who employ this argument are attempting to do is maintain their chronology \textit{without evidence}. Barber concludes: “Despite renewed archaeological interest in recent years...further evidence [for European Bronze Age tin mines] proved disappointingly elusive.” Why “disappointingly elusive” instead of just “elusive?” Here Barber has betrayed his and his profession’s “desire” to find proof that such tin mines existed. One is only disappointed when one wants something and can’t find it. This shows how deeply scholars in this field have had the established chronology engrained in their profession and points to a lack of neutrality respecting evidence. For those of us committed to the scientific method this desire to find what their paradigm requires is what “proves disappointing.”

The “Tin Problem” will remain a thorn in the side of prehistorians; so long as they cleave to their chronological framework it will be “elusive” and unresolved. There is simply no physical evidence for prehistoric tin mines in Britain or central Europe. Barber concludes his book with the following:

“It is...clear that the straightforward assumption about metal that once dominated its study can no longer be supported even by basic empirical and descriptive data. This has been apparent to many prehistorians for some time, but has been slow to penetrate beyond those actively involved in researching the period.”

There is absolutely nothing to sustain the belief that Bronze Age tin mines existed in prehistoric Britain or central Europe except their unyielding belief. The

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42 \textit{Ibid.}
43 \textit{Ibid.}
44 \textit{Ibid.}, p. 175
support from bronze metalwork and mining or independent support for radiocarbon
dating or for pottery sequence dating or artifact typology dating has failed. Not
only do these phenomena fail to support and uphold one another, in each and every
case there is nothing to internally support any of these methods. They have all
failed on their own terms.

In Ireland we run into the same problem for the chronology of the Iron Age.
Raftery explains:

“The use in Ireland of the term ‘Iron Age’ is...problematical...and the very
small number of surviving iron artifacts of the period greatly exacerbates the
difficulties. Factors such as poor preservative quantities of this material and the
relative unlikelihood of its retrieval by chance finders have doubtlessly contributed
to the dearth of evidence, but the possibility that iron working was practiced on
only a limited scale for much of the so-called Iron Age in Ireland must be
seriously considered.”

Raftery adds that:

“For Ireland...the contemporary situation is much less clear and the Iron Age
is seen as a vaguely defined era, following the Bronze Age and assumed to end at
some (as yet undefined) point in early historic times. Neither the beginning nor the
end of this Iron Age can be satisfactorily established, but it is not unlikely that the
period involved may have spanned as much as a millennium.”

As we can see, in Britain and Ireland the chronology of the Stone, Bronze and
Iron Ages is a muddle without a solution. Raftery titles his book in part The
Enigma of the Irish Iron Age because that is what it is. We shall come across other
aspects of this enigmatic Iron Age repeatedly below, just as we have with the
chronology of the Megalithic Age. What Raftery states about the Enigma of the
Iron Age in Ireland applies well:

“These centuries are obscure in archaeological terms and have been termed
rightly as a Dark Age. We have, in truth, no clear idea of what was happening in
the country at that time.”

Emmet J. Sweeney points out:

46 Ibid., p. 120
47 Ibid., p. 37
“Possibly some of the most damning evidence of all comes from one of the mounds at New Grange, Carn H. Here archaeologists found a whole series of artifacts of clearly Iron Age date. These included amber and glass beads, various iron objects and bone plaques. It was the latter which caused the most disquiet.

“The ornament on these bone plaques is by very general consent agreed to belong to La Tène art and probably to date to the first two centuries AD. The problem presented by these objects of La Tène art together with iron objects was to explain their presence in a Passage Grave, the walls of which were decorated with typical megalithic art and which was assumed by people to have been constructed around 2000 B.C. It was argued by most archaeologists before Dr. Raftery’s re-excavation that Carn H at Loughcrew had been used as a workshop in the Early Iron Age—perhaps the atelier of a Celtic artist. Professor MacAlister, for example, believed the metal-workers of the Early Iron Age produced these plaques as samples for the ornamentation of luxury items of bronze. Dr. Raftery disagreed with this viewpoint and in 1943 reexcavated Carn H.

“The 1943 Raftery excavations found no objects characteristic of the normal megalithic assemblage; what was found, however, were blue, green and yellow glass beads, small bronze rings, pieces of iron, and 2000 bone plaques of which 200 were ornamented in the late La Tène style. Raftery argued that all these finds dated to the Early Iron Age; he found some of them in what he described as an undisturbed foundation layer, while some bone plaques were actually in the stone hole of one of the orthostats in the passage. In describing his excavations to the International Congress of Prehistoric and Protohistoric Sciences in Zurich in 1954, he thought the evidence in his excavations susceptible to only two; first that the site was a normal passage grave constructed, say, in 2000 B.C., but entirely destroyed, removed and rebuilt in the Early Iron Age; or secondly, that it was an old style tomb still being used in the Early Iron Age. Having found no evidence for the first solution, he was put on record that Carn H was constructed in the Early Iron Age and that therefore, megaliths in Ireland survived not only to the end of the second millennium B.C. but to the beginning of the first millennium A.D.

“Thus on the one hand radiocarbon tests seem to place the megaliths centuries earlier than expected, whilst on the other archaeological digs suggest the exact opposite. Which is to be believed?”

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48 Emmet J. Sweeney, The Lost History of Ireland: An Enquiry into the pre-Christian History of the Gaels (Derry, Ireland 1992), pp. 59-60
BARROWS, SKULLS, AND CHRONOLOGY

Another methodology by which the early prehistorians attempted to date the Megalithic Age was the form of numerous burial mounds or barrows that dotted the British landscape and the shape of the skulls of the skeletons found in them. Once the antiquaries of the 18th and 19th centuries concluded these barrows were dated to the Late Neolithic and Bronze Ages they naturally used the stone, bronze or iron found in them to corroborate that chronology. However, upon excavating the mounds they discovered skeletons there and analyzed the skulls of these bygone people to derive the order in which people came to Britain first and second. This categorization led them to organize the barrow shapes and skull shapes such as to correlate them with their chronology in a fixed ordered way. They assumed they knew and understood how these fit into that time frame. Other burials that contained Saxons with Saxon artifacts were labeled as later intrusions of the post-Roman era. That is, they employed the same categorization for these barrows and skulls as they did with pottery sequence dating, artifact typological sequence dating and stone bronze and iron sequence dating to prove that all these forms of evidence were correlated and integrated in such a way that they corroborated each other. These antiquaries believed they had a thorough-going comprehensive picture of the prehistoric megalithic world. This concept was to dominate research for over 120 years from 1872 to 1994 when it was shown to be spurious and to some degree even racist.

The researcher who established this dichotomy and chronology was J. Thurnam who examined the skeletons in the two barrow types and reported his results in 1872. This division of barrows and skull types occurred at a time when racism, namely Eurocentric racism, dominated the intellectual milieu.

Prior to analyzing the crania of skeletons in British barrows, Thurnam and J.B. Davis in *Crania Britannica* had compared the skulls of the British people with other groups as these relate to intelligence levels of these diverse groups. H.M. Bracken discusses the inherent racism endemic in British and European intellectual and scientific circles at that time:

“The racism of a Voltaire or a Hume became scientifically well-founded in the nineteenth century. The historical antecedent of colour/IQ correlations was the work of the craniologists. ‘In the human brain we find those characteristics which particularly distinguish man from the brute creation. The differences between the various races of
Charles Ginenthal, *Pillars of the Past*, vol. IV

men are fundamental differences in intellectual capacity, as well as in physical conformation.’ Professor A.H. Keane of University College, London, writes as follows about the negro in the ninth edition (1884) of the *Encyclopaedia Britannica*.

“Nearly all observers admit that the Negro child is on the whole quite as intelligent as those of other human varieties, but that on arriving at puberty all further progress seems to be arrested…“We must necessarily suppose that the development of the Negro and White proceeds on different lines…”

“Keane later held that the Chinese ‘seem in some respects to be almost as incapable of progress as the Negroes themselves, the only essential difference being that the arrest of mental development comes later in life for the yellow than for the black man’. The discovery that neolithic and modern Europeans had the same brain capacity inclined him to place his emphasis on brain serratures being more complex ‘in the higher than in the lower races’…

“Nott and Gliddon, and Dr. Samuel Morton provided evidence on Celtic brain volumes as well as blacks. The data are included in J.B Davis and J. Thurnam, *Crania Britannica* (London, 1865). The scientific evidence shows that English brains were best [largest] with 96 cubic inches [in volume] and Germans were next with 95. But the ‘native Irish’ were a mere 87 and the ‘native African Family,’ 83.7. Dr. Robert Knox, *The races of men* (London, 1868), p. 12 writes, ‘There never was any Celtic literature, nor science, nor arts…’ He advises us that ‘the object of [his] work is to show that the European races, so called, differ from each other as widely as the…Esquimaux from the Basque’ (p. 44). Since he speaks of ‘exterminating’ (e.g. 229f) various races, it is not surprising to read, ‘Sir Robert Peel’s Encumbered Estate Bill aims simply at the quiet and gradual extinction of the Celtic race in Ireland: this is its sole aim, and it will prove successful. A similar bill is wanted for Caledonia [Scotland]…’ (p. 27).


Therefore, for the British people to be so highly superior, they had to have descended from ancestors with the same large, complex brains. Thus, when Thurnam and Davis examined the crania of the people found in British barrows they expected that these, too, should exhibit these superior qualities. They write: “If we revert to the size of the brain as a test of the mental capacity of the [British]

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race, we shall find that the results embodied in the tables fully support the high position claimed for the ancient British.”\textsuperscript{50} They went even further to suggest that the central districts of England, unsullied by foreign intermarriage, still retained characteristics of the superior aboriginal British of the Neolithic Age.\textsuperscript{51}

Archaeologically and chronologically speaking the main point of Thurnam’s work is the craniological distinction he drew between those found in long barrows and those in round barrows. These he dated to two separate ages—the Neolithic skulls belonged to long barrows, the Bronze Age skulls to the round ones. The differences were clearly distinct and thus told a chronological story:

“The skulls are in fact remarkably long and narrow [and] designated as dolichocephalic…by modern craniologists…

“The contrast in form between the[se] long skulls from long barrows and short or round [brachycephalic] skulls [that] prevails in our…circular-barrows, is most interesting and remarkable, and suggests an essential distinction of race in the peoples by whom the two forms of tumuli were respectively constructed.”\textsuperscript{52}

Thurnam had also analyzed the contents of the two types of barrow and from their contents of either stone, bone, bronze and, rarely, iron inferred the chronology as he reported elsewhere:

“The author inferred the relative date of the two classes of barrows from the archeological evidence. He observed that in no well-authenticated instance had objects of metal or of the finer decorated pottery been found with the primary [central site in the barrow as opposed to secondary—away from the center] interments in long barrows, but only those of stone, bone or horn, and a peculiar kind of pottery. [Because of this evidence H]e…refers the long barrows to the Stone period…and believes that they are the earliest sepulchral monuments of the inhabitants of this island…

“In the round barrow, on the other hand, objects of Bronze (very rarely of iron) and richly decorated pottery are often found, with or without objects of stone. The

\textsuperscript{50} J.B. Davis, J. Thurnam, \textit{Crania Britannica} (London 1865), p. 237
\textsuperscript{51} \textit{Ibid.}, p. 212
\textsuperscript{52} J. Thurnam, “On ancient British Barrows, especially those of Wiltshire and the adjoining counties,” \textit{Archaeologia} vol. 48, pt. 1 (1872), pp. 198-199
author hence refers the round barrows to the *Bronze period*…, and to that of the bronze [to] iron transition…”

The foundation of this dating was not the skulls themselves but the stone, bone, bronze, and rare iron in the barrows which has failed to support that chronology, as described above, but also the forms of pottery that also failed to support that chronology, also described above. In terms of the typological evidence of these ancient barrows we cited A.F. Harding above who described “the study of megalithic tombs, where detailed typological study is a quick route to insanity.”

The problem is that these round-headed people found in round barrows who replaced the long-headed people found in long barrows continued to build both long and round barrows, which apparently were not part of their culture, and bury people in them with long skulls. That is, Bronze Age round burial mounds were being constructed in the Neolithic Age before these round-headed people arrived. Frances Lynch explains:

“Large round earthen barrows are traditionally believed to date from the bronze age, but nineteenth-century excavators in Yorkshire recognised that several covered deposits indistinguishable from those under neighbouring long barrows. While it is probable that some of these were, in truth, damaged long barrows, modern re-excavations and the occasional presence of a circular kerb or ditch have confirmed that the round mound was certainly an acceptable variant from an early [Neolithic] date in the north-east of England.

“Some forty round [Bronze Age] mounds can be shown to cover earlier neolithic multiple [mound] burials with very limited accompanying goods. At Callis Wold, East Yorkshire…and Seamer Moor, North Yorkshire, foundation trenches for wooden façades and enclosures…were identical with those under long barrows. Similar burial spaces…are rather more common and more widespread…”

The concept that both long and round barrows were in fact contemporary was resisted for Scotland’s mounds as Gordon Barclay and Gordon S. Maxwell show:

“The most recent overall consideration of [Bronze Age] round barrows [existing] in the Neolithic of Britain is that of Kinnes [1979]…The Royal

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Commission specifically resisted ‘the temptation to assume that many of the large lowland round barrows [were] of Neolithic date’...However, the comparison of the...excavated sites allows us to consider the clearly strong and long-lived tradition of massive round mound building in the area.”

They go on to describe these supposed Bronze Age round mounds found in Neolithic contexts. Timothy Darvill specifically states that

“Oval barrows have their origin in the early Neolithic, but continue to be popular through the middle and later Neolithic.”

Castleden reports that

“Charcoal from the barrow has been radiocarbon dated to...(3600 BC) proving that some round barrows are quite early.”

Sally Exon et al. warn:

“But, before leaving the Early Neolithic period, two cautionary notes must be offered. Firstly, the record of excavations of the early [burial] monuments is very sparse and any new interventions are likely to amplify the database very significantly. In particular, many more absolute [radiocarbon] dates are needed. Secondly, although the complement of long barrows known from excavation and survey may be a near complete record, [yet] we do not know how many of the round barrows in our study might have originated in the Neolithic period. Strong hints of the existence of Neolithic round barrows have been gathered…and there may have been many more. How [Neolithic round] barrows such as these functioned within the changing patterns of long barrow intervisibility, pathways [between long and round barrows] and locales can at present only be guessed at.”

Peter C. Jupp and Clare Gittings were not so reticent regarding the contemporaneous nature of long and round barrows.

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58 Exon et al., *op.cit.*, p. 42
“We now know that round barrows were constructed throughout the Neolithic period, and that...both inhumation and cremation took place beneath round barrows...and within circular ditched enclosures at Dorchester-on-Thames.”

These people clearly lived at the same time.

Let us then return to the interpretation that the long-skulled people in the long barrows preceded the round-skulled people in the round barrows. This requires that the round-headed people either migrated to Britain gradually or came en masse as conquerors. The invasion hypothesis was accepted for a long time and is discussed by Barber:

“Evidence of [round-headed] invasion (or migration) came from a longstanding belief that a physical—and primarily craniological—distinction could be drawn between the occupants of Neolithic long barrows and the dead who resided beneath the later round barrows. The latter seemed to possess more rounded (‘brachycephalic’) skulls than the people they apparently replaced. Some also argued that the [later Bronze Age] Beaker folk were generally tall too, proof it appeared that ‘two widely-differing races had occupied these islands prior to the invasion of Julius [Caesar]’...

“Inevitably, for some the seemingly rapid dominance and spread of a new physical type and their associated, superior material culture warranted more than mere [skull] measurement and description. Thus for Beddoe (1895, 17), ‘Whencesoever they came, the [superior] men of the British bronze age were richly endowed physically. They were as a rule tall and stalwart, ‘their brains were large; and their features, if somewhat harsh and coarse, must have been manly and even commanding.’ More recent analysis of the physical remains, particularly the skulls, suggests that few if any significant distinctions can be drawn between [Bronze Age] Beaker associated skeletons and those of the Neolithic…”

Although certain types of skulls predominate in long and round barrows, they do not, as formerly believed, show an absolute and clear-cut distinct separation. If there was some intermarriage between these two peoples there would exist to some extent offspring with either round or long skulls among both groups. We will go into this aspect of these burials below. Neil Brodie in his discussion of this racial interpretation which separated the peoples with these two skull types explains the biological foundation for the racial attitude that British scientists and prehistorians applied to the barrow skulls in the 19th and even 20th centuries:

60 Barber, *op.cit.*, p. 22
“[D. Wilson in 1851 held to] the theory of monogenism championed by J.C. Pritchard… which held that all humans were members of a single species. The distinguishing features of the various ‘racial groups, including head shape, were thought to be acquired, their acquisition being determined by their relative level of civilization. Thus it was thought that the physiognomy of people would alter in association with their intellectual facilities and their social customs’… Nevertheless, despite his evolutionist tendencies, Wilson considered that differences in the cranial form that existed between the kumbecephalic [long-headed] and the brachycephalic class was of such magnitude that it was justifiable to propose that they had originally belonged to two separate races.”

Importantly, Brodie points out that the separation of the skull types in the barrows by one of the major researchers respecting their having a chronological order was a concept made by the prehistorians and not by this craniologist:

“Throughout his discussion in *Crania Britannica* [J.B.] Davis took pains to deny that there was any evidence for a prehistoric succession of races, or at least cranial types, arguing instead that dolichocephalic and brachycephalic skulls were present in Britain throughout both the Neolithic and the Bronze Age. He supported his position by refusing to accept that the small round barrow did, in fact, postdate the larger and more elaborate Neolithic chamber-tombs.

“[Davis argued that the large, round, supposedly Neolithic, mounds were far too advanced, stating] ‘But to regard the colossal mound of Newgrange, and the elaborate galleries and chambers of Willow, Uley and other [round] barrows of this kind as the most primitive [and hence the oldest] is difficult if not impossible [to accept] unless support of other very convincing evidence can be adduced.’

To suggest that the great round mound at Newgrange, clad with a wall of whitestone that rises several feet, is more primitive than the long mounds appears to be inordinately incongruous as a chronological factor in dating these barrows. Brodie chides his colleagues for failing to reevaluate this concept for several decades in the 20th century when better techniques and knowledge were available to determine the truth or falsity of this craniological thesis:

“The idea that the human skull may be used as a stable indication of genetic distance is a legacy of the cranial studies performed during the 19th and early 20th

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62 Ibid., p. 37
centuries. It is disconcerting to discover that since then there has been no concrete programme of research initiated to investigate the veracity of this idea and perhaps to explode a myth.  

What Brodie found was that there was a trend in the shape of skulls from long to round which he attributed to one people but which evolved. He attributed the change to either cultural practices like binding infants’ heads or to natural factors. He showed that head shape varies in populations over time and that head type succession was an element of chance. In this way Brodie appeared to have saved the established chronological paradigm, or so it seemed, but at the same time he admitted:

“The results of craniological study and consideration of comparative material do not contradict the suggestion [that environmental conditions affect crania]. However, this should not be taken as confirmation of the non-existence of the [Bronze Age] ‘Beaker Folk.’ Rather, it serves to emphasize that brachycephalisation of prehistoric Britons was a biological phenomenon, and one which cannot be utilized for the investigation of an archaeological entity such as the beaker culture. Biology cannot provide easy answers to complex archaeological questions…”

“Overall, the conclusions that can be drawn from cranial study are disappointing.”

Here Brodie admits that, like radiocarbon dating without independent confirmation, or pottery sequence dating without independent confirmation, or typological artifact sequence dating without independent confirmation, or stone, bronze, and iron sequence dating without independent confirmation, skull type organization cannot be utilized to determine complex archaeological problems and by inference the chronology of these people. Related to this is the problem of why, if, as Brodie maintains, these were one people burying their dead in both round and long barrows throughout the Late Neolithic into the Bronze Age, the people supposedly stopped constructing long barrows but supposedly continued burying their dead in round ones. Keith Ray outlines this enigma:

“In an even more difficult problem than why long barrows with their massive proportions, side ditches and elaborate forecourts were built is why they went out of use. The Hazleton [barrow] sequence showed that this could happen at an early point in time. Well before the end of the fourth millennium [B.C.], many chambers had been carefully blocked and façades closed off. Could this have represented a conceptual and physical distancing of the ancestors and collective burial from the

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63 Ibid., p. 43
64 Ibid., p. 80
living? That this happened at an early stage in some areas has also been taken to mean that the importance of monumental forms, and of a particular view of ancestors as a source of legitimacy for land tenure, declined rapidly.”

Burl admits the answer to this problem is unresolved:

“Whether there was an epidemic, starvation, whether with difficulty people adjusted to even greater hardship, these are questions archaeology cannot answer definitely. But the building of the long mounds seems to have stopped around 3100 BC.”

Thereafter, it is assumed that the Neolithic Age long-headed people continued on but were often buried in round barrows. Greenwell and Rolleston write:

“The round barrows [supposedly built during and long after long barrows ceased being constructed] contain two very distinct forms of skulls, a long and a round one, together with less characteristic forms which may be supposed to have belonged to people who were descended from intermarriage between persons whose heads were of the two different types in question [the long-headed population was] probably intruded upon and conquered by the more powerfully made round-headed folk, who, as is nearly always the case, would in the course of time become intermixed with them…”

Since it is known that round barrow burials were being built at the same time as long barrows, whatever caused the people to stop constructing long barrows, whether epidemic, starvation, etc., would have also affected the people who were building round ones; they, too, should have halted construction. Because the chronology of the Megalithic Age is erroneous, it is naturally fraught with archaeological problems that remain intractable. If we are correct in moving these two peoples into post-Roman/Saxon times, this evidence should correlate with our chronology and will be discussed below. However, we maintain both peoples built their respective barrows about the same period and ended around the same time. To prove this we must first turn to France and other areas on the continent.

In France, across the English channel, people were also burying their dead in long and round barrows. But there is a singular difference between those in France

and Britain; namely, in these French barrows both long-headed and round-headed people were interred together in both barrow types! This information surprisingly was known from the very beginning by Thurnam and those who followed, but this situation or condition is very rarely, if ever, mentioned in the modern literature when it is engaged in discussing skull shapes as these relate to barrow shapes in Britain. F.W. Rudler explains that this information was well recognized quite early:

“'It remained for Dr. Thurnam to formulate the relation between the shape of the skull and that of the barrow, in a neat aphorism, which has become a standing dictum in anthropology; ‘Long barrows, long skulls; round barrows, round skulls...’ No doubt exceptional cases may occur in which round skulls may have been found in long barrows, but these have generally been explained as being due to secondary interments. On the other hand, the occasional presence of long skulls in round barrows presents no difficulty since no one supposes that the early dolichocephali were exterminated by the brachycephali; when round tumuli were in general use, the two people may have dwelt side by side, the older race being perhaps in a state of subjugation.

‘It is not pretended that Thurnam’s apophthegm has more than local application. ‘This axiom,’ its author admitted ‘is evidently not applicable unless with considerable limitations to France.’ Although it is here [in Britain] called an ‘axiom,’ it is by no means a self-evident proposition, the shape of the skull and the shape of the burial mound being purely arbitrary in France.”

In the *Anthropological Review*, vol. V (London 1867), pages 96-97 we find:

“We are at variance with the French in our estimation of the relative antiquity of the two skull forms...The evidence [in Britain] is strong that the long type preceded the round...but it was not the same in France: the two types are found together occasionally, whence we infer that the races came in contact earlier there than in England.”

But this still does not explain why round barrows in Britain were being built in Neolithic times nor why long barrows ceased to continue beyond about 3100 B.C. Ella S. Armitage adds “but in France these chambered barrows, which are both long and round, contain both long and round skulls; while in Scandinavia the chambered barrows contain only round skulls. So that the uniformity of custom

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was not an accompaniment of uniformity of race."\(^{69}\) Above and beyond this British-French connection, the *New International Encyclopedia* vol. VIII (New York 1915) page 192 offers this amazing commentary:

“It must be carefully noted at this point that in Sweden, France, Switzerland, Germany, Austria, Spain and Portugal, crania of short [round]-headed people are found mixed with dolichocephalic skulls. This tells an important story, for it clearly shows that with progress [over time] race mixture had begun to take place…Another fact worthy of notice is that [with this intermixing] the erection of huge stone and earth monuments, called barrows by ethnologists, indicates the consolidation of society.”

England evidently is the one and only exception to the rule of both peoples being contemporary across Europe. Only in Britain did these races come one after the other.

Therefore, these people have different burial customs at the same time. This not only applies to the shapes of skulls and mounds but to the types of burials. That is, they also practiced various forms of burial as Ronald Hutton points out about burials known from the Saxon period:

“What is impressive about these…discoveries is the sheer range of burial practices which they reveal. Corpses were laid in coffins, upon biers and among pieces of charred timber, and in many different postures.

“It is obvious from all this that the early English had as many different traditions of burial as the [earlier] people of the Iron Age or of Roman Britain. Indeed customs seem to have become more varied as time went on, even within the same community, until they were at their most heterogeneous upon the eve of arrival of Christianity. People who attached great importance to attiring bodies for the next world and laying them out with great care shared graveyards with those who burned their dead to ashes. Members of the same dynasty, or at least the same court, followed utterly divergent practices….

“This is all that is at present known about the religions of the Anglo-Saxon invaders.”\(^{70}\)

It is self-evident that the two peoples found in the British barrows were contemporaries. They are clearly unknown in terms of the established chronology

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\(^{69}\) Ella S. Armitage, *A Key to English Antiquities* (Sheffield UK 1897), p. 308

but in terms of our chronology they lived in post-Roman times. They should therefore be reflective of the people who lived in Britain at that period. In this respect the two people who inhabited Britain were the Germanic Saxons, who invaded the island after the Roman exodus, and the Celtic people. That being the case, they should have the same craniological features and distinctions as found in long and round barrows there. As a matter of fact that is precisely what is found.

According to Gerald Baldwin Brown, “Craniologically the Anglo-Saxons are dolichocephalic…, whereas the Celts were round-headed or brachycephalic.”\(^71\) Thus, unlike the unknown peoples in the barrows dated to late Neolithic and Bronze Ages we have known peoples with the correct craniological features found in these barrows for the post-Roman epoch. Beyond this identification we can say nothing, but this evidence does corroborate and correlate with a well known craniological chronology and known peoples. The craniological chronology espoused by proponents of it is based on unknown and unknowable peoples that have no pillars of support.

In point of fact, every one of the phenomena upon which the established chronology of the prehistoric Megalithic Age is built has failed to stand up to the evidence. In spite of these failures these phenomena are repeatedly invoked by modern researchers as a solid basis for their knowledge of that past world. Rather than having an array of substantial evidence to do that they have a veneer, a façade that they call evidence upon which they have erected with gossamer. To paraphrase Ronald Hutton on another subject which aptly accounts for this behavior we claim:

“Many people have studied and continue to study the impact of evidence as it applies to the chronology of the Megalithic age…Less frequently discussed is the impact of the established chronological paradigm upon the minds of modern historians and archaeologists. The reason is obvious enough: that in the later two-thirds of the 20\(^{th}\) century these professional scholars are generally supposed to have been immune to the lure of false evidence confining themselves to pragmatic evaluation of objective data. Such a view tends to be associated with the concept that the academy is essentially a sealed community, immune to the influence of false and inadequate forms of evidence which have a minimal effect upon it. It is supposed to educate with fact, not to inspire with a dogmatic attachment to its paradigm. This, at

least, is the ideal to which it aspires. Had that ideal been even remotely attainable then the story of modern scholarship to the chronology of the Megalithic Age would have been infinitely less engrossing, less endearing, and less sad.”72

What we have shown time and again is that historians and archaeologists have accepted data and evidence that was highly dubious, and admittedly so as if it was and is indisputable fact, and upon it built a chronology of the Megalithic Age bereft of actual scientific, technological and historical support, and worst of all educated the public to accept this. Sadly these authorities were well aware of the dubious nature of their evidence yet were willing to give it their largely unqualified acceptance. In this undertaking they have created a myth of the megalithic world that does not stand up to the evidence, nor did ever do so. These professional scholars have created a fantasy instead of reality. This, of course, they can never admit at this time, having committed themselves to that paradigm.

However, there is one other scientific form of evidence called forth as proof that these monuments should be placed in Late Neolithic and Bronze Age times; it is the queen of the sciences—astronomy—or, more accurately, astro-archaeology, to which we will turn in the next chapter.

THE GREAT MOUND OF SILBURY HILL AND THE EVOLUTIONARY DEVELOPMENT OF BRITISH FORTS

Silbury Hill is the largest man-made mound in Britain and probably Europe. It stands above a plain, some 121 feet (37 meters) high, the height of a twelve-story building, covering two hectares or five acres, and its form is that of a truncated cone. The circular base of the hill at ground level is some 148 feet (45 meters) in diameter while the truncated flat top of the mound is 98 feet (30 meters) in diameter. Importantly, in terms of chronology, it is radiocarbon dated to about 2400 B.C. In the mid to latter part of the 1800s there was a contentious debate as to whether this artificial mound was a pre-Roman or post-Roman structure. On one side were James Fergusson and his proponents who held that Silbury Hill was built in the post-Roman epoch, but the majority of antiquarians maintained that this great mound was prehistoric, that is pre-Roman in date. Fergusson had at first attempted to argue that the Roman road, which ran from east to west, must therefore have run directly across and beneath Silbury Hill and, being below it, would indisputably prove that it was post-Roman. In 1860 he wrote

“There is another indication of much more value which is that the Roman road from Bath to Marlborough, either passes under the hill, or makes a sudden bend to get around it in a manner that no Roman road, in Britain at least, was ever known to do. Unfortunately the spread of cultivation has obliterated the road for nearly a mile on either side of the hill itself, for like all the roads in the country it was neither paved nor metalled, so that no traces of its course remained when once the plough had passed over it. No one standing on Oldborough Down, and casting his eye along its straight unbending line, can avoid seeing that it runs straight at the centre of Silbury Hill. It is true, it may have diverged just before hitting it, but nothing can be more unlikely. It would have been just as easy for the Roman engineer to have carried its arrow-like course a hundred yards to the right. This indeed would have been a preferable line, looked at from a Roman point of view,—straight for Marlborough to which it was tending and fitting better to a fragment of the road, found beyond the [nearby] village of Kennet. But all this was disregarded, if the hill existed at that [Roman] time, and the road runs straight at its heart, as if on purpose to make a sharp turn to avoid it,—a thing as abhorrent to a Roman road-maker as a vacuum is said to be to nature. From a careful examination of all the circumstances of the case, the conclusion seems inevitable, that Silbury Hill [actually] stands on the Roman road, and consequently must have been erected subsequently to the time of the Romans leaving the country.”

It was a daring hypothesis Fergusson had offered to prove his post-Roman date for Silbury Hill. After all, he had claimed the original road had been obliterated by the plow on either side of the great mound, and therefore might also have been obliterated where this great mound stood before it was erected in post-Roman times. This challenge was taken up by Sir John Lubbock who investigated Fergusson’s claim and writes:

“Startled by this argument, and yet satisfied that there must be some error, I turned to the ordnance map [of the region], and found, to my surprise, that the Roman road was distinctly laid down as passing, not under, but at the side of, Silbury Hill. Not content with this, I persuaded Professor Tyndall to visit the locality with me, and we convinced ourselves that upon this point the map was quite correct. The impression on our minds was, that the Roman engineer, in constructing the road from Morgans Hill, had taken Silbury Hill as a point to steer for, swerving only just before reaching it. Moreover, the map will show that not only this Roman road, but some others in the same part of England, are less straight than is usually the case.

73James Fergusson cited in The Living Age vol. 66 (Boston MA 1860) p. 604, and in Sir John Lubbock, Pre-historic Times (see next reference, pp. 53-54)
“Mr. Fergusson admits...that the pieces of the road, on the two sides of Silbury Hill, are NOT IN THE SAME STRAIGHT LINE, so that by his own showing there must have been a bend somewhere. On the whole, therefore, I quite agree with old Stukeley, that the Roman road curved abruptly southwards, to avoid Silbury Hill, and that 'this shows Silbury Hill was ancienter than the Roman road.'”\textsuperscript{74

The reason for Lubbock’s conclusion was as Thomas Codington also reports: “In 1867, to test Mr. James Fergusson’s contention that Silbury was upon the Roman road, some sections were cut across the road, and the trenches on each side of the road were found at a distance of 18 feet apart and were traced around the hill [from the east side] to the straight portion on the west of it.”\textsuperscript{75 Thus, it appeared that Silbury Hill was a pre-Roman mound. However, Fergusson also took a look at the ordinance map discussed by Lubbock and reexamined his original contention that the road was “straight as an arrow,” but that the two sides of it not being straight in alignment clearly indicated that at some point there had to be a bend or a curve in it. He frankly admitted and explained:

“At one time I hoped that the Roman road might be found to have passed under the hill, and if this were the case, it would settle the question as to whether it was pre- or post-Roman....As traces [of the road] seemed undoubtedly to mark the existence of the road running past the hill, at about 50 to 100 yards to the southward,...the line of the road [was] considered as established. Owing to various mishaps, no plan of these discoveries has yet been published, but the annexed woodcut which is traced from the Ordnance Survey sheet, will suffice to explain its bearing on the question....This [straightness earlier presented], however, is singularly contradicted by the line of this very road westwards from the Devizes road. According to the Ordnance Survey it is set out in a curve for 3½ miles till it meets the Wands-dyke. Why this was done is not clearer than why the road should have been curved to the eastward [side of Silbury Hill] of the Devizes road. But, on the other hand, supposing the hill to have been where it now stands, and the Romans wished the road to be straight, nothing in the world was so easy for them to set out a line mathematically straight between the [eastern] Devizes road and the point where it passes the hill. The country is and was perfectly open and quite as flat as any Roman road-maker could desire, and signals [used to keep the road straight] could have been seen throughout with perfect facility. It is crediting the Roman surveyors [who set down the road line] with a degree of stupidity they certainly did not show elsewhere, to say, if they wanted a straight

\textsuperscript{74} Sir John Lubbock, Pre-historic Times: As Illustrated by Ancient Remains and Manners and Customs of Modern Savages (London 1865), pp. 54-55 (capitalization added).
\textsuperscript{75} Thomas Codington, Roman Roads in Britain (London 1903) p. 330
road, that seeing the hill [from a distance] before their eyes, they first set out their road towards it, when they knew that before they advanced a mile, they must [then have to] bend it, so as to avoid that very obstacle. Even then they would have tried to make it as straight as possible, and would have adopted the line of the present coach-road, which runs inside their line and between it and the hill.”

That is, the road was curved somewhat even before it came to Silbury, and if the Roman road builders wanted to avoid this great mound they could have still kept the road straight so that it would come closer to it. Fergusson goes on to argue:

“At the same time, if any one will turn to Sir R. Colt Hoare’s map of the Roman roads in this district…, he will find that all are set out in lines more or less curvilinear, and sometimes violently so, when any object was to be gained by so doing. Though, therefore, as a general rule, it is safe to argue on the presumption of the straightness of Roman roads, it may lead to serious error to rely on such evidence in every instance.

“The inference drawn from the piece of the Roman road further eastward on Hakpen Hill is the same. It is perfectly distinct and quite straight for about a mile, but if it had been continued in that [same] line, it would have passed the hill at a distance of at least 200 yards to the southward, and never have joined the other [distance road] piece till long after…It was deflected northward in the village of Kennet, apparently to reach the bridge, and then to join the [road] piece coming from Bath.”

In this instance Fergusson was arguing that we don’t as a fact know why the Romans built straight or curved roads. Thus, he concluded that:

“The result of all this seems to be, that the evidence of the Roman road is inconclusive either way and must be withdrawn. Taking the point where it passes the Devizes road [then curves], and the piece which is found on Hakpen hill [and then curves] as fixed points, to join these it must have passed considerably [farther] to the southward of the hill; whether it did so in a mathematically straight line or in one slightly curved, was a matter for the judgment of the surveyor; but till we know his motives it is not in our power to found any argument upon them.”

Since other Roman roads curved violently in Britain, Fergusson had an explanation for why the road did not pass under Silbury Hill, but he did not prove the great mound was post-Roman. However, the more germane question was whether

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77 Ibid., 82-83
78 Ibid. p. 83
Roman materials stratigraphically lay beneath Silbury Hill. A large such find would weigh heavily in his favor and, in fact, during the 1867 excavation to test his thesis, that is just what was found as reported by David Field who informs us that:

“One intriguing aspect of Atkinson’s and the earlier excavations at Silbury is that many more Roman and early medieval finds were produced than Neolithic…Cutting into the mound at ground level in 1867, Wilkinson found a platform just below the surface on which a pile of ashes associated with Romano-British artifacts…”79

That is, digging into the hill at ground level, a platform was found that contained ashes from a kitchen along with Romano-British artifacts! Wilkinson himself reported the Romano-British materials found beneath Silbury. Although no evidence of the road was located at two other points, the third intrusive excavation, carried out at ground level under the direction of the Rev. A.D. Smith on the 24th of October, 1867, discovered a great many such artifacts. In Wilkinson’s own words:

“The excavation continued under the direction of the Rev A.D. Smith on the 24th and digging near the section farthest to the east, the workmen found a large hole twelve feet in length by eight or nine in width [the platform]. It contained a variety of what may be considered little better than rubbish, in fact a Roman ‘kitchen [ash] midden’, but taken together, the objects are not instructive and prove some Roman dwelling place must for some time have existed in the immediate neighborhood. The following remains [beneath Silbury Hill] were found:— three small bronze coins, one of Valentinian I…one of Constans I, the other illegible; an iron stylus…part of a pair of shears; several large headed nails, and other pieces of iron; two or three pieces of fine Samian ware, and several of the softer imitation Samian; part of the rim of a fine black drinking cup of Castor…, pieces of at least eighty vessels of ordinary type and of coarse ware, all Roman including dishes, amphorae, vases, cooking vessels [etc.,] pieces of thick earthen tiles and a stone tile…two of which were used as whetstones [to sharpen knives etc.]; the broken handle of a large amphora…With these were an abundance of bones of Ox, Deer, Sheep, Horse, Boar and shells of common oyster.”80

79 David Field, “Great Sites: Silbury Hill,” British Archaeology vol. 70 (May 2003); Internet under: http://www.archaeologyuk.org/ba/ba70/feat2.shtml Great Sites: Silbury Hill
The question is: how did all this Romano-British material get into and beneath Silbury Hill in the first place? Surely it did not migrate sideways to enter the bottom of the mound.

Of course it can and will be argued that the Romans dug a large hole into the mound to dump these materials. But it is just as feasible that there was no mound then and that the Romans dug a shallow hole into the surface to dispose of the middens. Nevertheless, there are Roman artifacts directly beneath Silbury Hill and these may well have been placed in the ground before the hill was built.

However, there is another form of historical evidence, namely typology, that informs us that Silbury Hill was constructed in post-Roman time, as again reported by Fergusson:

“If, however, the Roman road refuses to give evidence in this cause, the form [typology] of the hill offers some indications, which are of value. As before mentioned, it is a truncated straight-lined cone, sloping at an angle of 30° to the horizon, while all the British barrows known are domical [domed and not flattened at the top] or, at least, curvilinear in section. In all his experience, Sir R. Colt Hoare met with only one straight-lined monument of this class, which consequently he calls the Conical Barrow. Whether it was truncated or not is not quite clear. There are bushes and weeds, growing out of the top, which conceal its form….Be this as it may there are a range of [such truncated conical hills] at Bartlow, on the boundary between Essex and Cambridgeshire, which are all truncated cones, and undoubtedly of Roman origin….The point that interests us the most is, that the angle of the Conical Barrow quoted above is 45° to the horizon, that of the principal tumuli at Bartlow 37½°, and that of Silbury Hill 30°. Here we certainly have a sequence not long enough to be quite satisfactory, but still of considerable value, as an indication that Silbury Hill was post-Roman.

“On the other hand, we have undoubted evidence that the truncated conical form was common in post-Roman time. We have one, for instance at Marlborough…Mr. George Clark, in his most valuable paper on Ancient English Castles, enumerates ninety truncated cones erected in England, he considers, between the Roman times and the Norman conquest. ‘These earthworks,’ he says, ‘may be thus described: First was cast up a truncated cone of earth, standing at its natural slope from 50 feet to 100 feet in diameter at the top [of the mound], and from 20 feet to 50 feet high’…the [typological] point that interests us here is, that there are near one
hundred [post-Roman] examples of truncated cones of earth thrown up in England after the Roman times, and not one before [except Silbury Hill in Neolithic time].”

Viollet-le-Duc describes these post-Roman truncated cone mounds as being “formed from the contents of a broad and deep circumscribing ditch” just as found surrounding Silbury Hill. That is, the post-Roman mounds were constructed in exactly the same way as Silbury Hill.

Later research led to the conclusion that most or all of these were constructed by the Normans after the British subjugation by William the Conqueror in 1066 and are known as “mottes,” which is the French word for mound. These were defensive structures put up to check the Saxons who fought back against French/Norman domination of their country. At the leveled top of the motte, a wooden castle was built in which the Norman noble who ruled this Saxon area lived protected by high wooden walls. At these heights a Norman lord could survey his domain and see his Saxon enemies as they approached his castle. We know that Silbury Hill had the same conical truncated form as other mottes, some of which were constructed by leveling the tops of natural hills, and had a wide and deep ditch around it just like moats that surrounded motte castles. But was Silbury Hill’s ditch filled with water from the first? Alfred C. Pass carried out excavations regarding this and reported:

“My excavations were commenced in the month of September, after a long continuance of dry weather, so that the adjacent little stream, the Kennet, had been dried up for more than two months; yet water continually stood to the depth of 8 feet [below the surface] in the deep holes (21 feet), sunk at the foot of the mound, and I think [the ground water] would never at any time of the year fall much below that level.

“From these results it will be seen that when Silbury Hill was first formed, it was nearly surrounded by a deep and wide trench or moat, which at all times contained a considerable depth of water.”

The wooden castle at the top had either burned or was destroyed and could not, as at nearly all such Norman sites, be found. Therefore, we have Silbury Hill constructed like all the other artificial post-Roman truncated conical mounds by being built up of earth and stone from a deep and wide ditch. The ditch, like all the

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81 Fergusson, *op. cit.*, pp. 83-84
other ditches that surrounded these post-Roman mottes was, from its inception, filled with water. Here, then, typology comes into play. Archaeologists and historians have steadfastly maintained that artifacts and/or structures made of the same materials and methods that are highly similar in form and served the same purpose must have been made around the same time. But Silbury Hill was at that time the one huge exception to this typological rule in Britain. There is the well-known adage that “If it looks like a duck, walks like a duck, and quacks like a duck, it’s a duck.” But according to the traditional/established chronology, this does not apply to Silbury Hill. Even though it is a conical truncated mound built up from materials around it, as are the other post-Roman, Norman mottes, and even though its ditch, actually a moat, was filled with water when it was first built, just like those of other post-Roman Norman mottes, it was not a motte. Pass presented his own assessment of why all this existed in Neolithic times; and it is, as will be seen, a description of a motte and nothing else:

“For what purpose was this moat [around Silbury Hill] intended? There is one reason probable, and that is for the purpose of defence. By surrounding the hill with water it could be approached only by the narrow causeway situated on the south side, and this [like other mottes] could have been stockaded as a further defence.

“My conclusions are [although this was a Neolithic mound], that the builders of this mound selected its peculiar low situation for the sole purpose of obtaining the line of defence furnished by the water in the surrounding moat, and that Silbury Hill was erected as a tribal stronghold or place of retreat and defence in case of a sudden attack by enemies”

But somehow only one other Neolithic tribe built such a mound with a moat around it for defense from attack by enemies as we will see. Therefore, in spite of the typological facts and clearly understood defensive make-up of Silbury Hill, it was not ever a motte with a moat and wooden castle; it is in other respects just like a motte but not a post-Roman one. The logical “deduction” that one can draw from such an archaeological approach to this evidence is: “If it looks like a duck, walks like a duck, and quacks like a duck, it must be a camel in disguise.”

In fact in a recent book, The Story of Silbury Hill, Jim Leary, who investigated a well known Norman motte at nearby Marlborough came to the conclusion that it too was pre-Roman in spite of all the post-Roman materials found in it. His

84 Ibid.
85 Derived from Jonas E. Alexis, In the Name of Education (Maitland FL 2007) p. 204
conclusion was explained by a piece of charcoal that was found in this mound that radiocarbon dated to the Neolithic. Therefore, it was argued that, like Silbury, “…the prehistoric mound was re-used as a castle motte.”\(^{86}\) Thus the established chronology seemed to be preserved because the archaeologists could claim that if a Neolithic mound which contained many elements of a Norman motte, had to have been reused as such in Norman times, it was still Neolithic.

The theory seems highly attractive in spite of its dependence on radiocarbon dating that Whittle told us above is not corroborated in prehistoric times by any other form of evidence. The fact of the matter is that even highly accurate radiocarbon dated artifacts for Silbury Hill and Marlborough Mound found in these hills were originally lying in the ground prior to their construction. Yet it was from this ground that these hills were piled up to form a deep and wide ditch. Whatever materials had lain in that ditch before it was dug could of course have been thousands of years older than the time the mound was constructed. The material that was radiocarbon dated could be 4500 years old but the ditch could have been dug in Norman times.

However, we still have the very improbable condition that two and only two conical truncated mounds in Britain out of 90 were the only ones erected in the Neolithic age and no others for the next 3000 or more years. Because that interpretation now permits just two such mounds to have existed, we are still faced with the proposition that only these two ducks are also really camels in disguise.

The evidence showing Silbury was a motte is well known since excavations were carried out by Richard Atkinson. He found a plethora of medieval materials below the surface, just as at Marlborough Hill, but significantly in the upper portions of it where

> “small postholes containing iron nails [were] unearthed, early medieval potsherds and a silver coin of Ethelred II dating to 1010 [A.D.]…He also found an iron spearhead…He concluded that the…terraces [of Silbury Hill now known not to exist] had…postholes.”\(^{87}\)

That is, the mound had wooden posts placed in holes around it just like other mottes. It then had wooden planks nailed to these post with iron nails to build a wall or walls.

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\(^{86}\) Internet, http://blog.histouries.co.uk/2011/06/01/ough-mound-a-sibling-for-silbury-hill/

\(^{87}\) Ibid.
Stratigraphically we have the evidence that beneath Silbury Hill Roman coins were found, while above and in the high elevations a coin of Ethelred II is dated to 1010, or about half a century before the Norman conquest. It was in the Norman era that mottes began to be built all over England. The stratigraphy shows the first people to leave material underneath this hill were the Romano-British; the Saxons also left at least a coin, and the Normans after 1066 were the ones who actually built it up.

Each facet of the evidence points to this mound being built in post-Roman/Norman times. The typological shape of Silbury Hill, a conical truncated mound, points to this. The construction, whereby material around it was dug out and piled up to create the hill, points to the fact that it had a water filled moat from its very beginning. In terms of stratigraphy there are Roman artifacts and coins beneath the hill and a medieval artifact and an early medieval coin in the upper levels of it. Both the typological and the stratigraphical evidence point in the same logical direction and correlate each other to show Silbury Hill was a post-Roman monument. To wit: the duck is a duck, and this conical truncated British man-made mound is a Norman motte as is the Marlborough Mound.

Above and beyond all this is the clear evidence that will be presented below that there is nothing or almost nothing in the ground stratigraphically that shows the Saxons left any artifacts between around A.D. 500 to 950. As will be shown, there is no evidence of farms, settlements and, importantly, defensive fortifications. All are missing. Thus we arrive at the last element of our discussion as to where the Saxon fortifications exist.

Nevertheless there are other Bronze Age, Iron Age, and Medieval fortifications that were built on natural hill tops, not conical truncated mounds, also dated to pre-Roman times, known in the literature as “Hill Forts.” The Internet Wikipedia under “Hill fort” shows that these were to some extent like mottes: “A hill fort is a type of earthworks used as a refuge or defended settlement, located to exploit a [natural] rise in elevation for defensive advantage. They are typically European and of Bronze and Iron ages. SOME WERE USED IN THE POST-ROMAN PERIOD. The fortification usually follows the contours of a hill, consisting of one or more lines of earthworks, with stockades or defensive walls.”88 We maintain these hill forts in Britain were Saxon forts used prior to the Norman invasion. They were not set in particular places as were the Norman mottes along defensive lines, but wherever a Saxon lord or king

88 Wikipedia: Hill fort; Internet [capitalization added]
ruled. They were not made of cut blocks of stone but of field stones piled one upon the other and/or wooden stockades with wooden buildings within.

When the Normans conquered Britain, they ruthlessly destroyed the Saxons who opposed them and then built defensive lines of mottes to maintain their rule. These hill forts, in our chronology, because of the few Bronze and Iron Age artifacts, came before the Norman mottes. As we were told, above, certain of these “were used in the post-Roman period” because in them there are clear-cut medieval artifacts. We, however, conclude they were all of post-Roman times. These hill forts, in our chronology, came after the Roman exodus from Britain. When the Normans conquered it, they destroyed these and built defensive lines of mottes with wooden castles to maintain their control over the country. Later these wooden structures in the motte forts, many of which exist today, were replaced with cut stone blocks. With our chronology there is a logical historical evolution of fortresses in Britain, the Saxon hill forts were conquered and then the Normans built defensive lines of mottes with wooden castles, and these were later replaced by stone. In terms of the established chronology nothing is connected. Silbury Hill and Marlborough Mound were Neolithic conical truncated constructions. Then, instead of continuing to build such mounds, the later peoples began to move to the hills where they erected hill forts. Then for several centuries hill forts failed to be constructed until Norman times. There is no clear-cut evolution of fort development in Britain, no homogeneity and no connections of one type to the others. Our chronology shows a logical evolutionary development. Each type of fort only existed for a certain time—hill forts from ca. A.D. 500–1066, from after 1066 to the 13th century Norman mottes-and-baileys which were over time replaced by castles of stone. The typology, stratigraphy and chronological evolution of these defenses all fit together into a logical sequence, something wholly lacking with the established chronology. It is plagued by typological, stratigraphical and chronological uncorrelated developments without any logical connection or relationship to one another.
CHAPTER 5

ASTRO-ARCHEAEOLOGY AND CHRONOLOGY

PART I

The final pillar of support for the established chronology of the Megalithic Age is astro-archeology. There is a vital distinction between astro-archeology and archaeo-astronomy which we wish to draw and define because of the importance we attribute to each. Archaeo-astronomy we define as a science because it entails both written descriptive records of celestial bodies and events which give sufficient data about these based on observations in the distant past upon which one can retrocalculate accurately to a particular day or even hour. The work carried out by Lynn E. Rose in volumes I and II of this series employed just this form of scientific/astronomical analysis. Astro-archeology, we maintain, is not a science because it lacks these written descriptive elements that give the vital data necessary to date these phenomena accurately; astro-archeology we define as a branch of archaeology because its interpretations are based on mute stones aligned to different areas of the horizon and sky. The investigator finding a particular set of stones assumes that they were constructed to align with, say, a position of the Sun during a solstitial rising of the Sun on the horizon in the dead past. Archibald Thom, the son of Alexander Thom, a theoretical astro-archeologist in Britain, explains that “No written evidence exists; the evidence is only that obtainable from the stones themselves.”

Thomas Crump puts the problem of astro-archeology thus:

“With so many stones it is not difficult to discover an axis, defined by two prominent megaliths, pointing in one direction to sunrise at the summer solstice, and in the other to sunset at the winter solstice. None the less, the process requires reading into the configuration of the stones a system of organization for which there is no other evidence. The only argument, which is largely intuitive, is that the monument must have had some purpose that that purpose was astronomical,

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and that any other purpose seems more far-fetched...in the absence of any other records the answer to such questions remains entirely speculative.”2

Since the evidence is derived solely from the stones themselves as well as in some cases from distant natural horizon markers such as mountains, valleys or even other standing megaliths supposedly built in the deep past, one must also assume that the position of the geographical poles has not shifted on account of a sudden Velikovskian catastrophe intervening between the present and that deep past. If, as Velikovsky maintained and as do we, such sudden pole shifts occurred around 1500 and 800 B.C., any alignments that prove the Earth’s orbit and orientation in space has remained as it is today—taking precession of the equinoxes into account—would be a devastating disproof of Velikovsky’s and our thesis. However, because we maintain that these megalithic monuments and alignments were constructed in the post-Roman/Saxon epoch, the data related to these would of course not refute Velikovsky, with the important proviso that we can prove the prehistoric chronological framework into which these alignments are set is invalid and that one least disputable, well-known alignment does conform to the chronology we have adopted.

Velikovsky in a meeting with astronomer Gerald S. Hawkins understood this problem and came to a similar conclusion as have we, namely that these monuments had to have been erected after the 8th century B.C. catastrophe which changed the position of the Earth’s axis. Hawkins writes:

“Velikovsky realized that Stonehenge worked today as it did when it was built...If it was built 2000–1700 B.C., there have been no cataclysmic axial or orbital changes. As I understand his argument...he is assigning Stonehenge to a later date, specifically one later than 687 B.C., arguing that the archaeological date was wrong, radiocarbon was unreliable, an old bone relic could have been dropped into holes during construction at a later period. Moving Stonehenge up to 600 B.C. would of course place it in the present earth-axis era, after the [last] great cataclysm. But I also pointed [out to Velikovsky that] the fitting of Stonehenge into prehistoric chronology was a specialized and difficult task and in my own research I left it to the archaeo-experts.”3

But, as we have seen above, the archaeo-experts have admitted that all the methods employed to date Stonehenge and the Megalithic Age have failed. And at

2 Thomas Crump, Solar Eclipse (London 1999), pp. 64-65
3 Gerald S. Hawkins, Beyond Stonehenge (NY 1973), p. 267
this point one rightly wonders about how astro-archaeologists can extrapolate data from these non-supportive, failed dating processes to an astronomical one? Since there is no basis for establishing an accurate chronology for the Megalithic Age, how can astro-archaeologists be certain their alignment interpretations are valid without a scintilla of corroborating or correlating chronological evidence? We submit that employing all these illusory failed forms of evidence as corroboration, one cannot logically attain a solid basis for dating astronomical events. The astro-archaeologists are dating their alignments, etc., on foundations that exist nowhere but in their minds. The astro-archaeological data is therefore correlated and corroborated by itself, that is, not at all.

The question is: does the astro-archaeological evidence clearly and, above all, scientifically date Stonehenge and the hundreds of other megalithic monuments to the Late Neolithic into the Bronze Age? We propose to show that there is no such evidence. In fact, as will be presented below, the evidence that is evoked to support the established chronology is so broad and non-specific, as well as ridden with problems, that by itself astro-archaeology is just as unreliable to accomplish this as was radiocarbon dating, pottery sequence dating, artifact typological sequence dating, Stone Age/Bronze Age/Iron Age sequence dating, bronze artifact dating, and barrow and craniology dating. In dealing with this evidence we must also examine other forms of astro-archaeological evidence than the megaliths. If it can be proved this other evidence proves the established chronology valid, then again Velikovsky and our revised chronology will lie in ruins.

ALEXANDER MARSHACK

Since Velikovsky posits that the orbit and rotation of the Earth altered greatly enough to affect the period of the Moon and the length of the year, should there exist evidence that these have been the same as they are today in prehistoric times, then that would be a strong refutation of his catastrophic thesis; a prehistoric known 29-30 day month would surely refute Velikovsky. In this regard Alexander Marshack in The Roots of Civilization maintains that is just what these early peoples expressed by carving lines on bones that reflect a 30 day month with lunar phases of the same length just as these occur today. John David North explains:

“It has been claimed that sequences of moon-shaped marks cut into bone artifacts, found from cultures as widely separated in time as 36,000 BC and 10,000 BC, represent the days of the month. The length of the month, from new moon to new moon, is approximately twenty-nine and a half days, but any primitive tally
would naturally have introduced extra days when the Moon was invisible. Since in some cases counts might have been made from the new crescent to the last visible crescent, and in others up to the next new crescent, we should not be too disdainful of the fact that groupings found on these pieces of bone, ranging from 27 to 31 [days], have been claimed as evidence of lunar counting. There is much variation in the numbers of marks in groupings that have been said to distinguish between the four quarters of the month[. new moon/first crescent to first half moon, to full moon to second half moon to new moon and again first crescent]. Such evidence is intrinsically difficult to handle, even statistically. The thesis is not implausible; the marks on these bones do often seem to have been gouged out to resemble a lunar crescent; and more than that we cannot say with confidence.”

Anthony Aveni has enlarged on this evidence, as it relates not only to astronomy but to writing and arithmetic using pictures or symbols of the Moon in its various phases, and states:

“If we accept the traditional definition of writing as an ordered set of symbols that appear on a surface, a set that could be taken to represent a tally, we can trace the written [and arithmetic] calendar back more than twenty thousand years to the last ice age. In The Roots of Civilization, Alexander Marshack has interpreted engraved marks found on bits of bone from central Africa and from paleolithic caves in France to be rudimentary forms of an early lunar calendar. The evidence lies in distinct clusterings of notches on the bones—marks that could not have been grouped together by chance…

“To the untrained eye, these marks look random, almost accidental. But by handling the objects and examining the grooves under a microscope, Marshack was able to specify the direction in which several of the proposed notations were laid down as well as the manner in which they were grouped. For example, in the bone from the Dordogne Valley [France], the marks on one part of the sequence were made with a definite direction or turn of the tool, which is apparent from the comma-shaped gouges visible under a microscope. In another part of the chain, the commas [symbols of the Moon] take on a different direction. By holding the bone tablet in his hand and changing its direction, Marshack discovered that the marks must have been laid down in a ‘boustrophedon’ or alternating left-right [then] right-left sequence…the way a farmer plows his field. In fact, Marshack deduced that the maker must have taken 24 turns to produce the 69 marks.”

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More provocatively Aveni adds:

“Most surprising of all, when he laid these gouge marks out in a linear sequence...he noticed that they could be divided into repeatable groupings. Could they have been counting the days of a periodic [lunar] cycle? Assuming each mark to symbolize a day, he fitted the pattern with its turns to a ‘lunar model’: in other words, the groups of tallies could be so arranged that the major turns in the boustrophedon notation happened about every 14 or 15 days. This interval corresponds to the interval between the first sighting of the waxing crescent moon in the west after sunset and the full moon, or between full moon and the last sighting of the waning crescent in the west.

“The motive for keeping a lunar record should be obvious...a major portion of the lunar-phase cycle provides extended light [at night] for accomplishing many useful activities. Also, it helps to plan if one knows or can anticipate when light-time will come.

“Keeping track of lunar events would offer the paleolithic inhabitants of Western Europe a means of abstractly correlating what Marshack calls ‘time-factored’ events [through a process found] in Hesiod’s Works and Days—the first step in the evolution of traditional writing, where a mark stands for a thing, in this case one day.”

How did Marshack’s colleagues evaluate his thesis? According to Aveni:

“Psychologically, it is comforting to think that writing as we know it might go back such a long way. To imagine that our earliest ancestors were abstract thinkers like us—arithmeticians, perhaps even computists—offers a broader and higher historical pyramid to support our modern accomplishments. Though his basic ideas about the beginning of the arithmetic intellect in humans are accepted by a majority of anthropologists, Marshack’s work, even after nearly twenty [now 30] years, remains somewhat controversial. Some detractors say permanent calendar keeping is not consistent with what we know about the level of intelligence of these early people. Counting the days [of the month] would have been a concept too narrow, too abstract for them to fathom. Besides, cave dwellers did not need to count days. They knew when to hunt [during the day, not at night when they were apt to be attacked by night hunting animals], when to gather, and they could certainly tell when the extended light of the moon would come, simply by spotting the first lunar crescent in the west after sunset. Why bother to write it all down [since even knowing this, the sky could be cloudy and the moon’s light obscured]? The predictive capacity implied by

6 Ibid., p. 70
Marshack’s lunar hypothesis would constitute unnecessary baggage in their seminomadic way of life (Marshack himself had never interpreted the marks to be arithmetical or predictive, only notational). Still more conservative opponents have suggested that Marshack’s bones contain no ordered pattern [of days of the month incisions] at all, that he has not provided enough examples, and that those he offers include a lot of imaginary interpretations at best. Are these marks only doodles and decorations, or were the bones only tool-sharpening devises, pure and simple? Slash marks along the edges of some of Marshack’s bones... resemble the knife sharpening grooves that can be seen all over the stone pillars…”7

In 1996 James Elkins wrote a highly critical analysis of Marshack’s work, titled “On the Impossibility of Close Reading: The Case of Alexander Marshack.” Because Marshack used a binocular microscope to analyze the bone incisions to derive his interpretations, Elkins also examined these refined slits and presented counter-evidence from these same markings. For example on one ancient ‘broken bâton’ bone from Le Placard Charente, France, he counted the number of gouged slits, but according to Elkins:

“He looks closely, literally with a microscope...at every mark on a surface or artifact, and his looking does not cease until he has satisfied himself that he has distinguished all intentional marks [made by a stone tool] from unintentional or random marks [and then] ordered the intentional marks in chronological sequence, distinguished directions in which marks were made, noted where tools were lifted from the surface and where they remained in contact, and determined how many tools or cutting edges were used to make the marks.”8

Having distinguished intentional marks from unintentional ones, Marshack made his count and arrived at roughly 27 to 30, and interpreted these as day counts for the lunar month. But the carver of the bone may have incorporated the unintentional marks as part of his count and saved himself/herself the trouble of adding these extra ones to the count or design. Therefore, the count would be greater than 30 in many cases and not reflect the days of the month. How does Marshack know this was not done? He cannot know this, but must assume as fact this did not occur. Thus, instead of a scientific testable analysis, he has an interpretative one. Even if several such bones have similar numbers of intentional and unintentional marks, this does not prove his interpretation correct. If he had several such bones from one site or nearby sites that displayed these same

7 Ibid., p. 70
systematic numbers of the intentional and unintentional marks, his thesis would be quite strong, but this is not the case. Elkins further points out:

“An initial difficulty, well exemplified in the analysis of the Le Placard Bâton, is that the ‘signs’ and ‘symbols’ have no role in the final lunar tally: instead they are picked apart into individual marks, and each mark [no matter what its size] is made equal to each other mark...It does not make sense, on the face of it, to propose that the meaning of the Placard Bâton is a series of single marks, when it consists of collected and arranged ‘signs’ of various types.”

Instead of a clear set of marks, all aligned in the same way, some lean in one direction and are not deeply cut into the bone, others are deeply and more broadly cut and lean more greatly, others are cut almost vertically to the length of the bone while others are connected and even lean in opposite directions, and some are very small compared to the rest. See figure 1 drawn from Elkins, p. 190

![Figure 1](image)

Suppose Marshack had a bone that looked like that in Figure 2?

![Figure 2](image)

In this case the numbers would be clear and the lengths of the marks would in general correlate with the phases of the Moon—the shortest lines would represent the crescent Moon, the longer ones moving to the longest would show the Moon.

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9 Ibid., pp. 191-192
waxing to the full Moon, then waning back to the last crescent and new Moon. That would be a piece of evidence that decidedly and forcefully depicts the monthly cycles. And it would be extremely difficult to interpret in some other way than the lunar cycle. While the Le Placard bâton is fairly simple, other materials presented by Marshack are extremely complex such as the rib from the Grotte de Taï with nine sets of lines and numerous marks set into them running in several directions and one set of marks altogether discounted.

As for where to begin the count from right to left and vice verse or counting up and down on this rib or others, one must assume how the count was done. When lines intersect we cannot know if they represent two marks or one, e.g., \[\text{\text{\#\#\#\#\#\#\#\#\#\#}}\]. Are these marks to be counted as 8 or 16, and what does the slanted line represent—stop counting and begin a new count or either an additional number of 1 or 2? Of course Marshack has responded to these criticisms in the same journal.\(^{10}\) While he disagrees with most of Elkins’ criticisms, he cannot and does not prove that his analysis is scientifically valid because there is no such proof regarding this interpretation. “He [Elkins] has indicated no knowledge of the Upper Paleolithic materials or their variability.”\(^{11}\) But what scientific knowledge does Marshack possess that proves his interpretations are correct? There simply is none. As archaeologist Denise Schmandt-Besserat stated, Marshack’s theory of lunar record “…cannot be proven or disproven, nor can it be ignored.”\(^{12}\)

The arguments pro and con Marshack with Elkins’s articles are interesting, but the question of these markings discussed therein are “arguments,” “interpretations” but are not shown to be “scientific,” that is, definitive proof. Clive Ruggles, regarding one of the more provocative forms of evidence that Marshack has presented, has arrived at the same problematic conclusion—namely the proof of a lunar count is simply not available and Marshack’s interpretation is fraught with problems.

Speaking of the Abri Blanchard bone radiocarbon dated to the Upper Paleolithic period some 30,000 years ago discovered in a cave in the Dordogne valley, Ruggles reports:

“…it contains a series of notched marks in a serpentine pattern. Marshack proposed that these represent a tally of [monthly] days. The assumption is that the earliest [or beginning count] marks are those in the center of the pattern [and not at

\(^{10}\) Ibid., pp. 211-214

\(^{11}\) Ibid., p. 214

\(^{12}\) Ibid., p. 186
the ends to the right or left or top or bottom], and that [ensuing] marks were accumulated [circling] around existing ones. By following the [serpentine] line outwards [from that center] and back and forth, we discover that there are about fifteen marks in each sweep before the direction changes.

“…The period of the lunar phase cycle (synodic month) is between twenty-nine and thirty days, so one interpretation of the Abri Blanchard bone is that it represents a tally in which the days of the waxing moon are marked off in one direction, and those of the waning moon in the other; in other words it forms a rudimentary lunar calendar [showing the phases of the month pictorially], maintained for about two months.…

“How can we judge a particular interpretation against the alternatives? At least one of the [serpentine] turns is not sharp, which gives greater flexibility in interpretation. Two of the lines could easily be interpreted as separate straight lines rather than part of the serpentine pattern.…Finally, although some of the [pictorial] marks appear round [as full Moon] and others crescent-shaped, there is no apparent correlation between the shape of the marks themselves and the lunar phases.

“All of these points introduce doubts in the interpretation of the Abri Blanchard bone as a lunar calendar.”

With respect to another bone fragment with its several lines and intersecting marks Ruggles goes on to say regarding its complex design: “…more complex designs give us greater flexibility in interpretation.”

Again we are faced with an “interpretation” rather than a “scientific” method that arrives at factual knowledge. Colin Wilson adds this cogent assessment:

“In The Roots of Civilization, Marshack comments: ‘Though in the Upper Paleolithic, explanations were by story and via image and symbol, there was high intelligence, cognitive rationality, knowledge and technical skill involved.’ In other words Stone Age man possessed all the abilities needed to create civilization.

“And yet, although he was poised on the brink of civilization 35,000 years ago, living in a community sufficiently sophisticated to need a knowledge of

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astronomy, we are asked to believe that it took him another 25,000 years before he began to take the first hesitant steps toward building the earliest cities.”

This very same question, we will see, arises and applies to the megalithic people of Europe who were extremely advanced according to later astroarchaeologists. Clayton Eshleman sums up the nature of Marshack’s work:

“After carefully observing an engraved bone from La Marche with a horse head, an apparently pregnant mare, and many notational marks, he [Marshack] writes:

“The Mare drops its foal in the spring after an eleven month gestation and so the mare may be a seasonal image. The associated darts and signs may then represent rites, sacrifices or acts of participation related to the time of foaling. The combination of naturalistic ‘art,’ sequences of darts and signs, and a lunar notation hints at a complex time-factored symbolism and mythology.’

“At work here is almost sheer guesswork based on a primary assumption that we are dealing with people who think like we do. The seasonal message that Marshack extracts from the composition is based on counting the notches..., and coming up with a count which he interprets as ‘a possible lunar phasing’ which ‘gives a perfect tally for 7½ months. To make a solid case for lunar phases..., Marshack would have to demonstrate repeated sets of 28 to 31 notches, representing lunar months. Such groups of notches within the ‘7½ month’ period do appear, but many other groups do too, with much larger and much smaller numbers.

“...Marshack (on the basis of portable art alone) has come up with a provocative if very questionable theory (that constantly disappears into circling generalizations in his writing) to interpret, as he puts it, “the roots of civilization.”

Eshleman here puts his finger on the fundamental flaw in Marshack’s thesis. As with radiocarbon dating and pottery sequence dating etc., there exists no scientific corroboration. The interpretation stands “alone” and isolated from other evidence. In this respect it lacks scientific support from other disciplines; thus, as evidence against Velikovsky’s catastrophes, it is without merit. In discussing any reading of symbols, such as those of Marshack, to obtain an accurate understanding of reality in prehistory, Elkins cites Barbara Johnson who calls this systematic counting

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procedure “rigorous unreliability.”17 We will expand its meaning to include astroarchaeologists interpreting megalithic structures and alignments to obtain an accurate understanding of prehistoric astronomical reality. When one has a plethora of megalithic alignments that are astronomically interpreted but are without other scientific corroboration from other disciplines, even though these may number into the hundreds, what we have is “rigorously unreliable.” As we examine the various forms of astro-archaeological evidence below, we will repeatedly discover it suffers from “rigorous unreliability.” It is never corroborated by any other form of scientific evidence, and in nearly all cases even the alignments upon which the analysis rests are fraught with problems. Having shown that radiocarbon as well as pottery sequence dating, etc., of the Megalithic Age are rigorously unreliable, let us turn to the megaliths themselves.

NEWGRANGE AND KNOWTH

“From a country home of mine near Florence I plainly observed the Sun’s arrival at, and departure from, the summer solstice, while one evening at the time of its setting it vanished behind the top of a rock on the mountains of Pietrapana about 60 miles away, leaving uncovered a small streak of filament of itself towards the north, whose breadth was not the hundredth part of its diameter. And the following evening, at the similar setting, it showed another such part of it, but noticeably smaller, a necessary argument that it had begun to recede from the tropic [its most northern point].”18

In the above description, we would class the alignment discussed by Galileo with the discipline of archaeo-astronomy rather than astro-archaeology. The reason for this is that we know where and when Galileo lived and therefore one could derive from his data the alignment of the Sun as it rose above the horizon and a fairly accurate date for this event, particularly if we knew precisely where Galileo stood. In this case the documentary evidence can be correlated with the observational evidence and they corroborate one another. On the other hand when one does not have an accurate date for the time when an alignment of the Sun or the Moon supposedly occurs, one is placed in the position of assuming both the time of the celestial event as well as which markers were utilized to measure the celestial event’s occurrence when in fact neither of these are known. The two

17 James Elkins, Our Beautiful, Dry, and Distant Texts: Art History as Writing (University Park PA 1997), p. 92
assumptions are that one has an accurate timeframe and that one has an accurate marker alignment; more precisely, they constitute two tautological arguments: the time implies the markers, and the markers imply the time.

The alignment of a celestial body or bodies, the Sun and Moon on the horizon as observed over, or between, a set of megaliths and horizon markers when retrocalculated to a past time prove the alignment is accurate.

The past time framework or chronology is proved to be accurate because the celestial body or bodies, the Sun and Moon when retrocalculated to this time frame align over or between a set of megaliths and horizon markers.

The alignment proves the chronology valid, the chronology proves the alignment valid. Q.E.D. Since the methods of dating these monuments have failed, the astronomical alignments based on this tautological reasoning are the only proof for any astro-archaeological conclusion. In this respect let us turn our attention to the great mounds of Newgrange and Knowth in Ireland to examine the evidence. Clive Ruggles has outlined this evidence along with the inherent problems for the dating of the alignment at Newgrange:

“Newgrange has been described as a tour de force in megalithic tomb architecture. The [circular] mound, carefully constructed of layer after layer of pebbles and turf, is over 80 m[eters] [260 feet] across. The fine façade that confronts the visitor today, with its high walls [of white quartz stone blocks rising from the ground] is the product of restoration following excavations by Michael O’Kelly between 1962 and 1975. This frames an entrance on the south-east side [facing the rising Sun on the winter solstice] from which a 19 m[eter—62 ft]-long passage…leads to a large central chamber…”

“Lockyer had noted in the 1900s that the passage at Newgrange was approximately aligned upon the rising sun at winter solstice. However, the true nature of the interplay between the light of the rising solstitial sun and the architecture of the tomb…was only realised more than sixty years later, when it was witnessed at first hand by Michael O’Kelly on 21 December 1969 and again in 1970:

“At exactly 8.54 hours gmt (Greenwich Mean Time) the top edge of the ball of the sun appeared above the local horizon and at 8.58 hours, the first pencil of direct sunlight shone through the roof-box [opening well above the tunnel entrance] and
[penetrated] along the passage to reach across the tomb chamber floor [downward] as far as the front edge of the basin stone in the end recess. As the thin line of light widened to a 17 cm [about 6.7 inch]-band and swung across the [end] chamber floor, the tomb was dramatically illuminated…At 9.09 hours the…band of light began to narrow again and at exactly 9.15 hours, the direct beam was cut off…For 17 minutes, therefore, at sunrise on the shortest day of the year, direct sunlight can enter Newgrange, not through the doorway, but through the specially contrived slit which lies under the roof-box at the outer end of the passage roof…

“It is this box [above the entrance passageway], and not the entrance below, that admits the midwinter sun’s light into the interior [on the solstice].”

The first problem related to this alignment evidence is that radiocarbon dating places the construction of Newgrange to about 3300 B.C. However, when the Sun rises during the winter solstice 5300 years later—being the present day—it still shines through the roof-box into the center of the tomb. It supposedly did so in the prehistoric past and still continues to do so today.

The astronomical alignment evidence required to corroborate the general date in the past for the construction of Newgrange could have, as assumed, occurred not only around 3300 B.C. but in terms of the chronology we offer about A.D. 450-900. J. Patrick, who measured this Newgrange alignment, tells us:

“It therefore seems that the Sun has shone down the passage of the chamber ever since the date of its construction and will probably continue to do so forever, regardless of secular changes in the obliquity of the ecliptic.”

In other words, one must assume the date of Newgrange’s construction as a fact to maintain the established chronology. Corroborating evidence for its dating does not exist.

It must further be pointed out that other such alignments for the Sun and Moon suffer from similar impediments in terms of dating. Aubrey Burl states that

“The targets…the sun or moon and those bodies moved so little across the skyline in the centuries between 3200 and 1000 BC [or A.D. 2000] that delicate definitions [for alignments to a precise timeframe] are impossible.”

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20 Ibid., p. 17
What astro-archaeologists possess is not a fairly accurate date for the building of Newgrange but a vast span of time when it could have been erected. The alignment tells us nothing about the chronology; the chronology tells us nothing about the alignment. In addition to this there is the nagging problem of the purpose of arranging the stones at this site; was it fortuitous or done to see the winter solstice? Here Ruggles elucidates the problem:

“The evidence at Newgrange does seem to weigh in favour of the deliberate rather than the fortuitous. First, the roof-box is an anomalous feature without any obvious function in utilitarian terms (as it seems to us). Second, if the gap in the roof-box were merely 20 cm [8 inches] lower or higher, or the [lower] passage a few metres [10 feet] shorter or longer, then sunlight would never have entered the chamber. Third, at some time after its original construction, when the bones of a number of people had been placed within the tomb, the entrance was permanently blocked with a large stone weighing about a tonne [2200 lbs.]. The roof-box, however, was only covered with two small quartz blocks which could be, and evidently were, moved to and fro to permit the roof-box to be opened and closed. In other words, the design was such that although the living could no longer enter, by moving aside the quartz blocks at the relevant time the light of the midwinter sun could be allowed to continue to do so.”

But again, how would the people at Newgrange know when to do this unless they already possessed a fairly accurate calendar which told them when to remove the blocks? Ruggles concludes:

“Nevertheless, the evidence is not conclusive. It could be that the roof-box had a function that seems obscure to us, yet was of great importance to people at the time: perhaps as an opening through which people could communicate with their ancestors, or a ‘soul-hole through which the spirits of the dead could come and go’. We must also bear in mind that we are dealing with a reconstruction; the sides of the passage beneath the roof-box needed a good deal of rebuilding and some question must remain about the degree to which the stones and corbels of the reconstructed roof-box were replaced in their original positions, and to what extent this would effect [sic] the passage of sunlight.”

Geraldine Stout gives us this insight into how M.J. O’Kelly reconstructed Newgrange:

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22 Aubrey Burl, *The Stone Circles of Britain Ireland and Brittany, op.cit.*, p. 60
23 Ruggles, *Astronomy..., op.cit.*, p. 19
24 Ibid.
“O’Kelly believed that the cairn collapse indicated that the cairn had a near-vertical face. A highly controversial wall was erected at the entrance to Newgrange designed according to this hypothesis. The wall is made up of quartz and granite cobbles found during the excavation. O’Kelly believed that the builders used selected [rather than shaped] boulders to build a revetment on top of the kerbstones creating a drum-like shape. It is highly unlikely that such a steep profile was ever maintained using a quartz revetment.”

As one can see, Michael O’Kelly, even with good intentions, reconstructed Newgrange based on making assumptions that he could not prove were correct, and it is upon this assumptive remodeling of Newgrange on which so much astronomical/chronological evidence rests. It this regard A. Whittle reports that the collapsed mounds of Newgrange and Knowth were not complete:

“The mechanics of [re]construction were certainly also effective, the structures inside both Knowth and Newgrange still being stable 5000 years later, with skilful details [in place] like provision for shedding water from the passage roofs…”

Given all these problems, prehistorians and astro-archaeologists have been attempting to piece together a chronology for Newgrange. As for dating Site I there, we are informed in the Journal of Iberian Archaeology vol. I (2006), page 146 that a “total lack of any pottery at Newgrange Site 1 might be the result of such disturbances or even robbing, although one would expect to find at least one fragment, if deliberate deposition had occurred.” Brian Haughton further shows:

“The lack of pottery finds at Newgrange is typical for passage grave cemeteries…”

Let us examine this artifactual evidence in terms of the short chronology. Since we maintain that these monuments were erected after the Romans had deserted Britain in A.D. 410 and that these megaliths were raised after that time (ca. A.D. 450-900), any artifacts brought by trade to Newgrange and other sites in Ireland should reflect this chronology.

Historians and archaeologists have typically held that sherds of pottery found at a site indicate the time around which these pots were made and in use. Rarely does pottery survive for longer than a few hundred years at sites. Coins also reflect this dating since their minting dates are nearly always known. If Newgrange and other

25 Geraldine Stout, Newgrange and the Bend of the Boyne (Cork Ireland 2004), p. 37
27 Brian Haughton, Hidden History, vols. 1-2 (Franklin Lakes NJ 2007), p. 197
Megalithic Age sites across Ireland were erected in the post-Roman/Saxon period, one would naturally expect to find just these materials there. It should be noted that Rome never occupied Ireland. In terms of this evidence Raftery reports:

“There are sixteen reliably documented finds of Roman coins from Ireland. They vary from isolated copper coins [to] the gold coins from Newgrange…and the great hoard of silver coins from the north of Ireland. The huge hoard [is] dated to the early fifth century A.D. [the time the Romans abandoned Britain]…About a dozen brooches of varying Roman types are known from Ireland…Two late Roman disc brooches from Newgrange, a widespread late Roman type, are otherwise unknown in Ireland. The Newgrange excavations also produced a variety of other metal objects of probable Roman manufacture including…an important hoard of gold rings and neck ornaments.”

Although no pottery of prehistoric date has been found at Newgrange, Roman artifacts and coins clearly were. That is, in all the thousands of years between the supposed construction of Newgrange, no one left datable artifacts there except, it seems, in late Roman and Anglo-Saxon times. To get around this anachronism Raftery further reports:

“Sherds of Samian and Aretine [Roman] pottery have been recovered from a number of native settlement sites. These distinctive pottery forms were widely manufactured and exported across the far-flung empire, and such vessels might, perhaps, have been highly prized in an aceramic society…It has indeed been agreed that in many cases the Roman pottery fragments came to the country *many centuries after their manufacture* from long abandoned villas of the vanished empire. The point may be noted but it is not proven.”

Note that this pottery supposedly was brought there “many centuries after their manufacture”. The improbability that this Roman pottery could have actually remained intact for several centuries in Britain and not utilized by Anglo-Saxons or Celts living there seems never to have occurred to those who espouse this long term survival. And why would the Anglo Saxons and Celts trade these highly desirable pots that they themselves—as well will show below—deeply prized? If this Roman pottery did not last for many centuries, which is highly probable, since pottery is prone to breaking from daily usage, then this pottery arrived at the end or

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some short time after Rome evacuated England. Raftery continues in order to explain away the chronological implications of this material:

“The significance of the Roman finds in Ireland is variously debated, and it is likely that no single explanation applies to the material as a whole.

“But trade will not account for all the Roman material in Ireland and other interpretations are sought…Precious Roman material at Newgrange could have been brought there by provincial Roman tourists, pilgrims even…the coins and the gold hoard might in such a context have been offerings.

“Other mechanisms can also be surmised, Irish raiders, returning in the fourth and fifth centuries from their attack on a crumbling empire would have brought with them souvenirs and loot from plundered Roman villas.”\(^{30}\)

But why no other forms of pottery, etc., from other periods was left there is not explained. Another form of dating Newgrange and other passage graves is to assume that these round barrows date to the Neolithic period and then say since Newgrange is round like others of its type it, too, is of Neolithic origin—the same tautological rationalization we encountered earlier. Richard Bradley and Colleen Batey do just that:

“The best indication of a chronology for these monuments came from comparison with structures in other areas, including the stone circle enclosing the passage-grave at Newgrange (O’Kelly 1982, ch. 6).”\(^ {31}\)

Thus, Clava Cairns are dated because of their similarities to Newgrange and vice versa. But neither is dated by astronomy, nor pottery sequence dates, etc.

Each interpretation of Newgrange and the other passage-grave mounds is fraught with severe problems. These interpretations are intended to make them fit into the established chronology by dating them to the Late Neolithic period. If the consensus was that these mounds were constructed around A.D. 450-900, the Roman find would of course be hailed as evidence that fully supports that chronology. It could even be argued that when the Saxons invaded Britain and drove the Celts eastward, they would have taken their Roman valuables with them. Whittle, in his usual frank manner, admits “we do not know in any detail the early chronology of passage

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\(^{30}\) Ibid., p. 179

And he further goes on to say that “It may never be possible wholly to unscramble the chronology of the early horizon of monument building.”

Nevertheless, if these megaliths had been erected much closer to the present there would possibly remain some knowledge of it in the folklore of the Irish people who had seen it in either its original state or in its later collapsed state still retaining parts of its white quartz stone wall. Elizabeth Pepper and John Wilcox discuss this folklore:

“The mound or cairn at Newgrange was originally covered with glistening white quartz…Professor M.J. O’Kelly’s wife, Claire, has written the definitive Guide to Newgrange, a detailed account of the history and new discoveries about the prehistoric monument. She recalls a phrase in the old Irish literature describing the Brugh (‘house’ or ‘mansion,’) as ‘white topped,’ ‘with the many lights’, and ‘brilliant to approach.’ Although the eighteenth-century discoverer of Newgrange could conceive no greater antiquity than that built by the Danes, Claire O’Kelly surmises that ‘The Irish storytellers knew better.’”

How could ancient Irish storytellers know that Newgrange was clad in white glistening stones over 3000 years after it had collapsed and been buried in the earth by earthworm action—something we will discuss below—unless they had seen it or heard about it from their forefathers and mothers? It is clearly unreasonable to believe that this monument could have been constructed 5300 years ago, then abandoned in 1200 B.C. and remained unburied by soil and vegetation. Once Newgrange was abandoned, vegetation would have grown on it and buried it beneath a layer of soil and it would not be recognized as a monument. In fact, its discovery was made by a workman who was asked to dig there and found beneath the sod white stones as well as carved ones. Nevertheless, old Irish literature is replete with mention of this construction. Elizabeth Shee Twohig outlines this:

“Many instances are recorded of an association between prehistoric tombs or mounds and supernatural beings or gods of early Irish mythology. The best-documented is the identification (by George Petrie in 1845) of Newgrange as the Brú na Bóinne of early Irish literature. The name (broadly translatable as ‘mansion or abode by the Boyne’) occurs in many Irish records; the earliest that survives dates to the eleventh century AD, but all have their foundations in much older

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32 Whittle, op.cit., p. 244
33 Ibid., p. 249
34 Elizabeth Pepper, John Wilcox, Magical and Mystical Sites: Europe and the British Isles (Grand Rapids MI 1996), p. 266
literature and before that, in oral traditions. Newgrange can be identified as the
dwelling place of the powerful [Celtic] god Dagda, his wife, Boann, and son
Aonghus. It has been suggested that the belief that these were the abodes of
supernatural beings may go back to the time of the construction of the tombs.

“Newgrange in particular, as the home of Dagda, seems to have been specially
revered and this may explain the deposition around the tomb entrance...of many
valuable Roman coins and Romano-British ornaments in the fourth century AD.
The nearby [passage-grave] sites of Dowth and Knowth were used as settlement
places during the early medieval period, with Knowth especially important as the
seat of the kings of the territory...”35

Apparently Newgrange was not unknown to the early Irish people because they
knew it was clad with white stones (“white topped”), was a Brugh (house or
mansion) and “brilliant to approach.” A mound covered with sod for thousands of
years does not come to be known by such recent descriptions or belong to the
pagan Irish gods.

As for radiocarbon dating Newgrange, Michael O’Kelly and Claire O’Kelly
report: “Two radiocarbon dates were obtained for the Newgrange tomb from a
putty-like mixture of burnt soil and sea sand used to caulk the interstices between
slabs forming the roof of the passage...The samples came from two separate
locations...”36 The two radiocarbon dates from a “mixture” of burnt soil and sea
sand that date this monument are poor specimens for radiocarbon dating. Soil
is not a uniform material of one age wherein the top layer is the latest to have been
laid down and the layers beneath it are older. Earthworms turn it over
continuously, bringing to the surface at night deeper particles known as castings (to
be described below). This is known as bioturbation. Soils take thousands of years
to form and thus are extremely poor specimens from which to derive a radiocarbon
date. George Robert Rapp and Christopher L. Hill in this respect admit that
“Measured 14C ages of soil organic matter are always younger than the true ages
of the soil because of continuous input of organic matter into soils. Differences in
soil carbon dynamics can result in widely different 14C dates for soils for the same
age.”37 They further state: “Geoarchaeological problems in sample collection
include: samples taken from eroded or reworked deposits, samples taken from

35 Elizabeth Shee Twohig, Irish Megalithic Tombs (Princes Risborough UK 2004), p. 12
36 Michael O’Kelly, Claire O’Kelly, Early Ireland: An Introduction to Irish Prehistory
(Cambridge UK 2001), p. 110
37 Rapp and Hill, op.cit., p. 151
deposits mixed by bioturbation or cryoturbation, geochemical contamination from a fossil-carbon source (for example, limestone or coal) and geochemical contamination from nearby organic-decay products like humus. It is critical to ascertain that no recycled older carbon is in the sample.” 38 Yet they do not show how these recycled old carbon materials can be totally isolated and removed from a specimen.

With regard to dating shells, we cited above that the effect on the dating of shells suffers from similar problems. To put it bluntly, the date of Newgrange is based strictly on the supposition that it fits the paradigm archaeologists have enshrined so that every contradiction to that chronology is removed by ad hoc inventions that are themselves not subject to scientific proof.

KNOWTH

A lunar geographer claims, as did Marshack earlier, that at the nearby passage-grave mound at Knowth certain of a set of engravings on a stone inside can be interpreted as being the number of days and phases of the lunar month. He further suggests that another engraving depicts the face of the Moon through different stages of the lunar cycle. According to Francis Reddy and Greg Walz-Chojnacki:

“Leonardo da Vinci is generally credited as being the first to portray the markings on the Moon realistically, although some small Moon images appear in several of the works of Jan van Eyck (1390-1440). Da Vinci’s chalk drawings, only one of which is known today, were made between 1505 and 1514—surprisingly recent for a depiction of an object so easily. Apart from these efforts, all known previous depictions of the Moon appear to be symbolic rather than representational.

“In 1994, Philip Stooke of the University of Western Ontario described a contender for the oldest lunar map, one that would extend the history of lunar map-making back to the...period [of ca. 3000 B.C.]. It appeared on a stone that archeologists dubbed Orthostat 47 inside the passage tomb of Knowth, located in the Boyne Valley of Ireland. Constructed some 5,000 years ago—predating Stonehenge in England and the great pyramids of Giza...

“The ‘Moon stone’ of Knowth occupies the center recess at the end of the tomb’s eastern passage. Pecked onto the stone are three long arcs, a short arc, and several circular patches, that Stooke believes represent the face of the setting

38 Ibid., pp. 151-152
Moon. The markings match the relative positions of lunar features well enough that Stooke feels justified in calling it a map. Other patterns pecked onto the same stone appear to show the changing orientation of [these] lunar features as the Moon rises and sets.”

With respect to the lunar phase calendar they further report:

“Kerbstone 52, for instance, bears a complex design that some have argued represents a lunar calendar. Investigations into astronomical alignments at Knowth have shown that at certain times moonlight could stream down the entire passage.”

What has generally been overlooked is that at Knowth as well as Newgrange cloudy weather would obscure the shining light of the Sun and Moon. Brian Haughton comments on this:

“The decoration on one of the megaliths inside Knowth has…been touted by astronomer Dr. Philip Stooke…as the world’s first map of the moon. But looking at the series of arcs carved into the stone it makes an unconvincing moon map. Based on the decoration of this stone and other motifs at Knowth, there have been claims that…the builders of this monument had unparalleled knowledge of the complicated movements of the moon, enabling them to predict eclipses and other astronomical events. In the opinion of Dr. Philip Stooke, speaking to BBC News Online, in April, 1999, ‘They knew a great deal about the motion of the Moon. They were not primitive at all.’ I think Mike Pitts sums up ideas such as this succinctly when he writes in *Hengeworld* that ‘without science like mine, runs the clear subtext, these guys were savages.’”

Dr. Stooke on his Internet site “Neolithic Lunar Maps—Knowth Ireland,” page 2 shows the phases of the Moon from Kerbstone 52; we have numbered the figures to reflect the day and phase.

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41 Brian Haughton, *Haunted Spaces, Sacred Places* (Franklin Lakes NJ 2008), pp. 9-10

42 *Neolithic Lunar Maps at Knowth*, http://www.knowth.com/lunar-maps.htm
As one can plainly see, the supposed days of the lunar month on the lower part of the drawing are all crescents but these do no grow in any way as the Moon waxes and wanes. They also point in the same direction but should, after new Moon, point opposite to each other. Then suddenly there are seven circles that all resemble the full Moon. As with Marshack’s depictions this in no way reflects the phases of the lunar month. The only evidence that supports this interpretation is the interpretation itself.

With respect to the map of the Moon’s surface features, Stooke has selected one of a set of three carvings from a figure and applied that, and only that, as strictly representative of the full Moon’s face. The other two carvings are not highly similar to the one he chose. He maintains that figure B (below) is an excellent depiction of the lunar surface when observed by the naked eye at full Moon. But the lines could be anything else or nothing. The interpretation is in the eye and mind of the beholder. Compare depictions A and C to B.
For Stooke’s full evidence see his paper on the Internet. In neither case, the phase changes of the Moon during the lunar month, nor the physical aspects of the lunar surface are conclusive. Sharynne MacLeod NicMhacha explains how and why Stooke connected these features because of his occupation as a planetary cartographer:

“In 1999 Dr. David Whitehouse, the Science Editor of the BBC, released a report about a potentially remarkable discovery. It stated that Dr. Phillip [sic] Stooke… had evidently discovered what may be a map of the Moon…Dr. Stooke is a planetary cartographer who regularly works with NASA charting the planets of outer space. His knowledge of the Moon’s surface enabled him to view an apparently meaningless or undecipherable design with a different eye and see something in it.”

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44 Sharynne MacLeod NicMhacha, Queen of the Night: Rediscovering the Celtic Moon Goddess (Boston MA 2005), p. 98
An important aspect of Knowth in terms of chronology was also pointed out by NicMhacha: “The BBC report also mentioned that certain investigations at Knowth (which were not specified) showed that, at certain times of the year, moonlight shines down the eastern passage of the tomb and may have illuminated the lunar map.” This is quite similar to the Sun’s rays penetrating to the center of Newgrange during the winter solstice. If one were to assume that the phases of the Moon are indeed represented on the carving there and that the lunar map that Stooke has presented is valid, what can one say about the chronology of Knowth? Since moonlight can possibly shine down the eastern passage and fall on the map presently as supposedly well into the ancient prehistoric past some 5000 years ago, then it could have done so between A.D. 450 and 900, and again this possible evidence in no way refutes Velikovsky’s hypothesis. For example, Philip Coppens reports at yet another site, which relates to the Coligny Calendar that we will discuss below, that

“in the Knocknarea area is a small passage cairn with a roof box. The roof box opens to the northwest and might be interpreted as falling into the same category as an identical roof box at Newgrange. But this is oriented too far north for the setting sun, and too far for the northernmost reaches of the moon, where it goes once every Metonic [19 year] cycle.

“On the 17th March, 1980, Martin Brennan and Jack Roberts saw a beam of light from the rising sun that illuminated a carved stone at the back of the rock lined passage in the great mound called Cairn T at Loughcrew. Two weeks later, on the evening of the first of April, Brennan and his colleagues watched the rising moon from the same spot. As the moon appeared over the horizon, a shaft of light was projected along the passage and onto the same carved stone. The northern and southern reach of the moon is always around the equinox…Whereas Knowth ‘might’ be aligned, Cairn T at Loughcrew definitely [today] has both lunar and solar alignments incorporated into it. It was therefore not long afterwards that Philip J. Stooke…discovered that if moonlight were to shine on the back stone of the eastern passage at Knowth, it would illuminate the map of the moon itself.”

Not only does the winter solstice sunset still shine down the passages of Newgrange, and Cairn T at Loughcrew today but it does so at several other passage graves, as pointed out by Edwin C. Krupp, a staunch critic of Velikovsky who states:

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46 Coppens, *op. cit.*, p. 108
“Several other chambered passage graves and other tombs in northwestern Europe also are aligned astronomically [as in Newgrange], for example, winter solstice sunset at Clava in Scotland, winter solstice sunset at Maes Howe (Orkney, Scotland), winter solstice sunrise at [the great cist tomb] La Roche-aux-Fées in Brittany (France), and winter solstice sunrise and the major standstill southern moonrise at Gavrinis, near Carnac, Brittany.”

With respect to Maes Howe, Haughton comments: “Another aspect of Maes Howe shared by Newgrange is its alignment, the entrance of both structures southwest, in the direction of the midwinter sunset. At the time of the Midwinter solstice the rays of the setting sun shine down through the passageway at Maes Howe, illuminating the back wall of the central chamber.”

A brief perusal of these sites on the internet will confirm that these sites today have the setting Sun’s rays on the winter solstice still streaming down these passages. So how does one prove astronomically that they date back to Neolithic times? This evidence deeply undercuts any such absolute assertions that they can be astronomically dated.

Again, not only in Ireland, Scotland, and France does the Sun at the solstice presently align itself with the passage graves of Newgrange, and the Moon possibly with Knowth, and the Sun and Moon at Cairn T at Loughcrew, etc., etc., but we find the same type of present-day alignments exist elsewhere in the megalithic world. For example, on the Island of Malta Coppens reports:

“Joseph Ellul was the son of the caretaker of the Hagar Qim, one of Malta’s most spectacular megalithic temples. Ellul was thus immersed in a life-long interest in the Maltese megaliths, which led him to observe that the main entrance of Hagar Qim was aligned with the moon at the rising position of its major summer standstill. It wasn’t until 29th June 1980 that he was able to take photographic evidence of this phenomenon, and even then persuading the archaeologists—who at the time were very much like the rest of the scientific world, i.e., not interested in astronomical alignments of ancient monuments—was an arduous task.”

Here, too, at Malta the megalithic alignments cannot only be dated into Neolithic times but also to the present day which well allows for their construction in the post-Roman/Medieval epoch.

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48 Haughton, *Haunted Spaces..., op.cit.*, p.54
49 Coppens, *op.cit.*, p. 107
Lastly, there are also the designated ‘Sundial’ slabs at Knowth that are inferred may have been used to measure the hours of the day. If indeed these sundials correlated with the same length of the day as the Earth’s position and distance to the Sun that exists today, again this would call into question Velikovsky’s thesis. Wun Chok Bong outlines these so-called sundials at Knowth:

“One of the slab carvings—Kerbstone 15—inside the mound is said to have served as a solar clock tracking the movement of the sun, showing that Knowth was an observatory….K 15 is thought to be a sundial because of the straight lines radiating from the center to surrounding elongated dots forming a semicircular pattern. The ‘dot’ is a hole carved deep into the rock surface, which is believed to have once held a pole to cast a shadow indicating the approximate time of day. Its crescent-shaped carvings may also be interpreted as a lunar calendar [with over 18 ovals [left] some of which may be broken off]. The so-called solar clock is a semicircle with two centrally placed dots or holes (one off-center). So it is highly unlikely it is an instrument for telling time [which would use only one hole to hold a shadow marker]. There are nineteen radiating lines carved into the surface…. No archaeologist has tested the Knowth ‘sundial.’ Therefore its function for recording solar movement is pure speculation. [Of another so-called sundial kerbstone at Knowth we learn:] However, Anthony Murphy, author of the website www.mythicalireland.com states that ‘this sundial will never work now because it is hidden from the sun due to an overhanging concrete ledge which protects the kerbstone from rain.’”

These supposed sundials can nevertheless be tested. One need only make a copy of them on a large flat surface and place long pegs in the holes to determine if they work properly. In this case we have a unique situation in which the instruments can be scientifically tested and should be. Let us assume that they do work properly in the present conditions of the Sun-Earth relationship. What would this prove in terms of chronology for the construction of Knowth vis-à-vis Velikovsky? Nothing, because there is no solid evidence to date Knowth into Neolithic times. Unless, and only unless one has unimpeachable scientific evidence to do so, none of the evidence at Knowth refutes Velikovsky.

50 Wun Chok Bong, The Gods’ Machine (Berkeley CA 2008), pp. 15-16
PART II

GERALD S. HAWKINS, VELIKOVSKY, AND STONEHENGE

The same argument that we raised earlier regarding Alexander Marshack’s thesis challenging Velikovsky’s hypothesis applies even more strongly to Gerald S. Hawkins’s alignment thesis for Stonehenge. Hawkins argues: “the [astronomical] values found from Stonehenge for the obliquity of the earth’s axis and the inclination of the lunar orbit about 2000 B.C….agree with modern extrapolation [into the past which shows no evidence of a poleshift]. These Stonehenge values are perhaps the best rebuttal of Velikovsky’s thesis of cataclysmic shifts in the axis of the earth in the first Millennium B.C.”51 A year later Charles M. Fair echoes this argument.

“One line of evidence crucial to Velikovsky’s theory is that relating to the arrangement of the heavens prior to the two eras of catastrophe he is tallying…Professor Gerald Hawkins…purported to show that Stonehenge was (and still can be) used to sight the sun on the morning of the summer-solstice (June 21st). More, Hawkins said a ring of 56 holes, known as Aubrey or X holes had been used to predict lunar cycles [for eclipses].”52

Here, Fair inadvertently admits to the flaw in this argument. He specifically states that not only did Stonehenge produce this summer solstice in prehistory but it supposedly “still can be…used to sight the sun on the morning of the summer-solstice (June 21).” In terms of the Sun supposedly doing this today, how does one date Stonehenge to the Neolithic and Bronze Ages? Obviously, it is the lunar alignments that correlate with Hawkins’s dating of that monument. These will be analyzed below and especially at the end of the unit which deals with C.A. Peter Newham. Fair asks: “How could Thebes as Velikovsky suggests have changed, latitude and Stonehenge not?”53 Hawkins’s criticisms have been picked up by several critics of Velikovsky. Bertram F. Wilcox, Professor Emeritus at Cornell University, in Ithaca, New York, although sympathetic to Velikovsky’s theory, is

51 Gerald S. Hawkins, op.cit., pp. 265-266
53 Ibid., p. 163
“given pause by many doubts and objections, such as the chronology of Stonehenge…” Duncan Steel claims “All you really need to know about Immanuel Velikovsky’s absurd astronomical ideas [is found in] Hawkins, G.S., in association with J. B. White, Stonehenge Decoded.”

Interestingly, Steel, at the Second SIS Conference “Natural Catastrophes during Bronze Age Civilization,” held in 1997, presented a paper “which was more speculative, although based on…astronomical data and interpretations. Steel suggested that the construction around 3500 B.C. of the Great Cursus near Stonehenge, and that around 3100 B.C. of the first stage of Stonehenge itself were intended as predictors of catastrophes, since there were the approximate times when the orbit of the giant proto-Encke comet intersected that of the Earth.” Here Steel argued, as Velikovsky had first done, before claiming that Stonehenge was built after the 8th century B.C. catastrophe, that Stonehenge had been rearranged because of changes in the Earth’s orbital parameters.

Aubrey Burl suggests the same concept: “It is believed that the catastrophe [that for a long period brought on a megalithic Dark Age] was the result of a volcanic eruption in Greenland, but, whatever the cause, its effects were felt all over northern Europe and were disastrous for the inhabitants of Britain and Ireland. Helpless in a seemingly never-ending calamity, they turned away from the ancestors they believed had once protected them and, instead, looked to the threatening skies. Stone circles were born of desperation.

“People pleaded with those skies, hoping to calm them.”

Elizabeth Chesley Baity also asks: “I wonder why megalithic observatories fell into disuse; were the astronomers who manned them the victims of conquests or of natural disasters?”

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56 Trevor Palmer, *Perilous Planet Earth: Catastrophes and Catastrophism through the Ages* (Cambridge 2003), pp. 341-342
When Velikovsky originally suggested that the changes at Stonehenge over time reflected fear of catastrophes from the skies before changing that chronology, he was mocked for doing so. Yet when others suggest that similar catastrophes caused the people at Stonehenge to change their religious and megalithic constructions, no vulgar or mocking criticism is ever directed at them.

E.C. Krupp argues “Even Stonehenge [phase] I in Wiltshire with its recalibrated radiocarbon date of at least 2800 B.C. includes astronomical alignments that are understandable only in the absence of Velikovskian catastrophes.”\(^{59}\) Krupp further argues:

“Archaeoastronomy provides us with sites like Temple Wood in Argyll, however, which antedate the Venus collision, and yet which are accurately aligned on significant moonsets, as we might expect had no collisions [posited by Velikovsky] occurred at all.”\(^ {60}\)

This evidence regarding Temple Wood was presented by Alexander Thom, whom we will discuss below, to correlate with lunar horizon alignments that were employed to date this site.

Clive L.N. Ruggles, however, in his criticism of Thom and thereby of E.C. Krupp respecting Temple Wood, states:

“[Thom’s] idea immediately runs into serious trouble because, as emerged in the reassessment by this author in 1981, no fewer than twenty-one of the forty horizon features in the dataset [he used] are not actually indicated at all...A further five foresights [used for the lunar alignments] can not in fact be seen from the backsight because of the intervention of local ground, and one is non-existent. Thus only thirteen of the forty [foresights] actually represent indicated horizon features in the first place [for alignments with lunar positions]…

“Of course, it can be argued that [these] indicators may have disappeared since prehistoric times, or even that they were never necessary anyway, since if people were using horizon features for important astronomical observations they are likely to have known where to look, and only needed the observing position to be marked. The problem is that if we simply speculate that this was the case wherever we find a promising potential foresight, then we are going far beyond what the archaeological record actually tells us….In order to test the idea that horizon


\(^{60}\) *Ibid.*
foresights were used for which no indication (now) exists, we would need a
different and much more careful methodology capable of extracting something
believable from the data and avoiding circular argument.”61

In fact, in 1983 Krupp was forced to admit that the evidence from Temple
Wood was spurious at best. Of course, he has never retracted his critique of
Velikovsky regarding Temple Wood. Krupp writes:

“The uprights near the prehistoric Temple Wood stone circle form a line of
possible moon watching stations according to Alexander Thom. More than one
‘place to stand’ [to observe these lunar alignments] is needed in order to fine-
detail the changing positions of northernmost and southernmost moonsets. The
Thom interpretation has been challenged by some who cite a somewhat similar
arrangement of stones at Barbeck just 5½ miles north. That site does not seem to
have lunar sightlines”62

He finally was forced, because of the growing evidence, to admit:

“Those who challenge the concept of precise lunar observatories doubt that the
extrapolation methods by A. Thom really work. These methods still leave many
features of the stone grid unexplained and require intricate and abstract reasoning
to put into operation the features that do work. We are left somewhat mystified,
then, by alignments that indicate moonrises and moonsets with high precision,
higher than seem possible.”63

The lunar evidence that Krupp claimed requires “the absence of Velikovskian
catastrophes” has failed at Temple Wood because, as Ruggles stated, it is
apparently not capable of “extracting something believable from the set,” and that
evidence has failed to “avoid circular reasoning.” The precision of these lunar
alignments, which Krupp hailed as a refutation of Velikovsky, he admitted was not
at all “accurate” at Temple Wood, although that accuracy was exactly what he
maintained existed there. Having failed to retract this erroneous attack we are not
mystified by Krupp’s extreme hypothesis which holds Temple Wood’s lunar positions
“which are accurately aligned to significant moonsets” are based on “doubts that the
extrapolation[s]…really work,” because they “require intricate and abstract
reasoning,” and “leave many features…unexplained.” He holds that the original
“accuracy” is now based on “possible moon-watching stations,” not on “known

61 Ruggles, *Astronomy in Prehistoric Britain and Ireland*, op. cit., p. 59
62 Krupp, *Echoes of the Ancient Skies…*, op. cit., p. 44
moon-watching stations.” Krupp ultimately goes on to state the following regarding lunar alignments for prehistoric times based on megaliths and horizon markers:

“Unfortunately, there are too many variables and actual difficulties to permit such exactitude. Prehistoric lunar observatories pinpoint the moon only when it is on the horizon, and the time of moonrise and moonset might not coincide exactly with the moment the moon reached its limit. Even if the moments of the month’s extremes occurred close to the time of moonrise or moonset, the moon itself might [not be full but could] be anywhere in its monthly phases. The moon, therefore, might rise or set during the daytime. It would be difficult under those circumstances to see it low in the horizon. Partial phases [of the Moon] also could be difficult to measure even at night, if the moon’s dark edge [which cannot be seen] happened to be at the critical spot behind the foresight. On top of these difficulties, bad weather or clouds, at least now and then, would mask the moon and make the megalithic astronomers miss a night [or even a few nights].

“Other factors would also complicate the moonwatcher’s work…. [R]efraction…does vary somewhat…and that introduces a little error into the observations. Also, the moon’s orbit is not a perfect circle. Sometimes the moon is a bit closer to us, at other times a bit farther. These variations also can affect the exact position of the moon on the horizon. Finally, the moon is not a point of light but a fairly large object…Somehow the prehistoric astronomers had to make sure they always measured the position of the same point on the moon’s disk. Failure to do so would introduce a little more error.

“All these problems are real…The only way the megalithic astronomers could have side-stepped these problems was by devising a technique that would let them figure out the true extreme position of the moon without actually seeing the moon reach it.”

All these factors, Krupp admitted, make ancient knowledge of the lunar alignments during these major moonset or moonrise positions extremely dubious. However, this again is the very point Krupp claimed these ancient astronomers knew that disproves of Velikovsky’s hypothesis. Therefore, we have rather clear-cut evidence that the Sun’s positions at the solstices, as seen in the passages at Newgrange, possibly at Knowth, at Cairn T, Loughcrew, Maes Howe, Clava, and La Roche-aux-Fées can still be observed today which allows for these monuments to have been built in post-Roman/Saxon times. We also have the Moon shine down passages at Cairn T Ireland, at Gavr’inis near Carnac, and at Hagar Qim on Malta.

64 Ibid., p. 41
which allows these monuments to have been built in post-Roman/Saxon times. Both the Sun and the Moon have alignments that place the Megalithic Age well past any of Velikovsky’s catastrophes, but of course, this will not be addressed in the published literature. Related to the failure to address this evidence is the fact that Velikovsky has written a full response to Hawkins’s critique only one year after *Stonehenge Decoded* was written in the April, 1967 edition of *Yale Scientific Magazine*. What follows is Velikovsky’s answer to Hawkins:

“Stonehenge

“In 1963 and 1964, a young and talented astronomer, Professor Gerald S. Hawkins, published two papers in the British magazine *Nature* (October 26, 1963 and June 27, 1964). The subject of the papers was developed by him in articles (*Harpers*, June 1964; *American Scientist*, December 1965; *Physics Today*, April 1966); in a book (1965), *Stonehenge Decoded*; and in many lectures before scientific societies and the public. In the 1963 article Hawkins claimed that Stonehenge, a stone monument on Salisbury plain in England, was erected for astronomical observations…and that the purpose was to watch the sun rising on the summer solstices…but he claimed further that with certain four selected points as observational stations, the extent of the swing along the horizon between the rising and setting points of the moon in summer and winter can also be followed up. Also with some additional selected points the movements of the sun could be aligned with great precision for winter solstice as well. Such a purpose is readily conceivable; the problem then is: if the ancient alignments are still valid, how could my reconstruction of past events of catastrophic nature, with solstitial sun rising points repeatedly dislodged, be true? Not a small share of the public interest in Hawkins’ theory can be attributed to this predicament.

“Before we examine 1. whether the alignments are true today and 2. whether they were the same in ancient times, I would like to present Hawkins’ view on the motives that guided the ancients in erecting Stonehenge…as Hawkins says…

“They (the Stonehengers) had the means to confirm that the sun was on course. They certainly had reasons to be vitally concerned with the observations. If the sun ever failed to turn at the heel stone at midsummer and day after day rose further to the left [of it], then intense heat and drought would surely follow. Today we have absolute confidence in the regular movement of the Earth around the Sun. (Hawkins, *American Scientist*, December 1965, p. 395

“This concern of the ancient Stonehengers is, of course, hardly understandable if past experience had given no reasons for such apprehension. This, however, Hawkins
does not consider and thus he ascribes to the ancients on the one hand very advanced ideas like building an astronomical computer (his second article and thesis), and, on the other hand, an apparently unfound fear that the sun might go out of control.”

Along these very same lines, David D. Zink explains:

“…Velikovsky, criticising Hawkins’ analysis of Stonehenge, calls our attention to an issue which has by no means been resolved. On the one hand, he observes, Hawkins asks us to believe that those who built Stonehenge could design and build an astronomical computer. On the other hand, inquiring into their motives, Hawkins explained their actions by attributing to them a perfectly groundless fear that the sun’s movement might become irregular. Whether or not Velikovsky’s scenario…is valid, his point is well taken. The immense effort expended in the construction of Stonehenge and the astronomical understanding displayed therein is inconsistent with a neurotic concern with the sun’s actions.”

Velikovsky attributed this fear on the part of the Stonehenge people to their having psychologically remembered the catastrophes which affected the Earth’s orbit and axial tilt because of his chronology. We do not attribute such a fear on the part of these ancient people because our chronology places these megalithic constructions in the post-Roman epoch. The rest of Velikovsky’s critique, however, continues:

“In his second paper in Nature (1964), titled ‘Stonehenge: A Neolithic Computer’, Hawkins claimed that the Stonehengers dug out 56 holes in a circle (Aubrey holes from the name of their 17th century discoverer) around Stonehenge in order to predict lunar eclipses. Hawkins wrote in the preface to his book: ‘In retrospect it is a conservative hypothesis for it allows the Stonehenger to be equal to, but not better than, me. Many facts, for example, the 56-year eclipse cycle, were not known to me and other astronomers, but were discovered (or rather rediscovered) from the decoding of Stonehenge.’

“A 56-year eclipse cycle was unknown to modern astronomers, but known to the Stonehengers and learned from them by Hawkins who, in order to find this secret of Stonehenge, used a modern computer.

“How important was it for the neolithic (late Stone Age) dwellers of Salisbury plain to know in advance the times of lunar eclipses? Their computer was not built to predict solar eclipses.

“I could visualize Stonehenge being an instrument which was useful for giving some warning of the danger of an eclipse,’ says Hawkins in American Scientist, and in his book he details this warning system: ‘Not more than half of those eclipses were visible from Stonehenge but the good chance that the inevitable eclipse might have been visible from England would have made it well worth while for the Stonehenge priests to use winter moonrise over the heel stone as a danger signal. Far better to call the people out for a false alarm—and then perhaps claim that skilled intercession had averted the disaster—than to fail to call them out and have the eclipse come without warning!’ (Stonehenge Decoded, pp. 139-40).

“The ancient computer could predict lunar eclipses only during one winter month, when ‘the full moon nearest the winter solstice rose over the heel stone.’ Thus, the priests of Stonehenge could not spread the alarm during the entire year—lunar eclipses may occur in any of the twelve months of the year; but in order not to compromise themselves they alarmed their congregation, even of lunar eclipses that would be visible only in the southern hemisphere, because their computer was geared for such performance: Close to the time of the winter solstice it was in working condition. The Stonehengers, apprehensive of the danger of lunar eclipses, were unconcerned about solar eclipses because their 56-hole digit computer was attuned only to the 56-year cycle of lunar eclipses, which Hawkins refers to ‘as those most frightening things’ (Stonehenge Decoded, p. 147)

“According to Hawkins no other purpose of astronomical character will be discovered in Stonehenge since he has tried out every alignment: ‘I think there is little else in these areas that can be discovered at Stonehenge’ (p. 147).

“Since there are many more holes besides the Aubrey or X ring of 56 holes (closer to the sarsen monuments are 30 holes of a Y ring and 29 holes of Z ring, and inside the ring of the monoliths there are 59 holes prepared for bluestones, from which those stones were removed) and many stones large and small, as well, Hawkins subjected all possible alignments to a computer test to seek out their possible significance in observing celestial bodies.

“‘There are so many possible Stonehenge alignment—27,060 between 165 positions—that one could be found to point to practically anything in the sky, and, vice versa, there are so many objects in the sky—perhaps literally an infinite number—that hardly any line extended from earth could fail to hit at least one’ (Stonehenge Decoded, p. 104).
“Correlations

“With 27,060 alignments in a structure designed as an observatory it is surprising to read that ‘stars and planets yielded no detectable correlation’ (Hawkins in Nature, October 26, ’63). There was ‘no significant matching with planets or with bigger stars, Sirius, Canopus, Arcturus, Betelgeuse, Spica, Vega…’ (Hawkins in Harper’s, June, 1964). Not one planet, and not a single prominent star qualified, despite so many chances. The thought must occur that Stonehenge, if it was used for astronomical observations, must have been put together, let us say, originally, under a different celestial order. I say ‘originally’ because it will be shown that Stonehenge was repeatedly reordered.

“Visiting Stonehenge in the summer of 1957, I, like other visitors, could not but be greatly impressed by the huge monoliths capped by lintels, all shaped by human hand; there is a circle of such rectangular stones and inside the circle still larger stones capped to form trilithons. The larger of these ‘sarsen’ stones weigh up to fifty tons each, and all the ‘sarsens’ were brought south a distance of 20 miles to Stonehenge. Less spectacular features, not paid attention to by many a visitor, include a circular ditch with raised banks surrounding the area in which, in concentric rings, the already mentioned X, Y, and Z holes surround the sarsen monoliths. Inside the ring of these monoliths, but outside the horseshoe-like formation of trilithons (originally five in number), there are 59 or 60 holes, some of them still occupied by ‘bluestones,’ five or so feet high and weighing four to six tons each: inside the horseshoe there is another horseshoe of bluestones. Outside the circular ditch, but actually in an ‘avenue’ formed by two parallel extensions of the ditch, stands a roughly shaped (not trimmed by hand) stone with its apex leaning from the vertical—the so-called Heel stone. It is not located centrally in the avenue, but closer to one of the side ditches. Several holes found in the avenue suggest that at various times other stones the size of the Heel stone stood in them, or that the Heel stone itself was moved from one to another of them and finally to its present position in the avenue. Between the Heel stone and the sarsen stones lies the so-called ‘Slaughter Stone.’

“It is generally believed that on the summer solstice (June 21) the sun, viewed from the central position through an aperture between two sarsen slabs, rises directly over the Heel stone; this belief also served as the initial assumption of Hawkins’ theories. However, the official guide book on Stonehenge, written by Professor of Archaeology R.J.C. Atkinson and published by the British Government, states:
“It is commonly believed that on 21st June, when today large crowds gather to see the dawn, an observer at the centre of Stonehenge will see the sun rise immediately over the Heel Stone, and that it will cast a shadow of the top of the Heel Stone on the Altar Stone. Neither of these widely held beliefs is correct. Today the midsummer sun rises appreciably to the left of the Heel Stone, and when Stonehenge was built it rose even further to the left; it will not rise over the Heel Stone for more than a thousand years.’ Atkinson is the recognized authority on Stonehenge.


“Atkinson accused Hawkins of being very inexact with figures and measurement. Instead of making measurements on the spot, Hawkins used two different maps, one of them by Atkinson, which, as the latter stressed, was never made for such a purpose, being intended only to show the approximate positions of the stones and holes, ‘wholly inappropriate as a basis for accurate measurement.’ The other map comes from “a now obsolete Ministry of Works plan for earlier editions of the official guide. Further, Atkinson stresses that even then Hawkins permits himself an inadmissible tolerance of two degrees of arc in accepting non-alignment as perfect alignment. He does this ‘in spite of the fact that 2º is equivalent to about four diameters of the sun or moon’ whereas with a pair of sticks the rising or setting of the Sun can be fixed within “repeatable limits of 5 minutes of arc” or 24 times more accurately. ‘Translated into practical terms, it means for instance, that the Heel stone could be moved 12 feet to the north-east without affecting Hawkin’s [sic] claim.’

“Sunrise

“Hawkins says, ‘we have no record of what the ancients took to be the instant of sunrise. Was it the first gleam or the moment when the whole disk stands on the horizon?’ (Nature, 1963) Feeling free to select either one or the other, he mostly choses [sic] the complete emergence of the disk in fixing the rising point on the horizon, but occasionally half the disk, and then also (for 2000 B.C.) one full diameter above the horizon (Stonehenge, Pl. 18). This is hardly permissible: on the solar solstice the sun rises obliquely, and when it is in full view its lower limb is not even approximately where its upper limb is when the first ray of sunshine appears: in one instance, incidentally, Hawkins refers to a 2º displacement of the sun along the horizon during the time of emergence.
“Contrary to that assumption that the ancients have not left any tradition for what they regarded as the rising moment of the sun, we have records, from many ancient civilizations—Egyptian, Hebrew (Temple of Solomon), Mexican—that the shining forth of the first ray of the sun was the moment. The heliacal rising of a star, important in the reckoning of the so-called Sothis period in Egypt, was defined by the moment the first ray of the sun showed up.

“The Temple of Jerusalem was so built that on the two equinoctial days the first ray of the rising sun shone directly through the eastern gate.’ (Worlds in Collision, p. 318 with a reference to the Tractate Erubin of the Jerusalem Talmud).

“Atkinson showed by a number of examples that Hawkins in obtaining supposedly significant alignments for the moon and the sun, made “inadmissible” claims. Thus of eight alignments claimed for Stonehenge III (one of the several periods during which the monument was taking its shape) ‘four of them fall outside Hawkins’ own arbitrary limits of error; two more involve fallen stones; and one would almost certainly have been blocked by the Slaughter Stone when upright.’ Especially offended is Professor Atkinson by Hawkins’ claims based on Bernoulli’s law of statistical chance. ‘The probability quoted is wrong; the method of testing the hypothesis is wrong; and the restriction of the possible sight-lines...is wholly inadmissible.’

“The final blow came when it was shown that the 56 year cycle of lunar eclipses, first allegedly discovered by the Stonehengers, does not exist in nature. Yet this was the only basis for identifying the 56 Aubrey holes and with them the entire Stonehenge complex as an ancient computer. ‘Such eclipses repeat every 65 years (in periods of 19, 19 and 27 years) and not every 56 years (19, 19, and 18 years) as claimed by Hawkins’, write R. Colton and R.I. Martin in Nature for February 4, 1967, in a paper titled ‘Eclipse Cycles and Eclipses at Stonehenge.’ They also produce a table of eclipses for the last hundred years to demonstrate the true cycle. ‘The Aubrey holes at Stonehenge were not constructed to predict eclipses on a 56 year cycle.’

“Thus of the entire theory not one thing is left.”

For an in-depth analysis of Hawkins’s failure as well as that of Sir Fred Hoyle and C.A. Peter Newham to date Stonehenge via astro-archaeological evidence, see Peter Lancaster Brown’s Megaliths, Myths and Men. We will deal with Hoyle and Newham in the following units. Velikovsky’s analysis of Hawkins’s thesis

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67 Velikovsky, op.cit., pp. 24-25
68 Lancaster Brown, Megaliths, Myths and Men..., op.cit., pp. 100-161
based on astro-archaeological interpretations cannot be used to date Stonehenge; not only is it uncorroborated by any other form of scientific evidence, it is riddled with problems and contradictions. It is not even ‘rigorously unreliable’ but simply “unreliable.” And, as we will see below, radiocarbon dating and Hawkins’s alignments are in total contradiction to one another.

What attracted Hawkins and those who followed in the same astro-archaeological tradition after him was the fact that these theses had what appeared to be the trappings of science placed in an archaeological and chronological framework. The problem with all the astro-archaeological theses that came after Hawkins is that they suffered from the same defects. While they appeared “rigorous” in terms of astronomy, the scientific trappings—when later examined by other researchers—simply fell to pieces. To paraphrase Duncan Steel: “All you need to know about Gerald Hawkins’s absurd astronomical ideas is to be found in Velikovsky’s response and in all the work that followed.”
PART III

STONEHENGE ACCORDING TO HOYLE; LUNAR AND SOLAR ECLIPSES

Sir Fred Hoyle, who had been invited to evaluate Hawkins’s theory by Glyn Daniel, the editor of the journal *Antiquity*, presented his own ingenious solution for how these ancient people could have predicted eclipses based on the very same 56 Aubrey Holes. Hoyle’s solution, like that of Hawkins, nevertheless suffered from errors that precluded it as well. As we noted above, Velikovsky pointed out in the *Nature* (February 4, 1967) paper by R. Colton and R.L. Martin titled “Eclipse Cycles” that “The [56] Aubrey holes at Stonehenge were not constructed to predict eclipses on a 56 year cycle.” Lancaster Brown showed that Colton and Martin:

“…first took [the] opportunity [to] point out the false reasoning relating to the 19-year Metonic cycle being a hitherto unrecognized cycle as Hawkins claimed. Several writers had already referred to the fact that there is no true commensurability between the 18.61-year nodal swing [of the moon’s orbit] and the 19 year…phase cycle [when the Moon returns to the same position and phase as it did 19 years earlier]. Damningly Martin found that Hawkins had overlooked the fact that the moon is not in direct opposition (180º) to the sun after successive periods of this alleged cycle.”

This means that the Moon would not be at, or near enough to, the proper node [the point where the Moon and the Earth’s orbits intersect] to be eclipsed by the earth’s shadow or to eclipse the sun.” Colton and Martin noted that the 19-, 19-, 18-year (=56) sequence suggested by Hawkins is actually 19, 19, 18 years plus 11 days. As an eclipse ‘cycle’ it is very short indeed because there is no way to compensate for the error in the two 19-year periods.”

Hoyle, however, determined a solution that compensated for this which we will discuss below.

What he did was completely change the computing system of Hawkins to one which organized the way these Neolithic people interpreted the sky and applied that understanding to the Aubrey Holes. He set up three markers that were to be moved around the 56 Aubrey Holes to correlate with the yearly positions of the

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69 Lancaster Brown, *op.cit.*, pp. 149-150.
Sun along the ecliptic, and with the monthly positions of the Moon whose orbit is tilted to the ecliptic by about five degrees, and with the precession of the lunar nodes over the 18.61-year cycle. In this respect he had the two planes set in space for the Sun and the Moon’s orbits connected to the 18.61-year nodal cycle organized by the three markers set at the proper places on the Aubrey holes. Michael J. Crowe explains how Hoyle’s computer works:

“If we place a marker in Aubrey hole #10 and move it counterclockwise at the rate of 2 holes every 13 days, it will move through the 56 holes in \((13 \cdot 56/2 =)\) 364 days or almost exactly one year. This marker will consequently effectively represent the position of the sun [on the plane of the ecliptic]. Place another marker in alignment with the moon’s position relative to the ecliptic and move it counterclockwise at the rate of two holes per day. It will complete the circuit in \((52/2 =)\) 28 days or about the sidereal [stellar precise] period of the moon. Place a pair of markers opposite each other, say at [Aubrey] holes #18 and #46. Move each marker clockwise at the rate of 1 hole every four months. Each will complete the circuit of 56 holes in \((56/3 =)\) 18.67 years. If once aligned with the lunar nodes, these markers will come very close to preserving that alignment.

“Consequently, whenever the sun and moon markers are simultaneously near [1 or 2 markers from] the nodal markers, an eclipse is very possible.”\(^{71}\)

The difference between how the markers could be understood to predict a possible lunar or possible solar eclipse is explained by Jonathan Irving Lunine and Cynthia J. Lunine:

“When the Moon and the Sun [markers] are on opposite sides of the [Aubrey] circle, and less than one or two Aubrey Holes away from the node stone[ markers], a lunar eclipse would occur; when the Moon and Sun stones cross each other and are less than one or two Aubrey Holes away from a node stone [marker], a solar eclipse is predicted to occur.”\(^{72}\)

They go on to show however: “Because none of the solar, lunar, or nodal cycles are exact multiples of the 56 holes, the counting rules are not exact. The marker

\(^{71}\) Michael J. Crowe, *Theories of the World from Antiquity to the Copernican Revolution* reprint (Mineola NY 2001), p. 211

positions would need resetting regularly by sighting the Sun and the Moon in the sky at key times of the year."\(^7^3\)

The numbers which Hoyle derived from his mechanism were problematic. The length of the computer year was 364 days, not the correct number of 365.25 days. Thus every year his computer would be off by 1.25 days and in 10 years the marker for the Sun would be 12.5 days away from its proper Aubrey Hole, in 100 years 125 days off. As for the Moon, the length of the computer month was 28 days not the correct number, 29.53 days. Thus every month Hoyle’s computer for the Moon would be off by 1.53 days, in 10 months 15.3 days, or 15.3 days away from its proper Aubrey hole. Such a computer will not compute eclipses unless critical adjustments are continually and regularly made to correct these erroneous solar and lunar marker positions; since the Sun marker will be off by 1.25 days each year, one must create a method to correct this error once a year. That correction, according to Hoyle, could have been made at Stonehenge at the summer solstice. By precisely dating that solstice, every year the true length of the year could be computed from 364 to 365.25 days, and the Sun marker moved forward 1 day for 3 years and 2 days the next leap year. But how does one make this precise correction? Edwin Beggs \textit{et al.}, have outlined how Hoyle envisioned this:

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“Now we introduce [Hoyle’s] celestial corrections…To the Northeast of Stonehenge I [the first phase of construction] there is a 5 meter [16 foot] tall stone called the Heelstone. In the morning of the Summer solstice the sun…raises slightly to the north of the Heelstone. To know the exact day of the Summer solstice we wait for the day when the sun rises behind the Heelstone. The sunrise should then proceed north for a few days, and then back south [to again rise behind the Heelstone]. We count the number of days between the first sunrise behind the Heelstone and the second sunrise. The day of the summer solstice happened in the middle of these two events. With this information we can calibrate the second token to enough precision every year, so that Stonehenge I can predict eclipses indefinitely.”\(^7^4\)
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This process of dividing the times between the first and second appearances of the rising Sun behind the Heelstone by two, Hoyle called ”halving.” But how accurately could ancient man have measured those two alignments with the naked

\(^7^3\) \textit{Ibid.}, p. 12
\(^7^4\) Edwin Beggs \textit{et al.}, “Oracles and Advice as Measurements,” \textit{Unconventional computation, 7th International conference, UC 2008, Vienna, Austria, August 2008}, Cristian S. Calude \textit{et al.}, eds. (Berlin Germany 2008), pp. 36-37
eye? In order to do this, one requires an extremely accurate point at which one must stand at both times. According to Ruggles,

“…the observing position to be used from one day to the next [or from one sighting to the next, days later] would need to be specified to within a mere one or two centimetres, less than the distance from one eye to the other. While it has been suggested that the Heel Stone might have served to determine the solstice exactly by a process of halving the difference, even this would have required an observing position precise to 10 cm [4 inches] or so.”75

Consider a man making the first observation of the Sun as it moved behind the Heelstone and then coming back day after day to see when it would return. If he moved from the measuring position by four, five, six, or even seven inches, from where he made the original one, he would not have an accurate time separation to halve. It might be up to three or four days off either way. If it was, say, two days off, then the three markers would not properly work to suggest an eclipse might occur because the Sun’s position on the ecliptic would be slightly ahead or behind the proper one. The correction of the Moon marker, from its computed 28-day position to its 29.53-day position, requires an adjustment on a monthly basis or moving its marker forward an additional six positions every other month, which might have been conceivable. But we still run into other problems.

At Stonehenge in southwestern Britain, there are clear days and nights as well as cloudy and rainy ones. If around the time of the solstice the weather is rainy and cloudy, the first sunrise or the second behind the Heelstone, or even both, could have been missed by a few days. One would have to guess the date or wait a full year to correct the Sun’s marker position. But during that year, because the Sun’s marker is off by as much as three or four days, predictions of eclipse would be in error or highly unlikely. The same applies to correcting the Moon’s marker adjustment. If there is a long period of overcast days at the critical times adjustments were needed, it is highly problematic that eclipses could be predicted.

But how do we know that these Neolithic people set up Stonehenge as an eclipse computer in the first place? Surely we need some form of corroboration, and that is the problem; there is none. In this respect Stonehenge is the one and only megalithic monument in England, Ireland, Scotland, and Brittany that is built with 56 markers of whose function we have only Fred Hoyle’s assertion of its astronomical eclipse capabilities. Castleden explains that “in 1987, Aubrey Burl

75 Ruggles, *Astronomy in Prehistoric Britain and Ireland*, op. cit., p. 38
pointed out...that the whole hypothesis depends on there being fifty-six Aubrey Holes. Yet if that number had been regarded as significant or even useful for eclipse predictions, then it would recur in other comparable monuments with rings of pits of varying numbers...As Burl says, ‘the Aubrey Holes were not components of a computer.’”

To get around this problem that Stonehenge’s 56 Aubrey Holes are unique, Lancaster Brown submits:

“The lack of other supporting archeological evidence of numeracy among the Stonehenge peoples is not conclusive proof that they did not predict eclipses. Indeed it is my own belief that Hoyle, when he embarked on the Stonehenge problem, had firmly in focus the methods practiced by the nineteenth-century [A.D.] astronomers of southern India who, although lacking a profound mathematical knowledge, could predict eclipses in a pragmatic fashion by manipulating groups of shells placed on the ground before them.”

What Lancaster Brown has failed to recognize is that India in the 19th century already had a long history of astronomical development compared to the Stonehenge peoples of the Neolithic Age. That long developed knowledge would have enabled one of them, just like Hoyle, to organize a positional arrangement of objects that could be manipulated to predict eclipses. Furthermore, the 56 Aubrey Hole marks were not used by the Tamil astronomers. Stonehenge was unique in this respect, as North points out:

“Dr. Euan MacKie...draws an analogy between them [the Neolithic Age astronomers] and the procedure which enabled a Tamil calendar maker to make a mental prediction, accurate to four minutes, of an eclipse in 1825, without really appreciating the initial theory [behind it]. The analogy is one which should not be pressed very far, if we are to keep a proper sense of perspective over the different orders of theoretical structure underlying the ancient and the not-so-ancient routines”.

The battle lines between Hoyle’s belief that Stonehenge may well have been an eclipse computer and the archaeologists were drawn. R.M. Corfield describes the battle:

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77 Lancaster Brown, *op. cit.*, p. 138
“Hoyle’s findings led him to conclude that the Aubrey Holes at Stonehenge comprised a Neolithic orrery [a mechanism to show the organized positions of bodies in the heavens] that kept track of the positions of the sun and the moon…as well as the major axis of the moon’s orbit. Hoyle had gone native; the Plumian professor was a Stonehenge astroconvert…Hoyle…only further incensed the [archaeological] establishment. British archaeology, it seemed, suffered a catastrophic failure of imagination when faced with the prospect that Neolithic humans could comprehend math that the archaeologists could not master. Hoyle, however, was a scientist and was not interested in the bleatings of numerically challenged arts graduates; he responded like the typically bluff Yorkshireman he was: ‘It is not speculation to assert that we ourselves could use Stonehenge to make eclipse predictions. We could certain do so without making any substantive changes to the layout. While this does not prove that Stone Age men did in fact use Stonehenge for making eclipse predictions, the measure of coincidence otherwise implied could be quite fantastic.’

But Hoyle’s conclusion, based on probability, that because he, a highly trained mathematical astronomer could make the 56 Aubrey Holes work as an eclipse computer, does not mean Stone Age people did so even, though Hoyle maintained “the measured coincidence otherwise implied would be quite fantastic.” The fact of the matter is that with all his mathematical expertise Hoyle never calculated the probability of his assertion; the reason being that such a calculation, according to Chippindale, was impossible!

“The key question is, then, how ‘fantastic’ is the possibility of that coincidence?

“Propositions of this kind are impossible to judge statistically when only a single site is involved, and it puts them into a kind of limbo as attractive conjectures uncertainly capable of proof or disproof. The fact that a modern astronomer thinks Stonehenge could be used today to observe minute solar variations or to make eclipse predictions is beguiling but separate from the prehistoric question of whether it was [as a provable fact] used or designed deliberately with that purpose in mind. Here the uniqueness of Stonehenge is a severe obstacle, because it makes Stonehenge-specific astronomical schemes anecdotal in character. Statistical doubts make it very hard to assess their validity on the evidence of Stonehenge alone—and, because they are Stonehenge-specific, their validity elsewhere [e.g., among Tamil astronomers] is irrelevant.”

80 Christopher Chippindale, “Philosophical Lessons” …, *op.cit.*, p.76
The mathematical/statistical point Chippindale makes elsewhere is that: “A hypothesis developed on one set of data should be tested on another independent set; but unfortunately no duplicate Stonehenge exists to test Hawkins’s and Hoyle’s ideas about it.”  

Hoyle raised the probability as a mathematical/statistical method to prove that it could not be “coincidence” that the 56 Aubrey Holes fit his thesis, because it “otherwise implied” a probability that was “quite fantastic.” But as a mathematician he should have known that the probability of his hypothesis could not be mathematically/statistically determined. Perhaps that is why he never attempted to compute it. Hoyle, whose very raison d’être is mathematics, nevertheless pointed out to Velikovsky, whom he met at Princeton, the dichotomy between his standard of evidence and that of Velikovsky’s, in his autobiography:

“Velikovsky used to come to talk at the tea intervals that preceded the [Princeton] seminars. I managed to convey to him that our ground rules were different from his. He believed in the primacy of documentary evidence, whereas we believed in the primacy of mathematical rules, rules that enabled us to predict, with a high degree of accuracy, where and when the next total eclipse of the Sun was going to occur. This made Velikovsky look sad, which is how we parted.”

Both Hoyle’s description of Velikovsky’s ground rules and his reliance on mathematics, particularly at Stonehenge, are false. Had Hoyle actually read Velikovsky’s books, he would have known that Velikovsky maintained that scientific/mathematical evidence must overrule documentary evidence. In Ramses II And His Time, pages 212-217, Velikovsky discussed the length of the reign of Ramses II and his age at death. As we pointed out in volume III of this series, he maintained that the documents that were used to suggest that Ramses II reigned a long time and that he died at an advanced age in his late 80s or early 90s, were not to be accepted because the forensic anthropological/scientific evidence proved otherwise. Velikovsky wrote on page 217 “Between a figure on a document and anatomical expertise, it is always the latter that carries the greater weight. Would a Scotland Yard anatomist certify the age of a dead man—or a living one for that matter—on the basis of state of ossification or on the basis of a date on a wedding certificate?”

This shows that Hoyle, like so many other scientists who criticized Velikovsky, never read Velikovsky. His accusation, based on his ignorance of it, is yet another footnote in the Velikovsky Affair. That is what is truly sad about such an eminent

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man’s criticism. As to the second aspect of Hoyle’s ground rules where he maintains primacy is to be given to mathematics at Stonehenge, Chippindale stated that because Hoyles’ statistical argument could never be determined by the application of mathematics, his probability argument was simply “irrelevant.” That also goes for Lancaster Brown who raised this issue. In the journal, Archaeoastronomy, he explains that an “author who supports Hoyle’s idea of ‘pragmatic’ methods of Stonehenge eclipse prediction by a glib reference to the nineteenth century…Tamil case is thus irrelevant to the question…”83 The American Science Annual 1987 states: “Hoyle argued that since we could use Stonehenge for eclipse prediction, it would be fantastic if its builders had not done the same. That is tempting—but is actually irrelevant.”84 Hoyle’s mathematical/astronomical computer that predicts eclipses at Stonehenge is thus known to be “irrelevant,” and when a great mathematician/astronomer creates a mathematical argument that cannot be mathematically computed, that too is simply “fantastic."

Yet that is not the end of the problems respecting the matter. Neither Hawkins’s nor Hoyle’s computers actually predict all or even most of the eclipses. Ruggles states “Hawkins’s scheme only successfully predicted a fraction of eclipses; Hoyle claimed that his own method would have successfully predicted about half.”85 This, in retrospect, highlights the difference we outlined between archaeoastronomy and astro-archaeology, namely documents which Hoyle did not have to corroborate his eclipses. In the words of Ruggles: “Where historical or written records exist they give us concrete information about particular eclipse observations and their social impact. But we must beware of letting our eagerness to find notable accomplishments back in prehistory lead us to invent archaeological evidence that is simply not there.”86

The irony of Hoyle’s supposition is that given his splendid education in 20th century astronomy and mathematics he could easily have discerned that the farming and herding people at Stonehenge, lacking all this advance knowledge, were incapable of doing what he could do. It seemingly never occurred to Hoyle or Hawkins or the rest of the astro-archaeologists that since the discovery of Stonehenge about 1000 years ago, the best educated among them failed to see how simple it was to use the monument to predict eclipses and that only he and Gerald Hawkins, both trained

85 Ruggles, Ancient Astronomy, op.cit., p. 363
86 Ibid., p. 384
mathematical astronomers, were capable of recognizing this. Why didn’t anyone else over a millennium realize this, since it was so obvious? Nor did Hoyle or Hawkins ask why so many stone rings in Britain fail to employ 56 as a number when these circles were constructed. The obvious answer is that Hawkins and Hoyle brought their 20th century knowledge of astronomy to Stonehenge and imposed it on the monument.

In this respect Derek York offers what he conceives to be the “Achilles’ Heel” of Hoyle’s approach:

“The…flaw in Hoyle’s thesis, which in my opinion is…fatal…, is that while Hoyle’s recipe for moving the counters could be followed by any child who could count, the underlying theory was not available to the Stone Age Britons. In the presence of our [advanced] knowledge and education the theory is simple and could be explained to most high-school students. But in the early Britons such a state of sophistication is too much to accept. It requires an understanding of the relative motions of the sun, moon and earth. It is asking too much that people who did not leave a written record [such as the ancient Babylonians] had developed [a way to record and apply these celestial phenomena].”

Evan Hadingham puts the problem thus:

“Fred Hoyle…proposed the most complicated of all the theories about eclipse predictions at Stonehenge, demanding an ability to handle and record five-figure numbers, long division, and fractions. Although the Stonehenge people were as intelligent as ourselves, it is surely implausible that they could have developed such advanced arithmetical skills.”

North presents the problem from yet another vantage point:

“Eclipses, especially of the Sun, but also of the Moon, are difficult to predict without a tolerably extensive knowledge of astronomical theory, unless it is somehow appreciated that after certain periods of time the eclipse cycle tends to recur. And ‘tends’ is the right word. Both Professors Hoyle and Hawkins decided that the Aubrey holes told their user that there was a strong likelihood of an eclipse at certain times. Now neolithic man might conceivably have formulated general rules concerning eclipses on the basis of eclipses he had actually observed, but he would have been hard pressed to make generalizations from essentially

87 Derek York, In Search of Lost Time (Bristol UK 1997), p. 15
88 Evan Hadingham, Early Man and the Cosmos (Norman OK 1984), p. 72
undetected ‘danger periods’ [when eclipses are predicted but not observed]. How, then, did he proceed? Neither author explained the problem from that end.”

What Hawkins, Hoyle, and proponents of their theories are doing is projecting the modern knowledge onto a people living in impoverished simple conditions and saying these people developed mathematical and astronomical knowledge that took the rest of humanity, in terms of the established chronology, about 2000 years later to discover. Because these people could and supposedly did accomplish these feats, Hoyle, in the words of Lancaster Brown, suggested:

“The upshot of it all, Hoyle conjectured, was far-reaching. It not only required Stonehenge to be designed and built to operate as an astronomical device, but the consequences of his idea demanded a level of intellectual attainment for its builders much above that believed standard among a community of primitive farmers. ‘A veritable Newton or Einstein must have been at work—but then why not?’ wrote Hoyle.”

The point is that both Newton and Einstein had been schooled, their knowledge and understanding of astronomy and mathematics did not develop in a vacuum while the Stonehenge people, based on the established chronology, supposedly acquired this knowledge in an astronomical, mathematical and illiterate vacuum. North puts the case for the tradition of learning and mastering these disciplines into the past thus:

“...to read Einstein is to recognize a debt to Newton, and to read Newton is to be so often at only one or two removes from Ptolemy. And from Ptolemy to Hipparchus and the Babylonians is a step so short that no one could possibly dismiss it out of hand..., only the uncomprehending are likely to be left without a strong feeling of real continuity with the past,...one based on material every bit as enduring as potsherds.”

North elsewhere tersely sums up the concept that Stonehenge was used as an eclipse computer:

“All interpretations of Stonehenge as an eclipse computer are based on the idea that the counters (whether Aubrey holes or stones) were for keeping track of cycles of eclipses that had somehow been previously appreciated. [But none] have any serious regard for what was known of the history of computational techniques.

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89 John David North, Stars, Minds and Fate... (London 1989), p. 12

90 Lancaster Brown, Megaliths, Myths and Men..., op.cit., p. 147

91 North, op.cit., pp. 33-34
that would have been required. There is no historical evidence whatsoever for this approach to eclipse calculation. From a single location only about half of all eclipses would have been observed, and no explanation was offered of how any simple theory of eclipses could ever have been developed in the first place.”

We request the reader to recall our discussion of solar eclipses in volume II of this series, pages 166-183, wherein it was shown that it was impossible even for the Babylonians to predict such dramatic events. We cited Otto Neugebauer, who informed us: “It is now evident that even three centuries after Thales [6th century B.C.] no solar eclipse could be predicted to be visible in Asia Minor—in fact not even for Babylon.”

The obvious fact that the Babylonians, highly advanced in mathematics and astronomy, could not predict solar eclipses even in the mid-first millennium B.C. after a long period of observations, though having written records, never seems to have dawned on Hoyle, etc. He submits that Neolithic people could do without written records what took the Babylonian centuries with written records.

We maintain that by moving this megalithic monument and the others into Anglo-Saxon times in Britain that “continuity with the past” will connect it with the later medieval period that followed it which we will describe below.

However, let us assume that Hoyle’s eclipse predicting mechanism at Stonehenge is valid, does it refute Velikovsky? It can do that only if there is evidence—scientific evidence—to corroborate the dating of the Megalithic Age. As we have demonstrated, such evidence does not exist. It exists only in the minds of archaeologists and astro-archaeologists, and as such is “irrelevant.”

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PART IV

C.A. (PETER) NEWHAM: THE MOON, AND CHRONOLOGY AT STONEHENGE

Peter Newham was analyzing Stonehenge’s astronomical alignments prior to and around the same time as Gerald Hawkins. When he finally obtained a particular set of alignments for the Moon that agreed with the time line that he believed was the proper date of the building of that particular phase of Stonehenge, he submitted it to R.J.C. Atkinson. According to Lancaster Brown:

“Atkinson, however, was in general accord with all the ideas and advised Newham to seek publication, suggesting Antiquity as the appropriate journal.

“An outline communication setting out his results was duly submitted to Glyn Daniel, editor of Antiquity, along with a query whether Daniel would be willing to accept a more extended treatment. Newham sat back to await reaction. But for two months Newham’s communication apparently languished on the editor’s desk. Then, at last, in March Newham received a letter rejecting it. In reply Daniel wrote that since he was not an astronomer, he did not properly understand it—but in any case he did not believe it to be suitable material for his journal. What is still not clear is whether or not Daniel, while fearlessly admitting his own inability to judge its astronomical merits, had [even] submitted it to a referee who could assess it.”94

Newham felt his work was rejected because he was not a qualified astronomer. How was he able, then, to present it to the scientific world? This is reminiscent of Velikovsky who was also faulted for writing about astronomy without credentials. The answer came when a friend, who was the scientific correspondent of the Yorkshire Post, took note of the work and presented it there on March 16, 1963, seven months before Gerald Hawkins’s paper on such alignments appeared in Nature.95 Again note Newham’s work first appeared in a newspaper and not a peer reviewed journal and received no attention from the authorities because it appeared in a local and not a national newspaper. In a nutshell, if the establishment bars one’s work and it nevertheless is published, it will not be

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94 Lancaster Brown, Megaliths, Myths and Men, op.cit., p. 105
95 Ibid.
noticed and properly evaluated. When Atkinson spoke in *Nature* about Hawkins’s work which became widely known, he did not mention the fact that he was privy to Newham’s work that related to Stonehenge’s alignment. Atkinson did apologize to Newham pleading he did not mention him because the interviewer, when he had told him about Newham, did not raise the issue of priority. Yet Hawkins was given the acclaim in spite of the fact that he was pursuing “the same basic ideas as Newham.”

Like Hawkins’s, his work involved the Aubrey Circle and a particular hole nearby designated G2. Lancaster Brown explains:

“But hole G was not the fundamental issue raised by the [*Yorkshire Post*] article. What it set out to show was that the [four] Station Stones [which are slightly offset in the Aubrey circle and form a rectangle] could be used to indicate several alignments—particularly lunar ones.

“Back in 1846, the Rev. E. Duke had discovered that the north mound Station Stone ([no.] 94) lined up with [stone number] 93 at the last light of the setting Sun on the shortest day…Conversely, it was noted that a line that extended from the South Mound (92) to stone 91 aligned with the rising Sun on the longest day of the year…

“Newham had discovered an equinox alignment from [stone] 94 to stone hole C [outside the Aubrey circle], but he also noted that a line extending from [stones] 94 to 91 appeared to coincide with the point on the horizon where the Moon rises at its most southerly point during a nineteen-year cycle. Conversely, the line from 92 to 93 marked the moonset at its most northerly setting point. Newham believed that these two alignments were significant by the curious fact that the main sarsen circle is about 1 m (3 feet) out of the true centre with the Aubrey holes. Had they been concentric, the 92-93 sight line would have been obscured. This off-centre puzzle of the monument had long been a contentious discussion point among archaeologists.”

Using Station Stone alignments, Duke had aligned these to the summer and winter solstices, and Newham then aligned these to others, deriving the most northerly and southerly points of the Moon’s positions during its 19-year cycles, all in agreement with the radiocarbon dating of that time. On top of these significant alignments Newham found Mound 94 (stone) aligned with another hole, G, that marked the rising Sun on the winter solstice, while Mound Station Stone 92 aligned to G marked the moonrise at its most northerly point. He could also align the equinoxes from Stone 94 with stone hole C. Therefore Newham’s alignments

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96 Ibid., p. 109
97 Ibid., pp. 105-107
marked the sunrises of the winter and summer solstices, the vernal and autumnal equinoxes and the moonrises during its most northerly and southerly points during the 19-year cycle, all again in agreement with radiocarbon dating.

It must, however, be pointed out that Newham’s G2 hole was quite probably not man-made but the hole left by a tree or large bush that had been removed. Nevertheless, his achievement seemed to solidly support the concept that the first phase of Stonehenge had been erected to plot the movements of the Sun and Moon rather precisely. Yet there was further evidence to corroborate Newham’s analysis:

“Independently, Newham and the French architect G. Charrière—who had also made a study of Stonehenge—had noted that the ‘rectangle’ formed by the four Station [stones] corresponded almost [exactly] to the latitude (within a few km) required for azimuths of the Sun and Moon to be separated by 90° at their extreme declinations. Therefore it seems that the Stonehenge latitude (57°2 [N]) was a deliberate choice by the builders. Thus its location had been dictated by astronomical requirements rather than by availability of stones or other factors favoured to suggest the choice of Salisbury Plain. The evidence provided by the geometry of the four Station [Stones] is perhaps some of the most convincing of all to bolster the astronomical idea for Stonehenge.”

On the other hand, Lancaster Brown tells us “Only one of the four Station [Stones] (93) still retains a stump of stone defining its exact position, the exact position of stone 94 is uncertain, stone 91 has fallen, and 92 is known only by its stone hole.” Therefore the exactness of Newham’s alignment to the radiocarbon dating he used is somewhat in question. Only a slight change of inches would require that all these alignments did not necessarily date to the period Newham had defined for these alignments. In addition to this is the question of the date for precise equinoctial moonrises in the 18.61 year cycle. This significantly affects the date one assigns to the equinoctial alignments. Michael J. Crowe describes that when these points are reached the Moon stays at or very near to them for “a number of months”:

“…an important phenomenon…is known as a lunar standstill. This term refers to the limiting values of angular distance of the moon from the celestial equator…. [it] is evident that when the moon’s nodes coincide with the equinoxial points, the moon can reach as far as 28.5° (23.5° + 5°) from the celestial equator, or it can come as near as 18.5° (23.5° - 5°) to the celestial equator. When the moon’s nodes are not at the equinoxial points, its angular distance from the celestial equator will

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98 Ibid., p. 141
99 Ibid., p. 46
always be less than these extreme values. [However,] when the moon reaches these extreme values from the celestial equator, it tends to do so over an extended period. In other words, when during one lunar month the moon has reached this extreme value, it will continue to come very close to it over a number of succeeding months….The continuation of this phenomenon over a number of months [makes the Moon appear to stand still].”

Today, astronomers can distinguish the precise points and times when the Moon reaches these standstill points even though it stays quite close to these points for months. The problem is that ancient people did not have telescopes or other mechanisms to pinpoint these positions, nor clocks with which to make precise measurements when these occurred. No one has explained how these ancient astronomers ascertained these values in the first place so that they could then lay out the Station Stones to supposedly do just that. Beyond that, this necessary data is related to the timing of the eclipse cycles. As Crowe further explains, “the relevance of all this is that claims [by Gerald Hawkins and Sir Fred Hoyle] have been made that the megalithic builders were intent on observing these extreme points and that a knowledge of the period separating these standstills would help them predict eclipses.” The implications of this evidence are clear: since these early people using naked eye observations could not distinguish with pin point accuracy the Moon’s major and minor standstill positions or the precise time between them, they could only guess as to these. But Newham, Hawkins and Hoyle suggest that they did so without telescopes or decent time pieces. This, we submit, is extremely unrealistic.

The negation of Newham’s and Hawkins’s lunar alignments turned out to be the general radiocarbon date around which they organized these alignments. When Newham and Hawkins retrocalculated these radiocarbon dates, they had not been calibrated with tree-ring dates. While at that time their alignments did correlate with these early radiocarbon dates and everything seemed to fit and hold together neatly, if these original radiocarbon dates were to be moved significantly their alignment suppositions would not be accurate. While they wrote in the 1960s and thereafter, it was this calibration reassessment developed in the 1990s which exploded their alignment theories. That is, there was only a moderate leeway of perhaps around 200 years either way into which Hawkins’s as well as Newham’s alignments could be fit. If it was found that the Phase I construction which included the four Station Stones had to be moved 500 or more years either way,
then radiocarbon dating, which they accepted as valid, invalidated their astronomical alignments. Before there could be real corroboration, both sets of dating methods had to lie significantly close to one another to remove any doubts. This turned out not to be the case. Rosemary Hill reports just what was found by the announcement made in 1995 by:

“...the collaborative project that became Stonehenge in Its Landscape: Twentieth-century Excavations by Rosamund Cleal and others....It included the latest attempt to establish an exact chronology. This was done using carbon dating techniques and probability theory combined in OxCal, a software program developed at the Oxford Radiocarbon Accelerator Unit. The results were dramatic. They set Stonehenge back in time another thousand years.”

Castleden outlines the implications that these new radiocarbon dates had upon the theses of Hawkins and thereby of Newham:

“The astronomy of Stonehenge has been seriously jolted by new knowledge about Stonehenge. This has happened partly because more radiocarbon dates have been obtained for the monument and partly because the dates have been recalibrated [by tree-ring dating], which has made them earlier. Hawkins, for example, discusses the way the eclipse predictor worked in 1554 BC, basing his analysis in Van den Burgh’s Eclipses -1600 to 1207. He was looking at second-millennium BC astronomical events when he should have been looking at events of the late third millennium for Stonehenge [for phases] II and III and the late fourth and early third millennia for Stonehenge I....It looks as if the Aubrey Holes were dug in about 2800 BC and filled in shortly afterwards, some of them being obscured by the later Station Stones. The sunrise positions...calculated for 1554 BC would not have been valid for 2880 BC.”

Chippindale puts Hawkins’s and Newham’s dating vis-à-vis radiocarbon thus:

“The radiocarbon dates move most [Stonehenge construction] features by, in many cases, as much as 500 years: the initial elements move from about 2500 back to 2950 BC; the sarsen circle and trilithons from about 1900 BC back to 2400 BC; the bluestone horseshoe from about 1300 BC back to 1900 BC. These many large moves dislocate and reset also all chronological equations.”

102 Rosemary Hill, Stonehenge (London 2008), p. 183
103 Castleden, The Making of Stonehenge, op.cit., p. 27
104 Chippindale, Stonehenge Complete, op.cit., pp. 220-221
Not only do the ancient astronomers lack telescopes and accurate clocks by which to determine equinoctial moonrise points, but the Moon’s rays still shine down the passage of Cairn T at Loughcrew and at Malta down the passage at Hagar Qim, which suggests that these could have been built far closer to the present, but now it is argued that Hawkins’s and Newham’s lunar alignments are contradicted by radiocarbon dating. This is not corroboration or confirmation and these alignments are not supported by any other form of correlation from any other dating methodology. They are assumed to be valid based on the assumption that they are valid. Duncan Steel, who in general terms accepts the findings of Newham, Hawkins and Hoyle, sums up the nature of the evidence: “I BELIEVE that the INTERPRETATION of Newham, Hawkins and Hoyle is broadly correct…”105 Here in this abbreviated sentence Steel admits he only BELIEVES that Newham, Hawkins, and Hoyle are broadly correct, because he has no proof and substitutes “BELIEF” for the required evidence. Instead of evidence he accepts they are broadly correct in terms of their INTERPRETATIONS, again because he has no evidence to prove these INTERPRETATIONS are valid. Belief and interpretation is all that exists as Steel’s proof, or that of anyone else.

PART V

ALEXANDER THOM’S ALIGNMENTS

The same argument that used to supposedly refute Velikovsky vis-à-vis Hawkins’s astro-archaeological alignment evidence has also been raised by other critics who turn to the work of Alexander Thom, a professor of engineering at Oxford University. John David North asks: “How he [Velikovsky] would cope with the more accurately and closely argued work of Alexander Thom, done over a much wider area [of the megalithic landscape than Stonehenge], I cannot easily imagine, although Dr. Velikovsky is no stranger to ad hoc hypotheses, and no doubt some of the faithful are at this very moment rewriting Thom.”106 In our response to North,

105 Duncan Steel, “Before the Stones: Stonehenge I as a Cometary Catastrophe Predictor” Natural Catastrophes During Bronze Age Civilizations: archaeological, geological, astronomical and cultural perspectives, Benny J. Peiser, Trevor Palmer, Mark E. Bailey, eds. (Oxford UK 1990), p. 34 (Capitalization and emphasis added)
106 North, Stars, Minds and Fate..., op.cit., p. 40
who is now deceased, we will not have to rewrite Thom because North’s colleagues in the field of astro-archaeology have analyzed Thom’s work and while admiring his great efforts, have concluded his alignments cannot be verified and/or are so laden with problems as to cast serious doubt on their validity. North was well aware that his later work showed just that but never referred to this evidence as it related to Velikovsky. In a somewhat hypercritical vein Lancaster Brown argues:

“Velikovsky proposed that two major catastrophes occurred sometime in the past owing to the Earth’s dynamic interaction with a comet [Venus] and then Mars. The comet encounter…supposedly took place about -1500…, the Mars encounter supposedly took place -687. In both encounters Velikovsky claimed that the direction of the Earth’s axial spin was switched plus the angle tilt of the axis itself, resulting in a major change in the obliquity of the ecliptic.…”

“If Velikovsky’s ideas are correct the proof would be forthcoming in megalithic monuments, constructed before c.-1500 which would not now show positive alignments to the Sun and Moon…In attempts to counter the damning positive Stonehenge alignment evidence Velikovsky claims the monument was erected later than -687. He maintains that radiocarbon dates are completely unreliable, the archaeological data wrong and he is utterly convinced that the artifacts [such as pottery, etc..] from Stonehenge which have been dated as belonging to the early second millennium might easily have been placed there afterwards.”\(^{107}\)

Elsewhere Lancaster Brown argues

“In respect to the equally damning evidence provided by megalithic monuments Velikovsky claimed (shades of Fergusson c. 1870) that Stonehenge and other monuments were erected later than 678 BC in spite of the [radiocarbon] supporting calibration [from tree rings].”\(^{108}\)

One should note that Lancaster Brown has argued that he knows that the accepted and established chronology is correct without giving various forms of scientific and/or archaeological proof to support his assertion. He does not mention the fact that A. Whittle, cited above, has admitted, for dating Stonehenge and the Megalithic Age, that the ability of “radiocarbon dating on its own to solve [chronological] questions is now doubtful…and it is no use pretending like the emperor to be clad in sumptuous clothes that we do not possess,” for dating Stonehenge and the Megalithic Age. This, however, clearly agrees with Velikovsky who “maintains that radiocarbon dates are

\(^{107}\) Lancaster Brown, *Megaliths, Myths and Men…, op.cit.*, pp. 262-262

completely unreliable.” Nor does Lancaster Brown mention the fact that Whittle, above, on pottery and artifact archaeological dating, calls it “a methodology based on typology, a chronology imprisoned in tautological reasoning,” because, “in the absence of…(independent) criteria…the chronological ordering of materials rests on assumptions, indeed on intuitions whose scientific foundations remain unverifiable.” This also supports Velikovsky who claims “the archaeological data is wrong.” And, lastly, Lancaster Brown does not report the fact that Julian Thomas had described “a rich pottery sequence running from the Neolithic to the Early Bronze Age…appears to demonstrate the contemporaneity of a number of different styles of Beaker pottery and indeed other traditions,” and that archaeologists maintain that they somehow were still being made at, or later transported to, Mount Pleasant via trade after they had been manufactured. That is, to some extent they were placed at this site afterwards, as Velikovsky maintains. Lancaster Brown was alive and active in this field of archaeological research when this information was made known. He was either ignorant of this research or indifferent to what the great authorities stated. Yet he was willing to go on the record and pontificate on the evidence in the face of these authorities’ statements. This abject ignorance is well expressed by Vernon L. Grose whom we now paraphrase:

The chronology of the Megalithic Age must be protected from well-meaning but misguided archaeologists, anthropologists, and astro-archaeologists who fail to realize the limitations of their inquiry. They too often suffer from the temptation of over-extending their assumed knowledge, thereby unwittingly succumbing to the same delusion that ignorance brings to anyone—‘they know not that they know not.’

In the journal Centaurus, vol. 34 (1991), page 189, an academic contributor who esteems Thom and sees his work as a thorough invalidation of Velikovsky writes that Alexander Thom deserved and received “generous…praise [as a] well-deserving scholar. Thus Alexander Thom is championed and Immanuel Velikovsky is trounced…” As we will show below, the authorities who reexamined Thom’s work now deny its scientific validity!

Thom’s work has been championed by his son, Archibald Thom, and others. They, like Marshack and Stooke, have interpreted evidence from an astro-archaeological perspective, especially from a gold plaque found near Stonehenge, to provide evidence that Thom’s alignments there correlate with it and thus must be correct because of their correlation. The object is known as “The Bush Barrow Gold Lozenge.”

In *Antiquity*, Archibald Thom and his colleagues outline this corroboration in a paper titled “The Bush Barrow Lozenge: Is it a Solar and Lunar Calendar for Stonehenge?”

“Until now it has been assumed that the [Bush Barrow] lozenge was intended to be only decorative. We hypothesize that it was carefully fashioned for use at Stonehenge by an engineer–surveyor–astronomer–priest, as an aide memoire for a calendar. By fixing the flat lozenge on a plane table at eye level and orienting it with its shorter diagonal on the meridian [of longitude facing north], an observer could use an alidade-type instrument while watching the sun’s centre at sunrises or sunsets throughout the year. The pattern of intersection points at the ends of the zig-zag arrangement of scribed lines is suggestive that it was intended for practical use. Sight lines radiating outwards from the focus over these intersection points could have been used to fix dates at 16 epochs in the 16-month calendar [of Alexander Thom]. Some indication exists that four such lines could have been used for adding the inter-calary day. Eight additional lines are identifiable as indicating lunar standstill risings and settings...

“The average discrepancy of the solar calendar lines is $0.36 \pm 0.11$ days; the average discrepancy of the angles involved is $0.58$ degrees (about the diameter of the sun). Astronomical [retro-]calculations show that the calendar was made about $1600 \pm 150$ years BC.”\(^1\)

Note their dating contradicts radiocarbon dating. But let us examine this device. The evidence they supply in this paper divides the year into 16 months, aligns the lozenge to Stonehenge for the summer and winter solstices, the vernal and autumnal equinoxes as well as with the quarter-days that fall half-way between the solstices and equinoxes, and more. After presenting evidence for these alignments for a Stonehenge calendar they claim:

“The authors hypothesize that the Bush Barrow Lozenge was a ‘text-book’ for making the calendar. If angles to the required degree of accuracy could be engraved in gold, in a manner that would allow them to be retrieved when required, then a permanent record would exist. And exist it did!...

“Because of the high degree of symmetry achieved, it is likely that the designer intended the lozenge to be engraved with perfect symmetry about its axis.”\(^2\)


\(^{2}\) *Ibid.*, p. 496
In their “Conclusion,” Thom et al. outline the meaning of this evidence for Alexander Thom’s analysis at Stonehenge.

“Incomplete as this investigation is (many more markings are unexplained), it shows that the lozenge could have been used to record, with a high degree of accuracy, angular measurements which could have been retrieved subsequently without recourse to writing…”

“Oriented with its shorter diagonal on the meridian, the 18 lozenge lines show the calendrical 16-month division of the year.

“Four lines at equinoctial solar rising and setting could well have been intended to guide the calendarer as to when to insert the intercalary leap day.

“Eight lines can be identified as indicating major and minor lunar standstill risings and settings.

“Sufficient information is present to make it unlikely that so many of the astronomically related azimuths which Alexander Thom showed were incorporated in [other] contemporary megalithic sites can have been engraved on the lozenge entirely by chance…”

“The lozenge conforms in size to similar instruments such as astrolabes developed in a later era.

“When the date was known, the sun could have been used to find true north.

“An engineer–surveyor–astronomer provided with the lozenge could have built, for a site in the latitude of Stonehenge, a megalithic sun calendar and corrected it to the ‘ideal’ [proper alignment] by trial and error in a far shorter time than he would have required if he had had to work with nothing more than an oral tradition of how to do it. As a sun-moon calendar the lozenge provides material for study of the makers’ ability as mathematicians, astronomers, goldsmiths, draughtsmen and craftsmen…”

“The conclusion is that the lozenge is suitable for practical work.

“John Ker believed that the lozenge provided the only indication from an external contemporary source of the correctness of Alexander Thom’s solar calendar.”

\[112\] Ibid., p. 501-502
Figure 5 is a representation of the Bush Barrow lozenge

This beautifully designed parallelogram found in a tomb near Stonehenge appeared to offer an excellent form of corroboration for Alexander Thom’s analysis of the Stonehenge monument. If all the alignments did fall correctly on the monument as they appeared on the lozenge, then there could be little doubt that the instrument duplicated in miniature form the correct layout of Stonehenge and Thom’s findings there. The sought-after corroboration needed for the monument had supposedly been found. It was somewhat similar to finding a written record that explained how Stonehenge could be, and/or had to be, utilized. No other theory to explain this monument had anything like this for support and/or corroboration.

Ruggles, however, outlines the evidence against the alignments calculated by Archibald Thom et al. as they relate to astronomical alignments at Stonehenge.

“The Bush Barrow is one of a number of spectacular burial mounds in the vicinity of Stonehenge… These round barrows, the burial places of prominent Bronze Age chieftains in the Wessex region were [supposedly] built around 2000 B.C.E., several centuries after the main construction activity ceased at Stonehenge…”

“Rich assemblages of grave goods accompanied the chieftains to the afterworld. One of the most impressive was the Bush Barrow lozenge, a magnificent diamond-shaped plate of thin sheet gold 18 centimeters (7 inches) across. Finely decorated with distinctive patterns of incised lines, it is generally interpreted as an ornamented breast plate—an imposing mark of status. In the 1980s, Archie Thom and two colleagues claimed that the lozenge was a sophisticated astronomical observing instrument. By holding the plate horizontally and lining it up in the correct orientation, the various markings could have been used to indicate the sunrise and sunset positions on significant epoch dates in the ‘megalithic calendar’ that Archie Thom’s father,… Alexander Thom, had proposed. It could be used in a similar manner to determine significant rising and
setting points of the moon. The claim appeared to vindicate his father’s theories [that these people were astronomically and mathematically sophisticated].

“But attractive as the idea seemed, problems emerged when it was examined in detail. For one thing, there would be various practical difficulties using such a device, not least in determining its correct orientation. The most serious problem, however, is that the directions supposedly marked by the patterns on the lozenge do not really fit very well. Several of the alignments actually fall between the markings, while many of the markings do not fit any of the alignments at all. The fact that the markings actually form a regular and symmetrical design (while the astronomical targets are not regular[ly placed]) argues strongly in favor of their being purely decorative rather than astronomically functional. And as if this were not enough, other lozenges exist in nearby burials with a similar form of decoration but different [alignment] dimensions. Why should only this one function additionally as a calendrical device.”

John David North now comes into the picture as Ruggles further explains:

“By the 1990s it had become clear that the other evidence supporting the idea of a ‘megalithic calendar’ did not stand up to critical evaluation. However, the most direct blow to the calendrical interpretation…was delivered, ironically, when the historian John David North attempted to interpret the lozenge independently as part of his own astronomical interpretations of prehistoric monuments and artifacts in southern England. Vehemently criticizing the existing astronomical interpretation, he proposed an equally complex but entirely different one of his own, thereby showing how easy it was to do so and in the process undermining confidence in both theories.

“The example of the Bush Barrow lozenge demonstrates very clearly the dangers of trying to mould the evidence to fit a favored theory rather than letting the evidence speak for itself.”

There is a further glitch with Thom et al.’s paper as well as that of North, where Thom et al. write: “The authors are of the opinion that the thin piece of wood [backing] and the lozenge were originally flat (and not domed) so that the alidade could be used [to be correct alignment].” The great error in using the lozenge to interpret astronomical alignments was that it originally was not flat but domed. Burl explains what had happened:

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113 Ruggles, Ancient Astronomy…., op. cit., pp. 52-53
114 Ibid., pp. 53-54
115 Thom, et al., Antiquity, op. cit., p. 493
“…at Stourhead. In 1878 the entire collection [from its place with the lozenge] was loaned to the Wiltshire Archaeological Society…Then came the irony. In 1922 [it] was loaned to the British Museum for ‘safe keeping.’ Despite the confidence [it would be protected as it originally was found] the lozenge suffered ‘reconstruction’ even though ‘no approval to smooth out [flatten] the object was made and no permission given.’

“In 1984–1985…the lozenge…‘was irreversibly restored’ [and flattened] by the British Museum…

“There was a double irony in this misguided reconstruction. The elaborately radiating lines of the lozenge were interpreted as a hypothetical astronomical aide-memoire for a solar and lunar calendar ‘to fix the dates at 16 epochs in the 16 month calendar’ at the latitude of Stonehenge. For if this idea had been true ‘the authors are of the opinion that the thin piece of wood and the lozenge were originally flat (and not domed) so that the alidade could be used.’”

It was assumed that the wooden backing and the gold lozenge were both flat. That was an awful mistake because “The lozenge plate was recognized as being domed”. Thom and his colleagues by using a flattened lozenge to construct their calendrical alignments naturally held it flat such that it gave them values that agreed with their hypothesis. This only goes to show that they had mistakenly read into the lozenge what was not there, but only what fit their hypothesis. They found what they sought—not what was actually there. As we will see, Alexander Thom’s work itself is subject to similar forms of criticism, namely seeing what is not there. This we maintain is the problem inherent in astro-archaeology; it is the mind-set that holds our modern knowledge of astronomy that was not known to early prehistoric people. Nevertheless, Alexander Thom’s work is significantly deeper and broader than that of Hawkins, Hoyle and Newham, as Chippindale admits:

“A more productive approach—at least, one that seems more amenable to statistical study, and perhaps even to decisive testing—is Alexander Thom’s study of Stonehenge in the context of a wider megalithic science…The Thom scheme is wider in two distinct senses: it incorporates three complementary elements, adding exact measurements and exact geometry to exact astronomy, the three making up

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116 Aubrey Burl, Prehistoric Avebury, op.cit., pp. 263-264
117 Susan M. Pearce, “The lozenge plate was recognized as being domed.” Archaeological Curatorship (Washington D.C. 1990), p. 110
an intellectually coherent megalithic science; and it finds those same three skills displayed in several hundred other megalithic sites.”118

Thom’s vast research encompasses not only Stonehenge but hundreds of other monuments which all seem to exhibit very precise alignments to the Sun and Moon. He further analyzed these monuments as they relate to the calendar. He concluded the ancient people divided the year not only into four parts covering the winter and summer solstices and the vernal and autumnal equinoxes, but the quarter days—the mid-points between the solstices and equinoxes that divide the year into eight parts—but also he claimed that they segmented the year into 16 months. Lastly, he incorporated into this scheme a measurement between the stones of the various sites that indicated they were laid out using the same metrological unit he termed the “megalithic yard” or MY. Ruggles summarizes Thom’s work thus:

“according to Thom ‘megalithic man’ laid out configurations of standing stones all over Britain using precisely-defined units of measurement and particular geometrical constructions, and carried out meticulous observations of the sun, moon and stars. This widespread fascination with astronomy and geometry was unsuspected by archaeologists…yet Thom’s conclusions were not formed on the basis of a single site like Stonehenge, but backed up by widespread data, gleaned from several hundred monuments and supported by rigorous statistical analysis.”119

The breadth and depth of this massive compilation in its totality and with its integrated elements was so impressive, combining astronomy and mathematics to these monuments, that Atkinson, a strong critic of astro-archaeology, was overwhelmed by Thom and driven to acknowledge that his opposition to the discipline was incorrect:

“It is hardly surprising…that many prehistorians either ignore the implications of Thom’s work because they do not understand them, or resist them because it is more comfortable to do so. I have myself gone through the latter process; but I have come to the conclusion that to reject Thom’s thesis because it does not conform to the model of Prehistory on which I was brought up involves also the acceptance of improbabilities of an even higher order. I am prepared, in other

118 Chippindale, “Philosophical lessons …” op.cit., p. 77
119 Ruggles, Astronomy in Prehistoric Britain ..., op. cit., pp. 7-8
words, to believe that my model of European prehistory is wrong, rather than that the results presented by Thom are due to nothing but chance.”

In spite of Atkinson’s “Paul-like conversion,” other researchers began a long and arduous reanalysis of Thom’s work and reached conclusions contrary to that of Atkinson. Before engaging Thom at Stonehenge and elsewhere the reader should note that the same problems confronting Hawkins and Newham—namely lack of corroboration from other disciplines—were found: negative corroboration, but also the same chronological contradictions left many of his conclusions in ruins. Aubrey Burl in this respect points out:

“It has to be realized, however, that Thom specifically limited his entire archaeo-astronomical investigations in Britain to a period between 2000-1600 BC because ‘it is generally agreed that the dates of erection of standing stones lie between 2100 and 1500 BC’ (Thom 1967, 101). These archaeologically outmoded dates were, moreover, based on a radiocarbon calendar and thus further distorted by the need for calibration [by tree-ring dating] making 2100-1500 BC the analogues of C14 1675-1250 bc, and so part of the Early and Middle Bronze Age. Many of Thom’s sites need [instead] searching for celestial alignments [moved back in time to] between 3250-1500 BC (Burl, 1973, 170), starting a thousand years before the earliest of his hypothetical dates. It is only with demonstrably Bronze Age sites, that his chronology may be applicable.”

Thom himself acquiesced to these newer radiocarbon dates and admitted: “The radiocarbon method of dating and more especially its recent recalibration give an entirely different range of dates. Everything is earlier than was previously thought. In view of this it is necessary to re-examine the histogram of declination [with respect to the initial alignments] given in [Thom’s] Megalithic Sites in Britain…The star declinations shown there are for the period 2000 to 1800 B.C. We must look [to making these alignments fit] earlier; according to Burl, very much earlier.” Thus there is not only no corroboration from other disciplines to support Thom’s analyses such as the failed example of the Bush Barrow Lozenge, but radiocarbon dating places much of his work outside its supposedly proper time frame context.

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Before beginning this analysis of Thom’s thesis we wish to admit our great admiration for his immense efforts and painstaking measurements. Unlike Atkinson, who early bowed to Thom’s tremendous compilation, though we are deeply impressed by him, with the advantage of time and hindsight we respectfully but strongly disagree with his findings, because they have been examined by others and have been picked to pieces. We also disagree with his findings because we suggest non-literate, intelligent people could not have been knowledgeable in an advanced way of astronomical alignments. As Ruggles explains, “‘Megalithic man’, it seemed, had developed an intimate knowledge of the moon’s motions not to be rivalled until the seventeenth and eighteenth centuries [AD].”\(^{123}\)

Like Hawkins and Newham, Thom obtained the position of the Moon over the 18.61-year cycle and that of the Sun as well at Stonehenge, but also at many other sites. He maintained the levels of precision he adopted left nothing to chance and this implied that these alignments were correct and not fortuitous. Clive Ruggles examined these basic levels of precision.

“[Thom] accumulated survey data from several hundred ‘megalithic sites’, and it is through analyses of data from many of these sites taken together, rather than from discussions of individual monuments, that by far the most important evidence in favour of ‘megalithic astronomy’ derives. This evidence is cumulative in nature, and is most conveniently divided in four stages or ‘levels.’ Each stage involves analyses that test for astronomical alignments of greater precision than the previous stages, and at each stage evidence emerges of greater observational exactitude than before.”\(^{124}\)

On “Level 1,” the precision of Sun, Moon and star alignments Thom evaluated to a precision of at least half a degree—the diameter of the Sun—for the solstices, equinoxes, middle declinations between solstices and equinoxes that divide the year into eight and then again 16 parts as well as the lunar major and minor standstill positions.\(^{125}\)

\(^{123}\) Ruggles, *Astronomy in Prehistoric Britain ..., op. cit.*, p. 8
\(^{124}\) Ibid., p. 49
\(^{125}\) Ibid.,
On “Level 2,” Ruggles analyzed the “Level 1” findings occurring close to the solar solstitial declinations which include solar major and minor standstill points to 10 minutes of arc, a third of the solar or lunar diameter.\textsuperscript{126}

On “Level 3,” Ruggles analyzed natural markers such as hills, valleys, etc., as sight lines (foresights) distant from the megalithic back sight marker to determine the lunar motions with “great precision.” These were precise to 3’ [minutes of arc], about 1/10 the diameter of the Moon.\textsuperscript{127}

On “Level 4,” Ruggles evaluated Thom’s astronomical alignment to less than a minute of arc “indeed so precise that it seems they could only have been set up at the end of an averaging process lasting 180 years.” \textsuperscript{128}

After analyzing each level of precision adopted by Thom, Ruggles, based strictly on the astronomical/mathematical evidence, came to the conservative conclusion that Thom’s alignments, particularly with regard to the motions and standstills of the Moon, were not precise enough to support his contention that Neolithic people could have used these alignments:

“To sum up, we have seen that apparent trends in the crucial data at Levels 2, 3 and 4 can quite adequately be explained away by selection effects and the large number of free parameters that can be adjusted to provide a close fit between the high-precision lunar theory and the measured data. In any case, once we reach Level 3 there are enormous—almost certainly insurmountable—practical difficulties involved in observing and marking the moon’s motions to the precision claimed [by Thom]. Taken together, these factors lead us to the unavoidable conclusion that lunar motions were not in fact observed and recorded to high precision in prehistoric times....

“It should be pointed out that what we have just said does not conflict with Thom’s statement that ‘at no stage have we made any attempt to pull values this way or that way to produce a better fit.’ We are certainly not suggesting that the Thom’s were deliberately misleading people by carefully choosing only those lines which best fitted the theories they were trying to prove. Rather, the problem is one of implicit methodology: the values used in the Thom’s analyses are ones

\textsuperscript{126} Ibid.
\textsuperscript{127} Ibid.
\textsuperscript{128} Ibid.
favourable to the lunar hypotheses that had already been singled out from less favourable data by their prior selection. “129

The data Ruggles employed and the analyses of these to critique the Thoms’ alignments is quite technical and will not be gone into here. The technically competent interested reader is urged to attempt Chapter 2 of Ruggles, *Astronomy in Prehistoric Britain and Ireland*. Evan Hadingham further comments:

“The proof presented by Thom deserves to be considered properly, but the lunar theory, which depends so much on high-precision measurements and observations, has aroused skepticism in several expert astronomers…. How could a thin crescent moon be lined up against a stone or the horizon? Such practical difficulties might well have prevented the observer from seeing any regular pattern in subtle changes of the moon’s movements ever 18 ½ years.

“In his book *Megalithic Lunar Observatories*, Thom presents a selection of forty sight lines surveyed at twenty-three different standing-stone sites. Most of these sites are scattered in the Highlands of northern and western Scotland, where dramatic, jagged horizons are commonplace. Thom assumes that the ancient astronomers at each site lined up a distant mountain with the moon, even though the ruins actually present there often give little or no indication of the mountain involved. In fact, the monuments are frequently surrounded by so many dips and notches on the skyline that there is an obvious possibility of a potential moon alignment occurring quite by chance.”130

Thus, the level of precision Thom adduced to support the validity of his alignments at hundreds of sites is spurious and places them all under a cloud. With respect to these distant horizon markers used by Thom at Stonehenge at his Level 3 precision a curious story unfolds. Castleden begins:

“One great drawback with most 1960s Stonehenge astronomy was that most of the alignments inferred were marked by stone pits, mounds or gaps between the stones that were only 10-30 meters [33-99 feet] apart. Lines drawn joining points that close together and then extended out to the horizon are not likely to have been of any use for accurate observation. It was at Thom’s suggestion that long distance markers [natural hills, notches between or in mountains, etc.,] were added into the Stonehenge scheme to improve accuracy, although it could not be proved as we saw with Lockyer’s misuse of Silbury Hill [that he used although it was too far

130 Hadingham, *op. cit.*, p. 73
away to be seen with the naked eye] that they were actually used in antiquity. Peter’s Mound, a knoll discovered by Peter Newham and named after him, stands almost 3 km [1.86 miles] away to the northeast of Stonehenge and could have acted as a distant skyline marker for the midsummer [solstice] sunrise during the Stonehenge 1 phases [ca 3100 B.C.]. Thom proposed that other horizon markers, some as much as 13-15 km away, could have been used.”

The Thoms explained what these other horizon markers were:

“The hypothesis that the site of the [Stonehenge] monument is a lunar observatory invites us to examine eight sight-lines to find out if any of these could have carried distant foresights on the apparent horizon. The human eye can resolve two points about 20 arc seconds apart and so a foresight of 1 arc minute would have been sufficient, that is 1½ feet for every mile…[any distant] sight would need to be marked in a permanent manner by a stone or a mound of earth. We had hoped that traces of these mounds might still exist and indeed it might be claimed that there is evidence of this; but only extensive digging by archaeologists can settle this matter.”

They go on to suggest that these distant horizon foresights could have been used to align the Moon’s rising and setting positions:

“We set out to seek foresights for the eight limiting lunar rising and setting positions…of the lunar orbit. We believe that the positions for four of these may have been located, if not accurately pin pointed: (1) Gibbet Knoll…; (2)…Figsbury Rings; (3) Hanging Langford Camp…; and (4) Chain Hill…

“It will be shown that, for each of the first three lines above, the [Moon’s] ray grazes a ridge near Stonehenge; so at night with cooling ground the foresight would be easily seen down to the base at ground level. During the day all three [horizon foresights] would have been visible [from Stonehenge].”

To the Thoms’ great misfortune these natural horizon foresight markers did not exist when Stonehenge phase I was built nor during the supposed entire period of all the phases of that monument, as Ruggles shows:

“Of the four proposed foresights one, a mound at Hanging Longford, is part of a complex dating to the Late Iron Age or Romano-British period; one, an earthwork at Gibbet Knoll, is probably the remains of a Civil War gun battery;

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131 Castleden, *The Making of Stonehenge*, op.cit., p. 25
132 A. Thom, A.S. Thom, *op.cit.*, pp. 151-152
one, a mound at Figsbury Rings, overlies a modern ridge and furrow cultivation; and the forth, Peter’s Mound, has been shown by excavation to be a First World War military rubbish dump...The Thoms also identified the top of Chain Hill as the site of a further lunar foresight, but found no surviving candidate for the foresight itself. The Thoms identified these putative foresights by examining the eight possible sightlines radiating out from Stonehenge.”134

In other words, “their proposed artificial [horizon] foresights did not stand up to archaeological reappraisal.”135 They never existed at the times Stonehenge was built or used.

As to the astro-archaeological alignment interpretation by the entire panoply of researchers into Stonehenge as an astronomical observatory, Chippindale states:

“Those [astronomical theories] confined to Stonehenge,...share a common shortcoming. They show that the configuration of Stonehenge is compatible with an astronomical theory of its purpose—since a number of ‘astronomically significant’ alignments or other features occur in its plan. But some alignments would arise in the natural course of a plan made with no reference at all to astronomical events. It must be shown that the nature and/or number of astronomically significant features is such that they are unlikely to have arisen except by intelligent intent....

“Two other special difficulties have been found in some of the Stonehenge-specific schemes. Some schemes easily accommodate features which have subsequently been discovered to [radiocarbon] date much earlier than the main monument..., or to date much later than the main monument...The same features are used by different schemes in different ways [with different alignments]; since a variety of solar, lunar, and other astronomical alignments can be adduced, and a variety of features within the plan of Stonehenge and its environs can be aligned with them, the fear arises that astronomical schemes of the kind proposed for Stonehenge may be inherently indiscriminate. In that case there exists no clear means by which to judge whether they are likely to be true. If they easily include features which are several millennia older or younger, and surely can have no connection whatever with the main monument, doubts may arise, for this suggests a false astronomical order may easily be observed in unrelated monuments.”136

134 Ruggles, Astronomy in Prehistoric Britain ..., op.cit., p. 229 fn. 140
135 Ibid., p. 40
136 Chippindale in Critical Traditions ..., op.cit., pp. 76-77
Atkinson’s comment on this problem is germane: “One has only to think how difficult would be the task of future archaeologists if they had to construct the ritual dogma and doctrine of the Christian Church from church building alone without any written records.”\textsuperscript{137} Bernard Cornwell put the case in very simple but stark terms, asking:

“…why such an elaborate monument was necessary. After all, if marking the extremes of the sun and moon were all that was required, then it could have been done with just four or five stones…Cathedrals do have something to tells us about Stonehenge. If four thousand years from now [astro-archaeologists and] archaeologists were to discover the remains of a cathedral they might deduce all kind of things from the ruins of the building, but their first and most obvious conclusion would be that it faces the rising sun from which they would assume, reasonably enough, that Christianity worshipped a sun god. In truth the east-west alignment of most churches has nothing to do with the sun.”\textsuperscript{138}

If several spires of a great cathedral survived, these could also have been used to align solar and lunar extreme positions as well as equinoxes, etc. The perimeter of the cathedral could be measured and a unit of measure invented to connect that monument with the length of the year, or the size of the Earth or the distance to the Sun from the Earth. As Chippindale stated these must be “such that they are unlikely to have arisen except by intelligent intent.” Then suppose other researchers analyzed the same cathedral to show it related to several other phenomena using the same features, how do we determine which is correct when there are no criteria by which to do that. Then add that these researchers added horizon foresights to prove these alignments were precise enough to warrant their validity; again how do we determine this? This is the hodge-podge that exists at Stonehenge. Thus we wonder how it could in any way whatsoever be adduced to prove anything astronomically and refute Velikovsky. Nothing has been proved there that is testable via the scientific method. Mathematical and astronomical conjectures have been heaped one upon the other, all of which have the façade of science but fail to stand up to examination by various authorities. These negations of Stonehenge as an astronomical observatory are not our doing; rather they are the negations of authorities in these same disciplines.

\textsuperscript{137} Atkinson, Stonehenge, op.cit., p. 167
BALLOCHROY AND KINTRAWS

E.C. Krupp has argued that Thom’s work at the Ballochroy and Kintraw megalithic monuments in Scotland clearly disproves Velikovsky’s hypothesis:

“Velikovsky implies that the earth was closer to the sun prior to 1500 B.C. He does not appear to be any more specific than this…”

“Euan MacKie offers proof that earth at that time was not closer to the sun than it is now. Thom’s alignments at Ballochroy and Kintraw are in agreement with the setting of the upper limb of the sun at summer and winter solstice respectively. Both alignments if corrected for the apparent radius of the sun, give the same declination for the sun’s center. This is an impressive confirmation of the astronomically precise character of these two independent sites. It also permits us to deduce that the apparent diameter of the sun today, 32 arc minutes, was the apparent diameter before any Velikovskian calamity. If the sun appeared to be the same size it was [in that past age] the same distance from the earth, the orbital period could not have changed.”

The question then is, where do Thom’s alignments at Ballochroy and Kintraw place the Sun viv-à-vis these monuments prior to around 1500 and 700 B.C.? If his alignments are precise, as he claims, and not deeply flawed, then these megaliths could have been built at that prehistoric period. If, on the other hand, his alignments are highly problematic, then no such claim can be made as to their construction date. This is the same E.C. Krupp who above stated “Archaeoastronomy provides us with sites like Temple Wood…which antedate the Venus collision, and yet which are accurately aligned on significant moonsets, as we might expect had no collisions occurred at all.” We then cited Krupp when speaking of “the uprights near the prehistoric Temple Wood stone circle” needing “more than one ‘place to stand’” and that “the Thom interpretation has been challenged by some who cite a somewhat similar arrangement at Barbeck just 5½ miles north. That site does not seem to have lunar site lines.” And in the end he admits “those who challenge the concept of precise lunar observations doubt that the extrapolation methods [at sites like Temple Wood] really work. These methods still leave many features of the stone grid unexplained…We are somewhat mystified, then, by alignments that indicate moonrises and moonsets with high precision, higher than seems possible.”

Krupp now turns to the Sun at Ballochroy and Kintraw which he claims give precise alignments, but which unlike the moonsets at Temple Wood have stood the test of time. They haven’t, as we will see, and we are somewhat mystified, then, by alignments that indicate sunrises with high precision, which authorities claim exhibit “high precision, higher than seems possible.” Let us therefore proceed to examine Thom’s alignments at Ballochroy and Kintraw.

Owen Gingerich, one of the world’s leading astro-astronomers and bitter critic of Velikovsky, had actually gone to visit many of the megalithic sites in Scotland to examine Thom’s conclusions that these exhibited precise sightlines to the Sun and Moon at specific times in the ancient past. His conclusion contradicts Krupp’s enthusiastic acceptance of these alignments as they relate to Velikovsky. Ruggles cites him thus:

“In 1977 I visited [Callanish, Kintraw, Ballochroy, Temple Wood (Kilmartin) and Brodgar]. These sites proved psychologically devastating to my tentative acceptance of precision astronomy in ancient Britain.”

Apparently, Krupp understands that Gingerich doesn’t know what he is talking about! Ruggles has also examined these sites and has, like Gingerich, failed to see them in Krupp’s light. At this point it should be explained that “Clive Ruggles of Leicester University was appointed the world’s first professor of astro-archaeology.” Because of his unique knowledge of both astronomy and archaeology we have turned to him earlier, and will do so now with regard to Thom’s alignments at Ballochroy and Kintraw, presenting as fully and thoroughly as possible the evidence that pertains to these sites, omitting only the more technical materials.

**BALLOCHROY**

“…the possible astronomical significance of Ballochroy…for many people became the very embodiment of Alexander Thom’s ‘megalithic astronomy’…it is not a large spectacular monument…, but merely a small group of megalithic structures…found in the Argyll district of western Scotland…

“From this vantage point—a level area of high ground close to the west coast—one overlooks a broad stretch of the Sound of Jura [sea] including [across it] the small island of Gigha. Further away on a clear day can be seen the Isle of Jura itself, with the Paps [mountain peaks] of Jura, the group of peaks in its centre, being particularly impressive [as foresights against which to align the standing

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140 Owen Gingerich, in Ruggles, *Astronomy in Prehistoric Britain ...*, op. cit., p. 49
141 Rosemary Hill, *Stonehenge*, op.cit., p. 189
stones of Ballochroy]….The most conspicuous feature of the [Ballochroy] site itself is a 5 m[eter]–long row of three standing stones.”

Figure 6

Ruggles further adds:

“Thom, who had mentioned Ballochroy…came to regard it as one of the most important solar sites known to him. Having carried out careful…surveys to determine the declinations of conspicuous points on the horizon…., he suggested that the function of the monument was to pinpoint the longest and shortest days of the year by marking the exact setting position of the sun on both.”

An observer standing at the same spot behind the stones facing the mountains of Jura across the water could observe where the Sun sets each evening during the year. When it set farthest north and over the central stone and lined up with the notch in the distant mountain of Corra Bheinn, that would mark the summer solstice. When the Sun reached farthest south the observer would have had to move his position to a spot behind the smallest stone and peer over it to Cara Island to the south west to see it set during the winter solstice at a particular point in its mountains, as it descended at an angle until it could be seen in the notch of a valley which acted as a foresight.

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142 Ruggles, *Astronomy in Prehistoric Britain…, op. cit*, p.19
143 *Ibid.*, p.21
At this latitude in Scotland the angle between the summer and winter solstices is nearly 90 degrees. Thus the observer, to attain these alignments would have to move from observer 1 to observer 2 positions. Hadingham points out:

“There are…many problems at Ballochroy. The tallest stone points nowhere. The shortest stone is turned toward Jura [Island], but if one picks out a mountain slope…and measures it, no astronomical alignment will fit. The theory depends on [Thom] selecting [to use] only the middle stone [not the two others] and claiming that its flat face was intentionally aimed toward the correct slope on Jura [Island]. This is too much to claim for such a crude indicator as the [flat wide] side of a standing stone.…

“As for the long sight line to the southwest—the convincing layout of the three stones and the Carnmore grave toward the winter sun—the evidence surely weighs against the observing technique proposed by Thom. The [height of the earthen mound that covered the burial] cairn must have blocked the view of the distant horizon [so the winter solstice could not be seen by the observer]….

“Of course, it can be argued that the cairn was built exactly in line with the stones at a later date, after the observatory had fallen into disuse. But unless evidence for this becomes available, it is reasonable to prefer the simpler theory [that the Sun set over the burial cairn to give light to the dead in it, a symbolic alignment which would have occurred for several days], and to reject the idea that Ballochroy was operated as an accurate solar observatory.”

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144 Hadingham, *op. cit.*, pp. 59-60
With regard to the chronology of the standing stones at Ballochroy and the date of
the construction of the Carnmore burial cist which would have blocked the observation
of the winter solstice alignment, based on the established chronology, Ruggles adds:

“While we have no direct evidence on the relative chronology of the cairn and
standing stones at Ballochroy, we can rule out archaeologically the possibility that
such a cairn would have been constructed much later than the mid-second
millennium BC; if anything, it is more likely that it predated by several centuries
the astronomical date obtained for the standing stones on the assumption that the
high-precision foresights were intentional. Perhaps inevitably, the suggestion was
then made that the cairn [mound] might have been destroyed in prehistoric times
in order to permit the [winter solstice] observations. However, this idea is ruled
out by a sketch of the site made by Edward Lhuyd around 1699 [A.D.] which
clearly shows an alignment consisting not only of the three stones [with] the cairn
(still erect) [and blocking the view of the horizon]…”

In this respect Burl further explains:

“the name of Ballochroy, baile–cruach or ‘farm by the mound’, shows how
conspicuous this cairn once was. The nearby farm [called] Cairnbeg, ‘the little
cairn,’ still has a mound 3 meters [10 feet] high. This suggests that Ballochroy’s
cairn [mound] was even bigger…[W]hen…Edward Lhuyd saw it in 1700, he
called it ‘Karn mor,’ the great cairn. His sketch shows how it would have obscured
[the winter solstice sunset] sightline…”

It seems evident that the winter solstice alignment to Cara Island was blocked
by the cairn, and an ad hoc proposal was offered that “perhaps” someone stood on
the cairn mound itself and looked first at the three standing stones than turned
precisely 180° to see the winter solstice sunset on Cara Island. This, of course, as
with all the other observer positions at Ballochroy, has nothing to corroborate their
placement other than the assumption that they must have been used; why? Because
they must have been so used.

Then there is the problem related to the slowing of the Sun’s movement as it
nears the solstitial points which Ruggles addresses:

“…within a week or two of the solstices the movement of the setting position
becomes much smaller as it approaches its northern (June) or southern (December)

145 Ruggles, op.cit., p. 25
146 Aubrey Burl, Prehistoric Astronomy and Ritual (Princes Risborough UK 2006) pp. 22-23
limit. During a period of a week on either side of the solstices the sun’s setting path changes by only about a third of its own diameter. In most circumstances it would be impossible to detect [with the naked eye] any difference in the sun’s setting position throughout this [two week] period, even behind a mountainous horizon…How, then, could this have been achieved at Ballochroy”

Thom had suggested the long distant foresight mountain peaks and notches in them would solve the problem. Only on the solstice the Sun would set in a unique position such that it would be observed with visible edge sliding down a slope to disappear for a few moments behind the mountain and then that edge reappearing in the notch at the base of the distant mountain. This occurred because it set at an oblique angle. Ruggles points out the major obstacle to Thom’s thesis:

“A widespread misconception is that Thom’s method of using distant [mountain slopes and notches as] foresights allows the solstice to be pinpointed to the very day. Thom himself certainly believed this, and it is inherent in his own scenario for how the sightlines were set up, involving setting out stakes at the position where the last gleam of the sun could be seen on successive nights. However, the maximum possible difference between the sun’s declination at sunset nearest the solstice and that two days earlier or later is less than a minute of arc, or one thirtieth of the sun’s own diameter. Such a minuscule variation is almost certainly swamped by daily changes in atmospheric conditions.”

That is, the density of the atmosphere changes slightly from day to day, based on atmospheric temperature and pressure. This changes the position of the Sun, especially at the horizon, which must be observed through the thickest part of atmosphere through which the observer must look. At the zenith the distortion caused by refraction is the least, but as one looks lower toward the horizon the refraction becomes ever greater, and at the horizon it is the greatest. This raises serious problems for Thom’s solution as Ruggles explains:

“The extent of atmospheric refraction is dependent upon air temperature and pressure. From his own measurements Thom calculated that daily and seasonal variations in atmospheric conditions would lead to an uncertainty in apparent declination of at least one minute of arc [one thirtieth of the sun’s diameter]. Others, however, have warned that the uncertainly could be rather greater. More

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147 Ruggles, op.cit., p. 21
148 Ibid., p. 23
recent work\textsuperscript{149} indicates the variations in atmospheric conditions can alter the apparent declination of an observed low altitude [near the horizon] object in Britain by several arc minutes and possibly by as much as half a degree [the diameter of the sun].

“The evidence available today suggests strongly that the effects of variable refraction make it impracticable to detect the small differences in the sun’s motions for at least two or three days on either side of the solstice, and possibly for a considerably longer period.”\textsuperscript{150}

Beyond all these problems at Ballochroy, Ruggles adds the following:

“Standing stones may well have shifted in the thousands of years since their erection, or even been re-erected in relatively recent times, unbeknown to the modern investigator. Features just as important as the large standing stones that we can pick out so easily now may well be inconspicuous in the absence of excavations, or may have disappeared completely, as at Ballochroy. Even what is apparently a simple monument may have been used and modified over a considerable period…”\textsuperscript{151}

There is the further problem of overcast skies obscured by rainy periods at around these solstitial times at Ballochroy and elsewhere. These would occasionally obscure the Sun as it set at the horizon. It must also be pointed out that Thom deliberately chose the observer’s place as well as the particular stone or stones to be aligned with particular distant mountain foresights in order to make his alignments agree with the prehistoric chronological setting. But one can just as easily choose different observation points and different standing stones aligned to different distant mountain foresights that make these alignments fit or agree with a completely different chronological placement, say in post-Roman/Saxon times. That is the problem all the alignment choices incur; once one adopts one chronology and excludes others it becomes a foregone conclusion that one will find alignments that agree with that chronology. This is precisely the problem that Hawkins warned us about above. A “secure” astro-archaeological site must be dated “by other methods.

\textsuperscript{150} \textit{Ibid.}, p. 25
\textsuperscript{151} \textit{Ibid.}, p. 41
Otherwise one is postulating an alignment and then deducing the date that will make the alignment work.” Ruggles and Michael Hoskin discuss this concept of choice:

“Thom…believed that at Ballochroy the prehistoric erectors of the stones had overcome…problem[s] by the location they had contrived for the stones—one from which the Sun was to be seen at the winter solstice setting behind Cara Island which is on the horizon 7 miles away, and at the summer solstice behind Corra Bheinn, a mountain more than 19 miles distant…

“One problem with testing such a theory arises from our ignorance of when, to within several centuries, the stones were erected. Although the directions of solstitial sunrise and sunset at a given location alter only slightly from one millennium to the next, this is enough to make an important difference [especially] when we are observing with instruments tens of miles in length. At a site with distant mountains in roughly the right direction, it may well be possible to find a date for the site when it would have had the exceptional characteristics that Thom’s theory requires. As to the ‘indications’ supposedly built into the stones themselves, these are of the kind that tend to be identified by the investigator after he has already convinced himself of the astronomical purpose of the site. It is then that he is likely (in this example) to focus attention on the middle slab (which points roughly in the ‘right’ direction) rather than on the northernmost (which does not), and to specify the ‘intended’ alignment of the stones themselves, to a precision quite unjustified…”

Astro-archaeological alignments are interpreted alignments and are thus not empirically based. The phenomena that fit the theory and the timeframe into which it is placed are chosen. The entire correlations are based on circular reasoning. Burl dismisses Thom’s alignments at Ballochroy thus “It is not surprising that astronomers such as Gerald Hawkins have thought that Ballochroy is ‘an ingeniously neat combination of site selection and astronomical knowledge.’ Yet, although the evidence seems convincing, there are just as good reasons for believing that Ballochroy was not an observatory.” Ruggles puts it thus: “Taken as a whole, then, the evidence weighs overwhelmingly against the idea of there being [at Ballochroy] intentional solstitial alignments of the high precision envisaged by Thom.”

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153 Burl, Prehistoric Astronomy and Ritual, op.cit., pp. 20-21
154 Ruggles, Astronomy in Prehistoric Britain ..., op.cit., p. 25
KINTRA W

Kintraw, also in Scotland, is yet another megalithic monument that, according to Thom, enabled the ancient people to observe the winter solstice sunset with great precision around 1800 B.C. The problems with Thom’s alignment are very similar to those at Ballochroy. According to Hadingham, “…supporters of Thom’s theory suggest that Ballochroy and Kintraw were set out together as complementary observatories. One gave the exact day of midsummer, the other midwinter. Both contributed to the regulation of an astonishingly precise calendar by ancient observers who might indeed deserve the title of ‘scientists.’”155 T. McCreery, A.J. Hastie and T. Moulds analyzed Thom’s thesis for Kintraw and in 1982 published their criticisms in Astronomy in the Old World, edited by Douglas Heggie. This paper, which was reprinted by Lewis M. Greenberg in the Journal KRONOS, will now be cited. These investigators had made six visits to Kintraw to examine Thom’s claim that this obviously was an astronomical observatory and wrote:

“Of the many megalithic sites claimed by Professor Thom as remains of prehistoric observatories none has proved more contentious that the proposed midwinter solstitial site at Kintraw [not far from Ballochroy, Scotland]. This consists of [two burial] cairns and a menhir [standing stone] which Thom claims indicates a horizon foresight, the col [valley] between [the two mountains of ] Beinn Shiantaidh and Beinn a’Chaolais on the island of Jura, 45 km [30 miles] away. Since the view of this col from the area around the menhir is partially blocked by the intervening ridge of Dun Arnal [between them], Thom suggests that preliminary observations [to align and mark the winter solstice sunset foresight] were made from a ledge [behind the menhir up] on a hillside to the north east of the site, and overlooking it. [By moving the observer up that hillside to the ledge, the observer could see over the blocking ridge into the col between the mountains and thus observe the winter solstice sunset]. It is the question of the validity of this claim which has produced such a vigorous debate.”156

According to Thom this ledge was created by ancient man as a platform where observers could stand and look directly at the large menhir to see it aligned with the col. Whether that viewing ledge/platform is man-made or natural will be discussed below. The questions at hand are whether the average-sized ancient viewers could have actually observed this winter solstice alignment with the

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155 Hadingham, op.cit., p. 59
advantage of that hillside platform, but, more importantly, how could it have been planned in order to do so in the first place? That hillside on which that ledge exists is extremely steep. Since the sunset positions prior to the solstice move across the horizon, these ancient people would have to experiment by standing at different places on that hillside until they discovered the only spot from which they could observe the Sun setting in alignment with the menhir and the col. Between September 1980 and October 1981, these researchers came to Kintraw six times to test these questions. They report:

“To an observer standing on the stone layer behind the boulder notch, the col was sometimes visible and at other times obscured by the vegetation [of the intervening ridge] on Dun Arnal…We think three factors are responsible for this:

“(A) The observer’s height. Changes in eye level of [only] a few inches are critical.

“(B) Unpredictable, but substantial variations in terrestrial [atmospheric] refraction, which changes from hour to hour, along the length of the alignment from Jura to Kintraw.

“(C) The foliage on the clump of trees [on Dun Arnal]….In summer this is thick enough to be virtually opaque, but in winter and early spring, it is sparse enough for it to be just possible to see through to the skyline beyond. We believe that this [last] factor is less important than the two mentioned [previously].”

Ruggles echoes these same problems:

“Another doubt was cast upon the interpretation of the Kintraw ledge by the assertion that the bottom of the distant col could not, in fact, quite be seen from the platform because the intervening ridge just obscures it. [Euan] MacKie flatly denied this, and indeed the present author has seen the notch from the platform; however a movement of less than 0.5 m[eters] from the centre of the platform made a crucial difference. The disagreement may be accounted for by the differences in eye height of different investigators, by differences in vegetation levels on the intervening ridge at different times of year, and perhaps most importantly by differences in weather conditions altering atmospheric refraction.”

Hadingham also visited Kintraw specifically to evaluate these problems and reported:

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157 Ibid., pp. 46-47
158 Ruggles, Astronomy in Prehistoric Britain ..., op. cit., pp. 28-29
“During several successive visits to Kintraw, I became convinced that the narrow platform was an impractical place for the kind of observations demanded by Thom’s theory. The view of the distant horizon is still unsatisfactory. On some occasions the cleft in the peaks of Jura was visible just above Dun Arnal, but on other visits the ridge blocked the view, probably because of changes in atmospheric conditions along the thirty-mile sight line. Such variations in refraction would certainly have presented serious difficulties to an ancient astronomer who attempted to carry out the type of observing envisaged by Thom.”

Here we have McCreery, Hastie, Moulds, Ruggles and Hadingham all standing on the same ledge and having difficulty viewing the col on Jura where the winter solstice sunset disappeared. Now if these ancient scientists had the least bit of common sense, why didn’t they place their viewing station several yards higher on that hillside so that they would have an unimpeded view to that distant cleft? Why did they place their observation post at a place that suffered from so much obscuration? And finally, why didn’t they set up their entire apparatus at a site where they could stand only a few yards behind the menhir and see into that col? In essence, rather than setting up their observatory at a site that made this alignment easy to see, they constructed it at Kintraw which had all these problems. As a young man in the United States army I learned a military aphorism which went “there are three ways of doing something: the right way, the wrong way, and the army way.” The army way meant it was worse than the wrong way. Apparently the scientists at Kintraw were military men. But that is not the end. McCreery, Hastie and Moulds go on to discuss the problem associated with establishing this observation point in the first place:

“This controversy is, however, made meaningless by a further difficulty. According to Thom, several days before the solstices, the observer on the ledge would have so positioned himself that he saw the Sun flash in the col at the moment of sunset, and would have marked the position. As the setting position moved southwards, the position from which the observer saw the Sun setting in the col moved to the right (north west) along the ledge till the day of the solstice. On the following day, as the sunset once more advanced to the north [to compensate for this] the observing position [necessarily] retreated to the left, leaving the extreme right hand position, subsequently marked by the boulder notch, to identify the exact day of the solstice. It is evident that this [precise solstitial day] position could not have been obtained if the positions appropriate to

159 Hadingham, op.cit., p. 57
the days immediately before (and after) the solstice had not [first] been established. …160

As Ruggles states “The problem here is that moving away from the platform along the ridge…(that is, to the left) causes the bottom of the col to disappear from view completely behind the intervening ridge.”161 That is, there is no known way to observe the winter solstice sunset before and after it that could be planned to date it precisely since at these times no one could see where the Sun was setting. McCreery et al. go on to show:

“At Kintraw the observing position for the day before (and after) the solstice is 19 feet to the left…

“Regardless of conditions, the col was always obscured [on the days before and after the solstice] by Dun Arnal itself and not only by the vegetation on it. This was also true for the observing positions two and three days before midwinter, at 75 feet [for a day or two] and 170 feet [for day three] to the left respectively…It is quite clear, therefore, that the series of observations which Thom’s theory requires could no more have been made from the ledge [on the hillside] than from the field [below] beside the menhir itself.”162

Because the distances from the ledge observing place are 170 feet to the left on the third day before the solstice, 75 feet day two before the solstice and 19 feet day one, and during all these times the Dun Arnal ridge blocked the sunset points on the distant horizon on the island of Jura, how could these ancient scientist/astronomers know when the solstice took place? They had no way of knowing or preparing to determine this. And there is yet another problem which militates against moving to the left of the ledge observation station to attempt to prepare for the solstice. Here Ruggles shows “…there is no room to set out a line of people [left and away from the ledge], because the platform is situated on the side of a precipitous hillside above a deep gorge.”163 That is, in order to even attempt to see whether the Sun could be observed setting days before the solstice, people would have to take their lives in their hands because if they slipped they would fall down a precipitously angled hillside into a gorge. Even if they did not fall, why would they endanger themselves again and again, year after year, to

160 McCreery, Hastie, Moulds, *op.cit.*, p. 48
161 Ruggles, *Astronomy in Prehistoric Britain …, op.cit.*, p.29
162 McCreery, Hastie and Moulds, *op.cit.*, pp. 48-49
163 Ruggles, *Ancient Astronomy …, op. cit.*, p. 212
prepare for the winter solstice sunset when they could see it on any of the prior days? Again only a military-minded scientist would attempt this every year.

Finally, how do we know that this ledge is actually man-made? According to Hadingham “There is a distinct possibility that the platform is actually a modern cow track leading from Kintraw Farm to pasture land nearby [and not man-made].”164 If indeed ancient man year after year for centuries climbed to this ledge to observe the winter solstice, surely a few small items would have fallen onto it and been buried in the soil, even a flint arrow head, or scraper etc. In this respect McCreery, after carefully examining the evidence for this possibility, concluded:

“…we...have to admit that no valid evidence has been presented in support of Professor Thom’s hypothesis concerning the function of the Kintraw ledge. It is not, as he declared in 1969, artificial. No traces of archaeological litter concomitant with the supposed importance of the ledge were found. And finally, the claim that the excavated stone platform was man-made is clearly wrong…the very absence of any prehistoric activity on the ledge is a damning indictment of the astronomical significance of the ledge in particular and the Kintraw megalithic site in general.”165

While MacKie maintains the platform is man-made and John David North writes: “Professor Thom had predicted an observing platform of some kind at Kintraw, and all the evidence now marshalled suggests that there was in fact an artificial boulder notch and platform of the sort required.”166 this, as was shown above, is false, though on this very point of the platform, North added the “ledge was found…by…E.W. MacKie…to be artificial.”167

Ruggles adds:

“A major difficulty is the complete lack of human debris found during the excavation, which is very surprising if the platform was used as the astronomical theory supposes. A number of criticisms were also made of the way in which the petrofabric [soil structure] analysis had been carried out. While MacKie [who supports Thom’s thesis] responded forcefully to those criticisms, he later conceded

164 Hadingham, op. cit., p. 57
166 John David North, Stars, Minds and Fate…, op. cit., p. 31
167 Ibid. p. 23
that, since no material was found to allow the platform to be dated, the evidence from Kintraw was inconclusive.”

Ruggles concludes:

“It is clear, then, that there are severe difficulties for the theory that Kintraw represented a high-precision solstitial sightline using the deep col in the Paps [peaks] of Jura as a foresight. It is also clear that what promised to be an elegant and satisfying procedure—the archaeological verification of an astronomical hypothesis—has turned out in practice to be messy and inconclusive.”

Christopher Scarre puts Kintraw in the following light:

“Sites such as Kintraw in Argyll were interpreted as megalithic observatories. Many of these claims do not stand up well to critical assessment; above all, the high precision alignments that Thom proposed are now generally discounted.”

This is what remains of the evidence E.C. Krupp offered for Ballochroy, Kintraw, and Temple Wood which he claimed proved that Velikovsky’s catastrophes could never have happened. The very authorities in this field of astro-archaeology and archaeology have concluded as cited above: Burl “there are just as good reasons for believing that Ballochroy was not an observatory,” or Ruggles on Ballochroy “taken as a whole, then, the evidence weighs overwhelmingly against the idea of these being solstitial alignments of high precision.” While Ruggles calls Thom’s alignments at Kintraw “messy and inconclusive,” Scarre said they “are now generally discounted.” Krupp has never retracted these criticisms of Velikovsky. The “Velikovsky Affair” with all its outrageousness seems never to die.

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168 Ruggles, Astronomy in Prehistoric Britain ..., op.cit., p. 28
169 Ibid., p. 29
170 Christopher Scarre, “Introduction,” Monuments and Landscapes in Atlantic Europe: Perception and Society During the Neolithic and Early Bronze Age, Christopher Scarre, ed. (London 2002), p. 6
LE GRAND MENHIR BRISE

John K. Young describes the megalithic monuments at Carnac in Brittany, across from Britain in France, thus:

“Perhaps the most well known megalithic site in France is at a location named Carnac, in Brittany. At this site, precise rows of hundreds of standing stones positioned over very large areas can be seen...This megalithic site has been meticulously surveyed and studied by Alexander Thom...His findings show a definite interest in astronomy by the builders of the site.

“Thom proposed that at least two lunar observatories can be discerned...One observatory centers around a famous stone called Le Grand Menhir located on a [peninsula] site called Locmariaquer (menhir is a Breton word for standing stone). The Grand Menhir is unfortunately no longer standing upright—it appears to have toppled sometime before the seventeenth century [A.D., possibly from an earthquake or deliberately destroyed, or fell because it was top heavy]. However, when it was erect, it seems that about nineteen meters [67 feet] of its slender twenty-two meter [72.5 feet] length pointed at the sky. Since its weight has been estimated at 330 tons, an incredible amount of effort must have been required to quarry, move, and erect it in a pit in the ground. The Grand Menhir is the largest single megalithic stone known in Europe; its size compares favorably with similar Egyptian obelisks from the Temple of Karnek, e.g. the obelisk with a height of 19.5 meters erected by Tuthmosis I...While the Grand Menhir is not as elaborately carved as the obelisks of Egypt, the feat of creating and moving it suggests that the society of Brittany shared some of the abilities of the more advanced culture of Egypt.171

Young goes on to show how Thom aligned it to the Sun and Moon:

“Why go to the trouble of carving and erecting such an enormous stone? Thom maintains that the answer relates to astronomy. The Grand Menhir is located on a small peninsula that extends into the Bay of Quiberon. Since it is surrounded on three sides by water, it is ideally situated for observation of the rising and setting of the sun and moon on the horizon [as at Ballochroy and Kintraw]. Thom has proposed that the Grand Menhir was used to point at the positions of the moon, not unlike the foresight on the barrel of a rifle. However, if the stone was to function as a foresight where were the backsights located?” 172

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171 John K. Young, Sacred Sites of the Knights Templar: The Ancient Secrets Hidden in Stonehenge, Rennes-le-Château and Santiago de Compostela, (Gloucester MA 2003), p. 36-37
172 Ibid. p. 38
As to how Thom proceeded, Ruggles reports:

“The eight rising and setting positions of the moon [over 18.61 years] at its major and minor limits were considered to be significant by Thom. At first sight, the existence of suitable backsights in all eight directions from Le Grand Menhir Brisé seems very impressive indeed. But we must ask a crucial question: how confident can we be that the putative backsights identified by Thom and his team were actually intended as such? We know how the team went about locating candidates for backsights: they [first] identified the eight lines radiating out from the great menhir upon which such backsights would have to lie and examined what could be found along those lines. Such an approach has an evident danger [anything found on one of these lines may or may not be an intended alignment].

The area around the great menhir contains the densest concentration of dolmens and standing stones in the whole of Europe, so one would surely encounter features at least as promising in a great many different directions. It would strengthen the argument in favour of the astronomical interpretation if the putative backsights formed a coherent set of structures archaeologically [such as all being dolmens, the same size, or standing stones, or mounds, or hills or valleys]. They do not. On the contrary: the backsights turn out to be a diverse collection of stones, many of which are small and of doubtful prehistoric provenance; they include several boulders that are certainly natural and one cairn probably built a millennium later in the Iron Age. Finally, there is considerable doubt as to whether the great menhir itself ever actually stood at all: it is possible that it broke during the process of erection.”

Elsewhere Ruggles reiterates and adds:

“One problem that soon emerged was that the alleged backsights formed a motley mix of prehistoric monuments, and in four out of [these] eight [alignment] cases were probably not genuine prehistoric monuments at all. Yet the landscape in this area is strewn with impressive monuments. Wouldn’t it be quite easy to find an equally convincing backsight in almost any direction from the Great Menhir? The answer, it is now generally agreed, is: very likely indeed.

“The importance of this example is methodological. How could Thom have been so selective with the evidence without realizing it? As we know from the accounts of those who worked with him, he merely set out to identify suitable backsights in each of the eight directions he considered significant [and chose those he came upon that fit the theory]. Why didn’t he at least try some other, randomly chosen, directions as a control [to see if these non-astronomical

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alignments encountered a back sight object which would call into question the entire thesis of those that were found. The probable answer is that he was too convinced by his own theory—that Neolithic people observed the motions of the sun and moon to great precision—to feel it necessary. He was not alone in this. It is natural to want to find evidence that supports a favourite theory, especially if it is one that has taken a lifetime to develop. Yet it is precisely for this reason that correct field methodology, which ensures that one gives due consideration to all the evidence, is so critically important.”

Ruggles further shows:

“How could the positions of the backsights have been fixed [in the first place] without observing programs lasting many generations? How useful would the ‘inner’ target markers have been in practice? (The moon passes these positions twice every month!) What happened when a critical observation was missed owing to bad weather?”

The coup de grâce Ruggles delivers thus:

“In fact, The Grand Menhir is now known to have stood at the end of an alignment of more than a dozen stones [pointing in only two directions from the ends], varying in height. Most of the smaller stones in the row were subsequently felled and some at least were reused in the construction of tombs. A split carving shows without any trace of doubt that one former menhir—possibly one of this row—was split asunder…and another [was used] as the capstone [of a] tomb several kilometers away…Not only does the very early date [preceding Thom’s dating of these alignments] for menhir construction that this implies drive a final nail in the coffin of the lunar foresight hypothesis (since the lunar targets shift slightly over the centuries owing to the changing obliquity of the ecliptic). More importantly, it shows the potential complexity of the archaeological and chronological context, and the vital importance of taking this into account when formulating astronomical theories.”

Since there was a single row of stones which included the grand menhir, the sight line had to be along that row, and thus have nothing to do with the assumed backsighted alignments Thom created. Furthermore, some of these stones were felled and used elsewhere before the time that Thom envisages that these

174 Ruggles, Ancient Astronomy: An Encyclopedia…, op.cit., pp. 167-168
175 Ibid. p. 167
176 Ibid. p. 168
alignments were created and employed. Therefore, the actual alignment row was destroyed before the time that Thom envisages these alignments were created. What this suggests is that after creating a two-direction alignment row, the people decided to tear it down to incorporate a totally different eight-direction alignment. Why didn’t they do that in the first place? Lastly, because as Ruggles has told us it would have taken “observing programs over many generations” and the positions of the Moon’s changes with the change in the obliquity of the ecliptic, by the time they established the eight sightlines, they would then have to cut and haul the eight distant stones to these sites and stand them up. But that, too, would have taken time and during that interim, the obliquity of the ecliptic would have changed, so the alignment would not be highly precise and have become more so over further generations.

**BRAINPORT BAY**

Ruggles outlines the evidence and introduces it as “A calendrical complex.”

“What is most striking about Brainport Bay is that a number of stone structures occur in a single NE-SW alignment….At the south-west end, at an elevation of some 15 m[eters], is the so-called back platform and projection…, an area of natural [rock] outcrops that have apparently been converted into a platform… Some 60 m[eters, 200 feet] downhill to the north-east are two large boulders… oriented across the alignment…

“At first, these features were thought to represent the remains of some kind of ancient settlement. However, there was a troublesome lack of any clear evidence of dwellings or midden [waste] deposits. Furthermore, a number of other features seemed without explanation. These included two socket holes [that once held large stones]—one…at the north-east end of the central, highest area of the platform, and the other…in a V-shaped cleft between two boulders at its south-west end— together with three small stone slabs…appeared to fit well into the sockets [from which to observe an alignment].

“The idea that they were related to the distant horizon seems to provide a promising explanation…The alignment…is oriented to the north-east towards the only distant horizon visible from the site…From this vantage-point the rock outcrops of the main platform are silhouetted beneath the distant skyline, and the two small standing stones (as re-erected [in the empty sockets]) appear, one behind the other, in the V-shaped cleft formed by the rocks on either side of the nearer stone…Above the [two] standing stones is a horizon notch [between two mountains] some 45 km [28 miles] distant. The dramatic effect of the two stones
lining up upon the distant notch through a nearby cleft has been likened…to the sights of a rifle barrel [aimed at the sunrise solstice].”

Figure 8

Moving Sun in notch aligned with standing stones in ancient times

However, on the summer solstice the Sun does not rise in the notch at “the exact time of the solstice but some days before and after (about fifteen days in 1800 BC).” There are, however, serious problems, even assuming that the ancient observers determined the precise date of the summer solstice by counting 15 days forward in time and then 15 days back and halving the 30 days to date it. The first problem is the radiocarbon dates of charcoal found at the platform as reported by MacKie:

“…although flint implements of probable Neolithic date had been found on the old ground surface in most parts of the alignment, by 1982 the date of the paved areas and other artificial features was still not clear. Two C¹⁴ dates, in the second and fifth centuries AD were obtained for two separate deposits of charcoal on the old ground surface near the front (north-eastern) stone socket, but another much later one in the tenth century [AD] came from charcoal in a pit apparently dug through the paving on top of the main outcrop. Another date in the fourth century AD came from charcoal excavated from a trench full of ash and carbonised material next to one of the vertical rock faces on top of the Main Outcrop…”

177 Ruggles, Astronomy in Prehistoric Britain ..., op.cit., p. 29
178 Ibid., p. 32
These dates were assumed to be much too close to the present and were therefore disregarded as possible evidence for the construction of the monument. Moreover, Ruggles shows:

“However a shock was in store. Later excavations showed that, contrary to earlier expectation, the two boulders [that were replaced in the sockets to make the alignment] had actually not been moved into place but were entirely natural. It therefore had to be assumed that people in the second millennium BC made a chance discovery [that the Sun 30 days before the summer solstice crossed the notch and] of a set of features already roughly oriented towards midsummer sunrise, and improved them [by erecting the stones lying on the ground along that alignment after the fact].”\(^{180}\)

Furthermore, one wonders why these ancient people didn’t, as they had supposedly done at Ballochroy and Kintraw, orient the stones to the exact day of the summer solstice rather than 30 days before? Surely if they wanted to date the solstice, a precise alignment to some other distant point on the horizon would have been preferable as at these other two sites. Halving would, of course, enable the people to observe the Sun’s motion more directly 15 days prior to and 15 days after the solstice. But why, in one instance, do this at Brainport Bay but fail to do the same at Ballachroy and Kintraw? In essence the astro-archaeologists are applying two different methods for determining the summer solstice, according to what they wish to derive from the different sites. At Ballochroy and Kintraw they made their solstitial alignments point directly at the points on the horizon when these occurred, but at Brainport Bay they made their alignment point to the horizon 30 days prior to the solstice. The reason for using different methods is simply not explained except by a priori, post hoc assumptions by these researchers. Furthermore, Ruggles points out a problem even with using the halving method.

“A serious difficulty with the ‘halving the difference’ argument is the large number of distant horizon notches that can be seen from virtually any location in mountainous country. Picking such a location at random, there is only a relatively small chance that a fortuitous notch will be found exactly at a solstitial rising or setting position of the sun. However, the chances are appreciable of finding an acceptable notch within a degree [2 solar diameters] or so in the appropriate direction from one of the extreme positions [near the solstice point on the horizon]. Any such notch is susceptible to the ‘halving the difference’ argument, so that the mere presence of such a notch in the general direction of the alignment

\(^{180}\) Ruggles, *Astronomy in Prehistoric Britain ..., op.cit.*, p. 32
of features at a monument such as Brainport Bay does not constitute any sort of proof that it was used in this way by the people who occupied the site.

“...[T]he abundance of suitable horizon features contrives to make it almost impossible for us to convince ourselves that any particular one was indeed used for the purpose of solar observations close to the solstice. Neither can we ever prove that it wasn’t so used; the point is merely that on this subject, as in so many other matters relating to the activities of prehistoric people, the archaeological record...is unable to speak to us clearly.”

Aware of these difficulties, MacKie suggests a possible way around them:

“If the original construction could be accurately dated to late Neolithic times [by radiocarbon dating methods]—presumably somewhere in the late or early second millennia BC— and if their purpose as an accurate calendrical alignment could be proved beyond reasonable doubt with evidence from the site itself, then the Thom alignment hypothesis would be strongly supported....

“However since it is possible to use any notch to the right of the midsummer rising position..., such an indirect solstitial alignment cannot be regarded as convincing unless supported by independent evidence. Ideally this would take the form of another clear calendrical line nearby, or of clear archaeological indications from the site itself that a midsummer alignment, as opposed to an orientation, was intended. However, it is hard to imagine what form the latter could take here.”

Ruggles shows that overcoming any new evidence in order to maintain the original thesis that Brainport was a summer solstice observatory only piles difficulties on difficulties.

“In short, it is difficult to agree that the evidence from Brainport Bay ‘demonstrates to a high degree of probability that very long, potentially accurate [calendrical] alignments had...been devised.’ However, the example does serve to demonstrate some of the serious methodological pitfalls that can arise, even where, as in this case, an effort has been made to define a clear philosophical starting point. In practice, the archaeological verification of an astronomical hypothesis has been turned into a cyclical process in which astronomical predictions are tested by directed fieldwork and excavation, then modified [by the astro-archaeologists] and re-tested, and so on. The general principle [of adjusting the astro-archaeological interpretation to overcome the archaeological evidence] is

181 Ibid.
fair enough; the problem is that while the archaeological evidence is allowed to modify the specific predictions, for example, by adding more potential alignments, it is never allowed to influence the more fundamental hypothesis that Brainport Bay was a high-precision ‘calendrical’ site. Thus, as contradictory data confront each suggested alignment, more are suggested in an attempt to bolster the calendrical idea, and the structure of ‘supporting’ evidence becomes steadily more cumbersome....Instead, the idea of archaeological verification has been turned into mere post hoc [and ad hoc] justification, which in this case becomes less and less viable as the weight of evidence builds up against the hypothesis being proposed.\(^\text{183}\)

But that is largely what we have with the various alignments presented by the astro-archaeologists. They simply introduce ad hoc and post hoc interpretations to explain away contradictory evidence. The possibility that all or nearly all these alignments may not be correct as to time is never considered. Even when a critic such as Ruggles suggests that “the increasingly attractive alternative, that the astronomy of the main alignment was of lower precision and all other alignments were fortuitous,”\(^\text{184}\) still allows these monuments to have been built and utilized in the late 3rd and early to mid-2nd millennia B.C. Even if the alignments at all the various megalithic monuments are only broadly indicative of the chronology into which they have been placed, how does anyone know they belong to those times? By standing at slightly different places in and around these monuments one can find alignments that can be made to fit entirely different chronologies. Who is to determine which chronology is the correct one, and how is this to be done? Both the astro-archaeologists and the archaeologists are creating ad hoc and post hoc explanations to preserve their theory that the chronology of these monuments is valid when in fact their arguments in the words of MacKie “cannot be regarded as convincing unless supported by independent evidence.” At the conclusion of his book, Ruggles, “drawing together the strands,” delivers this devastating appraisal of Thom and the other astro-archaeologists’ efforts:

“...in forty years of research and fieldwork on megalithic monuments, Alexander Thom produced over six hundred drawings and index cards, filled over one hundred notebooks, and published three books and several dozen research papers...

“Thom’s contribution to the study of orthostatic stone settings bears witness to his own remarkable range of skills, both theoretical and practical...That much of

\(^{183}\) Ruggles, *Astronomy in Prehistoric Britain ..., op.cit., p. 34

\(^{184}\) *Ibid.*
this effort, while producing a valuable data resource, was ultimately misguided resulted from Thom’s total ignorance of (and lack of interest in) the framework of ideas being developed by archaeologists during this time. Thom was not alone in this; archaeoastronomy has been beset for many years by ‘otherwise sane and apparently sober physical scientists with respectable academic positions [who, from an archaeological perspective,] appear to lose all critical ability when utilizing archaeological data.’ The much broader range of skills needed to appreciate and address the archaeological and anthropological issues, as well as to develop and carry out astronomically related research to answer interesting archaeological questions, is even rarer.”

**FINAL WORDS ON ASTRO-ARCHAEOLOGY**

What appeared as a clear-cut explanation of the megalithic monuments as astronomical observatories has collapsed in the face of subsequent research over the past 25 years. The statements in the literature attesting to this are numerous. Regarding Thom’s alignments in Brittany, Hadingham states:

“The result in the case of Professor Thom’s research is a kind of science fiction in which the prehistoric Bretons operate their lunar observatory with a passion for precision suspiciously like that of modern engineers and astronomers. While the megaliths were indeed connected with beliefs about the sun and moon, it is clear that these were only aspects of a complicated mass of ideas and practices involving the afterlife…”

“We must be grateful to Thom and his colleagues for awakening interest in the intellectual skills of prehistoric Europeans. Yet many of the claims for a Stone Age science are now proving to be little more than an unconscious projection of our contemporary technical world onto the silent ruins of four thousand years ago. There is little ‘hard’ evidence to support the theory that accurate eclipse predictions were undertaken by the megalithic builders, who would have found their task extraordinarily difficult in the absence of written recording aids.”

Consider the fact that it took the peoples of the ancient Near East hundreds of years of not only making careful observations of the sky, but, most importantly, writing these down onto clay tablets before they began to detect a pattern in the

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186 Hadingham, *op. cit.*, pp. 80-81
data that enabled them to understand the pattern that gave rise to eclipses. Without some form of writing it would have been impossible for the megalithic builders to detect that pattern and then predict eclipses. Yet astro-archaeologists such as Hawkins, Newham, Hoyle, and Thom simply assume this could be done without a written data base to draw upon. It is simply assumed that these ancient people somehow accomplished this understanding without a written data base and then it is further assumed that, having done this, they erected their megalithic monuments to see these alignments. But the ancient Babylonians did not erect monuments to see these alignments. Once the pattern was understood they merely kept watch over the sky to see these eclipses. If the more ancient megalithic builders had achieved this pattern recognition, in spite of their failure to keep written records, they too, like the later Babylonian astronomers, wouldn’t need massive monuments to detect these eclipses; a peg board would have been a sufficient device to accomplish this. The Greeks, as we will see below, actually kept such peg devices to chart the heavens.

With respect to the statistical evidence that Thom and the others maintain supports their astronomical theses because there are so many accurate alignments, Douglas C. Heggie, an astronomer, looked carefully into Thom’s statistical proofs as well as that of others and reported

“Suppose a scientist notices some apparent pattern in a body of data and accordingly frames a hypothesis to account for it. The first question another scientist is bound to ask on hearing about this is whether the observed pattern could have arisen by accident, i.e., whether there is really anything to be explained, and the originator of the hypothesis ought to anticipate this question, satisfying himself that the pattern is not accidental. Now it is remarkable that in megalithic astronomy this elementary precaution has been ignored so persistently, even though the subject has been developed by people with a broadly scientific background, and such statistical considerations have been in use by a few authors since Thom’s work in the 1950s, and even before. Instead one sees statistical considerations ignored or abused, in the relentless drive to find an astronomical significance—the more elaborate the better, it often seems—in any ancient monument.

“It is interesting to reflect how different the development would have been had more careful attention been paid to the statistical approach. It seems to me that most of the conflicts have arisen over theories whose statistical justification is weak or non-existent. If we remove these from consideration I think we would be confronted with very few of the practical and archaeological difficulties I have
discussed in the previous section [of this paper]. The loss of these would make megalithic astronomy less exciting to many people, but unless the subject is based on a firm foundation the object of our excitement will prove to have been a delusion.”\textsuperscript{187}

Ronald Hutton argues:

“The evidence for Thom’s argument that the rings and rows were observatories for scientific astronomy rested upon the alignments which he had drawn from them to the moon and to specific stars. By 1982 these…had been subjected to detailed criticism. Because stars move around, to prove that one was in alignment with a megalith when the latter was raised it was necessary to know the exact date of the stone’s erection. This is, of course, impossible to fix by any current or foreseeable methods. Thom’s own estimations were all wildly wrong because they preceded the great revision made in the rough dating of prehistoric monuments following the correction of the Carbon 14 process. Thus all his stellar alignments have to be rejected, and nobody is anxious to replace them because of the impossibility of proof. As for his lunar alignments, the whole body of them was considered by the scientific astronomer Clive Ruggles, who found half to be either dubious or impossible and most of the rest unprovable. He noted that as Thom selected his monuments, his ways of surveying them and his foresights, it was very easy for him to come up with alignments, as a typical set of megaliths in hills or mountains would have hundreds of features on the horizon which could coincide with the movements. Douglas Heggie considered this problem from the point of view of a statistician and declared that it made all Thom’s lunar sightings inadmissible. Gordon Moir added valid but by now almost superfluous comments to the case for the prosecution, noting that Thom misreported the features on the horizon of certain sites, ignoring those of no value to his case and including some which could not be seen with the naked eye.”\textsuperscript{188}

Christopher Chippindale, writing in 1984, summarized the understanding of megalithic astronomy in an article subtitled: “Whatever happened to megalithic astronomy,” in which we are told

“If you ask modern visitors to Stonehenge what is was for, they will tell you it was an ancient astronomical observatory, as surely as their grandparents would have said it was a temple of the Druids. Yet, 20 years after Gerald Hawkins’s


Stonehenge Decoded declared it was a ‘neolithic observatory-cum-computer’, archaeological opinion is even more sceptical than ever, despite a shoal of variant theories from Sir Fred Hoyle, the Scottish engineer Alexander Thom, and any number of eager amateurs. The astronomer Douglas Heggie writes in his authoritative Megalithic Science that ‘the ground appears amazingly barren’ when it comes to statistically significant evidence for Stonehenge astronomy—beyond its approximate orientation to the midsummer sunrise. Heggie finds it ‘very hard to square the bleak record with the enthusiasm and confidence with which most accounts of the astronomical theory of Stonehenge are usually coloured.’ The astronomers have not quite given up: Hawkins himself has recently tried to prove again that the probability of his astronomical scheme arising by chance is infinitesimally small.

“There is good statistical reason why the attempts to prove exact astronomy in Stonehenge or the other British megalithic monuments are unlikely to progress further. A hypothesis developed on one set of data should be tested on another independent set; but unfortunately no duplicate Stonehenge exists to test Hawkins’s and Hoyle’s ideas about it. And Thom’s ideas, based on regularities among all the circles, again lack an independent data-set on which to test them. With rival astronomical theories in mutual contradiction, and the statistical evidence for any of them still not at all convincing, archaeologists are less and less impressed.”189

Ronald Hutton brutally summarized all the negative evidence for Thom’s thesis:

“As a result of all this, Alexander Thom’s beliefs in the existence of scientific astronomy in British and Irish prehistory is now only shared by his family, by Euan MacKie and by a handful of admirers outside academe, most of whom do not seem to be aware of the case against it.”190

Jacquetta Hawkes quipped in 1967:

“I am forced to the conclusion that nothing of any great moment has ever been established by the astronomical *nouveau vogue* [sic] flowing over Stonehenge.”191

If all or most of the megaliths were identical in their formation and oriented with the identical alignments, then that would constitute strong statistical evidence

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189 Christopher Chippindale,” Life Around Stonehenge,” *New Scientist* (April 5, 1984), p. 16
190 Hutton, *op.cit.*, p. 114
191 Jacquetta Hawkes, cited in North, *Stars, Minds and Fate, op.cit.*, p. 11
for accepting these as astronomical observatories. But that is the problem: except for a clear orientation of many of the stone rings to the winter solstice sunset, the monuments are a hodge-podge of standing stones from which one can assume the alignments work if one stands here at one monument or there at another, or somewhere else at a third, and so on. Had all the monuments been made of two or three standing stones aligned to various points on the horizon and all indicated, say, alignments to the solstices or to the major and minor lunar standstill points, it would have been statistically evident that these were valid. This, as we know, is not the case. Each monument has its own particular structure, and the place of observing alignments at each is unique. Thus the interpretation of each monument is also unique. Given the difficulties and problems that are also unique at each site, we are left with little or nothing that will support the astro-archaeologists’ conceptions. Is it any wonder that Hadingham above called these hypotheses “a kind of science fiction,” or Heggie suggested that they “prove to have been a delusion” and that “the ground appears to be amazingly barren,” and “a bleak record”?

In this regard Ronald Hutton adds about the numbers of monuments that align with solar solstices and equinoxes after discussing Newgrange, Knowth and Stonehenge etc:

“None of the other fifty odd cursuses in Britain has obvious solar alignments, nor do any of the other passage graves, which number over a hundred. An occasional one is found among other classes of megalithic tomb, but these cases may be coincidental; thus, of twenty ‘Clyde’-style tombs upon the Isle of Arran, one faces the midsummer sunrise fairly exactly but all the rest have very dispersed orientations. Much the same is true of the ‘henge’ monuments and stone circles of the third and early second millennium; thus, the large circle in Cumberland called Long Meg and Her Daughter has one entrance in line with the midwinter sunset, but none of the other stone rings of the Cumbrian region has apparent solar connections…

“To sum up, it is clear that at particular times and places in British and Irish prehistory the cardinal points of the sun [the solstices and equinoxes], and particularly the winter solstice, had considerable ritual importance. None the less, the vast majority of prehistoric monuments in these islands do not relate to any of them, so that no overall or enduring pattern of cult can be detected. Furthermore, a considerable gulf separates all these monuments from the pre-Roman British Iron
Age, not one of the temples of which has yet been found to have possessed a significant solar alignment.”

The entire field of astro-archaeology is faced with such impossible problems which have never been fully addressed. The discipline is confronted with more negative evidence than any positive support. The entire astro-archaeological edifice seems to have collapsed.

This brings us back to Velikovsky and his numerous enthusiastic critics who jumped at the chance to debunk him by claiming that the precise alignments at Stonehenge ineluctably prove his thesis impossible. To reiterate, Gerald Hawkins above stated: “These Stonehenge values are perhaps the best rebuttal of Velikovsky’s thesis of cataclysmic shifts in the axis of the earth in the first millennium B.C.” Charles A. Fair argued above: “How could Thebes as Velikovsky suggests have changed latitude and Stonehenge not?” Professor Emeritus Bertram F. Wilcox above questions Velikovsky and is “given pause by many doubts and objections such as the chronology of Stonehenge…” Duncan Steel above maintained “All you really need to know about Immanuel Velikovsky’s absurd astronomical ideas [is found in ] Hawkins, G.S., with J.B. White, Stonehenge Decoded.” E.C. Krupp above argued: “Even Stonehenge I [phase] in Wiltshire with its recalibrated radiocarbon date of at least 2800 BC includes astronomical alignments that are understandable only in the absence of Velikovskian catastrophes,” etc. What, then, is left of their criticisms regarding precision alignments? Nothing! Yet three years after Chippindale and eight years after Heggie and five years after Hadingham, John David North, a rabid critic of Velikovsky, argued above: “How he (Velikovsky) would cope with the more accurately and closely argued work of Alexander Thom, over a much wider area [than Stonehenge] I cannot easily imagine.” North, who was still imbued with the accuracy of megalithic astronomy which had lost its glitter, in the same 1989 book extols the precision of Alexander Thom’s work:

“If Professor Thom’s conclusions as to neolithic metrology and geometry were astonishing, what he discovered of the astronomy of the time was even more so. He showed for site after site a preoccupation with the Sun at the solstices. He proved that a calendar had been in use in which the year was divided into eight, sixteen, or even thirty-two parts. But he also left no doubt in the mind of an impartial reader that some of the alignments were lunar, while some pointed to the

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192 Ronald Hutton, The Stations of the Sun (Oxford UK 1996), pp. 4-5
rising or setting of one or other bright star. Dates are deducible from the directions of the latter…

“To appreciate the achievement of those who observed from the megaliths…we should at least keep half an eye on the probable evolution of their methods. Once it was noticed that the Sun rises and sets over a different point of the horizon on successive days, that at midsummer it reaches its greatest displacement along the horizon in one direction (north), and that in midwinter it reaches the other extreme, it would have been natural to fix the two turning points with permanent direction markers. These might have been a pair of stones, or an avenue, or a stake or stone taken in conjunction with some distant natural object, such as a mountain top, for a foresight. (This last method, found in many of Professor Thom’s examples, is obviously potentially very accurate, a minute of arc being easily achieved…It is not absolutely clear, incidentally, that Thom is right to ignore refraction in azimuth when sighting over a coastline)…”

“Perhaps comparable lunar observations were made almost simultaneously, but it is unlikely that the nature of the Moon’s fluctuating habits of rising or setting were fully appreciated at all quickly. Although in any month the Moon’s rising and setting points fluctuate between certain limits as do the Sun’s over a year, yet the limits themselves now vary as between successive months. The full cycle of the second fluctuation takes more than eighteen years…”

In essence, North completely ignored the criticisms of Thom by the respected experts/authorities—Hadingham, Heggie and Chippindale. He continued to subscribe to all of Thom’s thesis at that time even after it had been shown to be erroneous. Evidently, Hadingham, Hutton, Heggie and Chippindale didn’t know what they were talking about but John David North did! The accusation North has laid at the feet of Velikovsky and his supporters ironically applies to him, namely that “Dr. Velikovsky and his supporters have shown no inclination to heed the balanced criticism of the expert” because it is North himself who “appears to almost totally ignorant of the studies” by these same experts.

Finally, Elizabeth Chesley Baity raised the same critique, claiming that “[Robert L.] Merrit (1968) shows that megalithic astronomy establishes dates

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193 North, *Stars, Minds, and Fate…*, *op.cit.*, pp. 15-16
which invalidate the hypothesis of Velikovsky…” All in all, every critic who attacked Velikovsky’s catastrophic thesis based on the precision of megalithic astronomy was proven to be wrong. And as with the rest of Velikovsky’s other critics who were shown to be in error over the years, they failed to retract their statements.

In this regard Irving Wolfe has explained this syndrome, which we term “the academic propaganda syndrome” wherein scientists, historians and other academics present only one side of a debate, the criticism favorable to their particular paradigm, but make sure the other side, the answer to that criticism, is withheld from their readers so that only the propaganda is disseminated: Wolfe stated in a major response by six authors to Velikovsky’s critics:

“The…anomalous and disturbing syndrome to appear in the Original Affair (and which has continued to this day), is equally unscientific. It is the distressing sequence of events in which Velikovsky is criticized in print by scientists [historians or others], often quite wrongly. He [and his supporters] then write…a careful reply, pointing out each of the errors of the critics, but only the attacks were published. Velikovsky’s [and his supporters’] rejoinders were not, and the false accusations alone remain in the literature, and become part of the lore of organized science, not merely uneradicated, but unchallenged. They keep being quoted as if they had never been exposed. This is not merely the construction of bad science, it is suppression of debate, denial of the right to reply and blockage of access to the ears of the scientific community. It is totally contrary to the ideals of science (being more typical of a totalitarian dictatorship), but that is how organized science, for the most part, has handled Velikovsky—with intolerance and uncivility and high-handed fanaticism.”

For example, Wikipedia, an Internet encyclopedia, under the title *Ages in Chaos* presented the work of Abraham Sachs thus: “Velikovsky’s revised chronology has been rejected by nearly all mainstream historians and Egyptologists. It was claimed, starting with early reviewers, that Velikovsky’s usage of material for proof is often very selective. In 1965, the leading cuneiform translator Abraham Sachs, in a forum at Brown University, discredited Velikovsky’s use of Mesopotamian cuneiform sources, “ (as presented in *AEON* vol. III, no. 1, 1992, pp. 103-105) and by Leroy Ellenberger on the Internet under the title “Address of

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Abraham Sachs at Brown University 3/15/65.” Needless to say, this attack by Sachs on Velikovsky has been spread by Ellenberger across the Internet to the present day. For example Phillip Clapham, in a “Book Review” of Emmet Sweeney’s latest edition of The Pyramid Age (NY 2007), presented Sachs’s criticism in Chronology & Catastrophism Workshop, where he also cited Sachs’s criticism, to which this author responded. Clapham summarized Sachs’s linguistic argument thus:

“Velikovsky and Heinsohn, as far as I am aware, did not read cuneiform. Hence, they interpreted cuneiform texts that had already been translated without being able to appreciate differences in nuance and style [of the Akkadian and Babylonian languages] that appear to distinguish the different periods. In other words, the cuneiform in use during the Assyrian era was different to that of the Akkadian period in a way that an experienced cuneiform reader [such as Abraham Sachs] would notice straight away.”

The very same type of argument was raised by Ev Cochrane who writes:

“Hammurabi’s inscriptions were written in Old Babylonian [a form of Akkadian]. Indeed, modern scholars attempting to learn this ancient language still cut their teeth on the Code of Hammurabi, which is written in the purest Old Babylonian script…[Thus] the king engaged in some very curious behavior writing in the relatively archaic Old Babylonian when cataloging his laws yet adopting the more modern Babylonian and Old Persian scripts when celebrating his military accomplishments at Bisitun.”

According to Sachs, Ellenberger, Wikipedia, Clapham and Cochrane, the Akkadian language has a very long history that evolved through time, and cuneiform scholars can distinguish the archaic, middle and more recent forms of Babylonian used in southern Mesopotamia, and Akkadian [Assyrian] used in northern Mesopotamia. Nevertheless Erica Reiner, an authority in cuneiform in her book A Linguistic Analysis of Akkadian (London 1966), page 20, one year after Sachs’s cuneiform criticism of Velikovsky greatly contradicted Sachs stating:

“The written records of Akkadian [and Babylonian] form no continuous stream but fall into isolated groups of texts from areas geographically [distant] from each

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199 Ev Cochrane, “Heinsohn’s Ancient History,” AEON, vol. V, no. 4 (July, 1999), unpaginated but on the basis of the index p. 60
other...The [philologists such as Abraham Sachs] under the compulsion of tripartite division have quite naturally led Assyriologists to divide such [language] groups of texts [into Old, Middle and Neo forms for the] two main dialects of Akkadian into Babylonian [used in southern Mesopotamia] and Assyrian [used in the north].”

That is, the evolution of the Akkadian language, that looks isolated and interrupted, is to be organized along a continuous linguistic development. Cuneiformists like Sachs had organized this evolution without solid, linguistic evidence and then attacked Velikovsky for not adhering to their linguistic historical creation. Reiner on page 21 admits:

“I am inclined to consider [without proof] Old Akkadian and Neo Babylonian as distinct languages. For the chronologically intermediate periods (i.e., roughly 2000-600 B.C.) I assume the existence of two dialects, Assyrian and Babylonian. This assumption may be considered the statement of negative evidence, i.e., of the fact that there seems to be no convincing way of deriving the earliest attested Assyrian or Babylonian texts from the preceding states of Babylonian without at least considerable interference from other Semitic languages.” (emphasis added)

As is well understood, languages over long periods of time evolve, therefore the Akkadian language that evolved in southern Babylonia should have become very different from that in the north (Akkadian) which, according to Reiner, above, covered a span of “roughly 2000-600 B.C.” but the fact of the matter is that they did not evolve greatly from one another and this was pointed out by Georges Contenau, Everyday Life in Babylonia and Assyria (NY 1966), page 7, again just one year after Abraham Sachs’s criticism, wherein Contenau wrote:

“The two forms of the [Akkadian] language are practically identical in grammar and vocabulary and probably differed most in their method of pronunciation…”

All this was written one year after Abraham Sachs wrote his critique. All this and much more regarding Akkadian linguists etc. was published in Pillars of the Past vol. II, pages 89-114, and also in our “Response to Clapham,” Chronology & Catastrophism Workshop vol. 3 (2009), pp. 10-12. Of course Sachs would have read Reiner’s book a year after he attacked Velikovsky, because it was a major work in the history of Akkadian linguistics. Eric Leighty and Maria de J. Ellis in A Scientific Humanist: Humanist Studies in Memory of R. Abraham Sachs (Philadelphia PA 1981) p. VIII claim, “he had a wide sweep of learning.” If he did
not read this major work he was decidedly unprofessional; if he read it and suppressed it he was clearly dishonest. We ask the reader to consider how Abraham Sachs could not have known of the material evidence which Reiner presented, being one of the professional giants in this field. Yet he was either monumentally ignorant of what was going on in his own field, or dishonest.

The point is that, like the critics of Velikovsky’s thesis, regarding Stonehenge and the Megalithic Age, when the evidence became known that their criticisms were shown to be false, like those of Sachs, Ellenberger, Wikipedia, Clapham, and Cochrane about Akkadian linguists, they, too, simply suppressed that evidence and allowed rampant disinformation to be circulated under their names. They did not retract, and clearly will not retract, their criticisms. There are several other such episodes still ongoing in the latest parts of the Velikovsky Affair. Undoubtedly, Leroy Ellenberger is the major culprit in this part of the affair, as he has been extremely influential in the Wikipedia piece. He and Wikipedia are never to be trusted as reputable or reliable sources of information about Velikovsky because of their outright falsehoods and suppression of evidence favorable to Velikovsky, such as that regarding Sachs. Their behavior exhibits willful distortion of the literature and is merely part and parcel of “the academic propaganda syndrome,” and a continuation of *The Velikovsky Affair*. It is in the face of such unscholarly behavior that proponents of Velikovsky’s concepts must continue to build a case even though it will be ignored, suppressed, or buried in the footnotes of Wikipedia in order to mislead the average reader, who will not seek out these works. This is a strategy of propagandists, as explained by Merry Aronson, Don Spetner and Carol Ames: “The ultimate details—the [critical answers or] breakdowns—come last….It seems that the most useful and revealing information is buried in the footnotes [that few investigate], a possibly deceptive practice.”

Or as Claudia Trupp explains, “If there are facts that are particularly thorny for [critics] to deal with, something they want to downplay, those facts will always be buried in a footnote.”

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there in full, he, Ellenberger and Wikipedia are not to be trusted about anything regarding Velikovsky.

It has recently been brought to our attention that Ellenberger is now raising the contention that Egyptian language history is an accurate gauge of chronology and thus discrediting our chronology. Again, those who read pages 106-112 of Pillars of the Past vol. II will understand how ignorant he, and the sources he uses, are regarding the evolution of the epigraphic Egyptian language. This distortion may actually end up on the Internet, just as Sachs’s distortion about the epigraphy of Akkadian has.

Henry H. Bauer, an associate of Ellenberger, has written “Hawkins did find correlations with the Sun and the Moon, and it was observations of those bodies only that he claimed as a possible purpose for Stonehenge. Those correlations exist if the apparent motions of the Sun and Moon were the same when Stonehenge was built as now, certainly a stumbling block for Velikovsky’s views...” Here we have a critic of Velikovsky allowing his 1984 book to be republished in 1999 totally unaware that research into astro-archaeology by competent authorities showed, as did Velikovsky, that Hawkins’s alignments were without merit. He, too, has failed to utter a single word of retraction.

The anti-Velikovskian fires are still burning in those who use Stonehenge to criticize Velikovsky. In the words of Friedrich von Schiller: “this inconceivable blindness can only be accounted for as the result of his pride, which never retracted the opinion it had once formed...” They lacked the generosity of heart to admit that their criticisms were proved false. Unlike the great lexicographer Samuel Johnson, who when confronted by “A lady [who] asked him how he came to define Pastern [incorrectly as] the knee of a horse: instead of making an elaborate defense, as she expected, he at once answered: ‘Ignorance, Madam, pure ignorance.’” As Velikovsky often noted “ignorance and arrogance are twins” [Lynn E. Rose, personal communication, November 2010]. Or, in the words of philosopher David Hume:

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202 Henry H. Bauer, Beyond Velikovsky: The History of a Public Controversy (Urbana IL 1999), p. 131
203 Johann Christoph Friedrich von Schiller, History of the Thirty Years War (Charleston SC 2003) p. 294
“I am sensible, that nothing can be more unphilosophical than to be positive or dogmatical on any Subject; and that, even if excessive Scepticism could be maintain’d, it would not be more destructive to all just Reasoning and Enquiry. I am convinc’d, that, where Men are the must sure and arrogant, they are commonly the most mistaken, and have there given Reins to Passion, without that proper Deliberation and Suspense, which can alone secure them from the grossest Absurdities.”

Those who arrogantly attacked Velikovsky regarding astro-archaeology have indeed been shown to be guilty of “the grossest absurdities.” The point to stress is that megalithic astronomy does not refute Velikovsky, but that will not be openly admitted or it will be suppressed, i.e., buried in the footnotes.

There is a second point that must be pointed out at this stage of our work; it is, namely, that every dating methodology employed by investigators to place these megalithic monuments in a prehistoric setting has also failed. As we have shown above: “The ability of radiocarbon dating on its own to solve [chronological] problems is now doubtful, and it is no use pretending, like the emperor, to be clad in sumptuous clothes that we do not possess.” Or “it must be stressed that dating of Stonehenge necessarily entails a great deal of speculation, with the result that several disparate chronologies have been proposed. Even [the] English Heritage [society] refers to two completely different sets of dates.” And a great many of these are based on radiocarbon dating of bone or antler material. According to Taylor, above, “bone and antler were to be avoided” for radiocarbon dating, and bone and antler “are therefore least reliable for dating.”

The same dating failure applies to pottery sequence dating supported by the radiocarbon method, as shown above. “The results were unexpected, they did not support any of the [chronological ] schemes. They suggested that there was little evidence for a succession [over time] of different [Beaker pottery] types; even if it was indicated by artifact associations. Some of the dates were exceptionally late. This left students in a quandary.” Whittle, above, has told us some investigators hold that pottery and artifact “typology has been refined to a greater degree of accuracy than radiocarbon dating…” while others hold “radiocarbon dates are always more useful than those derived from [pottery and artifact] studies.” Lucas stated “…that British Beaker (and indeed other prehistoric pottery) has proven intractable to typological analysis…?” Or as A.F. Harding asserted, above:

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“detailed typological study is a quick route to insanity.” Or as Cohen and Gulot, cited by Whittle, above, stated: “On the contrary, so-called typological chronology is imprisoned in tautological reasoning...the chronological ordering of material rests on assumptions, indeed on intuitions whose scientific [radiocarbon] foundations remain unverifiable.” Or as Whittle stated, above: “There are lies, damned lies and typology.”

The same dating failure applies to dating the Megalithic Age via bronzes and skull shapes as well as the shapes of barrows (see Chapter 4). In each and every instance, the authorities have told us these dating methodologies have failed. And, here, also, there has been extreme reticence on the part of archaeologists to recognize all these failures. Yet nowhere in the literature have modern investigators expressed that, in the face of all these dating failures, one should reconsider the established chronology of the Megalithic Age. Researchers have come to this study with an idée fixe that the established chronology is inerrant. Barry Raftery has explained the inherent concept of what lies at the heart of such an approach when he discusses the notable enigmas of Irish Iron Age chronology and archaeology into which we will delve below.

“A basic defect inherent in many studies of the Irish Iron Age [or the Megalithic Age] is the imposition, often unconscious, of historical interpretations on the archeological evidence. An assumption is made, itself non-archaeological [such as radiocarbon dating] from which it follows that appropriate evidence for such an event [or chronology] must, of necessity be forthcoming in the archaeological record. Any apparent change or cultural break in the archeological sequence [such as pottery dating, barrow dating, etc..] at a more or less suitable date is thus interpreted in the context of what [archaeologists] assume to have happened. Any defects or seeming contradictions in the material evidence are either ignored or dismissed as weaknesses inherent in the nature of the subject. Implicit or explicit, such thinking has underlain most interpretations of [Megalithic Age] archaeology for the better part of a century.

“It seems, however, that this approach is at variance with the fundamental principles of archaeological interpretation. It is wrong to start with an answer and then arrange the archeological evidence accordingly. On the contrary, the archaeological picture must be established on its own [or on scientific] merits, and must stand on its own before correlation with evidence from related disciplines is attempted.”

206 Raftery, Pagan Celtic Ireland…, op. cit., pp. 223- 224
The answer the archaeologists started with is that radiocarbon dating was the *sine qua non* upon which to build their chronological edifice. Even though that foundation, that pillar of the past, has been shown to be invalid, as the authorities themselves have told us: “any defects or seeming contradictions in the material are either ignored or dismissed as weaknesses inherent in the nature of the subject.” Having started with the wrong answer it was inevitable that they had to “then arrange the archaeological evidence accordingly.” None of their dating methods that “must stand on its own before corroboration with evidence from related disciplines is attempted,” have been shown could “stand on its own.” To reject their chronology of the Megalithic Age is simply unthinkable. Having started with the wrong chronology, the subsequent findings of all these dating methods were doomed to failure, and that admission of failure is what is unthinkable.

Raftery’s ultimate comment on the *Enigma of the Irish Iron Age* applies to the entire spectrum of evidence employed to maintain the prehistory of the Megalithic Age. He cites J.R.R. Tolkien:

“To many, perhaps to most people outside the small company of the great scholars, past and present, ‘Celtic’ [or prehistoric megalithic man] of any sort is…a magic bag into which anything may be put, and out of which almost anything may come…Anything is possible in the fabulous Celtic [and Megalithic Age] twilight, which is not so much a twilight of the gods as of…reason.”

We contend that in view of these failures another chronological hypothesis should be presented and analyzed. If our chronology is valid it should exhibit none of the problems and contradictions found with the present scheme. As Terry L. Hunt, Carl P. Lipo, and Sarah L. Sterling explain:

“Although many believe that archaeological knowledge consists simply of empirical ‘findings,’ this notion is false: data are generated with the guidance of theory, or some sense-making system acting in its place whether researchers recognize this or not. Failure to understand the relationship between theory and the empirical world has led to the many debates and frustrations of contemporary archeology. Despite years of trying, the atheoretical, empiricist foundations of archaeology have left us little but a history of story-telling and unsatisfying...”

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generalizations about historical change…The real work…is in devising research questions to assess correctness—the basis of any scientific endeavor.”

Only exploring a hypothesis which contains none of these problems outlined above can lead one to a valid empirical picture of the megalithic world. Before proceeding, we must point out an enigma that has been known for a long time. No one seems to know where the megalithic builders came from and, more importantly, why they disappeared. The question of why the megalithic builders simply stopped erecting stone monuments and also whither they went has no clear-cut explanation. Surely if they were successful enough to be able to feed themselves and build thousands of standing stone monuments they would not vanish into thin air. Here Fergusson shows:

“We certainly have sepulchral barrows in this [British] country of the Roman period…and Saxon grave mounds everywhere; but … not one sepulchre of any sort between the year 1200 B.C and the Christian era,”

Why did these peoples suddenly stop burying their dead in these great long and circular barrows after 1200 B.C., but over a thousand years later resume doing so in Roman times A.D. 55-410, and to resume again in Anglo-Saxon times A.D. 410 and thereafter? What the established chronology requires is that these ancient peoples were supposedly burying their dead in mounds for about 2500 years and then just stopped doing so ca. 1200 B.C. In this case we have a burial mound Dark Age. Richard Michael Pasichnyk puts the situation in the starkest terms:

“As explained in a book that expounds on the complexity of Stonehenge and other such monuments, the end was quick and decisive:

“Indeed, there are signs that many people did not survive, because the familiar Early Bronze Age pottery types, beakers, urns and food vessels, all disappear from later excavation layers. So do the flint arrowheads and other small stone implements characteristic of the time, and the people no longer kept the same burial customs. By the time the Middle Bronze Age cultures appear in the

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archaeological record, they are different in almost every respect from those of their predecessors…

“When viewed as a whole, Europe appears like the scattered pieces of a puzzle, the full picture obscured….

“Everywhere the scene appears much the same. Norway began its Bronze Age, and few relics from the period can be found… The eastern Baltic cultures end with the emergence of a new people, the Battle-Axe culture. Centered on Bohemia and Moravia was the Unetice culture whose time had also come, as did its trading companions, the Wessex culture of Britain. Likewise the megalithic tombs… in Paris came to an end. The builders of the megalithic tombs of Scandinavia had built their last, as had occurred with the megalithic structures in Britain, France, Malta…”

This sudden decline also encompassed Asia, as Pasichnyk continues:

“At the other end of the Eurasian continent the picture is still one of great unrest… The Russian-Persian borderland, like so many other areas, shows that settlements did not exist between the Middle and Late Bronze Ages. The entire Caucasus region is one of shifting territories and transformations of existing cultures, while little archaeological evidence hints at how the changes took place.

“No less than twenty cultures experienced distinct changes in tradition, migrated to other locations, and/or had sudden appearances or disappearances. For example, the Siberian mining and bronze industry came to an end, with objects made of organic matter that became deep-frozen and well preserved, as if a sudden snow storm had engulfed them. The end of this period in Russia was met with the most widespread outbreak of disturbances imaginable.

“After 700 years of rule, the Hsia dynasty of China came to its end, and today not a single archaeological vestige has been found, suggesting that it had been wiped clean from existence. Many sudden innovations came with the culture that followed… This enigma offers no easy solution…”

All across the Eurasian continent the various megalithic people stop dead in their tracks and nothing that follows them shows any connection with them. What

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210 according to J.E. Wood, *Sun, Moon and Standing Stones* (Oxford UK 1978), page 199
212 Ibid., p.40
we have is a Dark Age encompassing early cultures from Britain to Japan which indicates to us that all these cultures belong to a later period and that something catastrophic in nature affected them. At the end of this volume we will address the nature of that catastrophe, but since we maintain that in Britain, Ireland, Scotland, Brittany and other parts of Europe the Megalithic Age belongs in post-Roman/Medieval times, so, too, these other cultures must be moved into the same epoch. Furthermore, we will demonstrate that all across this vast continental region, the cultures that are conventionally assigned to the post-Roman period, ca. A.D. 410-950, cannot be found in the archaeological record. That is, while the peoples of the prehistoric megalithic period exist in the archaeological record, the people who came around A.D. 500-600 to 950 cannot be found. Those who have been following us in the volumes of Pillars of the Past will understand that the absence of archaeological strata for the post-Roman/Medieval epoch must be filled by the people who left archaeological evidence in these same places but who were erroneously assigned to the Late Neolithic, Bronze and even Early Iron Ages. In this case, although Gunnar Heinsohn disagrees with our chronology, our analysis is typically developed along the lines he pioneered several decades ago. That is, all the missing strata from around A.D. 500/600 to 950, are the strata given to the Megalithic Age, dated to prehistoric times.

In a review of Alasdair Whittle’s *Problems in Neolithic Archaeology*, discussed above, Susan Kent points out:

“*Problems in Neolithic Archaeology* is a notable contribution to the current debate about how we can write prehistory... it reaffirms the central role of theory and interpretation while accepting as permanent the uncertainty which makes the testing of archaeological hypotheses difficult or even impossible. Dr. Whittle asserts in particular the need for greater self-confidence and FOR THE FORMULATION OF A NEW THEORY AND QUESTIONS MORE APPROPRIATE TO THE ARCHAEOLOGICAL RECORD.”

We must therefore do as Whittle suggested, bring forth a “new theory” with “questions more appropriate to the archaeological record,” which is not based on “hypotheses difficult or even impossible” and which must perforce exhibit “permanent uncertainty.” We suggest that our theory suffers from none of these problems, enigmas and contradictions because it stands mainly on scientific,

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technological and other pillars often far removed from the conventional methods employed by the archaeologists.
CHAPTER 6

WHAT THE CLASSICAL WRITERS DIDN’T SAY OR KNOW ABOUT THE MEGALITHS

Aubrey Burl, who is well aware of the problems that affect the present paradigm of the Megalithic Age, has presented this analysis:

“Archaeologists who have accepted themselves as no more than technicians have quite rightly been criticized.

“Ever since the time of Stukeley archaeologists, ever more concerned with the accumulation of factual detail, and less with the wider significance of the sites they investigate, have distracted attention from the possibility of an alternative approach to the problems of the past. Excavation can only test existing theories [and not those of outsiders]; and scientists, suspicious of the means by which they are reached, often fail to appreciate the views of poets and visionaries [outsiders].’

“This may be so but it must be added that such visionaries, welcome though they are, too often complacently ignore those factual details when constructing their own preferred models of the past. Undisciplined fancy can never be acceptable as a substitute for logic based on [archaeological] fact. Any evaluation of stone circles must begin with the tedious but unavoidable accumulation of data susceptible to physical[, scientific and technological] proof. Selection is not permissible. Omission is impermissible. Distortion is unforgivable. Any hypothetical model of prehistory has to be founded on this [scientific and technological] premiss. …‘Although self-styled experts on the meaning of stone circles abound, in fact, no-one knows.”

Burl claims the newcomers are really only “visionaries, welcome though they are, [who] too often complacently ignore those factual details when constructing their own preferred models of the past.” He in essence sees us not as scientifically-minded investigators but “poets and visionaries” who fancy ourselves as “self-styled experts.” Why, one might ask, after labeling us “poets, visionaries and self-style experts” would

1 Burl, *The Stone Circles of Britain, Ireland and Brittany*, op. cit., p. 7
he consider our work worthy of attention, let alone serious consideration? Never-theless, we accept his challenge and will apply not only scientific and technological evidence to explicate and understand these megalithic monuments, but employ, as we have in the previous volumes, the entire panoply of interdisciplinary evidence to achieve this. We maintain that it is these very authors who have failed to appreciate this evidence derived from science and technology and other interdisciplinary forms. Because this is our approach to the megalithic world, we hopefully will not fall into the traps Burl maintains are “impermissible” and “unforgivable.” While these lead to “distortion” and “omission” of evidence, we will endeavor to show that in many instances it is the self-styled “real experts” who are guilty of failing to live up to the standard that Burl has set forth. We further maintain that because our approach is multi- as well as interdisciplinary, it incorporates real evidence into the analysis which correlates, corroborates and is congruent with each element. Any investigator who fails to incorporate or ignores this multi- and interdisciplinary material is involved in distortion because he/she has failed to face these facts.

Before going forth, certain earlier forms of evidence discussed above must first be presented which have been “ignored,” of which Burl says “omission is impermissible.” As tedious as it is to repeat this evidence, we want to remind the reader of what we have already established. Pottery dating must now be seen in a severe light as it relates to chronology. The various forms of Beaker pottery, etc., are taken to have been produced for perhaps 1500 to 2000 years. Nevertheless, we quoted Oscar Montelius in this regard that:

“The various [pottery] types which are characteristic of any one period are very often met within the same find; but types belonging to different periods are seldom found together. If however the types of any two periods are occasionally found together these two periods are immediately successive in the series. Exceptions to this rule are remarkably rare, if not altogether absent. This proves that each period must represent a considerable length of time; for if the time had been short, the remains from the different periods ought to have been much more confused than is found to be the case.”

But the fact of the matter is that Julian Thomas, above, showed that at Mount Pleasant there was “a rich pottery sequence running from the Neolithic into the Early Bronze Age…. [T]he assemblage appeared to demonstrate the contemporaneity of a number of different styles of Beaker pottery, and indeed of other traditions of Bronze Age artifacts, whose relationship had often been considered in exclusively

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2 Montelius, op.cit., p. 308
developmental terms.” In terms of our chronology, this pottery was created from about A.D. 450 to about 950, a period not of thousands of years but only around 500 years. Montelius’s understanding is simple and direct. The confused nature of the pottery assemblage at Mount Pleasant is a simple and direct contradiction to the established long chronology to which Burl and his colleagues have given their assent. Rather than heeding Montelius’s clear-cut archaeological understanding of the facts, they have ignored his injunction and failed to see what was staring them in the face. Although this does not prove the chronology of these pottery forms to any particular period, it does unquestionably suggest that the pottery sequence existed over a far shorter period than that of the established chronology. It certainly does correlate in terms of duration with the short chronology we espouse. We do not need the whole rigmarole of ad hoc hypotheses to patch together these pottery incongruencies that plague the established long chronology. The duration evidence of the pottery sequence nestles quite comfortably in the post-Roman/Medieval epoch.

A further point related to chronology in terms of pottery sequence dating is that of La Tène/Iron Age materials dated from the 5th to the 1st centuries B.C. Timothy Darvill describes the “La Tène art…[a] style of ornamentation current from the 5th to the 1st centuries BC in western Europe….In those parts of northwest Europe untroubled by Roman invasions, La Tène art styles continued to develop through [to] the mid 1st millennium AD and beyond. Also known as Celtic art.” That is, this style lasted for over a thousand years. N.K. Sandars wonders about “La Tène works of art…What was this art that appeared so suddenly…when we have analysed patterns into their parts and traced the pedigree of motifs we are left with [an] enigma….For a style to stay true to itself for over a thousand years it requires more than the artist’s usual conservatism and the tenacity of the schemata.”

Of course, a pottery style does not stay true to itself for over a thousand years. La Tène art exhibits this enigma only when a chronology is artificially lengthened beyond reasonable bounds. In the short chronology this pottery sequence again nestles quite comfortably in the post-Roman/Medieval epoch.

In terms of when these megaliths were erected, we also have the problem of when metals were employed. This evidence we presented above regarding R.J. Mortimer’s finding of a grave cut into flint which left large splintered concoidal fragments. He told

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3 Thomas, *Time, Culture and Identity, op.cit.*, p. 183
us “it scarcely seems possible that so deep a grave could have been formed by stone tools alone. Had only the latter been used some [of the stone tools] would surely have been broken and splintered…Not one was discovered.” The question of what metal tool was employed, as we said, can be tested to do just that. This, too, has not been discussed in the present-day literature. It is an “omission” which is “impermissible.” This question is “susceptible to physical proof,” but the investigators have failed to scientifically test it fully. In Burl’s own words “they…too often complacently ignore…factual details…when constructing their own models of the past.”

There is also the problem of the Skara Brae ornamental stone objects which James Macauley attempted to carve with stone tools. As Anne Ritchie, above, stated “In some cases the angle created in decoration of prehistoric balls proved impossible to achieve with stone tools and can only have been carved with strong metal tools.” They could not have been reworked at a later period because Skara Brae was suddenly buried and only discovered in the 1850s when a storm at sea uncovered it. In addition these balls had “Pictish” symbols which are clearly of post-Roman times. But this evidence has had almost no impact on the thinking of the “self-styled experts.” The test was actually carried out on these balls and published; if the authorities had a counter-test, surely they would have published it. In effect the evidence was discretely placed aside because it seems it “distracted attention from the possibility of an alternative approach to the problems of the past.”

Although this scientific and technological evidence is far from definitive, it does undoubtedly point to a chronology that places the Megalithic Age in post-Roman/Medieval times—especially the medieval Pictish symbols. Nevertheless, let us turn to historical evidence as it relates to our thesis.

If, indeed, the great megaliths that dot the British, Irish, Scottish, and Breton landscapes were actually constructed, as we have been told, around 3000 to 1500 B.C., then the Greek and Roman writers would surely have seen them and communicated this fact in their writings somewhere. Caesar traveled through Gaul and invaded Britain briefly, later followed by the Emperor Claudius who incorporated much of England into the Roman Empire. For about 360 years Romans lived and ruled there, communicating with the Senate, etc., and writers in Britain and Brittany wrote about these regions. Yet over this great expanse of time, not one Greek or Roman ever remarked that these lands contained thousands of great stone monuments. The first important investigator to point this out as fully and exhaustively as possible

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6 Mortimer, R.J., *op.cit.*, p. 60
was Algernon Herbert in his 1849 book *Cyclops Christianus: or An Argument to Disprove the Supposed Antiquity of Stonehenge and other Megalithic Erections in England and Brittany*. Before addressing his evidence, however, we must examine the single isolated comment about the possible knowledge of Stonehenge or some other stone monument by the Greek writer Diodorus Siculus. Many writers have cited Diodorus to just this effect. Atkinson forthrightly states:

“In view of the relative paucity of Classical accounts of Britain, it is hardly surprising that no certain mention is made of Stonehenge [or the other megalithic monuments] by any ancient author, even though it must surely have been regarded as one of the wonders of the world, had it been known to them. A passage from a lost *History Of Hyperboreans* [History of the Far Northern Peoples] ascribed to Hecataeus of Abdera (c. 300 B.C.) and quoted by Diodorus Siculus in his *History* [Bibliotheca Historica] (book V) has sometimes been taken to refer to Stonehenge though without sufficient justification. ‘Hecataeus and some others,’ says Diodorus, ‘tells us that over against the land of the Celts there is in the ocean an island not smaller than Sicily situated under the constellation of the Bear, which is inhabited by Hyperboreans…The inhabitants honour Apollo [the Sun god] more than any other deity. A sacred enclosure is dedicated to him in the island, as well as a magnificent circular temple adorned with many rich offerings…’ The recent discovery of the carvings of weapons at Stonehenge, which may be votive representations, makes it tempting to see in them a confirmation of the suggested identification of the monument with the ‘magnificent circular temple’ of Apollo. But the temptation must be resisted; for it is not even certain though admittedly probable that the island of the Hyperboreans was in fact Britain.”

It was quite surprising to find Atkinson translate Diodorus to the effect that the temple was “circular,” when in fact the Greek word he specifically used means “sphere” or “spherical.” In this respect Lynn E. Rose, who studied ancient Greek, criticized Emmet Sweeney who employed the word “round.”

“Diodoros wrote in the first century before this era, and indicated that his account of the Hyperboreans was derived from ‘Hekataios and certain others’…”

“A more responsible translation might be ‘sphere-shaped in form.’ (That is not redundant; Greek uses repetition for emphasis.) In any case, the Greeks knew the difference between a sphere and a circle. If they said ‘sphere’, they meant sphere, not circle. If they said ‘sphere-shaped in form’, they meant sphere-shaped in form,

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7 reprint 2010 Kessinger Publishing, Whitefish MT
8 Atkinson, *Stonehenge, op.cit.*, p. 183
not round. How dare the translator make this into ‘round’?...The translator, ‘knowing’ that the text is supposed to suggest Stonehenge, but realising that Stonehenge is circular and not spherical, chooses to translate according to received opinion rather than according to the Greek.

“In any case, there is no circular in the text, there is no mega in the text [meaning ‘great’] and there is no lithic in the text [meaning ‘stone’]. Any circular, megalithic structure here [in Diodoros] is conspicuous only for its absence!…

“As matters stand, there is no reason whatsoever to believe that either Hekataios or Diodoros had Stonehenge in mind. Trying to use this particular text as evidence for the antiquity of Stonehenge is totally without justification.”

The ironic aspect of accepting Stonehenge as the site Diodorus mentioned has to do with chronology. Not only Stonehenge, but all these megalithic monuments according to the established chronology were abandoned around 1200 B.C. No one was worshipping there in later Greek times that we know of. So how could the Greeks of around 500 B.C. have known that it was still in use when archaeologists and historians suggest it was no longer an occupied site for worship? These megalithic sites had supposedly been abandoned and neglected for 700 years before Hecataeus; he could not have been referring to any abandoned and neglected sites.

John David North has also injected himself into this translation of Diodorus with his absolute assured misunderstanding of Greek. “Was the spherical temple of the Hyperboreans a vast dome? (There’s a word ‘spherical’, that Krupp should have silently changed to ‘circular’. The Loeb translations are not yet sacred books.)” The Loeb Classical Library books are assuredly not sacred, but they are the most authoritative texts based on the best knowledge of the Greek language. The only reason North has decided, strictly on his own, to ignore that factual detail when constructing his own preferred model of the past is “to use this particular text as evidence for…Stonehenge…without justification.” North has taken it upon himself without linguistic proof to suggest that the editors of the Loeb Classical Library don’t understand the Greek language but that he does. Of course, he never presents any linguistic evidence to support his ex cathedra assertion that “spherical” means “circular.” The words are quite different even in ancient Greek.

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10 North, Stars, Minds and Fate..., op.cit., p. 24
For example, Burl himself applies the words of Diodorus to a megalithic site known as Callanish.

“The Greek historian Diodorus Siculus, of Sicily, quoted from a lost history by Hecataeus…It appears to claim that an early voyager, maybe Pytheas, sailing around northern Britain had seen a lunar ‘spherical temple’ on an ‘island no smaller than Sicily.’ In its path across the sky ‘the moon as viewed from this island appears to be but a little distance from the earth,’ an event unique to the latitude of Callanish. Diodorus added that ‘the god’ visited ‘the island every nineteen years,’ the 18.61-year cycle of the moon.

“The latitude of 58º north is critical. Nowhere farther south in Europe could the major moon between its rising and setting seem to skim the horizon. [Farther south] it would rise much higher in the sky. Not until 58º south of the equator, around Cape Horn, does the same lunar phenomenon occur.”

But Burl is well aware that all these megalithic monuments had been abandoned by about 1500 B.C., and says as much: “By 1500 BC, like all other stone circles in Britain, Ireland and Brittany, the ruin of Stonehenge was abandoned.” Apparently, the word “abandoned” doesn’t mean “abandoned.” Furthermore Burl tells us:

“No smaller than Sicily’ presents an insoluble problem. The British mainland is almost ten times larger than Sicily so that ‘no smaller’ would be an exceptional example…Conversely, although the island of Harris and Lewis [Lewis is the part of this double island where Callanish is found] is less than a tenth the size of Sicily the archipelago of which it is a part covers an area almost exactly that of the Italian island.”

Here Burl translates “an island not smaller than Sicily” to mean “an archipelago not smaller than Sicily.” But again, the Greek, word for “island” means “island” and not “archipelago.” Various investigators have tried to prove that the “spherical temple” must have been a description of Stonehenge or other megalithic site. For example Castleden points to a flaw in the logic:

“The spherical temple is more problematic. The word ‘spherical’ seems too puzzling a description at first, but becomes clear when we remember that the word was sometimes used in antiquity as a synonym of ‘astronomical’. The spherical temple is

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11 Burl, The Stone Circles of Britain, Ireland and Brittany, op. cit., p. 205
12 Aubrey Burl, Prehistoric Stone Circles (Princes Risborough UK 2005), p. 48
13 Aubrey Burl, From Carnac to Callanish: The Prehistoric Stone Rows and Avenues of Britain, Ireland and Brittany (New Haven CT 1993), p. 64
thus a reference to the incorporation of celestial sightlines into Stonehenge’s design rather than its circular shape as many have assumed…this [descriptive interpretation] is a window into the distant past [about 700 years earlier, before Stonehenge was abandoned] rather than a description of Britain as it actually was in 330 BC.”\textsuperscript{14}

In this case Hecataeus somehow determined that Stonehenge was being used to make astronomical alignments to the Sun and/or the Moon 700 years after it was abandoned. How Hecataeus gathered that long lost information, Castleden does not explain. He, like all the others, has a theory about Stonehenge and gathers and omits evidence to fit his model. There is no solid or substantial evidence for any of these models of Stonehenge, Callanish, etc. It is purely conjecture. Lastly, James Fergusson as long ago as 1872 pointed out the fatal problem in interpreting the site of the Hyperboreans with the British Isles from Diodorus’s own book: Fergusson’s words have been completely “ignored” in spite of the fact that they thoroughly contradict all that has been surmised by more modern exponents of the erroneous supposition that the Hyperboreans lived in Britain:

“It is not too much to assert that there is not one single passage in any classical author which can be construed as alluding directly or indirectly to the megalithic remains on these isles or on the [European] continent. With all their learning and industry, the antiquaries of the last century could only find one passage which, with all their misapplied ingenuity, they could pervert to their purposes. It was this—in his second book, Diodorus, quoting from Hecataeus, mentions…there existed among the Hyperboreans a circular temple magnificently adorned. Stukeley and his followers immediately jumped to the conclusion that the island not less than Sicily and opposite Gaul must be England, and the circular temple Stonehenge, which was consequently dedicated to Apollo and the serpent Python, and our [British] forefathers were the Hyperboreans, and our intercourse with Greece clear and frequent. It is marvellous what a superstructure was raised on such a basis. But against it may be urged that the whole of the second book of Diodorus is dedicated solely to a description of Asia [not Europe]. In the preceding chapter he describes the Amazons…In the following chapters he describes Arabia, and even in this one (xlvi.) he speaks of the Hyperboreans as inhabiting the northern parts of Asia. By the utmost latitude of interpretation we might assume this island to have been in the Baltic [sea]—Æsel [Island] probably, Gothland possibly, but certainly not further west. It is impossible Diodorus could

\textsuperscript{14} Castleden,\textit{ The Making of Stonehenge, op.cit.}, p. 240
be mistaken in the matter, for in his fifth book he describes the British Isles in their proper place, and with a very considerable degree of accuracy.”¹⁵

If Diodorus wanted to place this spherical temple of the Hyperboreans in Europe and specifically in the British Isles, he would undoubtedly have done so in his fifth book on Britain and not in his second book on Asia. All these allusions to Stonehenge or Callanish being the site of the spherical temple in Diodorus are fraught with so many serious problems, actually contractions, as to be held for naught.

For example Ronald Hutton tells us that:

“Diodorus had both much less interest in, and less information concerning, north-western Europe than the Mediterranean and Near Eastern lands: he himself seems only to have set foot in Italy, Sicily and Egypt. None the less, he included sections on both Britain and Gaul. The former section placed even greater emphasis on the primitive nature of the British than Caesar had done, calling them a simple and poverty-stricken folk who lived in homes made of reeds or logs.”¹⁶

It is these primitive people who supposedly built a spherical astronomical temple. However, it should not fail to be mentioned that if Stonehenge or Callanish was intended to be the site of this temple, there is never any mention of any “alignment” or “festival” to the Sun-god Apollo. In fact, mention is made to the Moon. What we have in this case is a historical document by Diodorus citing another historian, Hecataeus, about a place that cannot be clearly known and upon which a mountain of conjecture has been built. More than that, no god equivalent to Apollo could have been in the British prehistoric pagan religion since no records of that religion exist. The evidence for Diodorus’s account is simply conjecture and interpretation without evidence to sustain it.

The only way for modern investigators to prove that Diodorus meant the British Isles when he described this Hyperborean temple is to translate the word “Asia” to mean “Britain.” Why have modern investigators failed to discuss this and omit this damning evidence from their discussions? If “omission is impermissible” or “complacently ignor[ing] factual details,” by what reason is Fergusson’s critique to be so casually dismissed?

¹⁵ Fergusson, Rude Stone Monuments…, op.cit., p. 8
¹⁶ Ronald Hutton, Blood and Mistletoe: The History of the Druids in Britain (New Haven CT 2009), p. 6
But let us return to Algernon Herbert’s thesis that these megalithic monuments were erected in post-Roman times. Before doing so it should be pointed out that the absence of any mention of these megalithic monuments by classical Greek and Roman authors is similar to the failure of ancient Near Eastern authors in mentioning entire civilizations as we have discussed in the previous volumes. There was no clear discussion by the later empires that we know about in the literature of the Hittites, Mitanni, and Sumerians because, as the forensic historical evidence we presented shows, they did not exist. They were actually the Lydians, Medes and Chaldeans. So too, the absence of any mention whatsoever of these stone monuments by classical authors suggests that these megalithic structures were not seen because they did no exist at that time but were built after the Romans departed Britain. As Herbert writes:

“It has long since forcibly struck me, that there could have been no such megalithic system as we are now taught..., in the days of those ancient [Greek and Roman] authors who described Britannia. If the concurring silence of persons well disposed to mention such particulars [as stone circles and standing stones sometimes in rows or large circular and long mounds] be evidence of non-existence of the Stonehenges, Aveburies, Carnacs, and all such giant fabrics as modern ages have admired, did not exist in Britain or Gaul when Caesar, Strabo, Diodorus, Mela, Pliny, and Tacitus wrote. Nothing could be more new and admirable to the eyes of a Greek or Roman, than the sight of structures so rude and uncouth, and yet so stupendous. It is naturally possible, that Egypt might have been described by many, and yet the Pyramids unmentioned; but morally, it is impossible. And so, it is a thing passing rational belief, that such a system of colossal works should have remained unalluded to in prose or verse from the landing of the legions of Julius [Caesar] to the departure [in A.D. 410] of Stilicho’s [legions], had they been in existence [for nearly four centuries]…

“Opposite to the Rhine (says Pliny) is Britannia, rendered famous by the writings of our people and those of the Greeks...Many authors (says Tacitus) have treated of the territory and people of Britannia, and I do not pretend to compete with them in genius or diligence...A fair sprinkling of these authors, besides Pliny and Tacitus themselves, have come to our hands; and they, who are lost to us, furnished the extant authors of antiquity with their several allusions to this island and its people. Various and minute particulars were recorded concerning the productions, arts, and customs of Britain; and in so doing, their full share of attention was given to the Druids, their learning, religious rites, and magic. The consecration [by the Druids] of woods and groves had nothing to astonish the Greeks and Romans. The mistletoe [plant] was but an addition to the catalogue of...magic plants or herbs. But men whose observation was attracted by such a silly bauble as the ovum anguinum [mistletoe], had no eyes to see and admire
[throughout Brittany and Britain] an architecture, sublime and awful, and widely
distinct from other European modes of construction.”

Herbert answered the problem invoked by any critic who, knowing the classical
authors never mentioned the megaliths, still maintain they existed in Greco-Roman times:

“Monsr. de Caylus seems to have felt, to what conclusion his arguments were
leading him on, for he professes his inability to account in any way for the silence
of all antiquity [regarding the megaliths and great burial mounds]; but his mind
either shrank from that conclusion, or from the opposition and ridicule its avowal
would have encountered. He pronounces it impossible to form any opinion upon
the subject of their origin; with this remark, ‘no one would maintain, that these
monuments [in Gaul] and those of England were raised after the destruction of the
Roman empire.’ But that is the very truth which, from his own reasonings and
others, I have adopted and maintain.”

Jean-François-Albert du Pouget Nadallic gives one possible reason for this silence:

“We must not, however, omit to mention one serious objection. Roman
historians, exact as are their descriptions of Gaul, Britannia, and Germania, are
silent as to the stone monuments. Tacitus does not refer to Stonehenge or to
Avebury. Caesar was present at the naval battle between his own fleet and that of
the Veneti, in the Gulf of Morbihan, and if the megalithic monuments of Carnac
[in Brittany] were then there, would they not have arrested the attention of the
great captain? This silence is the more inexplicable as one of the earliest
geographers mentions the stone of Iapygia; Ptolemy speaks of a similar stone on
the shores near Cape Cuneus; Quintus Curtius of an important alignment in
Bactriana; Pliny, who mentions a…pillar in Asia Minor, says nothing of the
megalithic monuments of Gaul, which he crossed several times. Moreover
[Gaulish writers such as] Ausonius, Sidonius, Appollinaris, and Fortunatus who
are so eager to glorify their own land [Gaul] maintain a similar silence with regard
to these structures…We may therefore, perhaps, conclude that these decayed and
clumsy-looking monuments were despised for generations, no one realizing their
importance or caring to penetrate their secrets.”

On the other hand, a reviewer states:

17 Algernon Herbert, Cyclops Christiannus; or An Argument to disprove the supposed antiquity
of Stonehenge and other Megalithic Erections in England and Brittany (London 1849), pp. 19-20
18 Ibid., p. 28
19 Jean-Francois-Albert du Pouget Nadaillac, Nancy Bell (N. D’Anvers) translation, Manners
and Monuments of Prehistoric Peoples (NY 1892), p.. 223-224
“Yet all are silent. The fact is unimpeachable. Of one thing we may be certain that, whatever the cause of the silence, it is not to be attributed to their underrating such striking [stone] objects. We should as soon expect that Herodotus would have walked through Egypt, and omitted all notice of the pyramids. It would be, indeed, past belief that men like Caesar, Tacitus and Pliny—the Napier, the Macaulay, and the Heeren of the age, to say nothing of a host of other poets as well as prose writers, by whom Britain was rendered famous—should even if they themselves had not felt very deeply impressed by such monuments, have thought an Abury or a Stonehenge unworthy of notice in their pages…

“Many of these authors have minutely portrayed the Druidic system in Gaul as well as in Britain; they have dealt with graphic horror upon the fierce rites and incantations, the forest temples, the savage gods. It is inconceivable, therefore, that they should intentionally, and as if by concert, have been silent upon these the most stupendous features of a foreign superstition. We can hardly evade this difficulty by supposing that such works escaped the notice of observant men, especially Caesar…He it was who spared no effort to eradicate an order [the Druids] that exercised such dangerous influence. He invariably acted upon the method enjoined upon the iconoclasts of the scripture history, ‘to destroy the high place and cut down the groves’ [of the Druids]. Tacitus expressly tells us ‘that the groves sacred to cruel superstitions were cut down,’ but he says not a word about the destruction, still less of the existence of Temples or other monuments in any way…Even the materials of which they were composed might offer a temptation (supposing there had been no other) too strong to be resisted by an active nation of improvers [and builders], who could feel no interest in such remains. Here were abundant quarries [of stone] for the construction of roads, fortifications, or other works to which their attention was turned.”

Provocatively he adds, “The difficulty that strikes every thoughtful writer, without question, is to find a satisfactory date for these monuments. One is too remote, the other too modern.”

Fergusson concurs with Herbert:

“As before mentioned, no classical author alludes, either directly or indirectly to these megalithic structures; yet they could not have been ignorant of them if they existed…The Romans occupied Old Sarum [near Salisbury, Britain] probably during the whole time they remained in this island, and the [Roman road] Via

20 “The Christian Remembrance” [Review of Cyclops Christiannus], Quarterly Review vol. XXIV (July-December 1852), pp. 5-6; published by John Petheram of High Holborn
21 Ibid., p. 6
Badonica passed so immediately under Silbury Hill that they could not have been ignorant of either Stonehenge or Avebury. Nor in France could they possibly have missed seeing the numerous dolmens with which the country is covered. Notwithstanding all this, the silence is absolute. The [spherical] temple of the Hyperboreans is the only thing any one has ever pretended to quote against this; and that, for reasons given above [by Fergusson] being inadmissible, any argument based on it falls to the ground.”

William Long gives what he conceives to be the most objective explanation for why these stone monuments were ignored: “To the writer, the best reason for the absence of any notice of Stonehenge by the Romans is this; that to any educated Roman who was familiar with the grand and magnificent works of Rome and the neighbourhood under the late Republic and early Empire, such a work would appear to be rude in form, puny in effect, and scarcely worthy of any special notice.” What Long has done is project a snobbish character onto the Romans, in order to evade the implications of the fact that the Romans are all totally silent regarding these monuments.

We wholeheartedly agree with Herbert, et al., that “it is a thing passing rational belief, that such a system of colossal work should have remained unalluded to in prose or verse.” Here we observe the struggle of those proponents of the established chronology to get around a simple fact. Because they assume, without a vestige of scientific proof, that these monuments had to have been constructed in pre-Roman times, they are forced to invent assumed reasons to that effect such as the Romans were effete snobs who would not deign to write about such rude, crude monuments. And how do we know or test this explanation? We don’t know, nor do they.

One of the most unusual aspects of all this is that the Romans not only didn’t describe these megaliths but they also failed to use them as quarry stones for their temples, villas etc. Surely these stones should have found their way to Roman sites, but this is not reported. Here was a great quarry of stone that the Romans never used. Now if the educated Roman snobs failed to mention these monuments, the common folk had a use for them which was a common Roman practice. Many Romans must have visited or stopped at these sites, had they existed, and would have had a use for these large standing stones.

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22 Fergusson, Rude Stone Monuments..., op. cit., p. 20
According to Burl:

“Two thousand years ago it [Avebury] was very much as it had been when it was built. Maybe a stone or two had fallen amongst the thick grass. …Yet it is likely that a Roman visitor saw the circles in their entirety uncluttered by huts or barns. Since 2000 BC they had been almost deserted. Scarcely any Iron Age pottery or equipment has been excavated here and any native being asked what Avebury was might have replied as Africans did when questioned about the ancient Gambian stone circles, simply and with a shrug of ignorance ‘that the olden people did it’…

“Avebury survived in the safety of neglect. The Romans altered nothing. In the early centuries AD tourists came from…nearby towns…travelling along the new road that had been aligned on the landmark of Silbury Hill. They were fascinated by prehistoric places just as their contemporaries were by the pyramids. One visitor lost a bronze brooch almost immediately after the Claudian invasion of AD 43. Sherds of Romano-British pottery washed down into the ditch. Sometimes these semi-Romanized Britons continued native traditions…‘Roman coins have been found in…the [Silbury] mount’ wrote Stukeley.”

It is assumed the Romans as tourists visited far and wide to see various megalithic monuments even as far as Ireland where Roman coins are found at Newgrange as votive offerings, but even given this assumed veneration to foreign gods, no one deemed it necessary to write of these. But the more shocking point is that the Romans, famous for writing, scratching, engraving and painting graffiti, failed to leave a single graffito on a single standing megalith stone there. Wherever the Romans reigned, they scratched graffiti, just as they did in Rome and in the towns, villages, and cities of Italy. But if, in fact, these monuments existed during the almost four centuries of Roman occupation of Britain, the common people who were not known to be effete snobs—quite the opposite—would have found these standing stone monuments ideal places to paint, scratch, engrave graffiti. Are we to assume that the common people were also too aloof to deign writing MARCUS GRAFFITO (like our famous KILROY of World War II fame) WAS HERE? The fact of the matter is that the Romans were famous or infamous for writing graffiti wherever they stayed. Amanda O’Neill reports that “Hadrian’s Wall [built by the Romans across northern Britain] is rich in Roman graffito—much like modern soldiers’ inscriptions ‘Lucius was here, Our centurion is a [expletive...

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24 Burl, Prehistoric Avebury, op.cit., pp. 25-26
deleted], British weather sucks’…” John Barrett and David Iredale further report: “The quarry workers of Roman Britain carved informal graffiti in rustic capitals and cursive script on exposed rock faces. A famous group of runic graffiti was carved on the walls…at Maes Howe in Orkney [by Norsemen in post-Roman times], including such gems as ‘Ingigerd is the sweetest woman there is’…”

Frank J. Korn describes how

“…[t]hroughout the Old Empire, ancient [Romans] used walls and monuments in lieu of flyers and leaflets for propagandizing…”

“Suetonius informs us that during the tyrannical reign of Nero, the populace took potshots at their ruler by painting insults on the pedestals of his ubiquitous statues. One likeness was bedecked with a leather sack around its neck. (Suffocation with such a device was the customary form of execution for parricides back then.) This was meant to express the widely-held opinion that Nero had murdered his mother Agrippina…

“To a bucket of paint and a brush, some scribes preferred a sharp stilus—graphium—for etching their sentiments on surfaces. (Hence our word graffiti.)…

“These scrawlings were not all political or revolutionary, however. Some were religious or philosophical, some commercial, many amatory in content…Cicero mentions that down in Siracusa there were graffiti throughout the town cataloging the extramarital dalliances of Pipa, the wife of a certain Aeschirion…

“There were also mischievous, prankish ‘Kilroy was Here’-type notices. One such notice in Greek, found in the seaport of Ostia, says ‘Polloi poll’epegrapsan. Ego monos ouk epegrapsa,’ which loosely translates to: ‘Everybody scribbles here except me.’”

With respect to Brittany and Britain there are stones set up by the Romans; according to Thomas Wright, that these are

“often of the Roman or post-Roman period…is proved sometimes by inscriptions. Several such inscribed stones have been found in Wales and Cornwall; and there is a celebrated one near Joinville in France, with the inscription in Roman characters, VIROMARVS ISTATILI F (Viromarus the son

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26 John Barrett, David Iredale, Discovering Old Handwriting (Princes Risborough UK 2001), p. 87
27 Frank J. Korn, Hidden Rome (Mahwah NJ 2002), pp. 65-66
of Istatilius). Two, found in the neighbourhood of Neath in Glamorganshire…were perhaps boundary-stones or mile-stones.”

Although Wright believed them to be ancient megaliths, this is simply not the case because this author sent an email to Christopher Chippindale on September 9th, 2009 wherein I asked: “Could you kindly inform me whether Roman graffiti was ever found at Stonehenge or any other megalithic site in Britain?” Either he or one of his aides responded on October 13, 2009 “Not to my knowledge…” There are indeed axes and a sword carving at Stonehenge, and axe-heads at other sites. These are thought by some to be prehistoric graffiti, as Jenny Blain and Robert J. Wallis suggest: “Physical inscriptions of meaning in the monument include those constructed as damage (recent graffiti), those seen as archaeological evidence (Bronze Age dagger, axe-heads—constituted as ancient ‘art’ or ‘graffiti’ depending on which discourse one subscribes to)…”

Lastly, not only didn’t the Romans inscribe graffiti on these monuments but neither did the indigenous Celtic Bretons of Gaul do so, according to Sheppard Sunderland Frere:

“In Gaul, after the [Roman] conquest, inscriptions in the Celtic tongue are not unknown [but are not found on megalithic monuments], though now carved in Roman characters. In Britain, however, no Celtic inscription of the Roman period has been found…

“The evidence [there] of graffiti scored by the owners of pottery and other objects is so widespread that we can be certain that at least in the towns the use of Latin…penetrated very deeply, and this class of evidence extends to villas also.”

In Brittany the Celtic people failed to leave a single graffito on a single megalith. Only in prehistoric times, supposedly (axe-heads and a dagger as possible graffiti), and post-medieval times did anyone inscribe graffiti on them, and nothing in-between these periods, as Rosamund Cleal et al. conceded:

“…the monument[s] have been inscribed with graffiti. Although the majority are almost certainly post-Medieval in origin, a number [of axe-heads and a dagger] are apparently prehistoric.”

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What this comes down to is that in prehistoric times the axe heads etc., found carved into Stonehenge and other megaliths may have been either initially art or graffiti, and in post-medieval times, long after the Romans left Britain, no one carved graffiti into these monuments. While the Romans carved and wrote graffiti into Hadrian’s wall and on pottery when they inhabited Britain, they failed to do so on these megaliths which littered the landscape. While the Scandinavians carved runic inscription at Maes Howe in the post-Roman era, no one did so prior to or during the Roman occupation. While the Bretons of Gaul left inscriptions of their language carved with Roman letters, they too failed to leave a single graffito on any of the Brittany standing stones. But long after the Roman exodus of Britain and the collapse of its empire on the continent of Europe, people got the itch and suddenly began to carve graffiti. Between 2600 B.C. or earlier, only axe-heads, etc., not proven to be graffiti, were engraved into these monuments and then for perhaps 3000 years no one deigned to immortalize himself on these stones with a single graffito. To paraphrase Algernon Herbert, “It is a thing passing rational belief that such a system of colossal works should have remained [uninscribed with graffiti] from the landing of the legions of Caesar to post-Medieval times.”

The response to the problem of the missing graffiti has been, so far as we have found, conspicuous by its absence. However, the argument against these monuments being unknown to the Romans has been the subject of criticism. William Long argues:

“If Stonehenge [or the other numerous megalithic monuments] were erected within the three hundred years which preceded the Christian era, it would have been in existence when Hecataeus wrote. At all events, we shall never, from this vague statement, be able to emerge from the region of cloudland, and take a step upon ‘terra firma.’ Mr. Herbert, in his ‘Cyclops Christianus’ has devoted a large portion of Section I to prove that by this island Britain could not possibly have been meant.

“No Roman makes mention of Stonehenge.

“No Gildas, Nennius, nor Bede make mention of Stonehenge. The Saxon Chronicle makes no mention of Stonehenge.

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“Nearly 1200 years of the Christian era roll away before the curtain is raised at all, and we get a peep at Stonehenge by Henry of Huntington, who died after 1154.”

The point is seemingly telling. If Stonehenge and all the thousands of standing megalithic stones were set up after the Romans left Britain, surely the Venerable Bede in A.D. 672 or 673–735, or Gildas ca. 500–570 A.D., or Nennius, who lived in the 9th century A.D. or other writers in Britain prior to A.D. 1100 would have surely known of them. What possible reason could there be for every one of these writers not to ever mention them, nor even those who composed the Saxon Chronicles that cover this period? If our thesis is correct, we must present rather unimpeachable evidence to show how these monuments existed all through this time but were not seen. While in the 1800s this contradiction to our thesis and that of Algernon Herbert and James Fergusson, etc., was valid, subsequent environmental research has cast an entirely new and unsuspected light on this question. The first point was made by Sir John Lubbock who tells us:

“When we turn to England…the first thing that strikes us is the singular paucity of megalithic remains that are found in fertile and inhabited parts of the country. Those of Cornwall…belong to the Aquitanian province. Those of Dartmoor, Wales, and Cumberland have not been described [as of 1870]…One thing about them is that they must have been erected after the people to whom they must belong were driven from the fertile plains into the inhospitable fastnesses where we find them. It seems impossible to believe that the moors of Dartmoor and Cumberland, or the rugged hills of Wales were [greatly] inhabited…but though we find in the cultivated parts of England earthen mounds and earthen burying places of all sorts, and in great abundance, the megalithic remains are very few and far between."

Lancaster Brown in this respect states: “Many megalithic sites are situated in remote spots amid boggy moorland far from roads and even cart tracks.” Brad Olsen, speaking of France, says of the menhirs: “Sometimes their remote locations saved them from being dismantled.” Related to this happening in post-Roman times, David Clegg writes of “the greatest concentration of…megalithic tombs in Britain” are located in “Cornwall dominated by its location at the remote and isolated far west of England…extraordinarily rich in [megalithic] prehistory. It

32 Long, op.cit., p. 7
33 Lubbock, “Non-Historic Times,” op. cit., p. 246
34 Lancaster Brown, Megaliths, Myths and Men, op.cit., p. 164
35 Brad Olsen, Sacred Places Europe: 108 Destinations (San Francisco CA 2007), p. 91
provided a haven for the Celtic culture, that was driven out of lowland England by Romans and Saxons, and which continued in the far west, largely untouched, until the Norman occupation…it was only with the coming of the railway…that Cornwall’s isolation truly ended.”

Not only are the megalithic sites remote, they appear to be constructed mostly by Celts who were driven out of the lowland agricultural regions of Britain. The other answer is not that these monuments were either ignored by the various British and other writers on the Continent, or that these megaliths did not exist, but because they were hidden in the immense forest growth that re-grew all over Europe after the fall of the Roman empire. John J. O’Neill points to what occurred with the fall of Rome:

“The land had dropped out of cultivation; trees and shrubs rapidly encroached upon the once cultivated land…the forest and the malarial swamp regained their sway over vast tracts of country which had been covered with prosperous farms and waving fields. The word *eremus*, wilderness, recurs with significant frequency in mediaeval land charts.”

We shall have need to return to O’Neill’s words in the latter part of this book but it is clear, even from excavations at Stonehenge, that sometime after the supposed first phase of it was begun, the land returned to forest. This environmental change has been explained by John Evans, *Land Snails in Archaeology* (London 1972). Different types of snail habitats are occupied by different species. While there are preferences, these are not exclusive. By careful sampling of a representative population at a site one can determine the nature of that environment. For snails moisture in the soil is the greatest indicator of whether the region was forest or grassland. Certain snails strongly tend to occupy dry grasslands, while others favor the shade and moisture of woodlands. By taking a large enough sample of species, a histogram can be constructed that tells whether the environment was grassland or forest or something in between. Chippindale has briefly summarized Evans’s work with regard to the environment around Stonehenge:

“Stonehenge today looks pretty well placed for astronomy. The horizon is reasonably flat, and at a good distance in every direction, so a long-distance

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36 David Clegg, *Cornwall and the Isles of Scilly* (Leicester UK 2005), p. 15
alignment can be made with good accuracy to the Sun or Moon as they rise or set. . . . [However] the precise direction of rising or setting depends critically on the vegetation cover. This is most noticeable at the midsummer solstice, when the Sun rises behind woods, and the exact point at which it becomes first visible . . . depends on the height of individual trees. If prehistoric Stonehenge stood in a small forest clearing, or even in mixed scrub which obscured the far horizon in many directions, then its ‘astronomical alignments’ are meaningless.

“So what was the prehistoric vegetation around Stonehenge? Pollen analysis, the standard technique for vegetation history, is impossible because pollen is destroyed in the dry, alkaline soils of the chalkland. Accordingly, John Evans, an environmental archaeologist at University College, Cardiff, has studied the ancient land snails whose shells preserve well, to reconstruct the environment.

“Evans tracks the populations of different species of snail through time, as shown by their shells in buried prehistoric sol deposits, taking special note of those that are restricted to woodland or grassland habitats.

“His major discovery is an entire period, not suspected before, during Stonehenge I when the entire site was abandoned. At a date around 3100 BC the Stonehenge ditch silted up naturally, and the mollusks show the site was covered with scrub or woodland. Later, the mollusks show evidence of the site being cleared a second time; then a predominantly grassland environment continues through the later phases when the [later sarsen] monumental structures were built . . . But even then, some woodland indicators are present, suggesting patches of scrub or even forest not far away, which may well have blocked a view of the horizon, rather than an unbounded spread of clear grassland.

“A feature of the most astronomical views of Stonehenge is an intellectual continuity in what was observed from the earliest phase of bank, ditch and circle of Aubrey holes right through to the [central] sarsen monument [built] a thousand years later. Evans has now proved a distinct break in detectable human activity on the site, and one which tallies with the survey evidence of occupation in areas well away from Stonehenge.

“The [supposed] unbroken sequence of astronomical learning, [that is assumed] over so many generations, has never seemed satisfactory to the archaeologists; now there is evidence that it may after all be illusory [because forest growth made seeing alignments out to the horizon quite impossible].”

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It is thus no surprise that the identical environmental change occurred after Rome collapsed as described by Gilbert F. LaFreniere:

“Perhaps ‘The Dark Ages’ was not a misnomer for the early Middle Ages after all. The half a millennium between the fall of the Latin Roman Empire and the second wave of invasions (Viking, Magyar, and Muslim) by around 1,000 [A.D.] was a period of relatively low population which continued the population decline of the late Roman Empire for at least several centuries. Decline of agrarian populations throughout the provinces of the former empire continued as a result of: (1) chronic warfare as the Germanic tribes defined their territories; (2) the flourishing of plague and other diseases under conditions of malnutrition exacerbated by the decline of agriculture and trade; and (3) depopulated cities languishing behind their new walls and surrounded by limited agricultural land. Under these conditions the forest returned to abandoned fields and pastures and the native fauna followed its return…Regarding new evidence from aerial photographs in 1990 indicating ‘that the Roman fields had reverted to wilderness,’ C. Warren Hollister wrote that ‘the silent catastrophe they record may well have resulted from the violence and depopulation following Rome’s collapse...aggravated by a great plague cycle that commend in the 540s [A.D.].

“Thus there has been a general belief among medieval historians through the later twentieth century that Western Europe in the early Middle Ages consisted of a sprinkling of depopulated towns surrounded by a vast, unpopulated forest wilderness. Writing in the mid-twentieth century, H.C. Darby observed that during the Germanic barbarian invasions, ‘the woods crept back over upon the many neglected fields.’...Darby further observes that by around 800...a considerable retreat of the forests had already occurred in parts of the empire. However, the [new wave of] barbarian assaults which followed over the next two centuries [the 800s and 900s] would have set back the process of deforestation. Writing in 1991, environmental historian Clive Ponting corroborated Darby: ‘Early medieval Europe was still a vast wilderness with a scattering of small, largely self sufficient villages which had only very limited outside contacts.’ Because the overall population within Western Europe remained low...forest clearance was limited until about the end of the tenth century. After that, forest clearance was completed in the Île-de-France about 1080 and in southeastern England shortly thereafter.”

This vast forest wilderness encompassing Europe from around 500 to 1000 AD obscured the megalithic monuments hidden in that dense growth, as LaFreniere

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39 Gilbert F. LaFreniere, The Decline of Nature: Environmental History and the Western Worldview, Palo Alto CA 2008), p. 100
explains: “Following the second wave of barbarian invasions, the late tenth and early eleventh centuries witnessed the beginning of relentless and continuous deforestation.”

The medieval people living in largely scattered and isolated villages, sometimes near a monastery, did not venture abroad, as Linda Kalof shows:

“It comes as no surprise that medieval people went to such lengths to ensure protection of family and property. Fear was their constant companion. In the early Middle Ages, wilderness and wild animals [bears, wolves, boars, etc.] were sources of fear and anxiety. At that time there were vast areas of wilderness, and what cultivated land there was consisted of small patches at the edge of dense forests, wild areas were perceived as dangerous, dark, ominous and unknown. In both rural and urban areas, cottages, gardens and fields were enclosed against intruders and wild animals by protective fences and hedges, suggesting that the medieval population lived in a constant state of alarm and insecurity, and that people considered it normal to live in this way. As agriculture expanded, predatory animals were driven from the wilderness into towns and villages, and sometimes attacked humans if their…supply of prey animals was depleted. As a result wolves were hunted ruthlessly and so loathed that they were often strung up alive.”

It was only thereafter, at the end of the first millennium, that the forests were cleared and the megaliths emerged from the dense vegetation that they became prominent features and were then recognized by the few literate observers and then reported. Fred S. Kleiner describes what the landscape looked like and what ensued as the population grew: “Monks reached into the surroundings and helped reduce vast areas of daunting wilderness of early Medieval Europe. They cleared dense forest teeming with wolves, bear and wild boar; drained swamps and cultivated wastelands and built roads, bridges [etc.,]…”

This clearance of the forest wilderness is identical to that described for the supposedly 2000–3000-year older Neolithic farming peoples. The early prehistoric farmers could not maintain their fields for long periods of time because they did not know how to replenish with manure the fertility of the soil they cultivated. As a result the early prehistoric farmers were forced, after clearing the land and planting

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40 Ibid., p. 102
41 Linda Kalof, Looking at Animals in Human History (London 2007), p. 57
42 Fred S. Kleiner, Gardner’s Art Through the Ages: The Western Perspective (Boston, 2010), p. 298
it, to move away to nearby lands after exhausting the soil.\textsuperscript{43} We will specifically cite these sources below.

Because the archaeologists displaced the megalithic builders/farmers in the Late Neolithic period, they cannot see that their description of these early agricultural peoples mimics that of the post-Roman/Medieval farmers. As Georges Duby shows in his unit on “Land Clearance” during early medieval times:

“Breaking new ground was a normal and regular operation in the agrarian system of the early Middle Ages. Every year it was necessary to abandon some of the erstwhile fields that tillage had exhausted and open up new ones on part of the uncultivated area. The slow rotation of ploughlands around the arable, an enormous portion of which was temporarily given over to natural vegetation, made habitual pioneers of the peasantry. They so remained as long as the shortage of manure kept up the tradition of fallowing. In the properly organized system of mixed farming that was emerging at the close of the period under consideration, the first ploughing of the fallow [land] represented the ultimate, residual form of seasonal assarting [recolonization of fallow, reforested fields]. Hence land clearance was firmly integrated into the group of practices associated with cereal-based agriculture. It was primarily a palliative against deterioration of the soil, an indispensable measure for maintaining it on a par with output.

“This process took on quite another economic significance when it ceased to occur in the context of village farmland with stable boundaries, but moved outside them. It then assumed the aspect of a veritable conquest, resulting in a lasting extension to the food-producing area. This defeat of the wilderness was beyond question the great economic venture of the twelfth century in western Europe. Land clearance was [thereafter] brought on by demographic pressure [via the growth of towns and cities] and technical [agricultural] improvements. To farm uncultivated tracts of land, to clear away the natural vegetation and to control water-courses made it essential to have better tools. The heavy waterlogged soils of Schleswig could not have been profitably sown before it was possible to till them with ploughs that were sturdy enough to hollow out deep furrows between long ridges and so create some sort of drainage.”\textsuperscript{44}


\textsuperscript{44} Georges Duby, \textit{The Early Growth of the European Economy: Warriors and Peasants from the Seventh to the Twelfth Century} (Ithaca NY 1974), pp. 199-200
Thus, the medieval farmers were doing exactly what the prehistoric farmers were doing, and only around the 11th to the 12th century had so much land been cleared that the megalithic monuments were exposed. This decline in agriculture we maintain is identical because the same people were involved who lived in post-Roman/Medieval times.

More to the point, if the megalithic structures had existed in pre-Roman times they would have been seen and noted because the land of Britain was cleared of forest to allow for agriculture. Even out-of-the-way stone monuments, especially in southern Britain, would have been accessible. Recall a Roman road passed Silbury Hill near Avebury and Stonehenge. But since the Saxons drove the Celtic peoples into inhospitable mountains, bogs, and moors, these people would have erected these monuments in these isolated areas. With the great reforestation of the land soon after the Roman exodus, the isolated and inhospitable areas would have been covered by forest growth to keep these monuments largely hidden. The people of the tiny villages and towns isolated in this vast wilderness, where few ventured from, even those near monasteries would in general not know about these monuments. But if they did, and were carrying out pagan rituals at these sites, they would hardly have reported these locations to churchmen hostile to these pagan religious practices. The largely pagan population would not inform the Church about them, and may even have murdered Christians who intruded upon their holy sanctuaries. That explains why neither Bede nor Nennius nor Gildas, nor others who wrote the chronicles of those times, knew about them. We further maintain that in large measure this prehistoric chronology goes down into the early Iron Age. Therefore, this identical environmental condition should exhibit itself as evidence in other regions. This can be seen quite clearly in Iron Age Ireland which presents the same change in the habitat from cleared land to forest as discussed by Raftery:

“The evidence for agricultural activity during the centuries of the [Irish] pagan Iron Age is rather surprising. For in several parts of the country, pollen studies in raised bogs strongly suggest that an actual decline in agricultural activity may have taken place during this period. Investigations [in two counties] are particularly informative, and the results have been repeated elsewhere. The picture that emerges seems to suggest that after a period of forest clearance and agriculture in the later Bronze Age, the evidence of agriculture gradually fades away. Weeds traditionally associated with cultivation disappear, then cereals and bracken [also disappeared]. At the same time grass and plantain [weeds] fall in value. Simultaneously, hazel [tree pollen] values begin to rise, followed by
increases in the pollen of ash, elm and oak. The low value of herbaceous pollen suggests that there must have been a considerable expansion of secondary woodland, leaving only restricted areas of the countryside still open. It was not until about the third century A.D. [or in our short chronology until about the 10th or 11th century AD] that this situation was again reversed with what seems to have been a dramatic expansion of agriculture. At Corlea, too, the botanical evidence indicates a significant increase in hazel scrub shortly before construction of the Iron Age [wooden] road…which continued unabated for several centuries. This implies again that the clearings were being filled with secondary forest growth, hinting once more at a decline in agriculture. Lesser rises in elm and ash at this time may be further evidence of such a decline. The decrease in oak pollen at Corlea could well reflect the extent of oak clearance [to obtain massive amounts of logs] for the construction of the road.”

What we have encountered here ties the farming and environmental development of the megalithic people to the farming and environmental development of the post-Roman/Anglo-Saxon people in with the further development—the forest clearance—that followed. This indicates a connection of all these people right through into the 10th and 11th centuries A.D. What we maintain is that this type of historical/environmental connection will also be found with other phenomena that will also be investigated. In each of these cases we will show that what has been assigned to the supposed prehistoric people belongs to the post-Roman era and then continues to develop into later medieval times or even beyond. For example, the end of the Megalithic Age assigned to 1200-1500 B.C. is without a cause. These prehistoric people come out of nowhere, go through a period when they supposedly clear much of the land and build large stone monuments and then practically all disappear from the earth. But in the short chronology they are the Anglo-Saxons who came to the British Isles, who fail to advance agriculturally so that the forest encroaches over the fields and the people take hundreds of years before they clear the land and recommence agricultural development in the 10th and 11th centuries. They stop building pagan megaliths as they are absorbed into the Christian church. We know who they are, where they come from, what they achieved or failed to achieve and why they stopped constructing their pagan temples. All these connections make evidential, logical, and historical sense. With the established chronology nothing connects or makes evidential, logical or historical sense. And this type of incongruity we will encounter again and again.

Raftery, Pagan Ireland..., op. cit., pp. 121-122
All in all this evidence clearly suggests that the sudden growth of forests was not a phenomenon that should be dated to prehistoric Europe but rather to post-Roman, medieval times. The unknown enigmatic collapse of the megalithic world, which supposedly occurred around 1200-1500 B.C., in the short chronology is the end of the pagan belief system in early medieval Europe as its peoples were brought into the folds of the church. The enigma of the Irish Iron Age habitat change that Raftery refers to is no longer an enigma but also correlates with the habitat change that followed the fall of Rome.

EROSION, GRAFFITI, AND THE MEGALITHS

We nevertheless will attempt to date these monuments by both scientific and technological evidence, not by what is missing—the Roman graffiti—but by what is still present on these stones. That, we have maintained, is the only evidence upon which to determine the chronology not only of the ancient Near East but also of the Megalithic Age. In this unit and the following ones we will present several scientific and technical forms of positive evidence to do just that. We believe the first person to suggest this method by which to date Stonehenge itself was the great English astronomer Edmund Halley. According to Anthony Johnson:

“One possibility [for dating Stonehenge] was suggested by Edmund Halley [who] visited [it] in 1720, probably at Stukeley’s invitation. In the same year he exhibited to the Royal Society a piece of polished sarsen [stone] from Stonehenge [from] which he deduced from microscopic study of the weathering ‘that the work must be of an extraordinary antiquity, and for ought he knew, 2 or 3 thousand years old.’”

“Stukeley realized that although the alignment of Stonehenge corresponded to the approximate midsummer sunrise, it did not ‘correspond precisely enough’. From this observation he concluded that the monument had been set up ‘by the use of magnetic compass to lay out the works, the needle varying so much, at that time from true north.’ He attempted to calculate the change in magnetic variation between the observed and theoretical (ideal) Stonehenge sunrise, which he imagined would relate to the date of construction. To achieve this it was necessary to introduce a ‘calibration’ which was apparently achieved with Halley’s help, by reference to 200 years of magnetic records. Halley concluded that geomagnetic variations followed a regular cycle and that every 700 years the compass needle returns to the same point with regard to geomagnetic north. Using this guide, three dates for the construction of Stonehenge were suggested: c. 460 BC, AD 220 or
AD 920, the earlier date being the one accepted by Stukeley. What they had attempted was a precursor to archaeomagnetic dating, a technique developed in the 1950s, based on almost exactly the same principles of measuring temporal shift in geomagnetic declination applied to magnetically enhanced burnt deposits as in kilns and hearths. Stukeley and Halley’s magnetic survey, and Halley’s microscopic examination of the weathered sarsen [stone] amounted to the first attempts to use scientific methods to date Stonehenge.”46

What we will use is Halley’s method of dating monuments on the basis of erosion. However, we will use comparative erosional features not only at Stonehenge but at other megalithic sites that were produced at different times. If we have a graffito that is dated to a particular century, then we can compare it to those that are dated supposedly to prehistoric times. If the graffiti dated fairly close to the present exhibits relatively strong erosional features, we would expect those dated to prehistory would all be eroded away. If, on the other hand, the graffito cut into the stone more recently exhibits strong erosional features and that supposedly of prehistory is not highly eroded, then we can judge on a general level that the supposed prehistoric one had to have been cut into the stone far more recently than is assumed by the proponents of a very ancient Stonehenge.

According to the established chronology, the monuments were erected from about 5000 to about 3500 years ago. Yet prehistoric axes, etc., can be seen etched into their surfaces today. In addition, at other megalithic sites, such as Newgrange and Knowth, designs have been cut into the rocks, standing in the open air and subject to all forms of natural erosion for five millennia. The climate of England, Ireland, Scotland, and Brittany is quite different from that of the Middle East that is generally hot and dry, which will permit engravings and graffiti to survive even in the soft limestone and still be readable. Northwestern Europe has relatively warm to hot summers with a great deal of rainfall but its winters are fairly cold especially at night. Summer forms of erosion are relatively mild on the surfaces of hard rock such as quartzite and granite but erosion does operate.

According to Nathan Cabot Hale:

“As to the effects of time on inorganic substances, there is almost always a wearing down of mass from the surface toward the center. We have all seen steel that has rusted and eroded, or granite rock that has broken from the cliff face and

46 Anthony Johnson, Solving Stonehenge: The New Key to An Ancient Kingdom (London 2008), p. 64
gradually had its jagged edges rounded off by friction, [by] the weather and seasonal changes…The external lines soften and round off; the atmosphere turns the surface to a soft and powdery sand, while the interior will remain fresh and brittle. Only over a great length of time do these great changes occur. The rock will be gradually rounded over thousands of years till only the [surface] sand remains, which blows and drifts like wind and water. From an inert and ponderous mass, the rock finally reverts to substance that can be moved by the purest form of movement…”

Most erosion in northwestern Europe is caused by the extremes of freezing and thawing temperatures that occur during the rather lengthy winter season. Speaking of winter weather in England, Ireland, and Scotland, Marion I. Newbigin reports on Britain:

“There is a tendency for the temperatures in a winter anticyclone to be low. This is because the air sweeping out of an area of high pressure has been dried by compression, and such dry air allows radiant heat to pass through it very rapidly. During a winter anticyclone, especially in February or late in January, when the sun has considerable power, the almost windless air may feel comfortably warm during the day, but no sooner does the sun go down than rapid radiation begins and we have severe night frosts characteristic of winter anticyclones. The length of the winter night and the shortness of the winter day means relatively little absorption [of heat] and very rapid radiation. A winter anticyclone then means clear keen frost, with often cloudless sky and sunny days, and bitter cold starry nights.”

With respect to Ireland, Wesley Johnston shows the same conditions pertain there as they do in Britain:

“Typical winter weather in Ireland is clouds and rain with the occasional sunny spell. The mountains may have snow on them for many weeks in winter, but [snow] falls on the lower ground on only a few days a year, and is generally not a feature of the Irish climate. Temperatures hover around a January average of 5ºC [40ºF]. Overnight temperatures often drop below freezing point, and ice and frosts are common. Each winter there are a few weeks when the temperature does not rise above freezing point all day, and lakes and rivers can partially freeze over.”

Of Scotland, Rosalind Mitchison reports the same climate exists:

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49 Wesley Johnston, “Climate of Ireland” (Internet) *http://www.wesleyjohnston.com/users/ireland/geography/climate.html*
“Scotland, lying far into northern latitudes, with little warmth even in the long days of summer, has a climate that encourages pastoralism…the rapid drop in temperature…comes with height [of mountains] in moist and northern lands, so that the tops of the plateaux are more like the arctic than like other European mountains…the temperature rarely falls low enough to kill grass in the winter.”

Britain, Ireland, and Scotland are subject to fairly long winters that are wet and where the temperatures hover above freezing during the day but drop below the freezing point at night. This kind of weather has a pronounced effect on rock surfaces, known as “frost-action.” If you ever leave a closed, full glass bottle of water in a freezer you will no doubt discover that the expanding ice will crack the glass or even a ceramic container. Because the water expands about nine percent in volume when it freezes, it can crack very hard materials. During winter months in cold, wet northern regions this same frost-action is exhibited in roads where water seeps into cracks and freezes at night to form pot-holes. The same action attacks stone and is extremely destructive. In England, Ireland, and Scotland this process will be enhanced because of the frequent alternative freezing and thawing from night-time to day-time when the temperatures hover near the freezing point. Water will seep into tiny cracks and then freeze to expand the crack so it seeps deeper the next day and refreezes during the next night, eventually creating a large deep crack that splits off from the rock surface. The product of frost-action can be seen at the base of cliffs in the higher latitudes as piles of debris at their bases called ‘talus.’ Frost-action is a very potent form by which rock is disintegrated, even hard quartzite rock such as that of the Sarsen Stones at Stonehenge.

These Sarsen Stones have a great deal of relatively recent graffiti etched into them. They also exhibit both natural and man-made destruction. Castleden describes the monument thus:

“It is nevertheless important that we should see Stonehenge as a wrecked monument; we should not assume that our stone and bronze age ancestors deliberately created such a rough and chaotic [surface] jumble. The rich texture of the stones, patinated by weathering and scumbled with half a dozen different colours of lichen, must also be seen as a past-building feature; when newly dressed and ground, the stone would have been more uniform shades of blue-gray and pearly pink, with the palest surfaces glinting in the sunshine like sugar.”

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It was thought that lichens growing on stone surfaces actually protected them from erosion. However, Allen I. Laskin et al. report:

“Certain lichens can grow endolithically [in stone surfaces]…They are slow-growing, stress-tolerant organisms…

“Under conditions of high abiotic weathering, lichens have been suggested to provide protection for the stone surface from wind and rain through the insoluble…layer [with which they cover the rock], or to limit erosion by reducing the level of water within the rock…; their retention of moisture within the talus [rock debris that will fall] reduces stress on a limestone surface…However, they are generally defacing and intrinsically damaging. Even when a protective effect can be shown…”

But in terms of frost-action, lichens by their very nature of retaining moisture actually enhance this type of erosion, as pointed out by D. Allsopp et al., who show that: “Lichens are able to withstand dessication and rehydration over a much greater range than other plants. This enables them to survive in extreme environments which could not be tolerated by other plants. The holding of water [by lichens] may, in itself, help in the physical weathering of stone caused by frost-action in cold climates.”

However, as we pointed out earlier, Stonehenge and the other megalithic monuments were supposedly built in the Late Neolithic and Bronze Ages (3100-1500 B.C.) or from about 5100 to 3500 years ago, and the Sarsen Stones about 4500 years ago. They still retain the supposed prehistoric carving of stone axes and designs. James Dyer explains that “A number of the stones carry carvings of [bronze] metal axes [at the cardinal point stones of the sarsen circle] and one has a hilted dagger. These may be mason’s marks or even the graffiti of the day. However serious consideration should be given to them as symbols of an axe cult surviving from earlier neolithic times. Stone axes are known from a number of henges…”

Fortunately in the case of Stonehenge there are also fairly modern graffiti carved into one particular Sarsen Stone, number 53, directly above the dagger and axe carvings, and here the amount of erosion on each can be compared and contrasted. Lancaster Brown describes this more recent graffito:

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52 Allen I. Laskin et al., *Advances in Applied Microbiology*, vol. 66 (San Diego CA 2009), p. 120
53 D. Allsopp et al., *Introduction to Biodeterioration* (Cambridge UK 2004), p. 143
“Carvings, and a whole miscellany of graffiti, dating from recent historic times were already well known at Stonehenge. There is hardly a sarsen which does not bear at least one inscription. The very sarsen photographed by Atkinson for a visitor that fateful afternoon (stone number 53) has one of the best known and deepest inscriptions: IOH: LVD: DEFERRE (Johannes Ludovicus (or John Lewis) de Ferre), positioned slightly above eye level…”

That recent graffito is about 350 years old while the axes and dagger are assumed to be 4500 or so years old. That is, the more ancient carvings are three times older than that “deepest” modern inscription. What we should therefore expect, given the vast difference in time of their carving is that the contrast between the erosive deterioration of each would show a very great degree of difference. They should exhibit a very distinctive amount of erosional difference if there is actually a difference of ten times in their ages. The modern graffito’s condition should show far less erosion than the ancient ones. But it does not! The difference of erosion between them clearly shows that the older ones could never be ten times more ancient. See figures below.

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55 Lancaster Brown, *Megaliths, Myths and Men…, op.cit.*, p. 98
Figure 9: Modern Graffito close up

Figure 10: Ancient Dagger and Axe close-up

Figure 11: Graffito and Dagger and Axe

←graffito

←dagger and axe
David Souden admits “The smooth surfaces of the sarsens [under the lintels] are testimony to the immense care and skill that went into dressing the stones. Despite the onslaught of time and erosion they have survived remarkably intact for more than three millennia. Some sarsens [are] decorated with incised patterns.”

However, the erosion that we observe on the modern graffiti and on the ones supposedly over three millennia old cannot be explained in terms of the established chronology. It seems inconceivable that the difference in erosion between them would allow the ancient markings to be about ten times older than that of the more recent “deepest” graffito. The erosion we see suggests about four to five times a difference in age but never ten times. William Gowland made a careful investigation into the many cup-like depressions observed on the sarsen blocks. These indentations are excellent sites for frost-action to disintegrate stone. They are in fact much deeper than the axes and dagger carvings on Stone number 53. His analysis is powerful evidence against the established chronology. According to Frank Stevens:

“…Mr. Gowland, who minutely examined the stone in 1901, is of opinion that the oval indentations…are more recent than the building of Stonehenge. Had they been contemporaneous with the erection of the Trilithons, he is convinced that the action of the water in the holes, combined with frost [action], would have caused a very much greater amount of disintegration [in these cups] than exists to-day…."

“These indentations, even if the stones are standing upright will still allow water to enter them since wind blows rain sideways. Like the cracks and indentations in cliffs, the water will seep into any tiny crevice in the indentations and frost-action will do its work, perfect, and as clean and sharp as it was when it left the hand of the craftsman about four thousand years ago.”

Since the oval indentations or cup marks are considerably deeper than the indentations of the axes and dagger at Stonehenge and could not have, according to Gowland, been “contemporaneous with the erection” of this monument, this means that the axes and dagger still visible on these stones would have completely or almost completely disintegrated and eroded away, and become unrecognizable.

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56 David Souden, *Stonehenge Revealed* (New York, 1997), p.95
58 Ibid., p. 44
According to Samuel Ferguson, examining the indentations of Stonehenge and speaking of a fallen stone lintel about “1620” AD:

“It lies on edge, presenting the flat of what was its under surface…This face has suffered comparatively little [human damage]…

“The cavities, about which I am more immediately concerned, on the…upper face, are nine feet six inches apart from inner, and eleven feet seven inches apart from outer, edge to edge. They are of irregular circular form. That towards the southern end of the impost is a dish-shaped indentation, thirteen inches in its greater by twelve inches in its lesser diameter, and 3½ inches deep…It is not impossible that the same process of erosion may have obliterated tool marks, if such existed within the cavities…”

Apparently these indentations did not exist and were eroded out of the stone since it fell about “1620” A.D. The erosion in terms of this analysis was between 3 and 4 inches in 300 years given that Stonehenge is assumed to be at least 3500 years old, it seems rather clear that incised axes and a sword would have been completely or almost completely eroded away. Robin Heath describes the level of erosion of the Sarsen Stones thus: “The relentless erosion of the gnarling and pockmarks in which some folks see faces, totems, animals and even goddesses.”

Given this level of erosion, every axe head and the one sword should have been eroded away. Yes, using scanning devices archaeologists have found a great many that could not be seen by the human eye but can still be detected. But we maintain all the ancient carvings should have been removed by erosion. Since the undersides of the lintels were protected from rain and frost, they of course, unless fallen and exposed to rain, have remained relatively smooth and ungnarled. The axes and sword would not appear as fresh as they do, compared with that geologically modern graffito! Another example of 17th century graffiti on Stonehenge is that of the great English architect Christopher Wren whose name is also carved there and also exhibits clear signs of frost-action. See Figure 12.

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60 Robin Heath, Stonehenge (NY 2001), p. 26
It is impossible to believe that it is about 350 years old while the assumed ancient axes and sword are ten times older. It defies geological science and reality. What must also be pointed out is that quartzite can have both a very hard surface in some areas and softer ones elsewhere. The softer layers naturally are the ones that are eroded most rapidly. Thus indentations that have survived 350 years like those supposedly ten times more ancient were carved into the harder layers of the surface and modern carvers would have been left with only the hardest surfaces in which to immortalize their names. This evidence of erosion via frost-action on laminated stone with interstices and covered with lichen precludes any possibility that these stones are as ancient as assumed.

The same type of evidence applies to the carvings on the portal stone in front of Newgrange. It was supposedly constructed 3100 B.C., or over 5,000 years ago. These carvings in granite, which is not as hard as the quartzite blocks at Stonehenge, are over fourteen times older than the modern graffito that we have just examined. Yet these engravings appear far too fresh to have remained in such obviously good condition for five millennia. See figure 12.
These carvings are supposedly about 500 years older than the axes and dagger depicted on Stone number 53 at Stonehenge. Again, it is impossible to believe that they could still be so fresh after 5000 years, and again this points to a far later time when they were incised. They are so fresh that in 1912 George Coffey admitted this kerbstone which has stood out in the open air subject to all the destructive elements of erosion “shows but little effect of weathering.”\footnote{George Coffey, \textit{New Grange (Brugh na Boinne) and Other Incised Tumuli in Ireland} (Dublin Ireland 1912), p. 17} Even earlier, William Copeland Borlase also admitted that at Newgrange “the spiral carvings [that] have been cut…and considering the exposed positions of the external slabs [to the elements] they show little effect of weathering.”\footnote{William Copeland Borlase \textit{The Dolmens of Ireland}, vol. III (London 1897), p. 357} Why, after being exposed to frost-action and whatever other forms of erosion affect such rocks, should they exhibit “little effect of erosion”? Since these engravings are supposedly more ancient that those at Stonehenge, they, too, should have been completely or almost completely eroded away.

We have found thus far in our research nothing by the authorities to explain this. Frost-action erosion is a real geological phenomenon and does operate on such stones; but in order to maintain the established chronology the archaeologists have to either ignore this evidence or suggest that these stones were so hard that erosion hardly affected them, in spite of that fact that on one of the very hardest of
stones, number 53 at Stonehenge, made of quartzite, which is about as hard as steel, a graffito carved in the 1600s shows indisputable evidence of severe erosion.

For those who may not agree and dispute the erosion evidence we offer the following by James Fergusson:

“[Megalithic structures] that give any sure indication which enables us to connect them to our own and with one another, are few and far between.

“Among the few that give any sure indications of their age one of the most interesting is Herrestrup in Zeeland, which has recently been disinterred from the tumulus [earthen mound] that once covered it. On it are engraved some half-dozen representations of ships, such as the Vikings were in the habit of drawing and which are [still] found in great quantities on the west coast of Gothenburg. According to the best authorities these range from 500 to 900 and some perhaps may be more modern. Those carved on the [buried] dolmen do not appear to be either among the most ancient or the most modern and if we fix on the eighth century as their date, we shall not be very far wrong. That they are coeval with the monument seems perfectly certain. We cannot fancy any Viking engraving these on a deserted dolmen and then covering it up…Had it never been covered up, any hypothesis might be proposed, but the mound [over it] settles that point…”

This carving of Viking ships on a buried stone dolmen is the coup de grâce for those who suggest these monuments are pre-Roman. We have been unable to find an explanation for this overwhelming contradiction to the established chronology. In fact, one might even conclude that the archaeologists have buried this evidence in order to escape its obvious chronological meaning. This evidence thus corroborates our erosion evidence no matter how myopic those who oppose our thesis are. While this evidence does not date these monuments precisely it indicates they were erected and incised much closer to the present than the specialists have claimed.

ENTASIS AND CHRONOLOGY

The final issue with regard to the Sarsen Stones that relates to chronology is that of the architectural nature of the very stones themselves. Elements of architectural design do not arise from nothing out of nowhere. The people of Rome constructed their great temples and state edifices copying the beautiful designs they inherited from the Greeks. The same applies to Stonehenge. Few people realize that the sarsen

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63 Fergusson, Rude Stone Monuments..., op. cit., pp. 303-304
uprights and the top lintels that connect them to one another are designed along lines laid down by the Greeks and Romans some 2000 years later. The form of this architectural structure is known as “entasis.” Castleden describes it for us:

“The entasis in the sarsen uprights has often been mentioned as an illustration of architectural refinement. This slightly convex swelling [gradually outward from the base] of the sides of each stone increases the effect of bulk, solidity and permanence. It became a regular feature of the much later classical [Greek and Roman] period, when every column was entastic. The stone avenues [attached to the megalithic stone circles] may be seen as rather informal forerunners of the colonnade, an idea that was fully developed later by the Romans [at] Palmyra.”

In this respect Chippindale reports:

“All the same, the archaeologists still faced a number of long-standing problems, chief of which was the exact shaping of the Stonehenge lintels and the seeming use of entasis, the adjustment of their elevation to offset the distorting effect of perspective, a refinement not otherwise known in Europe before Classical Greek architecture. If this was a ‘trick learnt at second hand from those who had some knowledge of the temple masonry of Greece and Rome’, then Stonehenge had to be [built] very late indeed, certainly of the Iron Age, probably of the last century BC. That date [or a date of post-Roman times] scarcely fit the Neolithic and Bronze Age affinities of the finds from Stonehenge or the [radiocarbon, pottery sequence, etc.] dates of other henges. Conversely, if Stonehenge was of the Bronze Age [ca. 2300 B.C.], was all that skill in its architecture really acquired inside Britain? And if it was so early, why were [radiocarbon dates of] the sets of Y and Z holes dug in the Iron Age [ca. 700 B.C.] the best part of a thousand years later, and why was the site so scattered with Romano-British pottery?

“Either way, [the chronology] did not make easy sense, though a tortuous amalgam of dates could be managed…Assuming you could bring the Druids [of Roman times or earlier] back into Stonehenge, it could be argued [but not proved] that a priest, a few decades before the Roman conquest, had taken over an existing sanctified circle with its patriotic attachment and tried to build it as ‘a grand temple of those refugees from Gaul who had seen [such structures] erected by the Greeks and Romans’ [and copied entasis]. This was possible but scarcely plausible."

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64 Castleden, *The Stonehenge People, op.cit.*, p. 244
65 Chippindale, *Stonehenge Complete, op.cit.*, pp 198-199
Chippindale, honestly, is unable to connect the various elements of Stonehenge, namely entasis dated to Greek and Roman times, the radiocarbon dates of the Y and Z holes dated to the Iron Age ca. 600 B.C., and the Romano-British pottery dated from about 100 to 410 AD. But if Stonehenge is a post-Roman structure, its builders would well have known of entasis, its Y and Z holes would date to an Iron Age, Bronze Age, Stone Age that encompassed the post-Roman British world. Romano-British pottery was the last element of that pottery that survived into the early post-Roman age and was, as we will see, highly prized by the post-Roman Saxons. While faced with chronological problems which he admits exhibit “a tortuous amalgam of...dates” that “could be managed,” the only solution Chippindale could concoct as “possible” was “scarcely plausible.”

This problem of the advanced design of Stonehenge reverberates through the literature: Nancy K. Sandars states: “Far more difficult to account for is the...entasis [of Stonehenge].”66 Gerald Hawkins called entasis there something that is “very advanced.”67 The point that very few researchers admit is that this architectural feature of the Greeks was so difficult to discern that it wasn’t known of or understood by western man until A.D. 1810. William Henry Goodyear writes:

“even the entasis of the columns had passed unnoticed in the Greek examples until 1810, and was then observed for the first [time] by Cockerell. Penrose is the authority for this astonishing fact, which is the more remarkable because the Roman entasis, which was derived from the Greeks, had been copied by the Renaissance architects and their successors down to our own time and since the fifteenth century [A.D.].”68

That is, only in the Renaissance, about A.D. 1300-1400, did architects in Europe understand and erect columns with Roman entasis, while of the Roman and Greek columns this phenomenon was not recognized until 1810. Yet we are asked to believe Stone/Bronze Age people about 1000 or more years earlier somehow, out of nothing, decided to erect the uprights at Stonehenge using an extraordinary architectural feature that is barely discernible to the untrained eye. However, if, as we maintain, these Stonehenge masons lived in Anglo-Saxon/post-Roman times in Europe before they invaded Britain after the Roman exodus, they surely would have had among them stonemasons, or slaves, who under Roman rule learned this

66 Sandars, op.cit., p. 291
67 Hawkins, Stonehenge Decoded (NY 1988), p. 54
technical form before coming to Wessex in the 5th century A.D. Rather than carving circular columns, which is extremely difficult into such intractable stone, they erected simpler rectangular columns with entasis.

The severe difficulty of trying to explain this entasis as an anachronistic design feature at Stonehenge drove Richard Atkinson to claim it was “not impossible.” However, in Chippindale’s words, “This was possible but scarcely plausible.” Consider how we would be treated by the authorities regarding the age or chronology of Stonehenge and the megalithic world were we to argue for a concept that almost defies probability by saying what we present for their consideration is “not impossible.” We would be excoriated. Yet archaeologists of Stonehenge, confounded by this anachronism, must maintain that their chronology is somehow correct in spite of entasis at Stonehenge being “not impossible.” Those who propose such a conclusion have no ability to calculate such a probability. They require entasis to be prehistoric, ergo it is!

There was also found in France another architectural feature that exhibits Greco-Roman design, namely that of a pillar set on a base block and a capital block atop it.

“In France there is a dolmen near Confolens in Poitou consisting of a rude stone block 15 feet long, 12 feet broad, and 2 feet 6 inches thick, supported on four slender columns of the architecture of the twelfth century. This [megalithic structure] appeared so anomalous that M. Rochebrune in order to get over the difficulty made a suggestion which Sir John Lubbock endorses to the effect that these supports [pillar bases and capitals] were probably carved at a period long subsequent to the erection of the monument.’ If this were so, it would not alter the argument to be derived from it…as is known that each of the [four] pillars consists of three separate pieces of stone—a base, a shaft, and a capital.”

How does one know this explanation is true? The reason is because the archaeologists wanting proof say it is, and presto, it becomes historical fact.

Let us briefly explain this in terms of what an “anachronism” is chronologically. Anachronisms are items of material culture that appear out of their correct time slot—ana-chron-ism meaning not in the correct time. In order to have a sound chronology, the objects and phenomena related to a culture should be

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69 Atkinson, *Stonehenge, op. cit.*, p. 37
70 *The Quarterly Review* vol. 129 (1870), p. 234
found in their proper contexts of time and place. If one were, for example, to find a sherd of clearly recognizable Roman pottery directly beneath the base of a large megalithic stone, that would be a clear-cut anomaly to the established chronology because that Roman pottery sherd could never have preceded the erection of that stone. When a chronology is unsound, anachronisms abound and the chronology cannot properly explain them in a clear, coherent, logical way. A more valid chronology has none, or at least very few, such anachronisms. Therefore, to resolve this entasis anachronism the archaeologists are forced to reason in a circle. Since entasis on the Sarsen Stones is taken to have been shaped over a thousand years before the Greeks constructed their columns with the base, shaft and capital, it had to be invented by the megalithic builders. That is, they knew how to carve pillars with entasis because they knew how to carve pillars with entasis. Q.E.D., a Latin term for: what was to be demonstrated. In this case we suggest we are dealing with a Quixotic Erroneous Deduction. A deduction is based on the premises one adopts beforehand and the only reason the proponents of the established chronology say that entasis was discovered about a millennium before the Greeks did so is their premise—the established chronology.

The pronounced value of a sound chronology is that it organizes human development on several levels into an orderly, coherent, logical, scientific, technological whole. Briefly, our chronology explains why the Romans who dominated Britain and Brittany for centuries never breathed a word about these megaliths: because these megaliths were not there at that time and people did not write historical comments or accounts of things that they did not see because they did not exist then. Our chronology explains why neither the Romans nor the Celtic Bretons failed to carve graffiti into these megaliths because these megaliths were not there at that time, and people cannot carve graffiti on monuments that don’t exist. Our chronology explains why only after the Romans left Britain the Scandinavians carved graffiti on the tumulus at Maes Howe and why graffiti begin to be written on such monuments in the medieval period, because forest clearance had exposed these and people who were disposed to write graffiti on them could do so because these monuments were visible. On the scientific level, our chronology explains why the difference of frost-action erosion failed to practically erode away all the prehistoric axes, daggers, and designs on megaliths out in the open as compared to the graffito on Sarsen Stone number 53, because these assumed prehistoric engravings were carved into this and other such stones only about 1500 years ago or somewhat later, and not 3700 or 5000 years ago, and thus did not have time to be erased by frost-action. Technologically our chronology explains why the Stonehenge masons built lintels with Greek architectural techniques because they
had known and experienced the long architectural tradition of this refined technique of the Greeks and especially the Romans, and they did not invent this out of nothing.

Again, a sound chronology explains and allows human/cultural development to evolve in a coherent, logical, scientific, and technological way as opposed to an incoherent, illogical, non-scientific and non-technological one which, as Chippindale, above, showed was confusing and “scarcely plausible.” This is what we have found with the established chronology and will encounter again and again in the following discussion. This we have delineated thus far and will show that there are many other developments of the Megalithic Age that have evolved in the same orderly manner, just as we did in volumes I, II, and III of Pillars of the Past for the ancient Near East. If our chronology is valid, it will continue to show orderly, logical, scientific, and technological continuity from Greek and Roman times into the Anglo-Saxon epoch and even into the modern era. Each human development will show this evolution repeatedly but fail to do so with the established chronology.

One final point before moving on has to do with at least some mention of stones by the churchmen of the post-Roman world in Europe. Surely, it can be argued that if these stones existed and pagan people worshipped them or at them, then the churchmen would have known of this and mentioned it somewhere. While we maintain that the growth of forests did make it extraordinarily difficult to see them, the rites carried out at them would have come to the attention of certain prelates and this would have been an affront to the church which would have sought to discourage or halt altogether these pagan practices. There is, nevertheless, direct evidence that in post-Roman times the pagan peoples of Europe did worship stones or worshipped their gods at stones. To the churchmen this distinction would have made no difference, and they sought to halt this veneration.

But if, as is known, these stones were abandoned and lost to memory about 1200-1500 B.C., why would the pagan Medieval people congregate at these megalithic sites thousands of years later, if they had no inkling of why these stones were erected? And why did they do this only in the post-Roman era and not in Roman and pre-Roman times? If the pagans of Europe were going to worship megalithic stones or at them, surely they would have been doing that in pre-Roman and Roman times as well. Yet as we well know, the Romans make no mention of them or of people worshipping at them. The obvious reason is that these stones did not exist in the Roman era for people to worship or at which to carry out their
pagan rites. This, as we see it, is yet another contradiction to the established chronology and gives strong support to our short chronology. Here Fergusson makes the point that the post-Roman churchmen knew of such stones and attempted to suppress the pagan worship of them or at them:

“Although the classical authorities are silent regarding the rude stone monuments, and contemporary records help us very little in trying to understand the form of the temples in which our forefathers worshipped, till they were converted to Christianity, still the Decrees of Councils [written in the post-Roman era] render it quite certain that Rude Stone Monuments were objects of veneration—certainly in France, and, by implication, in England—down to the times of Charlemagne and Alfred [the Great], at least.

“One often-quoted decree of a council, held at Nantes, exhorts “Bishops and their servants to dig up, and remove, and hide in places where they cannot be found, those stones which in remote and woody places [because they were hidden by forest growth] are still worshipped, and where vows are still made. Unfortunately the date of this Council is not certain; but Richard places it in 658 [A.D.], which is probably at least nearly correct.

“This, however, is of comparatively little consequence, as in 452 a Council at Arles decreed that ‘if, in any diocese, any infidel either lighted torches or worshipped Trees, Fountains, or Stones, or neglected to destroy them, he should be found guilty of sacrilege;’ and about a century later (567), a Council at Tours exhorts the clergy to excommunicate those who, at certain Stones or Trees or Fountains, perpetrate things contrary to the ordinances of the Church.

“Still another century further on (681), a Council held at Toledo admonishes those who worship Idols or venerate Stones, those who light torches or worship Fountains or Trees...Another council held in the same city, in the year 692, enumerates almost in the same words the various heresies which were condemned by the preceding Council. A Council at Rouen about the same time denounces all who offer vows to Trees or Fountains or Stones as they would at altars...

“Lastly, a decree of Charlemagne, dated Aix-La-Chapelle in 789, utterly condemns and execrates before God Trees, Stones, and Fountains, which foolish people worship.

“Even as late as in the time of Canute the Great, there is a statute forbidding the barbarous adoration of the Sun and Moon, Fire, Fountains, Stones and all kinds of Trees and Wood.
“The above [citations] which are taken from Keysler are not all he quotes, nor certainly all that could be added, if it were worth while, from other sources; but they are sufficient to show that from Toledo to Aix-La-Chapelle—and from the departure of the Romans till the tenth, or probably the eleventh century—the Christian priesthood waged a continuous but apparently ineffectual warfare against the worship of Stones, Trees and Fountains [fires with torches and the Sun and the Moon]…The testimony of these edicts is, therefore, not quite so distinct as we might wish…But what it does seem to prove is that down to the 11th century the Christian Priesthood waged a continuous warfare against the veneration of some class of Rude Stone Monuments, to which the pagan population clung with remarkable tenacity…This is, at all events, infinitely more clear and positive [evidence] than anything that has been brought forward in favour of their pre-historic antiquity. If, like the other branches of the written argument, this is not sufficient to prove, by itself, that the monuments were generally or even frequently erected after the Christian era, it certainly entitles that assertion to a fair locus standi in the argument…”\(^{71}\)

In this respect Burl concurs with Fergusson, saying “Classical writers such as Pliny and Strabo were ignorant of the [stone] rows and even in early Christian times the pagan standing stones were only mentioned as places at which worship was forbidden.”\(^{72}\)

In conjunction with all the other evidence given above it is clear that the pagan peoples of Europe worshipped “stones” that churchmen wished to have destroyed. Furthermore, the pagans, we are told, not only venerated trees and fountains, but most significantly the Sun and Moon, which suggests solar and lunar connections of these celestial bodies with these stones. This, we suggest, was related to the solstices and to the solar-lunar calendar to be addressed below.

Thus it is evident that some churchmen in post-Roman times were aware that stones were worshipped and surely, what other stones than megaliths were they referring to, hidden in remote forest places that followed the fall of Rome?

Related to these prohibitions, in terms of astronomy, would have been the Church’s condemnation of solar and lunar rites which the post-Roman people celebrated. Based on all the above one would expect to find written evidence by churchmen of this unacceptable practice of calendrical celebrations or worship of the Sun and Moon. According to McCluskey:

\(^{71}\) Fergusson, *Rude Stone Monuments…*, op. cit., pp. 23-26
\(^{72}\) Burl, *From Carnac to Callanish*, op. cit., p. 10
“In its expansion Christianity encountered pagan practices...of the Germanic and Celtic peoples...The conversion of the countryside only began in earnest in the seventh and eighth centuries; it was not until the thirteenth century that we can say ‘time, space, and ritual observances came to be defined and grasped essentially in terms of the Christian liturgical year [and not the Celtic year].

“Bishops Caesarius of Arles (470-542), Martin of Braga (ca. 515-586), Eligius of Noyon (ca. 588-659), and Pirmin of Reichenau (d. 753) formally warned their flocks against these deeply rooted pagan traditions. They all describe the kind of cult practices that archaeological investigations have associated with the Germanic and Celtic populations of Europe....The mention of sacred stones suggests the kind of rituals associated with lunar and solar observations...

“Eligius of Noyon rebuked those who attend to the Moon in the morning before beginning the day’s work. This sounds like a practice of noting the Moon’s phase or position in the morning, and in it we can see reflections of that lunar calendar keeping common to many folk societies, that is, of the folk traditions...Martin of Braga included in his collection of canons a condemnation of observing the course of the Moon or stars according to the traditions of the pagans to determine the time to build a house or plant trees [etc.]...

“Since the Sun and Moon are not lords, then the practice embodied in the calendar...of regulating activities by the dominion of the planets over the days of the week must also be rejected....

“Eligius’s most revealing condemnation is of pagan solstitial solemnities with singing and dancing on St. John’s day.”

This ties in directly with the orientation of Stonehenge and those others with avenues aligned to the solstices, just as has been found at Stonehenge, which we will return to below.

With respect to fountains, we claim that these were special watery places such as pools, lakes, and sources of rivers. We further will show that at these places, called fountains, votive offering were made. For example, Martyn Barber writes: “At Flag Fen....large quantities of bronze along with other metals and materials

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were deliberately cast or dropped into waters, a votive deposition in such places as well as facilitating broader acceptance of ritual depositions more generally."

In this regard James Dyer states:

“…many items and metal weapons have been recovered from rivers like the Trent and the Thames…Most of the tools were ostentatious...[I]t is logical to see them as votive offerings deposited in selected sacred places, in particular the sources of rivers, bogs and pools [which are good places to call a fountain]. There was something magical about the way that water burst out of the earth [like a fountain], particularly in the case of streams such as winterbournes, where the flow is seasonal and can be predicted…The water gods needed to be propitiated and this could best be done with costly military weapons… It is even possible that human sacrifices were offered to the gods, since human bones and skulls are sometimes found with the weaponry as at Clifton in the River Trent…but this practice is better demonstrated with more assurance in later Celtic Britain.”

C.N. Ó Dochartaigh further reports:

“It is easy to accept that the presence of underground water, and its emergence through wells or springs, represented a boon from the otherworld, and there is clear evidence for a Celtic interpretation of rivers as representatives of this chthonic world...The spring sources of rivers were especially revered, and at the spring site of Sequana [the River Seine] in Burgundy many votive deposits have been found, including bronze and silver models of parts of human bodies deposited as requests for healing. Even without a direct connection with a river, wells and springs have been recognized for centuries as objects of veneration within Celtic countries (or in their replacement cultures), particularly for their reputed healing properties, as indicated by the nineteenth century example of Lourdes and other such religiously over-written sites...

“Above ground the lake (loch, linn, llyn) acts as a natural reservoir, both physical and cultural, for its surrounding population, and here also we find evidence for the ritual use of lake sites across the Celtic world. In Continental and Insular Europe they were considered as important repositories for votive offerings, including human sacrifices. In the main such deposits consist of swords and other weapons, with their blades usually bent almost in two before deposition, to demonstrate that they were no longer functional in this world and that they took on the rôle of an offering to the otherworld. Lake Neuchâtel in Switzerland is an

74 Barber, op.cit., pp. 68-69
75 Dyer, op.cit., p. 112*
excellent type site for this practice, as projecting into this lake...a wooden platform was erected to act as a site of votive deposition. Several hundred brooches, spears, shields and swords have been recovered from its vicinity on the lake bottom.”

This process of turning to healing springs not only existed in the post-Roman epoch but indeed into the modern age. As a boy, this author visited Hotsprings, Arkansas, hot springs to which sick people still went to relieve their pains. President Franklin Delano Roosevelt traveled to Warm Springs, Georgia to ease the infantile paralysis from which he suffered.

What we have gleaned thus far is that the early medieval church had inveighed against pagan worship of stones. What stones were worshipped we are not told, but megalithic monuments standing in the depth of the medieval forests surely makes eminently good sense for them as objects of worship. The church further denounced the pagan worship of the Sun and Moon, and Eligius condemned pagan solstitial solemnities with singing and dancing. Again this makes eminently good sense since many stone rings and especially Stonehenge’s avenue are aligned to the summer solstice. The churchmen denounced the building of fires and torches as pagan celebrations. They inveighed against pagan worship of fountains which clearly were being given votive offerings in post-Roman times because the items found there are of iron, precious metal and weapons not available in the Neolithic or Bronze Ages in this part of Europe.

The Christian Church of the medieval era condemned several practices, especially the worship of stones that clearly connect the medieval period to that of the megalith builders. All this evidence correlates and corroborates with the evidence that preceded our discussion. Again, the short chronology allows human development to evolve in a logical, scientific, technological, and historical way as opposed to that of the established chronology.

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CHAPTER 7

METROLOGY AND CHRONOLOGY

Metrology is the study of measurements. Various cultures in the past had different standard measures that we know of from their writings and that can be seen were employed when they built monuments. Today Europe and most of the world use the metric system developed by the Napoleonic regime in France, while Britain and the United States still work with the English system of inch, foot, yard and rod. In this respect we have a technological/metrological phenomenon with which to test the chronology of the Megalithic Age. Archaeologists associated with the established chronology have dated this age to prehistoric time and thus any metrological unit or units found at Stonehenge or Avebury would be totally unrelated to the medieval world. We maintain that these monuments were built in post-Roman/Saxon times and therefore expect to find that Saxon units of measurement or other units of these ancient civilizations would related or connected with those used to construct these megaliths.

Since our thesis as presented in volumes I, II and III of Pillars of the Past claims history is much shorter and that the Indo-Europeans came to inhabit western Europe after 1500 B.C., it follows that in some instances they brought with them units of measure that are found in various settings in the ancient world and that these are related to the units of measure used in the post-Roman/Saxon Megalithic Age. Thus, we have a technology by which to test whether the short chronology we espouse or that of the establishment is correct. If it can be shown that the unit of measurement at Stonehenge and Avebury is a Saxon unit or that units found at other megalithic sites are closely related to those of the ancient cultures of the Near East or Indo-European peoples, then this correlates with and corroborates the evidence we presented in the previous chapter. In this case, however, our metrological technological proof is rather chronologically specific. If Saxon measurements were employed at Stonehenge and Avebury that would undeniably place the Megalithic Age in the post-Roman era. Q.E.D.

This approach was known from the time of Stukeley and others who attempted to correlate the units of measurement found between megalithic stones with some
previously known culture. It was known that having found that unit one could then identify it with a particular culture and one could not only discover the people who had erected these monuments but set them in a secure time frame. The logic was impeccable then and is still so today. Chippindale cites Stukeley on this matter:

“One was to measure Stonehenge accurately, for whoever [Stukeley said] makes any eminent building, most certainly forms it upon a common measure in use among the people of that place [and time]. Therefore, if the proportions of Stonehenge fall into fractions and uncouth numbers [rather than whole numbers, i.e., integers], when measured by the English, French, Roman or Grecian foot, we may assuredly conclude the architects were neither English, French, Roman or Greek. A Roman Stonehenge would measure in [whole or half numbers of] Roman feet…with every dimension convenient ‘in round and full number not a trifling fraction’.”

If for example, the unit of measure was in English feet, one would expect to find dimensions that were, say, three, ten or five feet or three and a half, ten and a half, or five and a half, but not three and 5/8ths, ten and 1/3rd, or five and 3/16ths feet. However, because the earlier and even modern investigators were imbued with the established chronology, seeking post-Roman or other historical units of measure were not considered. Stukeley, who had a strong religious disposition determined that the unit used at Stonehenge was the ancient Egyptian and Hebrew cubit, stating erroneously: “Father Brothais in his Observation on Upper Egypt found a door case made of one stone 26 ½ feet (8.1 m) long in height. this is 15 cubits. Dr. Huntington…says the Sphynx standing by the northern pyramids is 110 feet (33.5 m) in circuit, i.e. 60 cubits the very [same] measure Webb gives to the diameter of Stonehenge. The Hebrew & Egyptian cubit was divided into 6 palms [each of 3.6 inches] so was ours here.”

Burl summarizes the early views of the metrology employed by investigators:

“The idea that circle-builders did have a yardstick was first mooted by William Stukeley in 1723 or 1724 when he deduced that the dimensions of Stonehenge were based on multiples of a Druid’s cubit of 20.8 inches (53 cm) ‘most probably deriv’d from Noah and Adam.’ Seventy years earlier Inigo Jones thought that the ring was Roman…Preoccupied with his classical erudition Jones overlooked the ambiguity in that his architects had used not the Roman pes (foot) of 11.664 inches (29.6 cm) but Imperial Feet in their design. He supposed that Stonehenge was 110 English feet (33.5 m) across. But by an unfelicitous [sic] coincidence the

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1 Chippindale, *Stonehenge Complete, op.cit.*, pp. 74-75
2 William Stukeley, in Burl and Mortimer, *op.cit.*, p. 34
sarsen [stone] ring’s true diameter of 29.6 m was exactly 100 Roman pedes, something that Flinders Petrie was to realise three hundred years later. Neither Stukeley nor Jones was right and Petrie’s calculation was a chronological irrelevance [because Stonehenge could not have been built in Roman times].

“Since Stukeley further erroneous units have been computed. The ruined foundations of a round house near Hestingsgot in south Shetland [Island, Scotland] were supposed to contain evidence of a ‘Hestingsgarth Foot’ of 12.96 inches (32.9 cm), a measure also claimed for the much-abused Stonehenge as well as Avebury, Stanton Drew and others. Ludovic Mann speculated that Bronze Age toolmakers worked in an Alpha Foot of 14.85 ins and a Beta [Foot] of 13.78 ins (37.7, 33.7 cm)….

“Measuring-sticks undoubtedly did exist in prehistoric Europe. One of hazel [wood] was found in the Danish Bronze Age tumulus of Borum Eshoj…It measured 79 cm and had three notches cut at 15 cm intervals. A second, of oak, was recovered from Borre Fen, 1.35 m long, divided into eight equal parts of 16.9 cm. Of Borum Eshoj it was written, ‘There can be little doubt that it was a Bronze Age measuring rod marked out in feet…”

“Hicks has plausibly suggested that it was flexible body-yards, the distance from nose to tip of an outstretched arm, about 1 m, that prehistoric people employed, each community having their own subtly individual version of this. ‘Very similar sets of body-based measurements have been found…to be used not only in Europe…but also throughout much of the world…This suggests…their very great antiquity…”

A further important point that we will explore below is that many of these supposedly prehistoric units of measure have in various ways and places survived into modern times, which we suggest shows that there was a direct heritage and connection of these measurements, many of which date from post-Roman times and have not changed. Burl adds:

“Ratios of the human body, famously demonstrated in Leonardo da Vinci’s sketch of a naked man, ‘human figure in a circle, illustrating proportions’, arms aloft, legs apart, contained in a perfect circle, show what a range of lengths were available from a source that did not differ greatly between people of average dimensions.

“Such measurements are more relevant to proportions of megalithic monuments than the artificiality of the metric system…Although illegal in France since 1837 many country people there still think in pre-Napoleonic measures,

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3 Burl, *The Stone Circles of Britain, Ireland and Brittany*, op. cit., pp. 50-51
three pied to the metre, le pouce for the inch or thumb, eggs sold by le [sic] douzaine, and meat by le [sic] livre. Some farmers calculate the area of fields not by the hectare [but] by la perche, the old perch, pole or rod.\(^4\)

Alexander Thom believed he had discovered a megalithic unit of measure which appeared to be found over a vast region of northern Europe and made the deduction that in some way or other these supposedly prehistoric peoples had a uniform measure known as the megalithic yard (MY). His assumption indicated that these people were organized in a uniform way so that they were more like a national state rather than a heterogeneous collection of tribes. In that sense his work implied a national organization with people who had a precise unit of measure and employed it across that part of the prehistoric world. John David North was so impressed by Professor Thom’s deductions that he unabashedly claimed:

“There is little doubt that most of Professor Thom’s book will stand the test of time. In it he shows that the men responsible for the circles built nearly 4,000 years ago were capable of working within very narrow limits. Professor Thom underscores the urgency of accurate surveys. He shows that the Avebury site, for instance, was set out with an accuracy approaching one part in 1,000; and yet even scales of yards and metres on some modern plans might differ by more than one part in 100. Thanks to the accuracy with which the rings of megaliths were set out, Professor Thom was able to show that they were designed on at least six non-circular geometrical plans—as ellipses, for instance, or flattened ‘circles,’ with component circular arcs drawn from different centres.

“These centres were themselves at the vertices of right-angled triangles of at least four or five different sorts, but always having sides made up of simple multiples of a fundamental unit—what Professor Thom called the ‘megalithic yard’ (2.72 modern feet). This is not to suggest that the theorem of Pythagoras [the sum of squares of the sides of a right-angle triangle is equal to the square of the hypotenuse] was known…Here for comparison it is worth remembering the Old Babylonian ‘Pythagorean triangle’ tablet, Plimpton 322, which is of roughly comparable date, or a little older.

“Professor Thom showed how the architects of the megaliths were obsessed with lengths of integral numbers of megalithic yards, even to the point of distorting the circles—with due attention to a neat geometrical construction—to give an integral periphery [of these stone circles]. He showed conclusively that

\(^4\) Ibid., pp. 51-52
multiples of the unit (2½ and 10 yards) and submultiples (down to 1/40, in the cup and ring markings) had been used.”

North further avers “There is what seems to me conclusive evidence for the use there [at Stonehenge] of Thom’s so-called ‘megalithic yard.’” North, who gave us absolute assurances that Thom’s alignments were precise and correct and disproved Velikovsky’s thesis, has now assured us that “there is little doubt that most of Professor Thom’s book will stand the test of time” and that there “seems to me conclusive evidence…of Thom’s so-called ‘megalithic yard.’” He seems to be unaware, when he told us that the cup holes placed in the Sarsen Stones were apparently cut into them to measure 1/40 of the length of megalithic yard, that Gowland had claimed that if these cups in the stone had been there from the beginning, then “the action of water in the holes, combined with frost [action] would have caused a very much greater amount of disintegration [in these cups] than exists today.” As with his earlier claims, these were also over time picked apart by authorities and shown to be substanceless. Ralph Ellis, another of Velikovsky’s critics, describes Thom’s metrological work thus:

“Professor Alexander Thom of Oxford University…[a]fter a long and detailed investigation…stated that many of the Neolithic sites throughout the country had been designed using what became known as the Megalithic yard (my), as their units of construction; a unit measuring 0.83 m [32.64 inches]. This was, at first, hotly disputed by the establishment…. [I]t was disputed because such a theory led to some very thorny historical questions, such as: how is it that Neolithic man suddenly [from nowhere] appears to have had…nationwide organization…that maintained these [metrological] standards throughout the country; indeed across Europe?…[S]uch awkward questions have not gone away; they have been quietly shuffled under the table.”

Thom, of course, did not begin with this national organized standard of measure; it came from his findings which seemed to prove that the megalithic yard was employed everywhere in the megalithic world he explored. Nevertheless, as Ellis stated, there is a basic sociological disparity between what Thom’s evidence appears to show—a great culture spanning England, Ireland, Scotland, and Brittany with an organized bureaucracy using one standard unit of measure for its monumental constructions, as opposed to multiple tribes using multiple measures of somewhat similar lengths. Jay Kappraff reconstructs Thom’s achievement:

5 North, Stars, Minds, and Fate…, op. cit., p.15
6 Ibid., p. 19
7 Ralph Ellis, op. cit., p. 49
“Thom’s original measurements were reported in a paper written in 1955. He subsequently re-approached the question of a standard measure with new measurements, as reported in a paper written in 1962. Douglas Heggie...has made an extensive survey of the statistical methods used to examine Thom’s measurements. Heggie states,

“Thom...presents his measurements of the diameters of stone circles in a histogram. He discovered peaks at about 22, 44, 55, and 66 feet. This observation immediately suggests that many of the diameters lie closer to multiples of some [standard] unit. Thus we are led to frame a quantum hypothesis [based on finding a common standard unit that fit all these diameters] that the diameters were intended to be multiples of 11 feet. Actually Thom settled on [half of 11 feet or] a unit of 5½ feet (actually 5.435 feet), and since it is likely that it is the radius of a circle [rather than the diameter] which would be measured out [to form the circles], Thom thought that a unit of about 2.72 feet [32.64 inches] was in use, and this was subsequently called the megalithic yard.’

“Thom also measured the perimeters of the stone circles and found that units of about 2½ megalithic yards (2.072 meters) were most prevalent. He called this unit the megalithic rod.”

The problem lay in how to know with more certainty, namely statistically, that the numbers Thom used were artifacts of the circles he chose or were so statistically accurate that the probability of finding so many circles with these measurements was quite small, and thus statistically proved Thom’s case. In this regard Kappraff shows:

“The object of statistical studies of Thom’s work has been to test the significance of the quantum hypothesis. If the hypothesis was correct, one would expect that the measurements of diameters would exhibit only small deviations from the nearest multiple of 5.435 feet [the megalithic rod or 2½ megalithic yards].

“The problem of testing the null hypothesis was undertaken by several statisticians, most notably S.R. Broadbent and D.G. Kendall. Details of these studies are described in Megalithic Science...Although Thom derived his measure for the megalithic yard from his earlier data, he turned again to the megalithic yard in his later paper, and he reported measurements of many new sites [to see if they too employed the same unit as the first group]. If the old unit of 5.435 feet is

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applied to the new data,…a statistical test devised by Broadbent was most applicable. Heggie states:

“‘Omitting sites already discussed in 1955, or those with diameters noted as being particularly uncertain, we obtain a probability level [of matches between the different sets of circle measurements] far below 0.1 %. This is a highly significant result, for it implies that such good agreement with Thom’s unit would occur only once in many thousands of samples of random data.’”

It clearly seemed, that is, statistically seemed that Thom had very little probability support for his megalithic yard. Nevertheless Kappraff also presents his caveat:

“Heggie feels that as striking as these results are, there are several reasons why they may not be as decisive as they seem. One of these is the suspicion that the choice of [circle] geometry [of diameters and circumferences] open to Thom allowed the operation of a quite unintentional bias in favor of the megalithic yard. One way around this bias would be to analyze the results of other workers who have measured the megalithic sites but did not use Thom’s special geometries. Unfortunately, there is sparse data of this kind yielding inconclusive results and most of the support for Thom’s theory comes from his own measured diameters. According to Heggie, ‘This situation is unlikely to change until other investigators summon the energy to survey comparable numbers of sites with comparable care.’”

Many individuals such as John David North thought that the statistical case for Thom’s metrology was almost certain. Christopher Knight and Robert Lomas, among others, have come to that same conclusion:

“Thom dubbed this universal unit…the ‘Megalithic Yard.’ This apparently crazy claim was one of the principal reasons why his work was initially ignored by archaeologists, who thought it ridiculous to believe that these ancient people could have had anything as sophisticated as an international standard of measurement.

“Thanks to an objective study by statisticians, the ‘megalithic yard’ has now become fully accepted as a system of measurement that was in use over, what appears to be an impossibly large geographical area, at a time when people had been previously considered to be little better than cavemen.

“The megalithic measure had been employed in the building of structures in Scotland, Wales, England and Brittany; an area of some 150,000 square miles!

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9 Ibid., pp. 238-239
10 Ibid., p. 239
Thom felt that they [the measurements] were far too consistent to have been delineated by the use of measuring rods, created, copied and distributed across this huge area... He concluded that there must be some long-forgotten physical reality behind its creation—but neither he nor anyone else has been able to explain how it was derived.

“When reviewing Thom’s work, Colin Renfrew observed that these megalithic stone circles and other arrangements of stone are laid out in exact halves or full integer numbers of a unit of length Thom called the megalithic yard [which] could also have been a double unit which he adopted because it [32.64 inches] was conveniently close to the modern yard [of 36 inches].”

To arrive at his conclusion Thom measured a large number of circles of various shapes: egg-shaped, oval-shaped, flattened, ellipse-shaped, compound and nearly perfect circles, and wondered what they all had in common that would allow them to exhibit measures of whole and half units of his megalithic yard. Colin Wilson explains:

“Moreover most of these stone circles were not [true] circles: some were shaped like eggs, some like letter Ds. Yet the geometry—as Thom discovered through years of study and calculation—was always precise [in terms of the megalithic yard measure applied to its diameter and circumference]. How did they do it? Thom finally worked out that the ‘circles’ were built around ‘Pythagorean triangles’…”

“And why did they want these circles? That was more difficult to answer.”

Thom himself explains:

“We do not know the extent of Megalithic man’s knowledge of geometry and astronomy. Perhaps we never shall. He was a competent engineer. Witness how he could set out large projects to an accuracy approaching 1 in 1000, and how he could transport and erect blocks of stone weighing up to 50 tons. He had an extensive knowledge of practical geometry, and used the 3, 4, 5 right-angle triangle extensively. He also knew the 5, 12, 13 right-angle triangle, the 8, 15, 17 and the 12, 35, 37. There is a suspicion that he also knew the 9, 40, 41 [right-angle triangle]. He had in addition discovered many other triangles with integral sides that satisfied very closely the Pythagorean relation. These triangles were used in a peculiar geometry, in which he constructed rings, set out in stone, of various shapes: circular, egg-shaped, elliptical, etc. These constructions were made according to two rules: all linear dimensions had

11 Knight and Lomas, *Uriel’s Machine, op.cit.*, pp. 204-205
12 Wilson, *op.cit.*, p. 212
to be integral multiples of the unit, and the perimeters had to be multiples of $2\frac{1}{2}$ units. The unit was the Megalithic yard (MY) of length $2.720 \pm 0.003$ feet.”

According to Thom these non-literate people set up circles with such different shapes that not only had diameters and perimeters that showed the megalithic yard was used. They did so by setting out Pythagorean triangles that would be of the same unit that came out to be Pythagorean triplets, not just the well-known 3, 4, 5 triplet, but the 5, 12, 13, the 12, 35, 37, the 20, 21, 29 and the 8, 15, 17. Repeatedly proponents of the theories regarding the astronomical and metrological accomplishments of these megalithic builders suggest they were immensely advanced, in fact geniuses—Einsteins and Newtons. Knight and Lomas describe them thus:

“Alexander Thom…after examining some 600 sites and conducting a highly detailed survey of half of them,…produced a conclusion that upset the archaeological applecart. In his own words, he made surprising claims for the genius of these previously unsung prehistoric architects:

“A statistical analysis of the sites shows that they were so carefully erected that we can from them deduce

“(1) the inclination of the ecliptic
“(2) the inclination of the lunar orbit
“(3) the mean amplitude of the lunar perturbation and
“(4) the mean lunar parallax
“with an accuracy better than one arc minute….

“He proved that the more important rings of megaliths were designed using geometric rules” and “an accurate calendar. It has been shown [Thom 1967] that Megalithic man made use of a calendar linked to the Sun. In some Mediterranean countries the calendar was based on the heliacal rising of certain bright stars. Such a calendar does not have anything like the accuracy of Megalithic man’s calendar, which could not get out of step with the Sun by more than a day, if as much.”

Atkinson, understanding Thom’s far reaching implications regarding megalithic astronomy, claimed:

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14 Alexander Thom, *Megalithic Sites in Britain* (Oxford UK 1972), p. 27
15 Knight and Lomas, *op.cit.*, p. 203-204
16 Thom, *Megalithic Lunar Observatories, op.cit.* p. 11
“If we accept the evidence here presented by Professor Thom (in such detail that the reader can check all stages of the argument), and if we concur in even a part of his conclusions (which are drawn with the most scrupulous regard for the legitimate limits of inference), we must alter radically our current view of the intellectual caliber of man in Britain in the late third and second millennium B.C. Indeed, we must consider the revision of a whole chapter in the accepted history of science, in which primacy in the development of geometry, mensuration, observational astronomy and calendar that has been ascribed hitherto to the literate civilizations of the Ancient East.17

What we seem to have is a veritable intellectual and scientific society with great scholars employing unknown techniques that gave them knowledge that took future societies several centuries later if not millennia to achieve. It seems as if a fever swept those involved with this research to make claims that seemed to defy historical logic. It almost bordered on the suggestion that this knowledge came from the lost civilization of Atlantis. Euan MacKie, on the basis of Thom’s findings, drew the following conclusions regarding the nature of the society that gave rise to this litany of genius as described by Gordon J. Barclay:

“In 1977 MacKie, drawing on parallels with [highly literate and numerically developed] Mayan civilization, argued that the late Neolithic was a theocracy in which an elite of ‘wisemen, magicians, astronomers, poets, jurists and engineers with their families, retainers and attendants, craftsmen and technicians’ (MacKie, 1977, p. 186) lived in major ceremonial complexes and other special sites. Fed by the efforts of a peasantry living in primitive conditions, this elite undertook precise astronomical observations and set out complex ceremonial sites using advanced [Pythagorean] geometry and standard units of measurement. He rejected the possibility of a sophisticated and capable Neolithic society to communicate it from one end of Britain to the other (Thom and Thom, 1978:177). He also concluded that many non-circular stone rings were laid out using certain constructions, some of considerable complexity which involved knowledge of the techniques of Euclidean [and Pythagorean] geometry. The latter conclusion was challenged mathematically by authors such as Angell (1977), who showed that a number of different multi-parameter shapes, some less complex to construct in practice than Thom’s [Pythagorean right angle triangles] could be fitted equally well to ground plans considered by Thom. It was also challenged by archaeologists like Barnett and More (1984) who concluded that the majority of stone circles could simply have been laid out by eye to appear circular. Barnett and Herring (1986)

documented an experiment in which over 100 circles were set out by eye [not by Pythagorean triangles] by a range of individuals; it was found that these ‘circles’ could then all be described by the [same] ‘complex geometries’ of Thom even though they had not been set out using them. MacKie’s quotation (1977a: 16) from Dingle (1972) is particularly resonant: the greatest to believe that ‘everything that is mathematically true must have a physical counterpart; and not only so, but must then have the particular part that happens to accord with…that [which] the mathematician wishes to advocate.’

“It is well to remind ourselves that any shape may be described by complex geometry but that there is no evidence that the shape was laid out using it (Angell, 1977).

“The database used by Thom to deduce the existence of the M[egalithic] Y[ard] comprises the diameters of best-fit circles. Even if the data are taken at face value, the statistical evidence for a common unit of length is, at best, marginal, and even if it is accepted as existing, our knowledge of its value is only of the order of centimeters [one way or the other] far poorer than the 1 mm precision claimed by Thom (Kendall 1974, Freeman 1976; Angell 1979). The evidence is adequately explained by pacing.”

John Barnatt and Peter Herring explain their own work with the following:

“Volunteers ‘constructed’ 100 circles by eye and these were then surveyed to provide a body of experimental data for comparison [with] extant stone circles [analyzed by Thom]. Results did not support the view that geometric methods were used by Neo-BA [Neolithic and Bronze Age] builders. A further dozen circles laid out by peg and rope produced similar results. Using Thom’s shape-fitting methodologies it was possible to perceive flattened circles in the experimental results. Hence most stone circles were laid out by eye, though some were more accurately done.”

The point of this experiment is that rather than first constructing Pythagorean triangles and then circumscribing them in a flattened circle, where the vertices of the triangles touched the circumference, as Thom argued, the experimenters had the circles laid out by the volunteers by eye, yet the Pythagorean triangles inside still had vertices that touched the circumference. This was directly the opposite of

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Thom’s methodology! Above and beyond that, Angell was able to show that these same flattened stone circles could be laid out by first creating other geometric forms such as equilateral triangles where all three sides were of equal length with 60° angles between the sides that could be used for their construction. Burl lays out several different units of measure that could have been used. On page 53, he concludes: “At present it would seem that yardsticks were known with comparable but [somewhat] different lengths [to Thom’s megalithic yard] in separate communities and that elementary counting systems were in existence.” That is the unit of measure that seems to have been employed by the megalithic builders varied but was somewhat similar to the length of the megalithic yard—33.64 inches.

Furthermore there are other stone circles that cannot be made to agree with Thom’s megalithic yard unit. For example, Woodhenge with its seven concentric circles, of which Burl writes:

“The Sanctuary with its seven concentric circles offers a unique opportunity [to test] the validity of this [megalithic] ‘yardstick’ because consistency would be expected in the counting and measuring of these [seven] closely related rings. Yet although the counting base of four is manifest here from the number of parts in each ring, four is never used in the number of megalithic yards supposedly making up each diameter. Nor is each diameter an exact multiple of the Yard. Instead of the logical progression of 4 Megalithic Yards, 8, 12 and so on, one finds an unconvincing mixture of 4.4 Megalithic Yards, 5.0, 7.1, 11.4, 12.6, 17.2 and 23.8 Megalithic Yards.

“Such random multiples are incompatible with the precision claimed by the astronomers of Thom’s megalithic Britain.

“It is fair to say that important statistical tests have already shown that the Megalithic Yard, if it ever existed anywhere, had many competitors.”

On the other hand, Burl shows elsewhere that the Beaker Yard, 0.732 meters, 2.402 feet, when applied to Woodhenge’s concentric ring diameters as well as the Short Foot of 29.9 centimeters, 11½ inches, fits there much more exactly. At Newgrange, Powell concluded that a standard measure of 13.1 meters was used to construct the shape of the mound, while the plan of the passage way to the center

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20 See also Burl, *The Stone Circles of Britain, Ireland and Brittany, op.cit.*, pp. 48-53
21 Ibid.
22 Burl, *Prehistoric Avebury, op.cit.*, pp. 125-126
23 Burl, *The Stone Circles of Britain Ireland and Brittany, op.cit.*, p.52
of the tomb was and may even apply to the tomb at Knowth. These were found to apply by multiples and that these numbers “relating as they do to critical lines and widths [are] ‘all built around a rigid frame comprising two 4:5:6 triangles and one 3:5:6 triangle. These [non-Pythagorean triangles] give the monument its distinctive “heart” shape.’” The Boyne Yard, like that employed at Woodhenge, shows a counting base of 4. The numbers rise from 8 Boyne Yards to 16, 32, 48, 64, 80, and 96 [or in terms of multiples of four from 4:8:12:16:20 and 24.\(^\text{24}\) At Newgrange the Megalithic Yard failed to fit at all as either whole or even half units.\(^\text{25}\)

MacKie has argued:

“Though some of the details are open to discussion, I find the geometrical and astronomical theories quite convincing, mainly because of the way in which they are developed…Here is a vast amount of information, painstakingly collected over many years—plans of sites and carefully measured potential alignments in them. When one has enough of such information, it begins to fall into patterns of its own accord when analyzed in various ways. The patterns are real and we must accept them. If we do not like the conclusions from the patterns then we must think of better ones, but it is impossible to ignore the new data in any future assessment of Neolithic Britain.”\(^\text{26}\)

But the fact of the matter is that the patterns Thom suggested have been replaced by better ones. The essence of Thom’s work comes down to the process he used. He made the mathematics of the megalithic yard accord with his theory rather than seeing if other units of measure or other geometrical triangles and shapes could account for the evidence. And, these new units of measure and geometric figures did indeed account for the evidence better than Thom’s. For example, beyond the above better fits at Woodhenge and Newgrange, Burl cites the work of Barnatt and Moir on page 49 of *The Stone Circles of Britain, Ireland and Brittany* to show that by eye alone, these “authors decided upon the ‘hypothesis that stone circles are meant to be circular, and locally smooth to the eye.’ They concluded that their results were 780:1 times better than Thom’s.” Thus there are numerous ways these stone circles could be explained by mathematical hypotheses and one is free to choose which form of the evidence fits one’s desires, if it agrees with any particular theory which proves nothing. Johannes Kepler despised this specialized manufacturing of mathematics to make it fit one’s theory when he

\(^{24}\) *Ibid*, Table 6, p. 53

\(^{25}\) *Ibid*.

stated of a dispute: “The parties...should no longer say our mathematicians think thus, and the others so, they [the mathematicians] should rather say, we can make our mathematicians say what we please, for they are our slaves.”

Hutton sums up and reiterates the evidence against Thom’s megalithic yard and mathematics:

“By the end of the 1970s, the collusion between archaeologists, astronomers and statisticians necessary to consider Alexander Thom’s work had begun. By now he was dead and unable to answer critics, but perhaps it was just as well that he was thereby spared what ensued. John Barnatt and Gordon Mori noticed that rings which Professor Thom’s surveys had revealed to be eggs or ellipses actually looked circular to the eye. Dr. Barnatt experimented by getting students to lay out circles purely by eye and found the result was...of shapes which appeared perfectly round to the[se] builders but turned out to be eggs or ellipses when surveyed. The builders of the prehistoric rings, far from using pegs and ropes with marvelous precision, may in most cases not have used them at all, but set up rings which looked like circles, and had been happy with the result. J.P. Patrick and C.S. Wallace ran a statistical analysis of this problem and concluded that it was 780 times more likely that the rings which were not true circles had been laid out by eye than not. Meanwhile the mathematician Douglas Heggie tested Thom’s geometry and pronounced it faulty. He left the possibility that there had been such a unit as the ‘megalithic yard,’ but thought the odds were against this. A further count against the Thom view was that there was no trace among any British prehistoric finds of anything like a measuring rod, even among the wooden objects preserved in bogs. Indeed, this was true of all western and central Europe, save for one single and doubtful case, a notched stick from peatland in Denmark.”

John David North, who clearly knew this work regarding geometry had refuted all that he claimed to be established knowledge, just as with his criticism of Velikovsky, when it was refuted, failed to retract one iota of what he had written. His stance showed he was far more interested in saving face than admitting error.

Mathematics is a language, though a specialized one. Unless, and only unless, the numbers are connected to reality, like historical documents or archaeological interpretations of relics in the ground, they do not relate to reality. Claudius Ptolemy used mathematics to prove that the Earth was the center of the then known universe, employing various mathematical concepts such as epicycles, equants etc. Over 1000

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28 Hutton, *The Pagan Religions...*, op.cit., pp. 112-113
years later Nicholas Copernicus used these same devices mathematically to claim that the Sun was the center of that universe. Thereafter Tycho Brahe used various mathematical devices to place the Earth back at the center and have the Sun orbit the Earth while the planets and stars orbited the Sun. In each case, the mathematicians made the mathematics say precisely what they wanted it to say.

A great many other investigators have used mathematics to prove many concepts about Stonehenge and the other megalithic monuments, none of which can actually be proven to be the intention of these megalithic peoples. Alban Wall has presented an ingenious interpretation of Stonehenge as an ancient Celtic calendar. Robin Heath and John Michell have tied Stonehenge to the dimensions of the Earth. Ralph Ellis has tied Stonehenge to the pyramids of Egypt, to the ancient measurement of the cubit and to the Greek letter Pi (π). In each of these analyses, and in many others, the mathematics fits the monuments and the theories about them. But in spite of these meticulously measured and ingenious theories that connected them together, there was and could be no proof that this was the intention of the builders. Mathematics is not enough in such cases; to prove these various contentions requires proof of that intention. Because each of these precisely measured different mathematical theories proves different theories about these megaliths, it is the theory that governs the mathematical analysis. These mute stones only speak when many interdisciplinary forms of evidence prove the same thing.

Our approach, to paraphrase MacKie, is to rely not only on geometrical and astronomical theories and evidence, but the entire gamut of other scientific and technological disciplines as a whole in interdisciplinary patterns as these impinge on the theory in as many details as possible which we will continue to develop. Here indeed is a vast amount of information painstakingly gathered over years. When one has enough of such interdisciplinary information that begins to fall into a pattern of its own accord when analyzed in various ways, we maintain the pattern is real and we should accept it. If our critics do not like the totality of this evidence and the conclusions drawn from the patterns, these must be scientifically, technologically and in other ways shown to be invalid. But we maintain this data is impossible to ignore. We ask what new scientific and technological data in all

30 Robin Heath and John Michell, The Lost Science of Measuring the Earth: Discovering the Sacred Geometry of the Ancients (Kempton IL 2006)
31 Ellis, Thoth..., op. cit., p. 58
these cases proves the interconnections of the evidence for this pattern which has emerged respecting the short chronology.

In this regard we must turn to the metrological evidence that corroborates the chronology, indicating that the medieval/Megalithic Age belongs in the post-Roman/Anglo-Saxon era. The point that must be stressed is that the yardstick or sticks used at Stonehenge, Avebury, and elsewhere should be known to have existed prior to and into the Megalithic Age. In addition the unit or units of measure that were used should have been employed into later medieval times. In this regard Ralph Ellis has shown that:

“The results of Professor Thom’s research gave some very good whole-number units for the construction of many Neolithic sites...But, despite the success of the Megalithic yard in many cases, there is a small problem; it does not explain the design of all the Neolithic constructions. Most importantly of all, it does not seem to work for Avebury.

“One of the most important spacings on the Avebury site is the interstone spacing, especially in the [two separate] inner circles [contained within the great outer circle]. Despite the fact that some stones do not conform to this exact spacing length—because the perimeter ditch is not perfectly circular—it has been widely accepted that the distance between each stone center at Avebury was supposed to be 11.04m[eters] or just over 36 feet. Indeed, the more regular spacing of the smaller northern and southern [inner] circles at Avebury conform[s] quite precisely to this length. But the Megalithic yard does not fit this length at all, as it gives us a rather odd 13.3my or, when using the [megalithic] rod length, 5.3 m[egalithic] r[ods]. Avebury was the biggest henge in Europe, in the world no less, so if the Megalithic measurements do not work here, there must be a flaw in the system. The Megalithic yard cannot be the full story.…

“There are many possibilities, but what would be really satisfying in this case is a new unit of length that could resolve the Avebury stone separation into whole numbers and, at the same time, somehow form a link between the Megalithic [metrological] system [of Thom] and the new measurement system. Such a unit of length would form and confirm a real link between Avebury and Stonehenge.”

The new unit that Ellis discovered was a great surprise to this author because it was one that was used by the Saxons in Roman and post-Roman times. Ellis to his credit was, so far as we know, the first investigator to have found this unit which

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32 Ellis, *op.cit.*, pp. 49-50
he named in honor of a mythical king supposedly buried under the great flat cone mound at Silbury near Avebury, King “Zil.” Ellis continues:

“Creating whole numbers at any one site is an easy matter, but fitting that new system of measurements into existing theories and forming a compelling rationale behind that new system is much more difficult….Finally, after much juggling of numbers, it was found that there is a good argument to be made that there was, perhaps, a second length in use at these ancient sites—one that I shall call the Zil yard—and this unit measures 1.004 meters. In support of this theory, take a look at the following table of measurements from both the Avebury and Stonehenge sites:

<table>
<thead>
<tr>
<th></th>
<th>Zil yard</th>
<th>Zil rod</th>
<th>Meters [5½ Zil yards]</th>
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</thead>
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<tr>
<td><strong>Stonehenge</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Upright stone width</td>
<td>2</td>
<td>-</td>
<td>2.07</td>
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<tr>
<td>Distance between them</td>
<td>1</td>
<td>-</td>
<td>1.03</td>
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<td>99.40</td>
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<td>93.90</td>
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<td>Central horseshoe length</td>
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<td>4.5</td>
<td>22.30</td>
</tr>
<tr>
<td>Central horse shoe width</td>
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<td>16.60</td>
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<td><strong>Avebury</strong></td>
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<td></td>
<td></td>
</tr>
<tr>
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<td>2</td>
<td>11.1</td>
</tr>
<tr>
<td>Ditch perimeter</td>
<td>1347.50</td>
<td>245</td>
<td>1350.0</td>
</tr>
<tr>
<td>Stone perimeter</td>
<td>1078</td>
<td>196</td>
<td>1082.0&quot;³³³³</td>
</tr>
</tbody>
</table>

What will be shown below is that the Zil yard is the same length as the Saxon yard. What Ellis found at Stonehenge and Avebury appears to be unique to these monuments. In a certain sense they are unique megaliths because at Stonehenge the Sarsens made of quartzite were shaped by tools giving smooth flat surfaces while at Avebury only the inner facing surfaces were smoothed. Ellis goes on to state:

“Looking at the table again, most of these odd Zil yard lengths are suddenly improved by using the Zil rod, a rod length that uses multiples of 5.5 z[il] y[ards]. It is likely that nobody thought of translating the resulting yard lengths into larger lengths (rods) by using the rather odd multiple of 5.5, as it is not exactly the first multiple that springs to mind when trying to simplify fractions. But, surprisingly enough, this fractional unit does provide significant benefits.

“…However, the 5.5 yard Zil rod has not been created by the author just because it happens to fit the data; it also has some very good historical justification for its existence. The British Imperial rod, which has been in use for hundreds of years in Britain, was also 5.5 yards long, and this is where my solution to the problem originated. Although this Imperial rod was smaller in absolute terms, perhaps this evidence for an ancient 5.5 yard rod is an indication that the British Measurement System has a longer tradition than it has normally been credited with.”

In terms of our chronology which holds that Stonehenge and Avebury as well as the rest of the megalithic monuments only go back to post-Roman/Saxon times, the connection and the heritage of the British Imperial rod being 5.5 times the British yard makes excellent sense. Thus there is a fairly direct link between Saxon metrology running from the time when Stonehenge and Avebury were built through medieval and later British times. Of greatest importance, Ellis shows:

“The Zil yard length has not been conjured up by the author either; it is a measurement based on the old Saxon yard, which was the same length…”

Although Ellis suggests that the Saxons may have inherited this unit from the prehistoric megalithic builders, this is scarcely possible because the megalithic builders supposedly vanished around 1200–1500 B.C. and left nothing in any further constructions by which this unit of length could have been transmitted across an interval of a thousand years or more. What seems to be obvious is that the people who built Stonehenge and Avebury were post-Roman Saxons who employed their Saxon yard unit in the construction of these monuments. That is, we have a direct unit of measure—the Saxon yard—which could only have been employed in Britain in post-Roman times connected to the stone circles of Stonehenge and Avebury. This unquestionably follows only if these monuments were built by the Saxons! It fully correlates with the chronology we propose and it is in stark contradiction to the established chronology.

There is also a direct connection between the Saxon yard [Zil yard] and the smaller British Imperial rod which has a similar tradition although not of the same measure. Ellis further shows:

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34 Ibid., pp. 51-52
35 Ibid., p. 52 (emphasis added)
36 Ibid.
“…the Zil [Saxon] yard [1.004 meters] is related to his [Thom’s] Megalithic yard [32.64 inches]. Thus, it can be seen that 20 [megalithic yards] equals 16.5 [Zil/Saxon yards]. This may sound like an awkward ratio, but again it simplifies itself if we talk in terms of rod lengths, so in rods the relationship becomes 4 [megalithic rods] equals 3 [Zil/Saxon rods].

“For this simple ration between the Zil [Saxon] and [Thom’s] Megalithic systems, I have doubled up the Megalithic rod to 5 yards not [his] 2.5 yards, as it makes the ratio simpler. But this is not the only justification for using this length for the Megalithic system, for the Saxon rod was also 5 yards in length—5 Saxon yards…. [T]his may be a case of an ancient [prehistoric] system that has slipped into more recent usage.”

Ellis also shows:

“Amongst the many units of length in use in late Saxon Britain, there was the Saxon foot, which measures 33.4 cm [a little over 12 inches], and this unit would seem to be a direct descendent of the Zil [Saxon] yard. Three feet normally make a yard and so the Saxon yard equals 1.002 meters, against the Zil [Saxon] yard of 1.004 meters. For long measurements, the Saxons used a unit of 15 Saxon feet or 5 yards…which was known as the rod. This unit is either a simplification of the Zil rod which comprises 5½ yards, or perhaps it was a deviation from the Megalithic rod, which seems to have comprised 5 yards. Whichever the case, it would appear that the Zil system had [supposedly] survived [from its prehistoric origins] at least as far the Saxon invasion of Britain in the fifth century, despite the intervening tribulations of the Roman occupation.”

Although the Saxon units of measure were changed in the later part of the medieval era, the relationships between them were maintained. King Richard as well as Edward I changed the various lengths during their reigns.

Graham T. Smith sums up this later development:

“In England as early as 1000 the Saxon king Edgar kept a yardstick at Winchester as the official standard of measurement. However, it was not until King Richard I (the Lionheart) that any form of standardization of units of measurement was first documented. In his Assizes of Measure in 1196 it was stated: ‘Throughout the realm there shall be the same yard of the same size and it should be of iron.’ This metallic artefact was the first permanent standard measure

37 Ibid., p. 52
38 Ibid., p. 78
to be utilized in an attempt to control the vagaries of linear measurement. In 1215 in the reign of King John, the Magna Carta also considered units of standardized measurement…The yard (or *ulna* as it was sometimes known) came into existence during the reign of King Edward I (1272-1307). Here it is stated that ‘It is ordained that three grains of barley, dry and round make an inch, twelve inches make a foot, three feet make an *ulna* [yard], five and a half ulnas [just like 5.5 Zil yards] make a perch [rod] and four perches in breadth make an acre. The *perch* or *rod*, as it was known, was a traditional Saxon land measure that still survives into the twenty-first century…”

What we have is a long tradition of units of measure that are of Saxon origin, used to construct Stonehenge and Avebury, evolving through time through the medieval period into the late medieval and into the present, based on the Saxon yard. It also seems clear that metrological units in Britain varied somewhat in length and were not widespread, but each tribal group had somewhat similar units of length and used them at their different construction sites or may have in many cases simply paced out their plan. This clearly seems to explain these variations of measure found at different megalithic sites. It was only around A.D. 1000-1200 that the wilderness had been cleared and strong leaders arose from the incessant medieval wars, and forms of trade and construction were undertaken, that the forerunners of the great national states came into being, and standardized units of measure, weight, etc., were needed and created. Along these lines John J. Roche explains in terms of the ancient Near East how this development occurred:

“Standardization was presumably found necessary because the natural body units of length used initially, such as the palm, the foot[, etc.], varied too much. Standardization was apparently guided by the effort to relate the new fixed units integrally both to the older body-part units and also to more constant and widely available natural units which could allow the former to be reconstructed if high accuracy was not required or if they were unavailable…”

As in the Near East, it was only with the establishment of large states which undertook national building projects that units of measure came to be standardized. But in Britain the historic link of these Saxon measures is shown by aspects of its system that were kept throughout post-Roman British history into the present, as Stephen Skinner suggests: “The megalithic yard relates to the ‘measuring rod’, a

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very old British unit of length. Also known as the pole or perch, the rod measures 16.4 feet (5 [meters or almost 5½ present-day British yards]."

One critic, who happened to explain that Stonehenge and Avebury were consistent with the Saxon yard measure, claimed that Stonehenge had been largely reconstructed and thus this measure was valueless. However, if that was the case it would be impossible to rebuild the monument, especially with the uprights and lintels fitting into the mortise and tenon joints that connect them over the site and have them at the incorrect distances from one another even if the original uprights were moved slightly. Were the uprights moved out of place, then the lintels that top them and connect them would show large gaps between them. This is simply not the case. Even if it was, it would hardly be possible to move all these stones and still have the distance between all of them coincidentally just happen to fall into Saxon yards.

As for the historical antecedents of the various units, Jay Kappraff described the origin of these from the Indo-Europeans who migrated into these regions.

“What is the origin of the idea of the standard measure? The archaeologist, Colin Renfrew…has traced the movement of the first farmers from what is now modern Turkey and the fertile crescent both East and Westwards from [prehistoric times]. As a result, we now know that the first farmers to arrive in Britain in about 4500 B.C. were Indo-European. Renfrew believes these first farmers brought with them a farming package consisting of grain; domestic animals and pottery. Anne Macaulay conjectures that we owe the origin of measure to these ancient farming communities. Farmers had to know how much grain when sown in the earth would produce enough surplus to feed their people. From this emerged the bushel type of measure. In order to know how much land to sow grain upon, they produced a measuring rod or yardstick”

To this characterization, in large measure we concur, except that we propose that pastoralism dominated agriculture. If this is the case, then the various units of measure have an early ancestor or ancestors, and this should be observed in many of the regions where the Indo-Europeans settled. By moving the Indo-Europeans about 1000 years closer to the present, not only can we explain the evolution of the earliest proto-Indo-European language with that of the Hittites as outlined in volume III of this series, but we can connect the unit or units of measurement they used with those that followed them into the ancient civilizations of the Near East, Greece and into the post-Roman Megalithic Age. We pointed out that Thom’s

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42 Jay Kappraff, op.cit., p. 236
megalithic yard was 33.64 inches. This unit was incorporated into many monuments although it did not fit all of them. However, it did clearly relate to some rather precisely and others not so closely. We suggest that a unit of about this length was used by different early peoples and had its origin in the ancient Indo-European culture. In terms of the short chronology this should therefore apply not only to the megalithic European world but be found in parts of the ancient civilized world and exhibit the same sort of tradition we found for the Saxon yard and rod in the British tradition. Speaking of the megalithic yard, Knight and Lomas show that very similar measures were used in ancient India and in post-Roman modern times:

“The existence of an ancient civil engineering unit is truly remarkable…this [unit] was the granddaddy of all measurements!...

“Archaeologist Euan MacKie observed how fundamental the megalithic yard appears to be.

“…a traditional unit of length known as the gaz was in use at late as the nineteenth century in north-west India when it was standardized at exactly 33 inches (0.838 metres) by the British Government. In the 1930s excavations at Mohenjo-daro…in India and a part of the Bronze Age Indus civilization…contemporary with Early Dynastic Sumeria and Old Kingdom Egypt, revealed a small piece of shell, part of a longer [measuring] piece and with nine exactly equal divisions still remaining which had been cut into it with a fine saw. Five of these small divisions evidently made up a larger unit the boundaries of which were marked with a dot on the line. A.E. Berriman has pointed out that 25 of the major division, now called ‘Indus inches,’ totalled exactly 33 inches, the same length as the traditional gaz. It is difficult not to believe that the metrology of the Bronze Age cities was handed down intact over 45 centuries.

“Berriman also worked out the length of some small Sumerian length units from scales cut into stone statues of the Governor Gudea of Lagash in Mesopotamia… These units which he thought were probably the ‘shusu’ mentioned in the cuneiform texts, were on average exactly half the ‘Indus inch.’ Thus there were 50 Suneruab shusi in the traditional Indian Gaz…”

As can be seen, there was a continuity of the tradition of using a unit of length of about 33 inches from the Sumerian/Chaldean and the Indus civilizations down into relatively modern times. However, there is more evidence which relates this

43 Knight and Lomas, *Uriel’s Machine…*, *op.cit.*, pp. 205-206
measure to Spain, the Americas, and the mining district of the Tyrol in Austria, as Knight and Lomas go on to show:

“A similar ‘short yard’ of about 32.5 inches known as the vara, was in use in Iberia [Spain] and also seemed to have been taken to Mexico and Peru by the Spanish conquerors, while the traditional measuring rod of the mine overseers of the Austrian Tyrol are also the same length. These measuring rods are claimed to have been used in mining activities since the Early Bronze Age.

“So the ancient Indian ‘gaz’ was the same as a megalithic yard to an accuracy of one per cent and the Iberian ‘vara’ [and the unit of length used in the mining district of the Austrian Tyrol] was less than half a percent different.”

Now it is obvious that the Indo-Europeans in many regions to which they migrated did not continue to use this unit of measure, inventing over time other units. This is to be expected, for, unlike language imbedded in the Indo-European culture, metrology can be easily changed by whim or by historical experience which allowed whole peoples to change their early traditions. It is at the early beginning of prehistory that peoples became isolated and different rulers adopted different units of measure that reflected different parts of their bodies. A possible example of such an early change is outlined by Kappraff who shows:

“[Anne] Macaulay has shown that there is a strong possibility that the Greek fathom was identical to [Thom’s] Megalithic British rod…and is similar to the ancient Indus short yard of 33 inches and the Sumerian [Chaldean] shusti, 33 inches. As a result of [Thom’s] measurements, he conjectured that the megalithic rod was 2.072 meters (6.80 feet) in length. This resulted in a perfect match with the length of the Greek fathom reported in 1981 by E. Fernie…It is important to note that Thom died in 1978 before the discovery of the Greek measure and so he could not have been influenced by it.”

In terms of the connection of this ancient tradition with more recent times, Kappraff adds:

“Macaulay points out that the Megalithic rod = Greek fathom continued to be used for church dimensions in northwest Europe (but not in Italy) up until the [time of the] latest Gothic churches (c. 1500 A.D.). It was a great surprise to the team of Macaulay, Gordon Strachan, and Fred Robertson, that measurements of

\[44\] Ibid., p. 206
\[45\] Kappraff, *op.cit.*, p. 237-238
the base lines of the Chart[r]es Cathedral and St. Georges [Chapel], Windsor were integral numbers of megalithic rods. It was only after discovering Fernie’s article on the equivalence of the megalithic rod and the Greek fathom that this discovery began to make sense.”

Thus, it appears that different traditions of metrology existed in medieval British culture: the Saxon yard which evolved through to the British Imperial rod and the *ulna* of which 5.5 *ulnas* make a perch used in land measure into the twenty-first century, and the *gaz, vara, Tyrolian yard* and *Greek fathom* that shared a place alongside it. With the short chronology in which the onset of civilization begins around 1200 B.C. or later, the connections and interconnections of these units of measure from the past make evolutionary sense. With the established long chronology there is a very long separation between the disappearance of megalithic society—a Dark Age—with the subsequent adoption of this unit by the various other peoples. In the short chronology there is no such Dark Age.

However, there is another form of evidence relating to mathematics which indicates that other solid balls supposedly dated to the Late Neolithic had designs cut into them that belong in Roman times between A.D. 200 and 400 and possibly beyond. George W. Hart shows on his web site “*Encyclopedia of Polyhedra,*” under the subheading “*Roman Dodecahedra*” that bronze dodecahedrons were found in several countries of Europe. Hart cites the published works of Benno Artmann, Michele Emmer, and Joseph Malkevitch:

“Dozens of hollow, bronze dodecahedra survive from Roman times, the second to fourth century [A.D.]. They have been found in Great Britain, Belgium, Germany, France, Luxembourg, Netherlands, Austria, switzerland [*sic*], and Hungary. Ranging from 4 to 11 cm, they are embellished with spheroids at each vertex and circular holes of various diameters in each face. The articles by Artmann, Emmer, and Malkevitch (listed in the references) describe them further…Often there are concentric circles scored around the openings…”

“The function of these dodecahedra has not been determined [for no] one knows…”

“Artmann also reports one Roman icosahedron…”

The importance of this evidence to the short chronology lies in the fact that round stone balls with similar forms—not identical forms—have been found in

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Scotland, dating supposedly to the Late Neolithic. Alexander Thom and his son Archie Thom, because of their thesis which held that these ancient people were knowledgeable of mathematics, drew the conclusion that the stone balls found in Scotland were actually Platonic regular solids and, therefore, not only did these early people have some understanding of Pythagorean triangles but also of the five Platonic solids. As proof of this they offered the following:

“The most perfect proof of the ability of Megalithic Man to understand and use solid geometry (the geometry of the sphere) is provided by the fact that over 300 balls made from hard stone, each with a surface divided into triangles, squares, pentagons or hexagons have been found. The distribution of the ‘find spots,’ [is] chiefly in Scotland… [It] can be compared with the positions of geometrically constructed rings [that we propose were constructed with Pythagorean triangles] of standing stones…”

“It can be shown that there are only five so-called platonic solids …:
“(1) the tetrahedron consisting of 4 equilateral triangles;
“(2) the cube consisting of 6 squares;
“(3) the octahedron consisting of 8 hexagons;
“(4) the dodecahedron consisting of 12 pentagons; and
“(5) the icosahedron consisting of 20 equilateral triangles.

“These were described by Plato a thousand years after the above-mentioned stone balls were made. Among the stone balls found, all five of the platonic solids are represented on [sic] one form or another…”

“Possibly the makers of these [stone] balls made first a perfect sphere and then scribed out on it the figure they required. It is evident that they had a perfect mastery of their subject.

“The [precise] date of these balls is not known, but only in the period when Megalithic Man was setting out the sophisticated stone rings has a sufficiently high standard of mathematical knowledge and skill ever been reached before the fifteenth century A.D. Even today there are few archaeologists capable of appreciating the underlying geometry.”

The concept is reported in a book on metaphysics by Michael Poynder who summarized these stone balls thus:

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“In the Neolithic period of 4000-2000 BC in northeast Scotland around Aberdeen a remarkable series of carved stone balls have been discovered. They have been found in many different situations but seldom with human remains—some 400 in total—and they have confounded archaeologists and historians as to their possible use. The materials [from which they have been carved] differ from mainly serpentine to sandstone to granite quartz and even hornblende, an incredibly hard rock. They vary in size between a tennis ball and a large golf ball and are curiously shaped in many different geometric forms.

“These spheres are shaped to represent the different symmetries encountered in Platonic geometry…

“It is really remarkable that geometers of the Stone Age who are represented by historians and archaeologists as Pagan and ignorant could carve such highly sophisticated products in stone predating Plato by millennia.”

The investigator who originally drew attention to these supposedly prehistoric stone balls that seem to exhibit the form of Platonic solids is Keith Critchlow whom Alexander and Archie Thom cited to the effect that these were clear-cut examples of this ancient knowledge. There are two aspects of these stone balls that relate to chronology: If they are indeed Platonic solids, though there is some controversy about this, then we are left with the problem of explaining how Stone Age people derived, or knew of, this advanced knowledge of geometrical forms 1000 years before Plato. The second problem is: Why should we date them to the Late Neolithic when we have in fact examples of Roman bronze polyhedrons that are found through much of northwestern Europe which are small, like the stone balls of Scotland, with a few in Ireland and Britain, and have knobs or bosses on them similar to these stone balls? What we have is Late Neolithic people creating stone balls similar to the Roman bronze ones over 2000 or more years later. It is hardly credible that two peoples separated by over 2000 years should by coincidence create small, knobbed or bossed stone balls similar to one another in Europe. What is implied is that these two small, knobbed or bossed sets of polyhedrons were made around the same time, and that time was either in the Roman era for its Platonic solids and those in Scotland, or in Scotland shortly thereafter.

We add the fact that the builders of Stonehenge and Avebury used the Saxon yard in their construction of these sites, etc., along with the fact that neither the

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Romans nor the Greeks wrote about these structures; along with the fact that between about 1500 and 1200 B.C. into medieval times no one left graffiti or inscribed art on these monuments, a period of about 3000 years; along with the fact that frost-action and other forms of erosion failed to completely remove or almost obliterate engravings on these megaliths over a period of between 5000 to 3700 years; along with the fact that the builders of Stonehenge shaped the Sarsen Stones with an entasis form: all of this shows rather clearly that these monuments and the artifacts associated with them were built in Saxon times.

The problem we face in this regard is the consensus that has been reached by the authorities of Megalithic Age chronology. Bruce D. Smith discussed another topic of prehistoric archaeology which can be applied to the hard facts we have so far presented as contradictory to the established chronology:

“Although consensus opinion on the relative weight that should be assigned to positive supporting evidence versus supporting theory will inevitably continue [well into the future], few would take issue, even now, with the importance and impact of any extant negative empirical evidence. Whereas theory or opinion that runs counter to an explanation can be brushed aside as being irrelevant, a soft slap to be easily ignored, hard facts are another matter. Regardless of how much appeal an explanatory framework has for a broad audience, how easily it fits in a cameo textbook format, or how well it resonates with current theory, it is still...open to total destruction by a single well-targeted contradictory fact. Information derived from an empirical database, of course, can and often does function as both positive and negative evidence, filling the role of ‘critical test implication’ (Smith, 1977) by providing support for one explanation while at the same time undercutting or destroying another.”\(^{51}\)

At this relatively early stage in our investigation we have no doubt presented not just a “single well-targeted contradictory fact” but several “providing support for one explanation” [the short chronology] while at the same time undercutting or destroying another [i.e., the established chronology]. But there is more—much more.

CHAPTER 8

STRATIGRAPHY, EARTHWORMS AND CHRONOLOGY

Another method by which to gauge the general date when Stonehenge and the other megaliths were erected is that of stratigraphy and archaeology. In forming the surfaces of the sarsen and bluestones at Stonehenge, these had to be dressed and shaped to certain dimensions. In doing this task the masons had to remove stone, which would leave stone chips about the site. Although it has been suggested the stones were actually dressed at the distant quarry sites, this, we will show below, is not the case. Moreover, the stone masons would need to eat and drink and would have had pottery containers for the period while they dressed and erected the stones. Some of these clay pots in the course of handling would have been broken and the sherds of these would also have been strewn about the site. On the basis of the short chronology, these sherds and pottery pieces should be found slightly above or around the same level as Roman floor levels of abandoned villas, etc. Furthermore, if, as the established chronology demands, Stonehenge was constructed well over 1000 years before the very existence of the Romans and Saxons in Britain, the types of pottery sherds found with these sarsen and bluestone chips would reflect Neolithic and Bronze Age pottery. If, as we propose, Stonehenge was built soon after Roman times, the types of pottery sherds found with these stone chips should reflect those of the Roman vessels which the Saxons greatly admired and kept, as we will show below, as well as Roman coins that still remained in use on the island. It is well known that Roman and later pottery sherds were found at the same level as that of bluestone chippings. Chippindale, whom we cited above regarding entasis being in Iron Age-Greek/Roman time, asks: “And if [Stonehenge] was early [ca. 3100-2500 B.C.] why were the sets of Y and Z Holes dug in the Iron Age, the best part of a thousand years later, and why was the site so scattered with Romano-British pottery?”

Not only was this pottery found scattered about Stonehenge but it was also found in the Y and Z Holes. Burl claims: “There was a motley of finds in the holes…ware of the Middle Bronze Age, pieces of bluestone and rhyolite, a human tooth as well as Romano-British pottery and other later material…” It is assumed that this pottery

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1 Chippindale, Stonehenge Complete, op.cit., p. 198
was dropped in these holes long after they were dug and filled in. But this does not answer the question of why the bluestone and rhyolite stone chips are also found there. Because these were found at the bottom of these holes, it is again assumed they date to early prehistoric times and the pottery sherds to Roman, Saxon and later ones. The question comes down to the chronology of this site and the others in terms of stratigraphy. To do this we must address how the Romano-British potsherds came to be buried at the same level and how that level compares to Roman floor levels at abandoned villas, etc. This brings us to the work of Charles Darwin and his last book which, in part, dealt with Stonehenge as well as floor levels of Roman sites.

Darwin had come to Stonehenge in 1877, the same year as Flinders Petrie, to examine how some of its fallen stones were partially buried by soil or mould that had piled up around their sides over the centuries. Because Darwin was under the influence of Charles Lyell, a uniformitarian amateur geologist and lawyer who held that nearly all geologic changes occurred very slowly over long periods of time, Darwin, in his last great work, wanted to show how human and other artifacts left on the earth’s surface would gradually be buried over the centuries. He correctly understood that this gradual process would be done by the agency of earthworms. His book, published in 1881, a year before his death was titled: *The Formation of Vegetable Mould, Through the Action of Earthworms, with Observations on their Habitats*. In terms of chronology he showed the gradual burial of archaeological sites in his chapter, “The Part which Worms have played in the Burial of Ancient Buildings,” wherein he states:

> “Archaeologists are probably not aware how much they owe to worms for the preservation of many ancient objects. Coins, gold ornaments, stone implements &c., if dropped on the surface of the ground, will infallibly be buried by the castings of worms in a few years, and will thus be safely preserved until the land at some future time is turned up….In the present chapter I shall show that not only implements, &c., are thus preserved, but that the floors and the remains of many ancient buildings in England have been buried so effectually, in large part through the action of worms, that they have been discovered in recent times solely through various accidents. The enormous beds of rubbish, several yards in thickness, which underlie many cities, such as Rome, Paris, and London, the lower ones being of great antiquity, are not here referred to, as they have not been in any way acted on by worms.”

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3 Charles Darwin, *The Formation of Vegetable Mould, Through the Action of Earthworms, with Observations on their Habitats* (NY 1881), p. 52
A year later in a “Review” of Darwin’s book we are informed of the enormous work accomplished by these small creatures:

“The estimates of the amount of mould brought up by worms based on the actual weighings and measurements of the castings [brought to the surface at night] at particular spots, give results ranging from 7.55 to 18.12 tonnes per acre [an area 400 feet wide by 400 feet long] in one year, and a volume sufficient to make when spread out a layer from one to more than two inches in ten years. The remains of ancient buildings seem also to have been buried effectively, in large part through the action of worms.”

When Darwin went to Stonehenge he measured the depth of the worm castings on the side of fallen stones:

“At Stonehenge some of the outer…stones are now prostrate, having fallen at a remote but unknown period; and these have become buried to a moderate depth in the ground. They are surrounded by sloping borders of turf, on which recent castings were seen. Close to one of these fallen stones, which was 17 feet long, 6 feet broad and 28½ inches thick, a hole was dug; and here the vegetable mould was at least 9½ inches in thickness. At this depth a flint was found, and a little higher up on one side of the hole a fragment of glass. The base of the stone lay about 9½ inches beneath the…ground…

“A hole was also dug close to a second huge stone, which in falling had broken into two pieces;…The base was buried to a depth of 10 inches, as was ascertained by driving an iron skewer horizontally into the ground beneath it. The vegetable mould forming the turf-covered sloping border round the stone, on which many castings had recently been ejected, was 10 inches in thickness; and most of this mould must have been brought up by worms from beneath its base. At a distance of 8 yards from the stone, the mould was only 5½ inches in thickness (with a piece of tobacco pipe at a depth of 4 inches), and this rested on broken flint and chalk which could not have easily yielded to the pressure or weight of the stone.

“A straight rod was fixed horizontally (by the aid of a spirit-level) across a third fallen stone, which was 7 feet 9 inches long; and the contour of the projecting parts and of the adjoining ground, which was not quite level, was thus ascertained…The turf-covered border sloped up to the stone on one side to a height of 4 inches, and on the opposite side to only 2½ inches above the general level. A hole was dug on the eastern side, and the base of the stone was here found

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4 *Appleton's Annual Cyclopaedia and Register of Important Events*, vol. 6 (London 1882), p. 226
to lie at a depth of 4 inches beneath the general level of the ground, and of 8 inches beneath the top of the sloping turf-covered border.

“Sufficient evidence has now been given showing that small objects left on the surface...where worms abound soon get buried, and that large stones sink slowly downwards through the same means.”

In terms of chronology a stone lying on the ground or even in it would gradually sink beneath the surface by earthworms removing soil directly beneath the stone so it would move deeper into the ground, and by placing castings first on its sides then over it. The older the artifact or stone monument, the deeper beneath the surface it would sink over time. For example, the piece of the tobacco pipe was found 5½ inches below the surface because tobacco smoking only came to Britain around the 1600s from plantations in the Americas. Although earthworm castings grow in depth at different rates, these have in fact buried the oldest floors and the few remains of such cities as ancient Rome, Paris, and London beneath, as Darwin tells us, “[s]everal yards of this vegetable mould, except where buildings were kept in use and protected from these castings.” This is made explicit by J.S. Wacher:

“Most of the towns, both large and some small, of Roman Britain became perpetuated in the medieval and modern periods. Little of these is to be seen standing above the surface, as much of the Roman material lies deeply buried, sometimes by as much as 3-5 m[eters, 10 to 16 feet] beneath the modern surface, except perhaps where the modern version of the town is smaller than its Roman counterpart.”

Here and there ancient Roman floors and remains are still to be found at and above the surface but these are exceptions. In the 1500 years or so after the Romans abandoned Britain, Wacher makes it abundantly clear that their floors and relics are found buried to a depth of 3 to 5 meters or 10 to 16 feet. But many of the megalithic monuments were abandoned supposedly 3500 to 3200 years ago, roughly twice as old as the Roman remains, and the soil around them would have been raised by about twice the depth of the Roman floors and relics on average. There are provisos for areas which are water-logged or acidic soils where earthworm action would be attenuated and rocks set into chalk as at Stonehenge would not be able to sink below the surface by earthworm action because worms would not bring chalk up from under these stones. However, surface and subsurface build-up of the ground around these stones would still occur. As we

5 Darwin, op.cit., pp. 58-59
6 J.S. Wacher, A Portrait of Roman Britain (London 2000), p. 117
pointed out above, in terms of the established chronology, there were forests that supposedly enclosed these monuments around 3780–3800 B.C. until Roman times and from the Roman exodus after A.D. 410 to about 1000. Forests do build up the surface. In the *Encyclopedia of Soil Science* we read:

“Plants uptake chemical elements from soil, water and atmosphere and build up phytomass [plant decay mass]—the source of soil humus. The phytomass deposits on or in the soil *sic*. The annual fall of plant litter, mort mass [dead bark, leaves, twigs, branches and tree trunks] on the soil surface ranges from 1 to 30 ton/ha [while] roots in soil may reach 20 ton/ha in grassland and deciduous forests…”

There is no escaping the fact that in many regions throughout Britain, Ireland, Brittany, and perhaps Scotland the build-up of the soil would have been about 6 to 10 meters or 20 to 30 feet and would have buried all the smaller megalithic stones and many others of somewhat larger size. This would be especially notable in Brittany with its perhaps thousands of relatively small stone rows. One cannot have earthworms and the dead vegetation of forests fail to operate as they have always done except around megalithic monuments. The smallest estimate as related to Roman towns of 10 feet or 3 meters when doubled raises the ground surface by 20 feet or 6 meters and would completely bury innumerable megalithic stone monuments. But this is not always the case. Why did the earthworms and dead forest vegetation only bury those much more recent Roman structures deeply but fail to bury those much more ancient small ones, which in Brittany are the vast majority? It is simply impossible to have only the Roman structures buried by these processes and nearly all the small late Neolithic or Bronze Age stone monuments. One cannot selectively have these processes bury the Roman towns and villas but not these supposedly older megalithic ones. Even if we cut this soil build-up rate in half we still raise the ground surface by about 10 feet which would surely cover many prehistoric smaller megaliths. Darwin’s worms have eaten a deep hole in the established chronology and buried it. There is simply no escape from this scientific fact. Like erosion on these stones that indicates they are not anywhere as old as prehistorians suggests, earthworms and dead forest vegetation build-up of the surface also indicate that these stones are not anywhere as old as assumed.

On the other hand, should it be argued that there were erosive processes that offset the worm action and dead vegetation build-up, one would still be left with a contradiction. If erosive processes removed the surface materials around and over

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the more ancient prehistoric megaliths, why did they fail to do the same with Roman structures which should still be at the surface? Archaeologists and proponents of the established chronology are caught in a double bind: they are damned if they allow earthworms and dead vegetation to bury Roman towns and villa floors etc., because these processes of burial should have also buried most of the smaller megaliths etc.; they are also damned if they call upon erosional processes to keep these smaller megalithic structures still standing well above the surface because these same erosional processes would do the same with the Roman towns and villas. No matter which way they turn, their chronology simply fails. Those proponents of the established chronology who read this evidence, but fail to heed its consequences and fail to answer this contradiction with *scientific evidence*, will be playing fast and loose with scientific reality. The only response, as we see it, to this massive contradiction to the established chronology is denial. In psychological terms, denial is a retreat from reality to the protection of illusion, when reality, in this case *scientific reality*, becomes too painful. Velikovsky, a psychoanalyst, well understood this, and Robert Donald Romanyshyn explains this in terms of Velikovskian catastrophism, although he does not mention or maybe is unaware of Velikovsky. His statement nevertheless applies to the evidence we have just presented and to Velikovsky’s thesis of global catastrophism with its concomitant corollary: possible total destruction of the Earth and all life:

“…illusions become necessary when reality is too hard to bear. The destruction of the earth is, for the incarnated human soul, whose entire history is inseparable from and has been shaped by its place on the earth, an unbearable reality. At the most primitive and emotionally powerful levels, the very rhythms of the earth—the tides of the sea, the cycles of the seasons, the rising and the setting of the sun—have been a kind of guarantee of our own continuity. In knowing them, something of us, not individually but collectively, is also known, recognized and acknowledged. In the most extreme circumstances, we have been able to believe that there will always be another tomorrow.

“But what if tomorrow when the sun rises there is no living human being to see it? Then the earth which has linked together the generations of humanity falls into a kind of sleep…”8

In much the same way, those scholars nurtured for most of their working lives on the chronology that Stonehenge and the rest of the megalithic monuments are more ancient than civilization will also find this scientific reality too hard to bear.

The enormity of this contradiction requires that archaeologists must move the date of these monuments to a time close to the Roman era, if not sometime after it.

At this point we must reexamine the chronology as it relates to the stone chips and Romano-British pottery found at Stonehenge and compare this with the depths at Roman sites where Roman coins and pottery are also buried. If the short chronology is correct, the Romano-British pottery should be found slightly above or at about the same level as Roman floor levels, since many Roman towns and villas did survive into Saxon times. Gowland and Judd describe these scattered stone chips at Stonehenge:

“It must be emphasized that the stone chips cannot travel horizontally by themselves. They must be carried or thrown, and if they are allowed to lie on the surface for more than a few months they become buried by earthworms and can no longer move in any direction except downward. The casual collection of fragments by visitors, who subsequently lose interest and drop them or throw them away, would certainly produce in time a scatter of chips all over the surroundings of the site; but one would expect the number of chips to fall off fairly rapidly as one leaves the immediate vicinity of the stones [toward the center of Stonehenge]. In fact, while it is true that the highest concentration of chips occurs around the bases of the stones themselves [as placing in the holes dug into the chalk to support them in place before being filled in with soil], once one moves away from them the scatter is more or less uniform, and indeed there is at least [as] high a concentration in the ditch as there is in the vicinity of the Y and Z Holes. This is difficult to explain except in terms of the deliberate scattering of fragments, perhaps as a means of emphasizing the purposefulness of a partial [perhaps Roman] destruction of the monument [or that originally many stones were partially dressed at the site in post-Roman times]. The word ‘partial’ needs emphasis for although the number of chips over the whole site can be estimated to run into tens of thousands, their average size is small, and in sum they represent no more than the volume of the average pillar of dolerite and of rhyolite at the place of their erection. This is shown by the circumstance that within the limited area of the recent excavations, such a very large quantity of angular chips of these ‘bluestones’ have been found.”

Regarding these bluestone chips, Ronald Hutton explains:

“There is such a total lack of [bluestone material] in the area of any stratum which might have produced an outcrop of this sort [of stone]…So we are left with the apparently far less sensible idea that the ‘bluestones’ were moved at least 200

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9 William Gowland and John Wesley Judd, *Recent Excavations at Stonehenge* (1902; reprint Whitefish MT 2009), p. 79
miles from Presceli to Stonehenge….It is also very difficult to understand why the bluestones were not hammered into shape at the quarries, thereby shedding half the weight that would have to be shipped and dragged to Wessex: the quantity of [stone chip] detritus from them makes it indisputable that they were worked upon when they arrived at the Salisbury Plain.”

It appears obvious to Hutton that the amount of stone chip detritus in the soil around Stonehenge “makes it indisputable that they were worked upon when they arrived at” Stonehenge and at no other time. Therefore, having so many chips of this bluestone material at the same level in the soil as sherds of Romano-British pottery make it indisputable that they were left on the surface at the same time.

As for the Romano-British pottery found with the stone chips, Castleden points out that “Romano-British pottery along with a coin of the Roman emperor Claudius and bluestone chips are all buried in the same layer at Stonehenge.” To explain away these material differences of having Bronze Age/Neolithic age stone chips at the same level with Romano-British pottery Castleden argues:

“The inference is clear. The stones were broken up during the period of the Roman occupation, not before [but] by people using Roman pottery—a Roman military force…

“The slighting of Stonehenge was a purposeful act of destruction…. At the end of the operation only a few stones of the barbarian [pagan] temple had been overturned—it was probably heavier work than the Romans had expected.”

What Castleden has inferred is that since the established chronology is correct, then it must follow that Stonehenge was partially destroyed during Roman times, and while doing so they evidently broke Roman pottery during this attempt. It seems inconceivable that, had the Romans decided to destroy Stonehenge, they would not have toppled all the sarsen uprights and lintels. This could easily have been done by digging on one side of these stones, then tying ropes around their tops and pulling them down. Had the Romans meant to destroy the monument, they would not have left a single stone still standing. Again Hutton points out how unstably these stones were set into the earth and chalk beneath, showing:

“[S]igns of carelessness and haste. One upright in the outer circle was badly erected and so soon fell and broke. Its pieces were piled on top of each other and

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10 Hutton, The Pagan Religions, op.cit., p. 99
11 Castleden, The Making of Stonehenge, op.cit., p. 247
12 Ibid.
the lintel replaced, whereupon they subsequently collapsed again, this time permanently. The biggest of the central trilithons eventually toppled and broke because one of its uprights had been embedded in far too shallow a hole. The completed monument was visually stunning, but rickety in places.”

It seems inconceivable that, had the Romans determined to destroy this monument, they would have attacked it with hammers and not simply have dug down into the chalk to undermine and topple all these uprights. However, Castleden can prove none of his inferences which he merely takes as fact. But he also has overlooked the fact that the Romans were quite tolerant of other nations’ religions and temples. Robert M. Berchman explains:

“Tolerance of foreign religions and cults was a stable of Roman politics from the beginnings of the Republic to the closure of the Empire in the Latin West…Tolerance and respect, as well as a readiness to borrow and adapt foreign gods and goddesses was Roman custom.…

“Roman rulers confirmed the existing privileges of an ancient sanctuary when they conquered a region and welcomed numerous oriental cults into Rome. The principal motive for religious tolerance was political. Foreign cults were useful. As William Chase Green remarks, Rome did not want…to follow the Greek pattern of a land divided into a chessboard of petty [city] states [each with its own god]. One way to avoid a land divided is to practice religious tolerance.…

“Not all foreign cults and deities were accepted. Selectivity was Roman political policy. If beneficial to the state and if its adherents were able to coexist with the mores of the Roman state religion, then a religion or cult was tolerated. Little tolerance was exhibited toward religious practices thought politically subversive such as human sacrifice or divination or those actions thought to promote insurrection and immorality.”

It is well known that the Romans were intolerant of the Druids, but the Druids were known to have their places of worship in wooded forests and did not build stone monuments. Fergusson puts the case for Druidical worship at Stonehenge and Avebury bluntly:

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13 Hutton, *op.cit.*, p. 98
“[I]t may be asked, what is the evidence on which the Druidical origin of [and worship practices at] such monuments as Stonehenge and Avebury have been assumed? The answer fortunately is simple—absolutely none. It never was pretended that any direct testimony existed, and the negative evidence is perfectly complete. No ancient author, no one, in fact, anterior to the invention of printing, ever refers to any stones or stone temples, circular or in any other form, as connected with the worship of the Druids or the Celts.”

Thomas Low Nichols states: “The Druids worshipped the Supreme Being under the name of Esus or Hesus, and they made the oak tree the symbol of the Deity. They had no other temple than the grandest and holiest of all, the primeval forest…” Tacitus wrote that at a battle the Druids were conquered and “[a Roman] guard was placed on the conquered, and the groves [of trees], sacred to cruel superstitions, were cut down; for they held it lawful to sacrifice captives on their altars…”

There is no evidence that Stonehenge was a temple that the Romans wished to destroy. They might—if these stone monuments existed—have shown respect to them and have left votive offerings, but such is not the case. The only reason for raising this concept that the Romans partially destroyed Stonehenge is in the established chronology. The problem related to Roman pottery is that at other sites it is found directly below relics of medieval Britain as described by S.S. Frere:

“A feature of many sites in Canterbury is a thick layer of rather fine-grained black soil [probably the work of earthworms] which contains large quantities of late Roman pottery, but which intervenes between the late Roman and the Medieval layers or the Saxon when they are present. I understand that similar layers have been noticed at Winchester, Chichester and Cirencester. It is difficult to explain them except perhaps as the accumulation of wind-blown dust, household rubbish, decayed thatch, and disintegrating timber buildings, which is likely to accumulate when [Roman] discipline had disappeared.”

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15 James Fergusson, Tree and Serpent Worship: or, Illustrations of Mythology and Art in India in the First and Fourth Centuries after Christ (London 1868; reprint Whitefish MT 2000), p. 30
16 Thomas Low Nichols, Religions of the World: an Impartial History of Religious Creeds… (Cincinnati OH 1855), p. 29
In this case Roman pottery is found in a thick dark layer of soil which is the kind of soil laid down in forests where decaying vegetation produces humus and which earthworms turn over. This places this Roman pottery in Saxon times. Therefore it must be explained away. Yet in these instances the stratigraphy shows that there was Roman pottery still being used into the post-Roman period, which is the time we maintain Stonehenge was built. It must be understood that when the Romans abandoned Britain in A.D. 410, they did not all leave. There were left behind Roman cities, towns and villas that continued their Roman traditions.

Instead of following the dating procedures usually associated with pottery dating which indicates that the type of pottery found at a site usually is an excellent marker of the period in which the site was built; finding pottery sherds with building materials in the same stratum, this contradiction to the chronology at Stonehenge forced the archaeologists to create an ad hoc explanation to maintain their chronology. However, it is the depth of these that is pertinent to finding the correct chronology. When we compare the depth of these pottery and stone chip materials to the depths at which Roman floors and relics lay we can establish which came first and which came second. Wacher above has told us “Roman material lies deeply buried, sometimes as much as 3–5 m[eters, 10 to 16 feet]…” This of course is as great a depth as has generally been found. Some Roman materials are not found at such great depths, but much closer to the surface, as we will see. Anthony Johnson cites William Cunnington who did excavations in and around Stonehenge and wrote: “I have dug two or three times before the altar stone, once to a depth of six feet. At about three feet deep I found Roman and other pottery.” 19 While Roman floors and materials are found as much as 3 to 5 meters or 10 to 16 feet deep, Romano-British pottery and stone chips are found at about 3 feet. This large difference in depth, although extreme for Roman materials, indicates that the erection of Stonehenge came after the Roman period. Of course it can be argued that Roman pottery at Stonehenge is not proof that this material was deposited on the surface in Saxon times and could have been deposited in the Roman era, because the Saxons would hardly have access to that pottery. Nevertheless, it is quite clear that they did have access to Roman pottery and collected as well as admired these forms that were highly superior to the rude pottery of British origin. The reason we know of this is that the Saxons left Roman pottery in their graves, as outlined by Lloyd Laing and Jennifer Laing:

“To judge from cemetery finds, the early Anglo-Saxons found Roman objects attractive. The artefacts were not merely rubbish survivals, perhaps contained in

19 Johnson, Solving Stonehenge, op.cit., p.75; see also Fergusson, Rude Stone Monuments, op.cit., p. 105
the soil used to backfill the graves, or even curiosities picked up on abandoned Roman forts or settlements. Amongst the most common [Saxon grave objects] are glass vessels and COMPLETE ROMAN POTS. The chances of unbroken glass or ceramic vessels ‘lying around’ on abandoned sites to be picked up by the passing Saxons is [sic] extremely remote; they are more likely to have been acquired directly from their original owners…

“From this, certain possibilities emerge in the mind of the curious scholar. With a few notable exceptions, all the objects are exactly the sort of [expensive and showy] thing people call heirlooms.”

David Mackenzie Wilson echoes this concept: “There is little doubt from the distribution in Saxon shore forts, and near important towns and other Roman centres, that we have here a class of pottery made by Romano-British potters to meet the tastes of Anglo-Saxon mercenaries brought in [to Britain] during the fourth and early fifth centuries [A.D.].”

The nub of the change over from Roman culture to that of Saxon culture was analyzed by R.G. Collingwood who suggests it was not rapid:

“In 410, the year when Honorius told the British towns to look after themselves, the Romans, according to the generally received view, ‘left’ Britain… What was the nature of the event whose date is disputed?

“‘The Romans left Britain.’ To the reader familiar with modern empires, it sounds intelligible enough. If the Belgians left the Congo, if the Dutch left Java, if the British left India…, the first two cases are fairly straightforward; but in the third difficulties begin to loom. If the English raj ceased, if the English troops left, if English traders were expelled, would English ideas [as well as all developments] go too…?

“The fact is that in a century and a half [of British domination], India had undergone a process of Europeanization…Yet Britain was far more Romanized [after almost 400 years of Roman rule] than India is Europeanized. In race and traditions, in manners and customs,…an Indian, even of the most Europeanized, is still Indian; Indian civilization and British civilization are two different things, and they have not been fused into a single-complex whole. But in Roman Britain there was no visible distinction between Roman civilization and British civilization. The Romans in Britain were practically all Britons; Britons by pedigree, Roman in

20 Lloyd Robert Laing, Jennifer Laing, Anglo-Saxon England (London 1979), pp. 41-42 (capitalization added)
civilization; and there was no British civilization except that local development of Roman civilization which we have called Romano-British.”

When the Romans left Britain the remaining population was Romanized. Rome did not fall when this exodus occurred and trade with the continent continued. Therefore the Saxons lived for a period alongside the Roman-acculturated British and had goods available of Roman manufacture, especially pottery and necessary household utensils. This elegant ware they placed in their graves and utilized it for as long as it was manufactured and traded from abroad. This trade went so far that Saxon stylistic inferences began to creep into Romano-British pottery as reported by Laing and Laing:

“That Romano-Britons developed a Saxonized taste is also reflected by late Roman pottery. Roman factories started to produce pots with definite Germanic [Saxon] decorations in the fourth century. These pots have been named Romano-Saxon, and indicate a degree of cultural exchange between the two peoples in the later fourth and early fifth century. They are matched by what have been rather confusingly called Saxo-Roman pots, which are Roman in style and decoration but made in the coarse wares favoured by the Germanic [Saxon] barbarians. These hybrid pottery styles died out in favour of true Anglo-Saxon pottery…in the 430s.”

The date of this changeover is not at all established as the chronology after A.D. 410 is highly speculative as we will show below. It is nevertheless clear that the Saxons did use Roman pottery after the Romans abandoned Britain. Therefore, if the Saxons built Stonehenge in post-Roman times, then certain Roman towns, villas, etc., would have been either occupied or abandoned shortly before this event and we should expect a difference in the depth of floors, and pottery relics of the Romans would in some cases also be buried at approximately three feet depth, as at Stonehenge. This brings us back to Darwin who examined reports of how deeply certain Roman ruins were buried by earthworms:

“Silchester Hampshire: The ruins of this small Roman town have been better preserved than any other remains of the kind in England [which appears to indicate it was one of the last Roman sites abandoned or destroyed] by which many large buildings have been discovered. Mr. [J.G.] Joyce made careful coloured sections, and measured the thickness of each bed of rubbish, whilst the excavations were in progress; and he has had the kindness to send me copies of several of them…”

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22 R.G. Collingwood, Roman Britain, new revised ed. (Oxford UK 1953), pp. 143-144
23 Laing and Laing, op.cit., p. 35
“Mr. Joyce estimates that the town was inhabited by the Romans for about three centuries, and no doubt much matter must have accumulated within the walls during this long period. It appears to have been destroyed by fire, and most of the stones used in the buildings have since been carried off. These circumstances were for ascertaining the part which worms played in the burial of the ruins, but as careful sections of the rubbish overlaying an ancient town have seldom or never before been made in England, I will give copies of the most characteristic portions of some of these made by Mr. Joyce.

“An east-west section 30 f[feet] in length was made across a room in the Basilica now called the Hall of Merchants…The hard [cracked] concrete floor, still covered here and there with tesserae [tiles] was found 3 feet beneath the surface of the field.”

The depth of the earthworm vegetable mould castings was about as deep as that of the sarsen and bluestone chips and Roman pottery sherds at Stonehenge, which indicates that they are dated not very differently from one another. At another Roman site, Abinger, Surrey Darwin has shown:

“Late in the autumn of 1876, the ground in an old farm-yard at this place was dug to a depth of 2 to 2½ feet, and the workmen found various ancient remains. …On a trench being dug, a layer of concrete, still partly covered with tesserae (small red tiles), and surrounded on two sides by broken-down walls, was soon discovered… Many fragments of pottery, other objects, and coins of several Roman emperors, dating from 133 to 361, and perhaps to 375 A.D. were likewise found….From the different dates of the Roman coins we may infer that the building was long inhabited. It was probably ruined and deserted 1400 or 1500 years ago….

“The fine [earthworm] mould over the floor…varied in thickness from 11 to 16 inches; and on the sides of the trench was a little over 13 inches…

“[Elsewhere on the site] the mould varied from 9 to 14 inches in thickness.”

At Silchester, Hampshire the depth of the worm casting over the concrete floor was three feet while at Abinger, Surrey it ranged from between 11 to 16 inches in thickness which indicates certain Roman sites survived well into the Saxon period. Finally, we read in the Latter-Day Saints’ *Millennial Star* that: “The ancient capital of Britain, we are told, was Verulanium…The worms must have been busy for over a thousand years. The ruins are buried SEVERAL FEET beneath the sward.

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In A.D. 54 it became the capital. Then only a few years afterwards, came a terrible revenge, the city was sacked and burned by Boudicca”\textsuperscript{26}. This town was destroyed about 350 years before the Romans abandoned Britain and is buried because of this even more deeply than the Roman ruins described by Darwin. Again we return to the problem of how it is possible that the various megalithic stone monuments of relatively small size were also not completely or almost completely buried by worm action. This again we reiterate is a direct contradiction to the established chronology and demands an answer, which we dare say will never come from the authorities because they simply have no “evidence” to answer it.

Naturally it can be argued that if the short chronology is correct we would expect to find somewhere a Roman coin, tile, brick or pottery sherd directly beneath one of the megalithic stones. This would conclusively prove that the megaliths were erected in post-Roman times.

This then brings us back to the acrobatics that Roman pottery, tiles, bricks, and coins can do when the archaeologists call upon them to resolve their chronological problems. In \textit{Littell’s Living Age} we get the first indication that this may very well be the case at Stonehenge:

\begin{quote}
“Roman coins have been frequently found by those who have been digging in and about Stonehenge. These were never in such sites as would render it certain that they had existed before the monument itself [because they were not found beneath one of its standing stones]; but in 1797, when Mr. Cunnington was exploring the holes formed by the fall [at that time] of one of the great trilithons, he found fragments of black Roman pottery in the bottom of the pit…and consequently under the base of the great stone…[To avoid the implication of this finding h]e suggests that they may have fallen into the pit afterwards [with debris along the sides of the pit], and as this is possible, though not very probable, it will not do to rely too much upon the circumstances, but like the coins and other fragments of Roman pottery discovered about the place, it must be considered as strong presumptive evidence that the building was erected after Roman times…”\textsuperscript{27}
\end{quote}

The explanation that the two pottery fragments found by Cunnington fell from the sides of the pit with soil has generally been accepted by all the authorities, although it cannot be proved one way or the other. However, in Brittany we have

\textsuperscript{26} Latter-Day Saints’ \textit{Millennial Star}, vol. 62 (Manchester, 1840-1970), page 54 (capitalization added)

\textsuperscript{27} Eleakim Littell and Robert Littell, \textit{Littell’s Living Age}, vol. 99 (Boston MA 1860), p. 503
the condition wherein a megalithic stone was removed and the base of the pit that was immediately examined showed a Roman tile directly beneath it pressed into the soil. This was fully shown to be the case by Charles Cox who was at the site when the stone was lifted:

“In October 1879, I was specifically visiting and minutely examining that greatest and most famous megalithic monument, Carnac in Brittany. By good fortune at the time of my visit the authorities of the department were moving back one of the finest stones, that measured nearly 12 feet from the ground, in order to widen the public highway. The base was buried some 6 or 7 feet in the ground. I was the first to descend into the hole from whence it was taken. In the closely [compressed] pressed ground below the base was a wedge of Roman tile! Is not this tile irrefutable evidence that Carnac stones are historical [i.e., built after the Roman epoch]?”

This Roman tile was not found in soil or debris that had fallen into the pit from the sides but was found in the soil, compressed by the immense weight of the menhir above. The only way to have that tile set into the compressed soil directly beneath that megalith is that it had to be underneath it before the stone was placed in the earth. The megaliths of Brittany were supposedly erected over a thousand years before the Romans came to Brittany. Thus this finding is a direct physical contradiction to the established chronology.

According to T.D. Kendrick, who cites the *Bull. Soc. Arch. de Nantes*, T 39 (1898) p. 46, there is even stronger evidence for Roman materials to not only lay beneath a menhir but act as filler stone to steady it while it was being buried:

“Brittany supplies also an example of the erection of a megalithic monument at a late date…This is La Blanche Pierre near Guérande in the Loire-Inférieure, an upright block of quartz over 6 feet in height…I will not describe the excavation…but the quartz menhir…was resting upon, and steadied by, pieces of Roman brick…”

In this case, some Roman bricks miraculously worked their way down and then sideward beneath the soil to sit directly in contact with the bottom of the menhir to give the archaeologists the false impression that they were actually put beneath that megalith as a base to stand on. At the same time other Roman bricks worked their

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way down in such a way that they rested against the sides of the menhir to give the archaeologists the false impression that they were actually placed there to steady the monument while it was being buried. On top of all this, James Miln reports:

“A few years ago M. the Abbé du Marc’hallach, Vicar-general of the Bishop of Quimper executed some researches at the foot of a large menhir, forming part of an alignment of menhirs situated a few miles from the city in the parish of Plomelin. Underneath this enormous stone he discovered a Roman coin (middle brass) and several fragments of Roman roofing-tiles ( tegulae ).”

We suspect this evidence will also be ignored by the authorities or, as a last resort, Cox and these others will be attacked as unreliable investigators or, worse, that they misrepresented the evidence. One wonders, at what point does such clear-cut evidence of Roman material stratigraphically located in compressed soil directly beneath the base of a megalith reach these authorities? To reiterate, these Roman tiles, bricks, and coins could only have gotten into the compressed soil directly beneath these huge stones if they were there before the stones were placed over them. They were not found in loose or unconsolidated soil that fell into the pit from the sides but in compacted earth created by the weight of that megalith.

What we have is meticulously documented evidence that the megaliths were built in post-Roman times. But the critics, unable to explain it away, will simply not acknowledge it and thus deny it exists, or attack the author/researcher to undermine it. Such an approach, we suggest, is simply unscholarly. While Cunnington’s evidence is discussed broadly in the literature, these other evidences are seemingly suppressed. All in all, this evidence surely supports the short chronology and contradicts the established one.

In every case of finding Roman materials directly beneath these stone monuments, the proponents of the established chronology have either ignored (i.e., denied) them, invented highly improbable scenarios to explain them away, or may even have cast doubt on the reliability of those who reported these finds as well as their reports. In every case of finding Roman materials directly beneath these stone monuments the evidence indicates that the monuments were built after the Romans inhabited the area. This stratigraphical evidence, like that of Darwin’s earthworms, further buries the established chronology with contradictions.

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30 James Miln, *Excavations at Carnac (Brittany): A Record of archaeological researches…* (Edinburgh UK 1877), p. 119
STRATIGRAPHY AND THE Y AND Z HOLES

The Y and Z Holes at Stonehenge also contain stone chips and Roman pottery just as those found at the three foot (0.9 meter) depth all around the monument. The problem is that these holes were dug far earlier than the Roman occupation of Britain, at least 1500-2000 years earlier. Horace Judson reports “pieces of pottery found in the Y and Z Holes date from the Celtic Iron Age, probably from the second century B.C.”

In *Antiquity* for 1929, page 80, we are told that in “Hole Y II...104 pieces of Early Iron Age pottery were found.” Brian Houghton claims the Y and Z Holes contain pottery dating from “c. 800 B.C.-A.D. 43” and hold “20 Roman coins.”

Atkinson further points out a significant aspect of these materials:

“...in the middle and higher levels [20-10 inches, 0.5-0.25 meters] of the Y and Z Holes their [stone chip] number gradually increases. Now the vertical distribution of these [sarsen and bluestone] chips is very closely and strikingly matched by the vertical distribution of fragments of Roman pottery in the same holes. And since both [stone] chips and potsherds are of the same order of size, and will therefore have been buried by earthworms at the same rate, it follows that they must have been dropped on the surface at the same time.”

Here then is another stratigraphical contradiction. Both potsherds dating supposedly from 800-200 B.C. or A.D. 43 are found in the Y and Z Holes with Roman pottery and stone chips dating to at least 100 years later and possibly 200 years later. How could potsherds dating from 800 B.C., left on the surface for earthworms to bury, be found at the same level as Roman potsherds about 1000 years later? This is not so much an “enigma” that the authorities and researchers relate to, but a total contradiction to their chronology. Here is the problem: Someone around 800 B.C. drops an iron age pot that had broken, and places the sherds on Y or Z Holes so that they were buried by earthworm castings; some 900 to 1000 or more years later a Roman drops a Roman pot that breaks and leaves the sherds on that same Y or Z hole, again to be buried by earthworms. Yet by some miraculous processes the Iron Age potsherds after sinking down to a certain level in these holes, stop sinking; the earthworms can continue to bury the Roman potsherds so that they just end up at the same level as the Iron Age ones. This of course is scientifically improbable, not to say impossible, to have occurred in so...
many of these Y and Z Holes. It should not have happened in even one hole but has occurred in numerous ones.

The science of earthworm action cannot be negated. The earthworms would have been burying both sets of materials and they should have ended up at different levels, with the Iron Age one well beneath the Roman. This evidence again precludes the viability of the established chronology. In terms of the short chronology, since much of the Iron Age is of the same time as the Stone Age and Bronze Age in post-Roman Britain, then we would also expect the different forms of pottery contemporary with one another to exist at the same stratigraphical level. Philip Barker has attempted to explain this problem away by invoking vertical displacement:

“The classic case of the probability of an error [in chronology] is afforded by the Y and Z Holes at Stonehenge, in which the main part of the silt consists of earth, apparently [assumed to be] wind-blown and almost free from stones and rubble. This silt contained [at the same] Iron Age and Romano-British sherds at quite low depths, which have in the past been held to date the holes...But once the possibility of vertical displacement is recognized, it becomes clear that this pottery must have been deposited on the surface of the silting many centuries after the digging of the holes...”

Notice that Barker has not explained why pottery sherds from around 800 B.C. somehow could be deposited with those of the Roman period in the same Y and Z Holes. For vertical displacement to take place, since the two forms of pottery were left on the surface at far different times, somehow this vertical displacement allowed only the Roman sherds to again miraculously be vertically displaced so that they ended up just at the same level as those left about 1000 years earlier. Vertical displacement, in this case, selected only Roman potsherds to be the ones that move more rapidly through the same soil as the Iron Age ones or somehow slow down the burial of the earlier ones.

In this case the Roman pottery, and only the Roman pottery as a whole, was vertically displaced, or the stone chips and earlier pottery, and only these stone chips and earlier pottery forms, were displaced all at the same rate to end up in the same stratigraphical level. In other words, the vertical displacement in every case selectively moved only one or the other group of fragments. But why this should

be so, one cannot fathom, except that this explanation is correct because the established chronology requires it.

A second enigma/contradiction to the established chronology is that there were found pieces of bluestone chips at the very bottom of one of the holes. Atkinson reports the Y and Z Holes contain “a fragment or two of Bluestone chips (almost always of rhyolite) on the very bottom.”  

The Y and Z Holes are supposedly prehistoric and it is assumed by the authorities that the stone chips were hacked off the bluestones in Roman times since they are at the same level in the earth—three feet in depth—around Stonehenge. Again, we encounter an impossible chronological problem. To have these blue stone chips lying directly at the bottom of a Y or Z Hole requires/demands that the monument was mutilated at the very time the Y and Z Holes were dug. That could never have occurred in Roman times since by that time the Y and Z Holes would have been filled with soil. In this case worm action or vertical displacement must somehow make these bluestone chips sink more rapidly than the Iron and Roman Age potsherds so that they get to the very bottom of the hole while the potsherds only sink at a much more moderate rate. About these “enigmas,” which we rightly define as contradictions to scientific processes, Anthony Johnson writes:

“Anyone writing about Stonehenge is destined to employ the word, ‘enigmatic.’ If I allowed it only once, I would reserve it for the Y and Z Holes… Examples of almost every material, both natural and artificial, present elsewhere at Stonehenge, have been found in them…(including pottery of the Iron Age, Romano-British and Medieval periods, coins … and even human remains.”

Again in terms of the established chronology none of this makes the slightest scientific sense, and that is why this evidence is so baffling and called an enigma. But since we place this monument in post-Roman times and all the prehistoric ages found there as well, there is no “enigma” or “contradiction.” The evidence fully corroborates the short chronology. All these materials are found in these holes because they were laid there around the same time—post-Roman times.

The final piece of stratigraphical evidence that contradicts the established chronology is also related to earthworm action. As the earthworms would have buried completely or almost completely all the moderately sized megaliths over about 5000

35 Atkinson, *Stonehenge, op.cit.*, p. 75
36 Johnson, *Solving Stonehenge, op.cit.*, p. 167
to 3000 years, their action would have completely filled the Y and Z Holes long before the Roman period. These holes are not very deep, as described by Castleden:

"The Y and Z Holes are hard to date. They were dug 1 m [40 inches] across, 1.8 m [72 inches] long and 0.9 m [36 inches] deep….They were never backfilled. They were left open to the sky and filled up slowly with natural debris that people trod and the wind blew around the site." 37

In this case only wind-blown material and natural debris filled in these holes. But as pointed out above, forest grew over this area after Stonehenge was abandoned supposedly from 1500-1200 B.C., but this forest never impinged on the Y and Z Holes to fill them with material. Why? This we are never told, nor, I doubt, ever will.

The holes being about three feet deep in an area with earthworms would be filled by their vegetable mould castings just as the Romano-British pottery and sarsen and bluestone chips three feet deep. Following the established chronology, those sherds and chips were buried to a depth of three feet over about 1500 to 1600 years. Nevertheless, earthworms would have also emerged in the Y and Z holes at night to deposit their castings, just as they did on the level surface. Since Stonehenge was abandoned supposedly 1500 to 1200 B.C., that leaves anywhere from 1600 to 1300 years for the earthworms to fill in these holes. Remember, Cunnington found the Romano-British pottery and stone chips at about three feet in depth in the year A.D. 1797 or some 320 years ago. That means if we date the partial destruction of Stonehenge by the Romans to around A.D. 200, these materials were buried three feet in about 1600 years. Using the same rate of burial for the Y and Z Holes, they should have been completely or almost completely filled well before the Romans came to Britain. Any potsherds or stone chips left on their surfaces should also have been buried at the very bottoms of the Y and Z Holes over the ensuing 1600 to 1800 years. The science of earthworm action is a hard scientific fact that contradicts the established chronology over and over again.

Having shown that earthworms would have completely, or almost completely, buried the moderately-sized megaliths over 4000 to 5000 years if they were that old; having shown that earthworms would never have buried Iron Age and Romano-British potsherds at the same median levels in the Y and Z Holes, if the established chronology is correct; having shown that earthworms would have

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37 Castleden, *The Making of Stonehenge*, *op.cit.*, p. 74
buried all these potsherds at the bottom of the Y and Z Holes over about 1600 years if the established chronology is correct; having shown the Y and Z Holes would have been completely or almost completely filled by earthworm action before Roman times, we see no rational scientific explanation that will allow archaeologists and proponents of the established chronology to escape these contradictions. The stratigraphical scientific evidence makes no sense whatsoever for the established chronology. It makes excellent sense for the short chronology.
CHAPTER 9

STONEHENGE’S ONE ALIGNMENT, CALENDARS AND CHRONOLOGY

Thus far we have shown what we consider strong scientific technological and other evidence which indicates that the Megalithic Age should be placed in post-Roman times. This being the case, we might expect to find that the astro-archaeological evidence will do the same, correlating with this evidence and corroborating our chronology. Furthermore, as shown above, the astronomical alignments cannot be proof of the validity of the lengthy chronology for the Megalithic Age. Even today certain alignments, such as that at Newgrange and Maes Howe still allow the Sun to shine into the corridors on the solstices. Also, we have shown that by moving one’s observing position only slightly, these other alignments supposedly dated to prehistoric times will also fit that of the short chronology. What we wish to demonstrate now is that the astronomical midsummer solstice alignment at Stonehenge clearly fits in post-Roman/Saxon times, and, if our demonstration is successful, that evidence will not only corroborate, and correlate with, the short chronology but will once again be a clear contradiction to the established one. The nature of the astro-archaeological situation has been summarized by Stephen C. McCluskey:

“Although Stonehenge is the most famous megalithic structure at which… astronomical alignments have been noted, it is not the only one. But the question of whether these orientations were deliberate or merely fortuitous called forth careful consideration of the significance of various forms of evidence. Stonehenge and other megalithic structures share the ambiguity of most archaeological remains; the possible interpretations that we can assign to them are limited only by our imagination….

“Critical examinations of this evidence and new, more systematic surveys of carefully defined groups of sites have led to rejection of the more extreme of these claims. The evidence for highly precise ‘scientific’ observations is generally dismissed as the product of various kind of unconscious selection of data. What remains from these more critical analyses is evidence for low-precision calendric
observations of sunrise and sunset to mark the divisions of the year into eight equal parts and evidence for similar low-precision observations of the rising and setting of the Moon, perhaps associated with religious rituals.…

“A different pattern emerged from Burl’s examination of a cluster of large stone circles in Wales, southwest Scotland, and Ireland. Burl found little evidence for lunar alignments but a greater than expected number of alignments marking sunrise on the dates dividing the year into eight equal parts [viz. the midsummer and midwinter solstices, the autumnal and vernal equinoxes and the midpoints between them, the quarter year days]…. 

“In summary, these studies of megalithic alignments indicate that the prehistoric peoples of Britain had a tradition of observing the Sun and the Moon. They kept a solar calendar dividing the year into eight equal parts, and performed the kind of lunar observations that could lead logically, but not necessarily, to attempts to reconcile the motions of the Sun and the Moon [year periods into a coherent relationship with one another]…. 

“As we will see, certain elements of the solar calendar established by ‘megalithic’ observers of the heavens [somehow] survived into the Middle Ages, despite the cultural changes brought about [during about 3500 years separating these cultures] by successive incursions of Celts, Romans, and Anglo-Saxons. To the extent that we find the same [eight part] solar calendar playing similar social roles [in both the Late Neolithic and Middle Ages] in changing cultural contexts, we can begin to understand how survivals of prehistoric astronomical traditions influenced early medieval knowledge of the heavens.”

What is being suggested here is that the astronomical knowledge attained by Late Neolithic people, who divided the year into eight equal parts and possibly “reconciled the motions of the Sun and Moon” into a cohesive whole, was somehow preserved until the Middle Ages without written records. This knowledge survived after the megalithic people vanished around 1500-1200 B.C. and Europe was invaded by various peoples such as the “Celts, Romans, and Anglo-Saxons.” The Romans clearly had no such calendar and dominated most of northwestern Europe, imposing their own calendar on its peoples, yet somehow this eight part calendrical tradition was a fundamental element of the Celtic peoples and most probably the Saxons who lived at the same period and brought that tradition to Britain. All this we will outline below.

1 Stephen C. McCluskey, *op.cit.*, pp. 12-14
However, before doing so, the basic question to be resolved in terms of chronology is: is there a clear-cut astronomical alignment at Stonehenge that is agreed upon by the astro-archaeological community that can be tested to determine the time this monument was constructed? This alignment must be so clearly set up that it precludes any question as to its reality. This can only be shown to be the case if the builders of Stonehenge set up a series of stones that acted exactly like the sights of a rifle, and that this sight line alignment points undeniably to the summer solstice sunrise quite accurately, though not precisely, because these people did not have the observation tools to make precise alignments. This alignment must furthermore be dated to a span of time encompassing 300 or so years such that it fits into the post-Roman/Saxon period but absolutely not into the Late Neolithic or Early Bronze Ages.

Such an accurate alignment to the summer solstice sunrise as seen from the center of Stonehenge does exist and has been recognized since 1979 or for over 30 years. However, because the astro-archaeologists and other investigators are so deeply imbued with the established chronology, although well aware of this alignment, they have failed to determine into what time frame it actually belongs, other than the one that dominates their thinking. Instead, they have allowed this chronological fixation to deny its existence, and, rather than allow that alignment to speak for itself and demonstrate the period to which it actually belongs, they ignore it. Rather than following the scientific method of letting that datum determine the chronology—the correct chronology—they have allowed their chronology—their theory/paradigm—to determine where and when that alignment had to have taken place. They have allowed the theory to determine what the chronological and astronomical facts are rather than to let the chronological and astronomical facts determine the scientific reality that flows from those facts.

This summer solstice alignment at Stonehenge as observed from the center of the monument is down a long corridor facing northeast called the Avenue. Looking from the center between two stones in the sarsen circle, numbers 30 and 11, we then come to another two stones on the edge of the ditch, known as the Slaughter Stone and its missing partner stone to the left of it, which was standing in the chalk leaving an indentation there known as Stonehole E. Then still farther down the Avenue we come to another two stones through which the summer solstice sunrise was aligned, known as the Heel Stone and its missing partner to the left of it set in another indentation and called Stonehole 97. From these two stones there is a clear line to the horizon. The observer standing at the center of Stonehenge would have three sets of stones between which he could align himself to the horizon to observe
the first glint of the Sun as it rose over that horizon directly between all three sights on the summer solstice. The Heel Stone and its missing companion stone, being the most distant from the center of the monument in this alignment, are clearly important since they give the most accurate aim to the horizon. They are like the sights at the end of a rifle barrel. Without them the alignment could vary so broadly as to be of almost no value. Rosemary Hill discusses the discovery of the Heel Stone’s companion and the significance of this discovery:

“[1979] saw another of those coincidences of insight...that characterizes the [20th] century. An official of the General Post Office drew a line on a map for [laying] a new telephone cable which happened to run across Stonehenge. Not until the...bright yellow digger [had dug to] within yards of the Heel Stone, and then entirely by chance was it spotted. Mike Pitts, an archaeologist from the Avebury Museum, mounted a ‘rescue dig’ on the site and found evidence [of a hole in the chalk beneath the soil into which a stone to the left of the Heel Stone had been set] that there had once been a pair to the Heel Stone. This was a substantial discovery. It removed one of the principal objections to the idea of the solstice orientation, the fact that the sun does not rise directly over the Heel Stone. This, it now seemed, might be deliberate. The sun was meant to be seen rising between a pair of stones [as seen from the center of Stonehenge]. If, indeed, a later archaeological report conceded the purpose of Stonehenge had been to celebrate the midsummer sunrise, ‘the positioning of the two stones here would have achieved that aim.’ It was a substantial point to astro-archaeology.”

In this case we have what appears to be direct evidence of this midsummer solstice alignment, as Burl explains:

“Proof of an alignment demands the presence not only of a celestial target but also both an artificial back sight where an observer stood and a foresight towards which he looked. In stone circles it is assumed that the back sight was on the axis and that the foresight was man-made and remarkable such as a tall stone [or pair of stones between which one can align with the celestial body]...The existence of a convenient [natural distant] notch [in a hill] unindicated by any of these features is inadequate...‘Whereas it is logical to suppose that a row of stones or a pair of archways were designed to point to some celestial object...it is unjustifiable at the present time to assume that [distant foresight] natural objects were so used...’

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2 Rosemary Hill, *op.cit.*, pp. 175-176
3 Burl, *The Stone Circles of Britain, Ireland, and Brittany, op.cit.*, p. 59
All that Burl requires as “proof of an alignment” exists at Stonehenge. We have an axis—though not a precise one—running through the center of Stonehenge; we have the sarsen arch created by the two Sarsen Stones and its lintel; we have the two foresights of paired stones—the Slaughter Stone and its companion and the two Heel Stones; and all these point like the sights of a rifle to the point on the horizon where the Sun rises at the midsummer solstice. Burl fully acknowledges this set of foresights:

“The Heel Stone and its lost partner, Stone 97, inside the avenue…[t]ogether with the Slaughter Stone and its missing companion at the north-east entrance [to Stonehenge] once formed a narrow 9 foot [2.8 meter] wide, 100 ft long (30 m) rectangle through which the midsummer sun shone at its rising [in the ancient past.]”

Castleden echoes this:

“[I]t is now clear that the Heel Stone had a partner and that together they formed a ceremonial portal, straddling the main axis of the monument. It is also likely that, at this time, the Slaughter Stone and its missing partner made an inner pair of portal stones at the henge entrance, which would have made solar observations more accurate. The two pairs of stones defined and emphasized Stonehenge’s principal and least controversial alignment, on the newly risen sun disc on the summer solstice in 3000 B.C.…”

Ruggles comments:

“Burl argues that the entrance stones [the Slaughter Stone and its companion] were deliberately placed in line with the Heel Stone and its companion so as to produce a ‘corridor’ down which the light of the rising midsummer sun would have shown directly into the interior of the monument.”

Elsewhere Ruggles concludes:

“The axial alignment upon midsummer sunrise and midwinter sunset seems to have formed a vital part of the symbolism of the phases of the monument. This conclusion is strengthened by the discovery in 1979 of a probable companion stone to

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4 Burl, From Carnac to Callanish, op.cit., p. 50
5 Rodney Castleden, Neolithic Britain, op.cit., pp. 231-232
6 Ruggles, Astronomy in Prehistoric Britain and Ireland, op.cit., p. 138; see also Aubrey Burl, A Guide to the Stone Circles of Britain, Ireland, and Brittany (New Haven CT 1995), pp. 89-90
Charles Ginenthal, *Pillars of the Past*, vol. IV 313

the Heel Stone, the two stones forming a part of a [narrow] ‘corridor’ down which the sun would have shone into the interior of the sarsen circle around midsummer.”

There are several other researchers, investigators, and writers who express the very same conclusion regarding this alignment at Stonehenge. This finding brought to fruition the long hoped-for alignment that would ultimately date Stonehenge to a fairly narrow period that Stukeley first envisaged, as Chippindale explains: “The midsummer orientation of Stonehenge was first noticed about 1720 by William Stukeley, who spotted that it was approximate and not an exact line to the sunrise at all; he thought of Stonehenge as a solar temple, roughly facing the Sun…two-and-a-half centuries on, that is still the best opinion…”

The question of chronology now rears its head. If, as the proponents of the established chronology claim, the monument was built between 3000 and 2500 B.C., then surely by retrocalculation—even with problems in the slowing of the Earth’s rotation—the Sun should still rise between the Heel Stone and its partner around that time. But the fact of the matter, that is acknowledged by these same proponents, is that it categorically did not rise between these foresights at this time, nor even at the 1500-1200 B.C. date when Stonehenge was abandoned. Here is how Castleden has attempted to deal with this unambiguous contradiction to the established chronology:

“As far as Stonehenge is concerned, the general orientation of the entrance [Avenue] toward the midsummer sunrise is suggestive. The more exact orientation implied by the inner portal stones [the Slaughter Stone and its companion] and the other portal stones [the Heel Stone and its companion] is even more compelling. But the first flash of the midsummer sunrise in 3000 BC was not only further north than it is today, it was actually almost two solar diameters further north than the two sets of portal stones. Those who try to advance the view that the Heel Stone itself pinpointed the exact dates of the solstice itself are [also] in grave difficulty, because the principal sightline was not over the top of the Heel Stone but just to the north between the Heel Stone and its missing partner. But even allowing for

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7 Clive L.N. Ruggles, *The Observatory*, vol. 116 (October 1996), pp. 279-280
this, it is clear that the path between the two pairs of portal stones was never intended to align on the first flash of the midsummer sunrise, nor even the half disc.

“The sight-line could have been used to fix the solstice indirectly, because the first flash of sunrise between the Heel Stones in 3000 B.C. occurred exactly thirteen days before and again 13 days after the solstice...The alignment’s primary purpose was to celebrate and salute the full disc of the untrammeled sun just as it floated free of the horizon on the summer solstice—in 3000 B.C.”

In other words, the summer solstice alignment was off by 26 days in 3000 B.C. and because of this the people who built Stonehenge constructed it with this massive error built into it. This is the one ad hoc theory that is offered to deal with the falsification of the established chronology. Another ad hoc hypothesis is offered by Burl who suggests “that the empty socket on the opposite side of the original entrance contained not the twin of the Heel Stone but the Heel Stone itself which was moved to indicate more accurately the rising of the Moon, not the Sun.” Again, the established chronology offers this sort of ad hoc hypothesis and nothing else!

However, if it was found that the Sun had risen between the two sets of portal stones at the very time of the summer solstice around 3000 B.C., this would be heralded as unimpeachable evidence that Stonehenge had to have been built at that time.

To obviate the disconnection between what was found and what should have been found, Castleden offers: “what was happening was not an astronomical observation but a ceremonial greeting of the midsummer sun, a celestial encounter of the highest religious and emotion content.” That is, he disavowed his earlier contention above that “the real sunrise being taken...when the first limb of the Sun climbs above the horizon, and when the first rays of the Sun are seen” to fit the established chronology.

Against the accepted view that the Sun of the summer solstice should be observed rising between the two sets of portal stones stands this lethal problem for the established chronology. To begin with, it is not backed up by even one other piece of scientific nor technological evidence to suggest that this ad hoc interpretation is correct. Hadingham, on the basis of this utter alignment failure was driven to state:

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10 Castleden, The Stonehenge People, op.cit., pp. 129-130
11 A. Burl in Hutton, The Pagan Religions of the Ancient British Isles, op.cit., p. 115
12 Ibid., p. 130
13 Ibid., p. 149
“But could the entire idea of the midsummer rituals and the sunrise alignment be a mistake, a romantic delusion? The Heel Stone, it seems, was positioned appreciably ‘off target.’ Today the sun does not rise directly in its path but slightly to the left, or north, of it [on the summer solstice]. Moreover, because of an extremely slow shift in the earth’s axis over the centuries, the sun should have risen even farther to the left back in prehistoric times. A few minutes after the first flash of light, the sun would indeed have appeared right over the top of the Heel Stone, but by that time it would also have cleared the horizon [which is at the same level as the top of the Heel Stone], but a distance nearly equal to its own diameter. This apparent failure of the Stonehenge builders to mark the precise sunrise point is crucial to an understanding of the monument. It demands an explanation.”

No matter which way the proponents of the established chronology turn to manipulate this evidence, they simply cannot make this solstitial alignment fit their chronology. The interesting point Hadingham makes is that the builders of Stonehenge set up huge stones to mark this alignment in an utterly incorrect way, which “demands an explanation.” Why would they do such a thing—such an unreasonable and irresponsible thing? The astro-archaeologists have simply failed to grasp the implication of this failure for their chronological placement of Stonehenge and therefore of the entire Megalithic Age. Anthony Aveni’s comments are absolutely telling in this respect:

“Having studied it [Stonehenge] close up there is little doubt in my mind that Stonehenge was intended to be a sun temple from its very inception. It seems to have been arranged deliberately so that a viewer who stands in the center of the great stone ring could capture the rising sun’s disc in the Heel Stone gateway to the northeast [of it] on June 21…Stonehenge was surely built to celebrate his entry into the circular sanctuary at a very special time of year.”

This alignment was deliberate and the builders of Stonehenge did not construct the monument incorrectly. Littman, Espenak and Wilcox, state: “In that ancient time, the Sun at the beginning of the summer [June 21] probably rose between the famed Heel Stone and its now-vanished companion, and the alignment with sunrise at the summer solstice was probably exact.”

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14 Hadingham, *Early Man and the Cosmos*, op.cit., p. 43
16 Littman, Espenak, Wilcox, *op.cit.*, pp. 30-31
Hutton sums it up thus: “Most authorities agree that [the Heel Stone and its partner] formed one of a pair which framed the entrance and between which the midsummer sun was framed just as it cleared the horizon.”

We do not know, as yet, when Stonehenge was constructed because we cannot calculate from that date forward in time. We must work backward in time from the present where we can determine the precise point at which the Sun rises on the summer solstice. However, we must reiterate that we cannot determine this date precisely, even given this clear alignment, as Ruggles has explained earlier:

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17 Hutton, *The Pagan Religions of the Ancient British Isles*, op. cit., p. 115
“...the observing position to be used from one day to the next would need to be specific to within a mere one or two centimetres, less than the distance from one eye to the other. While it has been suggested [by Castleden above] that the Heel Stone might have served to determine the solstice exactly by a process of halving the difference, even this would have required an observing position precise to 10 cm [4 inches] or so.”\textsuperscript{18}

Elsewhere Ruggles and Michael Hoskin show “A precision equivalent to, at best, two or three solar diameters is involved: the popular notion that the Heel Stone defined the direction of solsticial sunrise more precisely is quite unsupportable, because the supposed observing position (the centre of the monument) cannot be defined precisely enough, while the Heel Stone is too near to provide an accurate foresight and the horizon behind it is featureless.”\textsuperscript{19}

Nevertheless, the corridor sightline between the Heel Stone and its companion will allow us to get a fairly accurate general alignment date for the time Stonehenge was used to celebrate the summer solstice. The point at which the solsticial summer sunrise occurs today can therefore be retrocalculated to that time. According to The Wiltshire Archaeological and Natural History Magazine vol. 84-86 (1991), p. 5, the Sun “rises about a foot and a half to the left [of the Heel Stone].” Fernand Niel writes identically of the Sun on this solsticial date: “It rises about a foot and a half to the left [of the Heel Stone].”\textsuperscript{20}

The distance between the Heel Stone and its companion is nine feet while the Sun on the solstice today rises just 1½ feet to the left of the Heel Stone. The midpoint between these two distant portal stones is 4½ feet, therefore in the past to have the Sun rise at that midpoint it must shift to the left by about an additional three feet. Therefore we must also determine the length of time back into the past for the Sun to shift those three feet. John Edwin Wood explains that the Sun “now rises a fraction of a degree east of its position in prehistoric times. In fact the difference...between A.D. 1900 and 2000 B.C. is about 0.85°. The rate of change is only about 0.02 ° per century.”\textsuperscript{21} Lancaster Brown also shows the “midsummer sunrise...is at present shifting eastward along the horizon approximately 1° per

\textsuperscript{18} Ruggles, \textit{Astronomy in Prehistoric Britain and Ireland}, op.cit., p. 38
\textsuperscript{19} Ruggles, Hoskin, “Astronomy Before History,” \textit{op.cit.}, p. 6
\textsuperscript{20} Fernand Niel, \textit{The Mysteries of Stonehenge} (NY 1975), p. 63
4300 years.”  

In about 1500 years from the present, going back to the Roman exodus of Britain in about A.D. 410, the Sun would have shifted about 0.372 degrees or about 75% of one solar diameter.

This may not appear to be a large enough shift to cover those additional three feet to the left and allow the Sun to rise between the two distant Heel Stones. But Lancaster Brown has told us that in 4300 years the Sun will shift its position on the horizon by one degree. In 5000 years that amounts to about one and an eighth degrees. The diameter according to Lloyd Motz and Jefferson Hane Weaver shows “The sun’s disk covers about half a degree in the sky.”  

That is, in 5000 years the Sun will shift over four diameters. As Castleden has told us, in 3000 B.C. it was well to the left of the Heel Stone’s companion and would not be seen rising above the horizon. It would be 10 to 12 feet to the left of its present position. But between about A.D. 410 to 750, or about 1600 to 1250 years ago, it would have shifted much less. Thus, while it now rises 1½ feet to the left of the Heel Stone, it would rise about three quarters of a solar diameter, i.e. 75 per cent of a solar diameter, farther to the left. It would still be between the two Heel Stones, far less than nine feet, but more like between four and five feet to the left of the Heel Stone. Since the sunrise occurs presently on the very day of the summer solstice 1½ feet to the left of the Heel Stone and its partner, it would still have risen there on the day of the summer solstice back about 1500 to 1250 years ago. This however needs to be fixed more precisely. This would clearly place the rising Sun quite close to the midpoint between the Heel Stone and its partner, as observed by the naked eye from the central area of Stonehenge on the summer solstice in post-Roman/Saxon times. It would further permit this observation to remain relatively unchanged for centuries because of the very slow change.

In this respect we need to know the azimuths of both the Heel Stone and its companion stone in order to determine the azimuth point directly between them. If we are correct in placing the construction of Stonehenge in post-Roman times, either the first glint of the Sun or the half solar disc or the full solar disk should stand extremely close to that midpoint. According to Burl, “the Heel Stone has an azimuth of 51° 3’”

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while its companion stone had “an estimated azimuth of 48º 21′.”

When we add these azimuths together and then divide by two we find the azimuth midpoint:

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\begin{align*}
51^\circ & \ 3' \\
+ & \ 48^\circ 21' \\
\hline
99^\circ 24' \\
\end{align*}
\]

\[
\begin{align*}
99^\circ 24' \\
\hline
2 \\
\end{align*}
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= \(49^\circ 42'\) mid point azimuth

Professor Lynn E. Rose, employing the *Starry Night* program, at our request kindly determined when the Sun did rise quite close to that azimuth point. According to this program, the first glint of the Sun stood with its upper edge touching the horizon quite close to that azimuth between the years A.D. 500 to 700. This measurement is not precise because the observer at the center of the circle could not stand at the precise center point. Nevertheless, it is close enough to give a general time for when this observation was made and when Stonehenge was built. We have a fit for this observation in post-Roman times and the proponents of the establishment chronology have no such fit for their chronology. Clearly the proponents of the established chronology have failed to do this basic analysis.

In this instance we have clearly beaten the proponents of the established chronology at their own astro-archaeological game, with the very forms of alignment evidence they called upon for support and with the very same rules that they required for that evidence. Although that summer solstice is not precise, and should not be because of the nature of naked-eye observation and the lack of a precise center point at Stonehenge from which to make this observation, it is, nevertheless, the only generally accurate, astronomical alignment that fits. It is not, as Castleden admitted about the established chronological alignment dated to prehistoric times on that plain, “an astronomical observation but a ceremonial greeting of the midsummer-sun.” On the other hand, our astronomical alignment is both a clear-cut “astronomical observation” of the “midsummer-sun” as well as a “ceremonial greeting” of it. In this respect this astronomical observation correlates

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with, is corroborated by, and is congruent with, all the other evidence we have thus
far presented for our chronological placement of Stonehenge and the megalithic
world in post-Roman times. Not only do all the forms of their own evidence fail,
on their own and together, to support their chronology, but now the strongest form
of evidence—the summer solstice alignment at Stonehenge—not only contradicts
that chronology but supports the one we propose.

The question that arises is: why did these proponents of the established
chronology fail to carry out this most basic research which they have maintained is
the “most secure” element in the astronomical arsenal of evidence for Stonehenge?

The apparent reason for failing to carry out this basic calculation is that the
general answer for this analysis pointed unambiguously to a period that
contradicted the established chronology. It was thus ignored, omitted and denied.

Before going forward, what must be pointed out is that at Stonehenge, as at
Newgrange, and other sites, when the Sun entered the corridor between the Heel
Stone and its companion stone, it would do so over a period of several days, not
just on one particular day, and therefore the precise day of the summer solstice
could probably not have been determined. The people would see, for all practical
purposes, the same lower edge of the Sun seeming to stand between these stone
markers for a week or more with its edge rising on the horizon. The only way they
probably could have known the closest day or the exact day of the summer solstice
is if they had an accurate calendar to tell them that. Like the average person today,
who could not use a naked-eye measurement to determine this date, one could
consult a calendar that informed him/her of when the summer solstice occurred,
and therefore such a precise observation was superfluous. The same situation
would apply to the prehistoric past. Unless one had a rather accurate calendar,
finding the precise day of the year for the summer solstice would be most
improbable. This is one of the advantages of placing the construction of
Stonehenge in post-Roman times because at that period there existed just such
calendars among the Romans, Greeks and, as we will show, the people of Gaul,
and the Celtic peoples of this region were in many respects interconnected with the
Saxons, Romans, and Greeks. In Britain, these Celtic people lived alongside their
Roman and the Saxon conquerors, as pointed out by Peter Clemoes:

“The complete disappearance of Celtic culture from the east and south-east of
Britain [but not from the west and south-west where many of these megaliths
stand] undoubtedly points to assimilation or extermination of the Celts in these
areas by the invading Germanic [Saxon] settlers, although some probably retreated to the north and west of the country. Many Celts in the southwest seem to have migrated to Brittany and even Spain [where megalithic monuments are found] probably to evade the onslaughts of the Irish. But there are traces of the survival of Roman culture [in the Saxon period] and both archaeological and literary evidence suggests that Celt and Saxon coexisted in same areas.”

The point to stress is that the Celts and Saxons did live alongside each other on the European Continent as well as in Britain, and that their knowledge of calendrics was surely rather common among them.

Although we have provided alignment evidence that fairly accurately dates Stonehenge and the Megalithic Age, we further suggest that it was not only used for this purpose, but that it and perhaps other circles were employed as calendars. In this instance Burl gives us the first clear explanation as to the intentions of these pagan people in constructing stone circles:

“There is a fact firmly to be established. Had the intention of prehistoric societies been to construct an astronomical observatory they would not have designed a circle, a line of stones or a horseshoe of pillars for computations would have been more efficient. A circle is an enclosure. It would have been an effective fence demarcating the sacred and profane worlds but its plan militates against an exclusive use for solar and lunar observations and the prediction of eclipses. Whatever astronomical purpose it contained was only a part, perhaps minor, of its function.”

That is largely the position we take with respect to Stonehenge and many other megalithic rings. We suggest that these rings, often oriented to the solstices, were also primarily used as calendars. The circle is a symbol of the eternal without beginning or end that runs its course forever; it is a symbol of the endlessness of time. At Stonehenge the only clearly linear element is the Avenue and its corridor running from the center out past the Heel Stone and its partner stone. The circles of Y and Z holes along with the 30 sarsen arches and the five horseshoe arches, along with the 19 bluestones inside the structure, can be employed to construct a calendar of days, months, seasons, and years. One such investigator, D. Lees, concluded that these stone rings could be calendars. His hypothesis was that the 42 and 16-stone

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26 Burl, *The Stone Circles of Britain, Ireland, and Brittany*, op.cit., p. 61
circles of the Sanctuary at Avebury were used as solar-lunar calendars. Thom and Thom present a brief discussion of Lees’ thesis:

“Lees (1984) shows how, by using movable markers, the complex of rings, stones, post holes, etc., at The Sanctuary could have been used as an accurate solar and lunar calendar. At the same time another marker was used on the inner ring to enable the months to be recorded. Owing to the incommensurability of the solar and lunar periods no calendar can be perfect but the Sanctuary calendar was as perfect as it could possibly be. An analysis of the stones showed that the Sanctuary calendar was in some ways superior to the present-day calendar…

“According to Lees, the Sanctuary Calendar can be operated like a circular abacus with 42 stones in the outer circumference. Other rings in Britain may have been used in the same way….

“Dr A. Burl has pointed out that there are some circles on the west coast of England which have 42 stones in their circumference.”

Before proceeding to the evidence we must explain the nature of the pagan Celtic and Saxon calendar. To the farming people of Europe, whether living outside Roman domination or under it, agriculture was their major employment and therefore their calendar revolved around the four seasons. However, these seasons did not correlate with the Roman calendar which divided them by the summer and winter solstices (maybe just June and December) and the vernal and autumnal equinoxes (maybe just March and September). Instead their four seasons were tied more closely to agricultural life and began on what we presently call cross-quarter days—the midpoints between the solstices and equinoxes. Their spring began around or on February 1st when the daylight periods became noticeably longer than the winter daylight periods, instead of our March 21st. Their summer began around or on May 1st when the weather became markedly warmer and the vegetation around them had turned green, instead of our June 21st. Autumn, the season of reaping began on August 1st which ran into late October instead of September 21st. Finally, winter began when the foliage around them had practically all fallen and the daylight period had begun to become noticeably short which they dated to October 31st or to November 1st or 2nd. In essence, their seasons, like our own, were 91 days in length where each of them fell on a cross-quarter day between a solstice and an equinox or an equinox and a solstice.

They also wrestled with the problem of meshing the lunar month year, where 12 lunar months or 29.5 days created a year of 354 or 355 days, with the 365- or 366-day solar year. This could be achieved by adding intercalary months every 2.7 years or in other ways. This meshing of lunar month years and solar years had been achieved by the Greek astronomer Meton who discovered that 19 solar years were almost precisely equal to 235 lunar months, so that after 19 years the lunar month days repeated themselves almost as they had during the previous 19 year cycle.

If, as we maintain, these pagan people had an accurate calendar, then they would have been able to determine the date of the summer solstice not merely from having discovered it or learned from either or both the Greeks and the Romans; they would simply have consulted their calendar to date that event as well as other celebrations. We further maintain that these pagan peoples were not at all advanced astronomers but used their calendar system to organize their year and its holy days.

It is extremely hard to accept that the Neolithic/Bronze Age megalithic calendar somehow survived after the disappearance of the Megalithic Age around 1200 B.C. to become part of early Medieval culture, while in terms of our post-Roman chronology, it is only natural that the astronomical and calendrical elements that existed would become part of early medieval culture. This, then, brings us to the Celtic calendar that existed in central Europe during Roman times into the first to third centuries A.D. that clearly incorporates nearly all these elements. It seems that the Celts of central Europe, like other peoples, were preoccupied with the heavens. Julius Caesar states:

“They hold long discussions about the heavenly bodies, and their movements, the size of the universe and the earth, the physical constitutions of the world, and the power and properties of gods; and they instruct the young men in all these subjects.”

The same interest in astronomy was also claimed by Pomponius Mela ca. A.D. 43. Berresford Ellis maintains that their calendar did not originate from their connections with either the Greeks or the Romans, and dates their calendar to between 200 B.C. and A.D. 200: “That the Celts were highly competent in astronomy is proved by the fact that they originated their own calendrical system The earliest surviving Celtic calendar is from Gaul and is dated to the first century B.C. This is the

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Coligny Calendar, now in the Palais des Arts in Lyons [France]. It is a lunar calendar on engraved bronze plates and was discovered in November 1897.”

Nevertheless, there is evidence of borrowing of this calendar from the Greeks as shown by Helen Benigni, Barbara Carter and Eadhmonn Ua Cuinn which they call the Sequani Calendar from the ancient Celtic area where it was found. They maintain that because “every two and half years, the calendar reveals an intercalary Moon [month], it makes…the Sequani Calendar more accurate than the Julian.” They go on to show there are close similarities of this calendar with that of the Greeks:

“Most importantly, the Calendar of the Celts or the [Coligny] Sequani Calendar and the calendar of the Greeks, or the Sacred Calendar of Eleusis are strikingly similar. The calendars mark lunar and solar time with both cycles in conjunction using an intercalary month to coordinate the cycles of the sun and the moon. The calendars are then successfully able to use lunar cycles for the twelve months of the year. The equinoxes and the solstices are included in the lunar month and the stars are included in the calendar as well. The stars are named in Greek mythology to follow a pattern of myths, and the Celtic mythology is easily transferable due to the similar archetypes in both cultures.

“In the lunar month both calendars celebrate their people’s holidays of the full moon…and the priests’ Holy Nights on the new moon. The third quarter-moon is a significant time in both calendars to begin religious practices. The lunar months are also a microcosm of larger lunar cycles that both calendars follow. Every 55 years the lunar cycle begins on one of the four major phases of the moon…

“The [Coligny] Sequani Calendar and the Sacred Calendar of Eleusis have a 55-night cycle for one of the most important celebrations: the coming of the fall equinox. Both calendars follow the ellipsis of the Sun’s orbit and its quickening pace at the equinoxes.”

Since we maintain that the chronology/history of the ancient world is far shorter than assumed and that these various Indo-European peoples shared a common language and heritage before they dispersed from their central homeland near northern Anatolia, they would have brought with them not only their common language, or one with several dialects, but their mythology, customs and

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31 Ibid., pp. 116-117
32 Helen Benigni, Barbara Carter, Eadhmonn Ua Cuinn, The Myth of the Year: Returning to the Origin of the Druid Calendar (Lanham IN 2003), p. XII
33 Ibid., p. 2
knowledge such as metrological lengths, discussed above, but a somewhat similar calendrical system as well. If our chronology is correct, there should exist certain common calendrical elements across the Indo-European world. In this respect the calendar of the Celts should contain not only elements of the Greek calendar but of more distant ones as well. Berresford Ellis reports one of these connections:

“There have been many studies of the [Coligny] calendar but in 1992 Dr. Garrett Olmsted’s seminal work, published in Germany, substantiated the Celtic parallels of the Vedic Calendar. Olmsted was both a Celtic scholar and a qualified astronomer. While accepting that the surviving [Coligny] calendar was manufactured in the first century B.C., Dr. Olmsted went further and demonstrated by astronomical calculus [based on the established chronology] that the calendar must have originally been computed in 1100 B.C. .

“Professor Eoin MacNeill, when he examined the calendar in 1924, posed the question: ‘Is it possible that the Coligny Calendar preserves the older Indo-European tradition of the Celts?’ He felt the answer was affirmative. Every study of the calendar has inclined to support this.”

That is, we have similar elements of the calendar found among the Vedic Indians, the European Celts, and the Greeks, indicative of their having, in the past, shared a similar culture, mythology, and calendar. Berresford Ellis elucidated the close connections between the Coligny and Vedic calendars because both divided the year into dark and light periods and use a thirty-year period.

“There are sixty-two consecutive months in the [Coligny] calendar divided into period of twenty-nine or thirty nights each. In Celtic fashion the calendar reckons periods by nights. Caesar observed: ‘They count periods of time not by the number of days but by the number of nights…Pliny implies that it was by the moon that the Celts measured the months of the year and also ‘ages’ or thirty-year periods. [Presumably the thirty years was regarded as a generation.] [Carl] Deroux shows: ‘The use of a 30-year generation value in antiquity is well attested to’ in his Studies in Latin Literature and Roman History vol. 3 (Bruxelles, Belgium 1979) p.7.

“Dillion and Chadwick comment:

“The Calendar of Coligny is evidence of a considerable degree of competence in astronomy and may reflect the learning of the Druids. Moreover, in the division of the year into a bright and a dark half, [also] in the month of thirty days with a

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34 Berresford Ellis, op.cit., p.117
five-year cycle, at the end of which an intercalary month was added, the Gaulish calendar resembles that of the Hindus.

“The Vedas and the Upanishads show that the Hindu year was indeed divided into two halves in a fashion analogous to the Celtic year. The Vedic references... indicate that the Vedic calendar was lunar, with variable 354/355 days, included intercalary months, and followed a thirty-year cycle like the Celtic one. Plutarch mentions a thirty-year festival among the Celts when Cronus [Saturn] entered the sign of Taurus.

“It is [however quite] clear that by the first century A.D. the majority of the continental Celtic peoples had adopted the new forms of astronomy and astrology that were used in the Greco-Roman world.”

As was pointed out above, the churchmen of France and Spain inveighed against the worship by post-Roman pagan people of the Sun and Moon. Citations we have just presented show that the pagan people of Europe also worshipped these bodies. Again we have evidence that supports the short chronology including the division of the year into eight parts as shown above by McCluskey. In terms of this borrowing of calendars from both the Greeks and Romans, McCluskey further shows that the Coligny Calendar had holes in it into which pegs could be inserted and moved from day to day to inform the user of the day of the month, season and year:

“Like much of Gallic culture, the Calendar of Coligny combines native Celtic elements with ideas drawn from centuries of contact with Romans and Greeks. It is an excellent example of the kind of inscribed calendar that had developed in Mediterranean city-states, yet its content is clearly indigenous. The earliest such Greek calendars described the appearance and disappearance of stars in the course of an entire solar year [as did the Celts], with sockets drilled next to each day into which pegs marking the current day, and perhaps the changing phases of the Moon, could be inserted. Roman examples were structured on the solar year but variously added places to mark the current day in the lunar or calendar month... and the place of the Sun or Moon in the zodiac.”

In addition to all this, Garrett S. Olmsted, in discussing the 19-year Metonic cycle in this calendar, where 235 months are almost equivalent to 19 years’

35 Ibid., p. 119
36 McCluskey, op. cit., p. 54
duration, suggests: “I think it most likely…”

Harry Mountain says that the Coligny Calendar gives “a 19 year cycle.”

All in all, the Coligny Calendar contained knowledge and elements of the Greek calendar as well as that of the Romans who dominated the Celts for centuries. From these sources or independently they divided the year into eight parts—four parts being the two solstices and equinoxes, and the other four parts being the cross-quarter days between the solstices and equinoxes that delineated their four seasons. They knew the length of the year. They had 30 and 29-day months and also knew of the 19-year Meton cycle. These were not ignorant barbarians with respect to the calendar.

Above and beyond that they would also, in their contacts with the later Romans, have known of the seven-day week which “was officially introduced by the emperor Constantine in 321 [A.D.].” John T. Koch further reports: “The seven-day week, though common among ancient cultures, was evidently borrowed into Celtic cultures through contact with the Roman world. The evidence for this comes largely from the names of the days of the week, which are all either borrowings from Latin or based on the medieval Christian calendar. The modern BRYTHONIC languages [such as Gaelic, Welsh etc.,] are unique in preserving all the Latin names for the days of the week.”

From all this we gather that the Celts and other peoples of Europe dominated by Rome knew of and celebrated these astronomical events, as Anna Franklin shows:

“The feast of the summer solstice, along with the other solar festivals [such as solstices and equinoxes], is often referred to as a Lesser Sabbat, as opposed to the four…cross-quarter festivals, of Imbolc, Beltane, Lughnasa, and Samhain…

“We know that the Saxons and Norsemen held the summer solstice to be a festival of major importance; in northern Europe it was generally considered to be the most important festival of the whole year. Academics are also now challenging

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37 Garrett S. Olmsted, *The Gaulish Calendar: A Reconstruction from the Bronze Fragments from Coligny…*, (Bonn Germany 1992), p. 43
the view that the observation of the summer solstice spread from there into Celtic areas. I often read that the Celts did not celebrate the summer solstice…”

Nevertheless, the Celts did live under Roman rule; they were aware of the solstice and equinox festivals and incorporated these into their calendar, as pointed out by Patricia Monaghan: “Although the Celts did not celebrate the SOLSTICES and EQUINOXES…, various pressures [caused by contact with the Romans] caused the addition of other seasonal feasts to the Celtic calendar…With the coming of Christianity the [celebration of the] winter solstice [around December 25th, the birth of Christ] became especially important.”

Given all this, the ring at Stonehenge appears to be a calendar in which only the summer solstice alignment played a prominent role. It was used by these people to move markers like the pegs in the Coligny Calendar from one point to the next point to organize the year, seasons, etc. To begin, there were the Y and Z circles with 30 and 29 places. These we suggest were used to move markers for the months, those months of 30 days moved once a day along the 30 holes; these months of 29 days along the 29 holes. In that way the lunar months were taken into account. The 30 sarsen arches with the inner five arches of the horseshoe would be used to tabulate the year and the season. Moving a marker around the 30 sarsen arches three times plus one on the inner five of the horseshoe would equal 91 days or an entire season.

They may have also added a 92-day season to the year to create a 365-day year. Four such seasons would equal 364 days, or an extra day was added to create a year of 365 days, and every fourth year two days were added to create a leap year of 366 days. In this way the lunar and solar years still created an incommensurable construct. However, there are 19 blue stones inside the sarsen circle. Having a marker moved once a year created in 19 years the solar and lunar years such that they became commensurable. With this simple organization the people at Stonehenge could have determined the lunar month, the solar year and the cross-quarter days that marked their seasons. By placing a second marker in the 30 arches of the sarsen ring 40 days prior to the first set, and using the five horseshoe arches in the same way, the solstices and equinoxes fall either on the exact day or a day before or after the time that they should fall. The simplicity of this calendar is that if Stonehenge was constructed as a calendar, its markers were only moved one

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space per day, which required of the man or woman who did the moving no elaborate knowledge.

All of this, of course, is tentative, but it does explain how these people could have applied their calendrical knowledge in such a way that they could date the year, the cross-quarter days, seasons, equinoxes and solstices and the lunar year together within the 19-year Metonic cycle. The year of 365- or 366-day period was integrated with the cross-quarter day seasons, with the solstices and equinoxes, with the lunar months and lunar year, and the Metonic cycle. Having dates for these that may have been off by a day or two would have had almost no effect on their calendar for some time. To all intents and purposes, this calendar would have worked well for hundreds of years.

Other rings, not structured as Stonehenge was, may have used a week calendar of 7 days and 52 weeks equal to a 364-day year plus one extra or two extra days to give a 365- or 366-day year. Where there are multiples of 7 stones in such rings this would have accomplished the same division of the year. All these methods may have been in operation at the same time, which enabled the celebrations of these dates to be kept.

Related to these pagan festivals is the fact that the medieval church over time incorporated these celebrations as saints’ or other holy days. These were then carried forward into modern times and therefore show a direct connection between these pagan festivals with the Church down into the present. We suggest that these connections and the evolution of these cross-quarter celebrations of the post-Roman epoch into medieval times to the present are clear evidence of the chronology we have presented. In this respect, McCluskey outlines these developments in his chapter titled: “Christening the solar calendar.”

“The solar division of the year at days falling midway between the solstices and equinoxes, indicated by…British megalithic monuments, implicit in the Calendar of Coligny and surviving in Celtic folk traditions, finds more direct expression in the later interactions of Christian and Celtic traditions, best recorded on the insular fringes of Europe.…

“This traditional Celtic division of the year was not mentioned just in texts. It can be identified more precisely by considering the transformation of the four mid-quarter festivals, Samhain, Imbolc, Beltaine, and Lughnasa, into Christian feasts… [L]ater generations of Christians transform[ed] the mid-quarter festivals into Christian feasts which took on aspects of their pagan counterparts. When a pagan
festival was transformed into a Christian holy day, its date...became fixed in the Julian calendar used by the Christian church. Since the Julian calendar does not exactly follow the Sun, but errs by some three days in 400 years, a particular Julian date can coincide with a particular solar date only for about two centuries...

“Considering the historical development of the Christian liturgical calendar, by investigating a group of Christian feasts that folklore and tradition associate with one of the mid-quarter festivals, we find that at the time of their establishment [between 642 to ca. 685 and 902 A.D.], these feasts actually fell within a few days of that solar mid-quarter festival.”

McCluskey goes on to list and explain these connections. The first is:

“The feast of All Saints (1 November) is the feast most widely recognized as a successor to a mid-quarter festival, in this case [the pagan day of] Samhain. Samhain means the end of summer and marks one of two primary turning points of the Celtic year; the festival concerns the bonds between the living and the dead, a point it shares with its Christian successor. Despite the occasional challenges by historians of liturgy there is little doubt that All Saints and Samhain have long been closely related.

“The establishment of the [pagan] feast [into that of the Christian church] can be firmly dated to the end of the eighth century...

“The feast of St. Brigit (1 February) is much less a rival to the pagan feast of Imbolc that it replaced, than it is a Christian adoption of pagan traditions....

“The date of the establishment of Brigit’s feast can only be approximately defined. Brigit’s death is assigned various dates around the year 525...

“No well-established Christian feast can be clearly associated with the pagan festival of Beltane [May 1st or 2nd]. Yet a number of early and modern texts indicate the connection of the Blessed Virgin with Beltaine, May Day, and the crowning of a May Queen as a symbol of fertility....

“One of the most thoroughly studied of the mid-quarter festivals is Lughnasa, a harvest assembly and festival dedicated to the god Lugh...Lugh’s festival is connected with assemblies, often held at hilltop shrines, throughout the Celtic fringe of pre-Christian and modern Europe. In many cases folklore, historical

43 McCluskey, op. cit., pp. 60-61
accounts, and local traditions connect the celebration with...Christian feast day[s] ...the feasts of St. Oswald (5 Aug.) and St. Justus of Lyons (4 Aug.).”

This cross-quarter day concept is strongly disputed by historian Ronald Hutton in his *Stations of the Sun*. The problem he faces however, is: why would the medieval Church adopt these pagan festival days if they never existed? The other question is why are (as McCluskey claimed) these cross-quarter days directly related to the Christian calendar as found in the literature? While Hutton argues on page 411: “that the notion of a distinctive ‘Celtic’ ritual year, with four festivals at the quarter-days and an opening at Samhain [November 1st] is a scholastic construction of the eighteenth and nineteenth centuries which should now be considerably revised or even abandoned altogether”, McCluskey two years later actually cites documentary sources for all these Christian medieval festivals and he points out: “As these Christian feasts emerged, they continued many of the social functions of the pagan festivals that they supplanted.” Again, the question arises: why would the Christian Church during medieval times not only place these pagan cross-quarter day feasts in the same time slots with their Christian holy days, but then use these, continuing the same social functions as those of the pagans? Is it plausible that just by chance the medieval Christian church placed these religious holy days into the very same time slots that the pagans had for these cross-quarter days, and also by coincidence just employed these to have the same pagan social functions? This we suggest is simply neither plausible nor probable. So far as we know, no one has explained these coincidences away.

There may be some who are uneasy about our suggestion that the peoples of Ireland, and elsewhere, did not use sight lines to determine dates of the year, but derived these strictly from a calendar as we do today. However, the Coligny Calendar presents us with just such evidence. As Miranda J. Green tells us:

“Of great significance for the antiquity of the major Celtic calendar festivals is the fact that the pan-Celtic assembly of Lughnasa, ‘Games or Assembly of Lugh’, held on and around 1 August, a first-fruit festival, is indicated on the Coligny calendar under the name Rivas, ‘great festival month.’”

She further reports:

44 Ibid., pp. 64-67
45 Hutton, *The Stations of the Sun*, op.cit., pp. 408-411
46 McCluskey, op.cit., p. 69
“The ancient Celtic year was divided into two season, the cold season (Gaulish giamon, Irish giamred, Welsh gaeaf), and the warm season (Gaulish samon, Irish samrad, Welsh haf). This division applied over the entire Celtic world in antiquity. It is noted in the Coligny calendar and in modern times.”

Beyond this she adds:

“The Celtic New Year began on the eve of 1 November... Many significant events took place at this time, according to tradition; for example, the great battle of the gods of Ireland, Cath Maige Tuired, is essentially a Samain myth..., the Samoni, ‘three nights of Samin,’ are noted in the Coligny calendar.”

However, in the Coligny Calendar this November 1st festival was somehow placed on the mid-summer equinox as explained by Koch:

“In the Celtic languages the name Samain is clearly related to that of ‘summer’... The Brythonic name for the month of June, Welsh Mehefin, Breton Mezeven, contains the same word including the suffix, and etymologically means ‘middle of summer,’ BRITISH, *medio-samoni, indicating an old[er] system in which summer figured as the three months broadly corresponding to May, June, and July, as likewise implied by Irish Meitheamb, June... and Welsh Gorffennaf, ‘July’, lit[erally] ‘end of summer.’ Why a Celtic May-July summer should give its name to a festival of 31 October/1 November is an unresolved question.”

It appears that at an earlier time the Celts celebrated the mid-summer solstice, but later transferred it to the start of their new year, October 31st/November 1st. Not only that, but it is clear the Celts divided the year into eight parts with four seasons and cross-quarter days, also noted in the Coligny Calendar. According to Ruggles, the Coligny Calendar:

“...is a public calendar... with dates and festivals marked, and it is luni-solar in character... Lunar months alternating between twenty-nine and thirty days, and even intercalary months, are marked; but so, too, are dates that recur once every three months or so, each mysteriously marked ‘PRINI LAG’ or something similar. These dates occur at intervals of ninety-one, ninety-three, ninety-one, and ninety-two days—intervals of almost exactly one quarter of the year (which has 367 days in the Coligny calendar)....

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48 Ibid., p. 434
49 Ibid.
50 Koch, op. cit., p. 1558
“Perhaps the regular intervals were introduced as part of the process of making the existing calendar conform to the Roman system.”

This is precisely the position we have taken regarding the Celts who lived for centuries under Roman domination. That is to say, if Stonehenge was used as a calendar, it would have been after the Romans left Britain, and the Celts there and the Saxons on the Continent would have learned the Julian Calendar and adjusted their own to fit it. The Coligny Calendar reflects an older tradition of the Celts; Stonehenge may reflect the later Celtic tradition. But since the Celts had a calendar that already divided the year into seasons of roughly 91 to 92 days, and had marked these, so pegs moving through time would give the various times of their occurrence, they did not have to take sight-lines for all except, as we suggest, at Stonehenge, for the summer solstice. Why would these people have to observe these seasonal dates by alignment when they could in only a few years have measured and discovered this 91-92 seasonal change? It seems evident that their great convocations in Europe, Lughnasa on August 1st, only needed to be inscribed in the Coligny Calendar, such that when the peg moved into the slot for that date, it told them to convene. Since the Coligny Calendar apparently worked in this way, any other great calendar that they set up after Roman times would have worked in the same way, but with more advanced calendrical knowledge because of their long association with the Romans and their Julian calendar.

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51 Ruggles, Ancient Astronomy..., op. cit., p. 76
CHAPTER 10

SKARA BRAE: A MEDIEVAL TIME CAPSULE
AND A DARK AGE

Skara Brae is a time capsule like Pompeii and Akrotiri because it was buried suddenly and has remained in a marvelous state of preservation since that burial. It was discovered in 1850 when a storm removed materials that had covered it. As Knight and Lomas show:

“...nobody is sure when Skara Brae was first inhabited...but it is certain it was abandoned very suddenly. A small pile of bone beads was found strewn along the main passageway, suggesting that the owner of the necklace had snapped its cord whilst rushing from apartment [number] seven, and had no time to collect the dropped beads. In one of the wall cupboards was a hoard of 2400 inscribed beads and pendants that must have had great value, yet they were left behind. The date put on this abandonment is circa 2655 BC.”

Because of this, Alison Hems and Marion R. Blockley maintain “...nowhere else in northern Europe are we able to see such rich evidence of how our remote ancestors actually lived.” In essence, we have a small settlement or village suddenly abandoned with nearly all its possessions preserved that could survive the ravages of time and, most importantly, dated to the same period as Stonehenge and most other megalithic sites. The question is: was Skara Brae a Neolithic Age site or did it date to medieval times? As we will find below, from its early excavations carried out in the early 1900s, the evidence of the period to which it belonged was so striking that until the 1970s, when radiocarbon dating was applied to establish its chronology, many archaeologists and even its excavators, Vere Gordon Childe et al., offered the strong possibility that it was not a Neolithic village but a post-Roman/Medieval one. It might have been one of the small villages or monasteries so common in medieval times that dotted the landscape. Skara Brae in the Orkney Islands, Scotland, near the sea of the Bay of Skaill is shockingly sophisticated for the Neolithic Age, as Brian Moore and Peter James readily admit:

1 Knight and Lomas, *Uriel’s Machine, op. cit.*, pp. 160-161
“...possibly the most interesting site on the island is the cluster of drystone walled Neolithic dwellings known as Skara Brae...This settlement comprised a complex of eight interlinked houses of an architectural sophistication almost unmatched in prehistoric western Europe...

“The site was first excavated scientifically in 1928-9 by V. Gordon Childe who, misled by the sophistication of the architecture, erroneously dated the site to the Iron Age and attributed it to the Picts. However, clues from the pottery types at Skara Brae unavailable to Childe show that the site was in fact Neolithic, dating to roughly 2000 BC. The excavations carried out in the 1970s by D.V. Clarke and Anna Ritchie supplied material for radiocarbon dating which confirms the Neolithic date. Results from the earliest level of the midden [waste materials] (all on bone) indicate that the site began around 2450 bc in $^{14}$C years (3150 BC calibrated)...

“As none of the dates is any later than 2200 BC, all the evidence points to the main period of occupation having terminated during the last centuries of the 3rd millennium BC on a calibrated timescale.”

As noted above the earliest radiocarbon dates were determined by dating “all on bone,” the worst material possible for this dating method. Furthermore, we will show that not only is the architecture of such sophistication that it led Childe et al. to offer that it was a post-Roman or even medieval site, but moreover the materials found at Skara Brae were also of great sophistication and could not be attributed to the Neolithic Age but to the medieval period; they state: “The actual occupation of the village may certainly be comparatively recent, perhaps indeed post-Roman in date.” The title of the book claims it was a “Pictish Village.” The Pictish period runs from A.D. 446-843 or well into medieval times.

Above we have shown that hard stone balls were carved with various intricate grooves and patterns found at Skara Brae and that an engineer, James Macauley, cited above, failed to replicate these using stone tools: he admitted “In some cases the angle created in the decoration of prehistoric balls has proved impossible to achieve with stone tools and can only have been carved with strong metal tools.” These stone balls with the rest of Skara Brae’s contents had been quickly buried when the site was abandoned and lay buried until reopened in 1850. Thus there was no other time between these dates when these objects could have been carved.

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4 Vere Gordon Childe et al., Skara Brae: A Pictish Village in Orkney (London 1931), p. 1
5 James Browne, The History of Scotland (Edinburgh UK 1909), p. 96
“Strong metal tools” to do this work strongly suggests these were carved in the Iron Age which we have dated well into the first millennium B.C. Furthermore, the designs on the artifacts buried at the site were clearly Pictish in character, which suggests Skara Brae was in existence in the medieval era. But this evidence has been largely shunted aside to maintain the facade of the established chronology.

From this point forward we will show there is an immense “Dark Age” in the chronology for Saxon times and the dating of Skara Brae and the rest of the Megalithic Age, in terms of peat ash and agriculture also exhibits this same Dark Age which apparently ends in the 10th century A.D. Each of the following examples of evidence all show that the Neolithic, Bronze Age world people did not exist in these prehistoric times but did so in post-Roman/Saxon times. These problems/contradictions are found throughout the archaeological literature and are fully admitted by the proponents of the established chronology and, as we will see, they have not been resolved to this day. Let us begin!

The evidence that further clearly contradicts the established chronology, which is extremely well known, is related in part to the pottery made at Skara Brae. Now this pottery, like all pottery, had to be fired in some kind of oven to remove the moisture in the clay. In this respect Childe et al. explain: “Pottery was manufactured locally. It was badly burnt, perhaps because peat was the fuel. The red [peat] ash, already described was a constituent of the midden.”

Note that we are told the pottery found “was badly burnt” suggesting “peat was the fuel.” The point is that peat ash is reddish pink in color as shown by Nancy Marie Brown: “Ash from burning peat [has] a distinctive pale rose-pink color.” As we will see, peat had to have been the fuel not only to bake the local pottery at Skara Brae but to also heat the houses and cook the food in this remote area. Wood ash, as anyone who has a fireplace or had a campsite wood fire knows, is white to gray in color; so too are most other fuels. The evidence that the ash piled around Skara Brae was red and derived from burning peat echoes through the literature. John Robertson Campbell Hamilton speaks of it as “a layer of peat ash… [whose]

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6 Childe et al., op.cit., p. 98
7 Nancy Marie Brown, The Far Traveller: Voyages of a Viking Woman (Boston MA 2008), p. 207
color was generally red owing to the iron salts...”

Jacquetta Hawkes states: “At Skara Brae... fuel peat was burned.”

Peat ash, we repeat, contains iron salts and when it is burnt produces a reddish ash. The midden pile surrounding Skara Brae was huge, as Childe et al. state: “The huts [at Skara Brae] are buried to the tops of the surviving walls in huge midden heaps consisting of peat ash, shells [etc.].” Here then is the massive contradiction to the dating of Skara Brae and thereby to Stonehenge and the rest of the megalithic sites. Over a period of supposedly 600 years from around 3150 to 2550 B.C., the people of Skara Brae were burning peat and throwing it around their houses with the rest of their rubbish, building a huge pile of red ash, yet there was no peat available there during that entire period. In fact that, too, is well attested in the literature. Peat only began to develop in Scotland at least a thousand years later. The contradiction is thus that the people of Skara Brae were somehow miraculously burning peat that only grew in future times, to heat their homes, cook their food and fire their pottery—a clear impossibility. Knight and Lomas describe the problem thus:

“There is another puzzling feature. It is difficult at first glance to see where they got the fuel for their fires, because Orkney at the time was open grassland with hardly any trees. There was no local supply of wood to burn on the many fireplaces at Skara Brae. Yet from Childe’s excavations we know they had sufficient fuel to heat volcanic rock to high enough temperatures to cause it to heat-craze [heat rock so it shatters when dropped into cold water to heat the water etc.], and they must have been able to heat the [fairly large] houses to keep them warm enough to live there for so long. The suggested alternative fuels of cattle dung or seaweed do not have a high enough caloric value to achieve the temperatures needed to heat-craze volcanic rock, or to fire the pottery the inhabitants made. What’s more, the peat burnt on Orkney today was not laid down until 1000 years after Skara Brae was abandoned. It would appear, then, that the village of Skara brae either relied on driftwood from across the Atlantic to keep its eight fires and workshop furnace burning, or it imported wood from Caithness or Scandinavia.”

This explanation simply fails because driftwood or any other such wood leaves white ash with bits of charcoal as residue, not red ash. For example David Clarke and

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8 John Robertson Campbell Hamilton, *Excavations at Jarlshof, Shetland* (London 1956) p. 112
10 Vere Gordon Childe et al., *Ancient Dwellings at Skara Brae* (Edinburgh 1933), p. 1
11 Knight and Lomas, *Uriel’s Machine...*, op.cit., pp. 163-164
Patrick Maguire tells us that at Skara Brae “They also may have burned some wood. But not much. Virtually no charcoal has been found.” In spite of the negative evidence for burning wood because its ash is white/grey, or seaweed and dung because of its lack of caloric value to heat-craze rock, Caroline Arnold and Arthur Arnold offer:

“The focus of each dwelling at Skara Brae, as in other neolithic homes, was a stone-bordered hearth. The hearth fire was both a source of light and heat as well as a place for cooking food. Fuel for burning probably came from animal dung, dried seaweed, heather and bracken, and from whalebone, rich in oil. The little wood available to the inhabitants of Skara Brae was not burned because it was needed for building and for making handles for tools.”

All this is based on assumption, not only devoid of proof but contradicted by facts. The chronology of Skara Brae, accepted by the authorities, is in stark contradiction to these facts. In fact, even knowing this, Anna Ritchie makes the following statement about Maes Howe, where the Sun shines down its corridor at the solstice. “Radiocarbon analysis of peat at the bottom of the ditches indicates that the tomb was built sometime before 2700 BC.” In essence peat which grew in this part of Scotland about 1500 B.C. later was able to give a radiocarbon date to 2700 B.C. or 1200 years before peat began to form in that region.

Here is a further contradiction to radiocarbon dating. The overall evidence of peat ash at Skara Brae according to the proponents of the established chronology indicates that the peat was formed at least 1000 years after Skara Brae was abandoned, yet it is used to date Maes Howe, another supposed late Neolithic site, to 2700 BC. How can peat that grew around 1500 B.C., as attested in the literature, possibly give a date over 1000 years earlier? Somehow, this scientific contradiction must be addressed by scientific evidence to explain this obvious contradiction. We have found, as far as we know, no scientific evidence to explain this contradiction.

Furthermore, the pottery made at Skara Brae was “Grooved Ware,” as Ritchie elucidates:

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“The type of pottery used at Skara Brae is known today as Grooved Ware, on account of its characteristic grooved decoration. These were flat-bottomed bucket-shaped jars…Grooved Ware was used by communities over a very wide area from Orkney to southern England.”

But this pottery, according to Childe et al., cited above, was “badly burnt, perhaps because peat was the fuel.” That is, the relation between the “Grooved Ware” that was “badly burnt” only makes scientific sense if this pottery was manufactured after 1500 B.C. Since the only fuel abundant at Skara Brea that produced red ash was peat, and peat only began to develop after 1500 B.C., the Grooved Ware pottery had to have been fired after 1500 B.C. On every level, the red peat ash shows that Skara Brae as well as Stonehenge and the other megalithic sites do not date before 1500 B.C. To get around this massive chronological contradiction, Ritchie suggests “Childe’s ‘peat ash’ at Skara Brae which was considered to be an important element in the midden deposit…may perhaps be explained similarly [by heather]. (Peat is chiefly derived from decomposed heather.)”

The fact of the matter is that if heather fuel was capable of heating houses at Skara Brae, it would have been used as a fuel by other Scottish people, but this author has been unable to find any citation in the literature that heather was used as a primary fuel in Scotland for long periods of time. Ritchie gives us no particulars (that is scientific citations) about burning heather, as to its caloric content, etc. She gives us a vague “perhaps” and nothing else. Interestingly she also presents us with the following:

“The search for fuel must have presented an almost daily problem until the sustained and widespread growth of peat began in the later second millennium. Before then, the options were necessarily varied—and equally varied in their effectiveness. Apart from low scrub which burned fast, homegrown wood was too precious to squander, and good driftwood was essential for building purposes. Dried seaweed, dried animal dung, turf [all of which lack high caloric content], bone, could all become reasonable fuel to eke out timber supplies. Even in recent centuries, the inhabitants of the islands of Sanday and North Ronaldsay, where there was no peat, had to make do with seaweed, turf and dung…Seaweed was at least an inexhaustible resource, whereas burning turf reduced the area of pasturage, and cattle dung might have been put to better long-term purpose as an agricultural fertilizer…

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15 Ritchie, *Prehistoric Orkney*, op. cit., p. 35
“Wheat as well as barley was grown by the people of Skara Brae…[which require fertilizer to keep the soil rich or agriculture will soon fail.]”\(^{17}\)

Ritchie further points out: “Evidence of manuring having been spread on the soil at Tofts Ness and Sanday…supports the idea of intensive agriculture in Neolithic times.”\(^{18}\) But none of these fuels produce red ash! And how many whales just happen to wash up on a shore to supply fuel? Very few.

Had heather been a reliable fuel in Scotland for 600 years in Neolithic times it would have been employed for this purpose in places such as Sanday and North Ronaldsay. Surely somewhere in Scotland we would have evidence that heather was a reliable fuel, but this is simply not the case, and the authorities are clutching at heather straw to answer the contradiction of red ash at Skara Brae.

People do not establish settlements in cold regions that lack fuel. When they first arrive they have to have a sufficient and sure supply of some sort of material to keep them warm at night and through the first winter as well as to cook their food. In a place where fuel in the form of driftwood, cow dung, whalebone are in very short supply they cannot survive until they build a shelter to protect them from the elements. Moreover, they would need a steady and constant supply of fuel to remain in the same place for some 600 years. Moving to Skara Brae long after peat had begun to form would allow them to keep warm, cook food and even fire pottery. Peat does take time to form and therefore coming to the Skara Brae area in medieval times would resolve the fuel problem.

Nevertheless, in terms of the short chronology the additional rainfall necessary for the growth of peat beds may not have commenced around 1500 B.C., the time of Velikovsky’s first pole-shift catastrophe, but around 800 B.C. While great aridification occurred in the desert belt, as described in the previous volumes of Pillars of the Past, regions adjacent to the Atlantic, when displaced northward, would have experienced much greater rainfall, and this is well outlined by Michael E. Jones, who points out:

“…between 1000 and 750 B.C…a sharp fall in temperatures along with increased rainfall [occurred]. Two climatic factors, rainfall and temperature, exercise a controlling influence over British agriculture. A fall in yearly average temperature of even 1°C [1.8°F] significantly reduces the growing season, while

\(^{17}\) Ritchie, Prehistoric Orkney, op. cit., p. 19
\(^{18}\) Ibid., p. 24
wetter than usual summers and falls are the most damaging circumstance for harvest yields. Changes in either temperature or rainfall are most destructive to western and northern regions...in general the higher the altitude or latitude [such as the higher latitude of Scotland] the greater the effect of climatic change...

“The climatic deterioration of the Iron Age produced an estimated fall in average annual temperatures of 2ºC [3.6ºF] in England, Ireland, Wales, and southern Scotland. This would have shortened the growing season by at least five weeks. At the peak of the cold episode (750-400 B.C.) areas of Britain experienced unmatched wetness [which promotes the growth of peat]. The growth rate for certain raised bogs dependent on surface water (rainfall and runoff) was 500 percent higher than for other times...The increased wetness is reflected in the archaeological evidence....These colder, wetter conditions in Britain were paralleled in central Europe and Scandinavia, where glacial advance and the rise of lake levels graphically recorded the climatic deterioration.”

This suggests that the great growth period for peat occurred not around 1500 B.C., but about 750 B.C., and that the people of Skara Brae must also be dated later than 750 B.C., which could place them in post-Roman/medieval times so that they would then exploit this precious peat resource. In either the established chronology or our short one, the people of Skara Brae could not have been burning peat to leave red ash over a period of 600 years from 3100-2500 B.C. They could only have done so either after 1500 B.C. or 750 B.C. This indisputably contradicts the established chronology for Skara Brae, Stonehenge, and the rest of the megalithic world.

THE DARK AGE: AGRICULTURAL STRATIGRAPHY/ARCHAEOLOGY

If, as we maintain, the supposed Neolithic farmers of the Megalithic Age are actually the Saxons of post-Roman/medieval Britain, Ireland, and Scotland, one of these people could not, or could only barely, exist in the archaeological record. When one equates one culture with that of another, only one should have left relics and evidence of its existence and the other not, or should barely have done so. This concept was pioneered by Professor Gunnar Heinsohn, as outlined in volume I of Pillars of the Past. When the Sumerians were discovered, who were actually the Chaldeans, all the stratigraphical/archaeological evidence of the Chaldeans vanished from that record. When the Mitanni were discovered, who were actually the Medes, all the stratigraphical/archaeological evidence of the Medes vanished. In these cases

the unknown older civilization was a historical/archaeological invention, and if the older Neolithic culture has been given the archaeological materials of the post-Roman Saxon culture, the Saxons, too, will disappear, or practically disappear, from the stratigraphical/archaeological record in Britain, etc. With regard to agriculture, the fact of the matter is clear. While the archaeologists claim they have found clear evidence of agriculture in Britain, Ireland, and Scotland of the Neolithic Age, dating from roughly 3200 to 1500 B.C., the agricultural evidence of the Saxons has vanished. That is, the archaeologists have given to the Neolithic farmers the evidence that actually belongs to the Saxons! Thus there should be no evidence for Saxon agriculture from around A.D. 500 to 900, and that is indisputably the case. This fact is prevalent in the literature. For example Peter Hunter Blair writes:

“Village communities which lived by agriculture and stock farming would require a wide variety of buildings which could be used as barns, stables, byres and store houses…, but only the discovery of an Anglo-Saxon village which had already been abandoned before the Middle Ages, and its subsequent excavation could supply the evidence which is at present lacking. Such a site might be expected to give valuable information about agricultural practices of the earliest Anglo-Saxon settlers, but for the present there is no counterpart in the Anglo-Saxon period to the Iron Age farm at Little Woodbury…No Anglo-Saxon plough has yet been discovered…”

P.J. Fowler, writing as late as 2007, admits: “The evidence of artifacts directly related to Anglo-Saxon agriculture remains sparse. Ploughs, or ards, or parts of them, continue to be increasingly conspicuous by their absence…In general the same is true of fields and field systems…The picture remains disappointingly similar to that [for other areas]…” Elsewhere he writes: “Whatever the discussion about the plough in Roman Britain, at least it is a discussion based on surviving models and parts of ploughs, whereas virtually no such evidence exists for the period A.D. 500-900 in England.” This is identical to the missing Chaldean materials in the stratigraphical record where D.O. Edzard stated: “of the Chaldeans (Kaldû…) …not even a brick, not even a potsherd could be discovered.”

22 P.J. Fowler, Farming in the First Millennium A.D.: British Agriculture Between Julius Caesar and William the Conqueror (Cambridge UK 2002), p. 28
On the other hand, we have clear evidence of agricultural tools such as plows for Neolithic, prehistoric Britain. For example, speaking of dry farming E. Cecil Curwen and Gudmund Hatt write:

“Some of the plots tilled…by the earliest British farmers can be traced on the moors of Devon and Cornwall. Their date is not absolutely certain as yet, but a rough estimate of 2000-1000 B.C. is not likely to be far out. Their outlines have been preserved because the edges of the plots have served as dumps for stones of all sizes which had been cleared from their surfaces, and the plots themselves are always associated with the ruins of the circular stone huts in which people lived. How do we know that these little plots were cultivated at all, and that they were not just small enclosures for cattle and sheep? The answer is that most of them show traces of lynchet formation; that is, on sloping ground there is a tendency for the soil to be denuded at the upper edge of each plot, and to be heaped up along the lower edge. This lynchet formation may be very slight in some cases and quite pronounced in others, but it indicates that soil was transferred from the upper part of the plot to the lower. This has not been done deliberately, but is the result of gradual creep of topsoil downhill during the process of cultivation. So long as the turf remains unbroken lynchet formation cannot occur and once the turf has reformed [over it] after cultivation ceases, the contours of the lynchets are preserved indefinitely—unless they are destroyed by modern cultivation.

“Very many groups of hut circles (the ruins of prehistoric round stone huts somewhat similar to those at Skara Brae) are to be seen scattered over Dartmoor and Bodmin Moor. Some of them are associated with the remains of cultivated plots…”

Fowler writes of the problem of finding evidence for Anglo-Saxon agriculture and therefore of history:

“We are immediately confronted by a paradox. On the one hand, a very wide range of evidence is now available to be taken into account in considering farming in the first millennium AD in Britain. On the other hand, there is a widespread perception of paucity of evidence. Writing of what is admitted to be a rather special part of Britain for example, a distinguished authority began her study of the second half of the millennium [A.D. 500-1000] by declaring ‘It is not possible to write a history of early medieval Wales that can stand up to the requirements of modern scholarship.’”

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25 Fowler, *Farming in the First Millennium A.D.*, op.cit., p. 28
He adds this stunning piece of information and analysis about this absence of farming: “In the British Isles projected in Davies’ (1999) monumental history, for example, no ‘farming’ occurred at all and ‘agriculture’ was apparently, to judge by the index [of his book], confined to the Neolithic period and the Bronze Age. What sort of history is it in which people did not eat after 700 BC? What did the bulk of the population do with its time after that?” Fowler further remarks: “In contrast to the field system of the 500 years or so on either side of the beginning of our era, little evidence has survived in the ground for the next half millennium.”

According to Fowler, there is no evidence of farming in Britain from 700 B.C. until around the time the Romans came in at A.D. 45; and after the Romans left, agriculture again vanishes for 500 years. Why would the evidence of agriculture only survive in the Neolithic and Bronze Ages, then disappear after 700 B.C., only to revive after the Roman conquest and then vanish after the Roman exodus for an additional 500 years? In essence there are two periods in early British history and prehistory when agriculture flourished—the Neolithic and Bronze Ages to 700 B.C., and the Roman era—and two periods when it disappeared—from 700 BC., to the Roman domination and from the Roman exodus to 1000 AD. That is, altogether for 1200 years there is practically no evidence of agriculture!

As for evidence of the plow the very same problem exists and is exacerbated by a further problem. According to Childe: “The native houses and fields of Roman Britain did not differ essentially from those of the latest Bronze Age…Even in the British Isles many elements of pure Bronze Age culture survived unchanged…For example, travellers describe…a foot-plough, exactly like those known directly or inferred in Bronze Age Britain, as still current in the Hebrides.” Bronze Age agriculture disappears after 700 B.C., to miraculously reappear almost “essentially” the same after A.D. 1000, and continued up until fairly recent times in the Hebrides of Scotland. However, when we move all these materials into the Saxon age, the continuity of these artifacts make eminent sense.

Nevertheless, there is a major problem related to the plows used for cultivation. Many of the lowland regions of Britain and Europe contain heavy soils which require a heavy plow to turn furrows. But as we will see, this heavy plow only comes upon the scene in and after Roman times. Light plows will till highlands

\[26\] Ibid., pp. 28-29
\[27\] Ibid., p. 28
\[28\] Vere Gordon Childe, *The Bronze Age* (NY 1930) p. 239; see also Collingwood, Myres, *Roman Britain and the English Settlements*, op.cit., pp. 20-21
where the soil is light and easy to turn, while heavy plows will till the heavier soils of the lowlands. Miranda Green introduces us to this issue:

“In Britain, evidence for Iron Age ploughs exists mainly in the form of score-marks [in the soil, cut by these plows]; an ox-drawn ard was used at Danebury. This simple plough or ard was an uncomplicated implement consisting of a wooden shaft ending in a spike set at an angle, sometimes iron-tipped, which simply stirred and made furrows in the soil...However, there is some evidence from the later Iron Age that fairly heavy [lowland] soils were nonetheless being exploited.”

The problem of course is, how could these heavy plows have existed from the later Iron Age after 700 B.C., when no such evidence for them exists? If, indeed, as we suggest, the missing agriculture of Saxon times is that of the earlier Neolithic and Bronze Age, then the Saxons would have learned of these heavy plows from the Romans and employed them after the Romans departed. That is, the people of Britain, who used light plows in Roman times, would have been cultivating the highlands with light plows while the Romans tilled the lowlands with their heavier ones. After the Romans were gone, the Saxons, knowing of these heavy plows, would continue to use them. J.S. Wacher explains that Roman plows were clearly strong enough to do such work:

“Once clearance had taken place and boundaries been made, normal cultivation could start. At its most primitive this could be carried out with hoes, but ploughs were already in use in Bronze and Iron Age Britain [before 700 B.C.], although it is doubtful if they were strong enough to cope with heavy soils...The commonest type was the bow-ard, fitted with a wooden, iron, or sometimes, as in the north of Scotland, a stone share; iron shares varied considerably in type and size in the Roman period...Ploughs of the type were only really of use on light soils with a high clay content."

“But the real improvement came with the introduction of the plough with winged share and mould-board. It used to be thought that this type was introduced during the later Iron Age, which now seems very doubtful, and it is better seen as a Roman product.”

Rodney S. Carlisle shows: “The scratch plow was essentially a large digging stick dragged through the earth by a pair of oxen. [It] did not normally turn the soil

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29 Miranda Green, Animals in Celtic Life and Myth, (London, 2002), p. 28
30 J.S. Wacher, A Portrait of Roman Britain, op.cit., p. 45
but simply created separate furrows. To properly churn up the soil required cross-plowing...However, this form of plow and cross-plowing were not suited to agriculture in northern Europe, where a combination of weather and soil made it inappropriate. With wet and rainy summers and much heavier soil, the scratch plow simply did not work well in lowlands, where the soil offered too much resistance.”\(^{31}\) Eric Kerridge further explains: “...the Ancient Britons confined their tillage almost entirely to the more easily worked soils...”\(^{32}\)

It is therefore self-evident that in order to plow the heavy lowland soils of Bronze and Iron Age Britain, they had to have used heavy plows which did not exist in Britain until the Roman occupation. Thus, even when historians and archaeologists claim that in early times these heavy soils were being cultivated, this simply could not have been accomplished with the early light plows.

Curwen and Hatt describe what was actually happening:

“During the Roman period in southern Britain two agricultural systems seem to have been practiced side by side. On the one hand, the native peasants continued to cultivate the light upland soils as their fathers had done before them, only much more extensively; and we have evidence suggesting that some of their surplus grain may have been required for the supply of the armies on the Scottish border or on the Continent. On the other hand, there was the system of farming associated with the Roman villas; of this, unfortunately, we know practically nothing, but what little we do know suggests that it may have been based on the use of the heavy plow and a system of long, narrow plots which is conveniently called strip cultivation. In the first place, Roman villas were commonly placed in low-lying situations and on the richer [heavier] soils for which the colter [heavy] plow would normally be required. A large plowshare has actually been found in one villa and a colter in another, and other colters have been found among the stock in trade of two Roman blacksmiths [in Britain]. Practically no traces of the fields cultivated by the owners of the villas have survived because unlike the remains of the upland...field system they have been completely obliterated by subsequent cultivation. There is, however, one site in the Fens where air photography has revealed a group of strip cultivations associated with the sites of two small Romano-British buildings with which they are clearly contemporary. Such finds as these suggest that the agricultural system of the Roman villas may have been based on the [heavy] plow and strip cultivation.

\(^{31}\) Rodney P. Carlisle, *Scientific American Inventions and Discoveries* (Hoboken NJ 2004), p. 130

“With the departure of the Romans the Celtic field system finally ceased in southern Britain. The Angles and Saxons brought with them the [heavy] colter plow and strip cultivation from their Teutonic homelands, and they made their use universal in England and ultimately throughout the British Isles.”

Only during and after the Roman occupation of Britain could the heavy lowland soils have been cultivated. Yet historians and archaeologists suggest these heavy soils were cultivated in prehistoric times with a plow that couldn’t do this work!

We have shown that the farms and field systems of the Saxons from around the time of the Roman evacuation of Britain to around A.D. 900 cannot be found, but this is only one aspect of this missing evidence. We encounter this same problem with regard to the population and towns as well. That is, the burial remains that the historians and archaeologists assigned to the Neolithic and Bronze Age people are actually the burial remains of Saxon times. Once these investigators gave these burial remains, found in long and round barrows, to the peoples of the Neolithic Bronze and early Iron Ages; the Saxon peoples from the 5th to the 10th centuries practically vanished, creating a Saxon “Dark Age.” This Saxon “Dark Age” is well known and explicitly has no solution. In this respect Hunter and Ralston report:

“It is no longer possible to imagine the complete disappearance of the population of Roman Britain [of post-Roman time]: aerial and field survey have shown a density of occupation in lowland Britain during the Roman period that reached, or exceeded that known from medieval England. A population of such size could not have completely disappeared, even in the face of prolonged war, famine and plague. It is true that the same surveys show a far less densely occupied land in the early medieval period, but not an empty one…In Scotland and Wales, it is extremely difficult to identify early medieval sites at all because pottery was not made or used in large quantities…”

Michael E. Jones shows: “Attempts to demonstrate conclusively significant continuity in specific urban or rural sites have run afoul of the near archaeological invisibility of post-Roman British society.” He later remarks: “Despite impressive archaeological activity, the number of identifiably Anglo-Saxon burials in Bernicia has remained tiny.” J.D. Richards adds: “We are forced, therefore, to

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33 Curwen and Hatt, op.cit., pp. 83-84
34 John Hunter and Ian Ralston, eds., The Archaeology of Britain: An Introduction from the Upper Paleolithic to the Industrial Revolution (London 1999), p. 181
35 Jones, op.cit., p. 2
36 Ibid., p. 23
accept that the sub[post]-Roman British population became archaeologically invisible…”  

Where did the Saxon population go? Barbara Yorke submits:

“...There has been much debate recently about the scale of Germanic [Saxon] settlement in fifth and sixth-century Britain. Population sizes are difficult to estimate and the numbers which can be calculated from the excavations of cemeteries are little help as on their own they would suggest a ridiculously small population when the number of years in which the cemetery is likely to have been in use is taken into account. Few West Saxon cemeteries have been excavated in their entirety, but it seems that few exceeded a hundred burials…”

Eric Klingelhoffer writes:

“The absence of the British population from the early Saxon landscape remains one of the major problems of early medieval research. There had to have been heavy population loss in late Roman times and emigration [of part of the Britons] to Brittany probably came from central southern England as well as from the Severn estuary. This population loss was probably further exacerbated by the sixth-century plagues that are known to have reached Britain. There is no evidence, however, that the Saxons were affected by any epidemic until the plague of 664 [A.D.], as recorded by Bede. This discrepancy may well be a major factor in the replacement of the British rural population by an Anglo-Saxon one during the period 550-700 A.D., but it does not explain why there is so little or no physical evidence of the presence of the British population after the first decades of the fifth century.

“The major tool of the archaeologist, pottery, is missing from the sub[post]-Roman settlements. The few sherds that have been found (comprising soft, crude hand-made, grass-tempered [fired] wares) have been categorized as Germanic…

“That the origin and use of grass-tempered pottery are attributable to a surviving Celtic population seems more plausible than the alternative assumptions that the Britons either completely abandoned most of the island or that they used only utensils of wood, metal and leather—anything but pottery...without greater

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38 Barbara Yorke, Wessex in the Early Middle Ages (Leicester UK 1995), pp. 43
sophistication in the archaeology of the post-Roman period, however, there is little chance that future light can be shed on the problem of British survival.”

Jones offers this “presumable” explanation for the lack of burials in Saxon times:

“In marked contrast to the archaeologically ‘visible’ rites of furnished cremation and inhumation practiced by the Germanic [Saxon] tribes, the overwhelming majority of the indigenous British rural population disposed of their dead in a manner rarely recoverable by archaeology. They are largely invisible in the Iron Age, remain so throughout the Roman era, and continue to defy archaeological discovery in post-Roman contexts. Presumably, corpses were disposed of singly or in scattered groups in a fashion that time and modern agricultural plowing have mostly erased. The total number of graves discovered from the Roman period is less than 20,000, almost all from urban cemeteries. If extrapolations were made from this basis alone, this would represent an average total population for Roman Britain of only about 2,000 people, obviously a gross underestimate.”

Jones further tells us in this regard, using the same type of statistical method, that “roughly 30,000 excavated inhumation and cremation graves from the Anglo-Saxon pagan era represent an average total population for the early fifth through early eighth century of only about 3,000 people.” Where did the population go?

C.J. Arnold epitomized the nature of this “burial Dark Age:”

“The Dark Age of British history has for long held a fascination for those interested in the past. This fascination is reflected in the eagerness with which publishers and authors alike incorporate the name of [King] Arthur into book titles, although interest in the Dark Ages existed well before the revival in popularity of Arthurian studies...The application of modern archaeological research to this period, with all its desire for precision, has not diminished the sense of mystery which surrounds it. Indeed, it is unlikely that it will ever do so because the enthusiasm for the Dark Ages is created simply because so little is known about this shadowy phase of British history; our fascination springs from our inevitable ignorance.”

40 Jones, op.cit., p. 23 (capitalization added)
41 Ibid., p. 26
42 C.J. Arnold, Roman Britain to Saxon England: An Archaeological Study (Beckenham UK 1984), p. 157
Not only is there no understanding of the lack of farming evidence, nor of the missing population as known from the very few graves, but the absence of pottery and artifacts during the post-Roman period has baffled the historians and archaeologists. About this material David Mackenzie Wilson tells us: “The rough handmade pottery found on Anglo-Saxon domestic sites presents a serious problem. The typical small globular cooking-pots are so simple that they are unlikely to have developed much in form over the centuries.”

Arnold enlarges on this: “A great stride forward was made after 1931 by J.N.L. Myres who began the systematic investigation of early Anglo-Saxon pottery..., yet there was no clear statement about the methods used to establish the chronology of the vessels under review, despite quite specific dates being ascribed. Myres fitted the material to a five-phase evolution of Anglo-Saxon society to which dates were attributed, but the basis for [these dates] was not discussed.”

He further adds:

“Most discussions of the chronology of specific artefacts, or artefact-types, of the period, are couched in predictably vague terms. This in a sense is unavoidable, but inevitably the flexibility that must be allowed can lead to varying opinions. Fundamental to all such discussions are assumptions about the rate of evolution of decorative styles and the definition of the circumstances in which such evolution took place. It could be argued that until those important issues have been resolved the role of subjective opinion in dating many artefacts is unavoidable.

“When dates for particular artefact-types are given, it is rarely made clear whether the date is one of manufacture of the artefact, its period of use, or the context in which it was found....Some writers quote dates to an accuracy of twenty-five years, others favour thirty years, but rarely is it made clear why or how such dates are obtained. There is unlikely to be any advance in dating early Anglo-Saxon archaeology in the near future...”

Charles Boutell sums up the great ignorance surrounding pottery dating in Anglo-Saxon Britain: “The remains of the pottery which was made during the long interval between the middle of the fifth century and the Norman Conquest [in 1066], show rather a decline than any improvement in the potter’s art. Many of the specimens of this period are singularly rude, and they all assimilate more closely to the ancient British than to the Roman types...”

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45 Ibid., p. 361
In essence, the Saxon period pottery has more affinities to pre-Roman types than to Roman types. What we have in this case is that when Rome abandoned Britain, the craft of pottery making was lost and had to be redeveloped. There was no continuity between the Roman period and the Saxon, and the methods of making pottery evolved along totally different lines as outlined by Mark Jackson and Kevin Greene: “Whereas the technical and aesthetic accomplishments of classical architecture and art were closely copied from medieval and Renaissance times onward, Greek and Roman pottery had no direct successors. When its forms and finishes were copied in the eighteenth century, potters employed completely different techniques.”

John Hines further reports:

“There is another radical change in the direction of cultural development in Britain in the fifth century AD…The ranges of material characteristic of both [highland and lowland] zones—principally pottery and metalwork—change from those of the Roman period, as does the set of sites of activity (settlements and cemeteries) that archaeology can attribute to this period. The volume and range of detectable continuity in what had been Roman Britain is surprisingly thin. Both Anglo-Saxon and post-Roman British deposits are familiar enough in the final phase of old Roman towns, villas and so on, but this late element is never demonstrably alike in an unbroken history. ASTONISHINGLY LA TÈNE [pottery and other] ART STYLES RE-EMERGE AS DOMINANT IN THE NORTHERN AND WESTERN ZONE.”

What is astonishing, not to say impossible, is that the La Tène European cultural art style that supposedly began, according to the established chronology, around 500 B.C., and ended just prior to the Roman conquest, reappeared after the Roman exodus of Britain, 500 years after it supposedly ended. This, again, clearly shows that the La Tène Iron Age pottery originally assigned to pre-Roman times throughout Europe was actually created in the Saxon/post-Roman epoch. Why, after a period of 500 years after the La Tène culture disappeared, would later people in Britain use its art styles and have them become dominant in the northern and western zone of Britain? Style of pottery do not re-emerge and become “dominant”

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after 500 years during which they were no longer used and clearly lost and forgotten by later generations.

Nor do the Saxon cemeteries show any correlation with what came before, as Jones shows:

“Although there are several examples of Roman cemetery sites later reused by Anglo-Saxons, crucially missing from the available evidence is an appreciable corpus of studies of cemeteries continuously in use from clearly Roman contexts right through the early Anglo-Saxon period. Wasperton in Warwickshire may be a rare exception. Without such a context there is no reliable way to distinguish between newly arrived [Saxon] invaders and the surviving indigenous [Briton] population…”

He adds:

“Chronology is perhaps the most intractable problem for cemetery studies, indeed for Anglo-Saxon archaeology in general. Dating rests on the uncertain and precarious chronology derived from typology… Before the seventh century, Anglo-Saxon archaeology lacks any fixed points to date securely and anchor the typological sequences…”

Just as with the La Tène art style re-emerging after 500 years, the Saxons reintroduce cremation burials that existed in pre-Roman times. J. D. Richards admits “…the arrival of Germanic [Saxon] immigrants seems to have led to the reintroduction of…cremation [burials] in what is now England.” Why, as with the La Tène art style re-emerging after half a millennium, would the Saxons also reintroduce cremation burials? The implication is again clear. Both inhumation and cremation burials ascribed to the Neolithic, Bronze, and Iron Ages were all employed in long and round barrows and elsewhere in post Roman/medieval Britain.

Aubrey Burl offers us this archaeological adage: “It is one of the many contradictions of British prehistory that the living disappear, but the dead survive.” But in post-Roman Britain the living and the dead nearly all disappear.

When we turn to the Saxon settlements of the early medieval period and beyond, the very same problem emerges; their settlements do not exist and this

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49 Jones, *The End of Roman Britain*, op. cit., pp. 18-19
50 Ibid., pp. 20
52 Burl, *Prehistoric Avebury*, op. cit., 269
also reverberates through the literature. Christopher Taylor states that there are “large areas of land [where] no Saxon occupation has been found. This may be because the remains of settlements of this period are particularly difficult to identify.”\(^{53}\) M.O.H. Carver tells us of the early Anglo-Saxon period: “Settlement sites of this date are notoriously hard to locate, and even when they are found it is very difficult to separate Iron Age and Pagan Saxon pottery when all the sherds are plain and abraded.”\(^{54}\) When it comes to Anglo-Saxon settlements in Northumbria, Fowler admits: “Richards (1999; 44-49), an archaeologist…, writing of Anglo-Saxon settlements in Northumbria...remarks that ‘one of the strengths of archaeology is that it is able to complement history based on documentary sources...Archaeology, it is argued, is about the everyday lives of everyday folk. Unfortunately, one searches hard for the everyday villages and farms to place alongside the rich manuscript and sculptural evidence of seventh and eighth century Northumbria.”\(^{55}\) He further comments: “The dearth of Anglo-Saxon earthworks [such as mounds] is a curious phenomenon and applies to settlements as well as fields.”\(^{56}\) Of Kent, John Williams reports: “…Anglo-Saxon settlement remains are relatively difficult to locate.”\(^{57}\) Della Hooke and Simon Burnell add: “Settlements are few because they are almost impossible to detect.”\(^{58}\)

All these researchers are being unusually disingenuous because they do not note any archaeological site that unambiguously represents an Anglo-Saxon village. When we turn to Ireland during the Iron Age, Barry Raftery explicitly speaks of “The Invisible People:”

“The great Corlea [wooden road] that runs for two kilometers (1.2 miles) encapsulates one of the baffling conundrums of Irish Iron Age archaeology. Its construction was clearly a substantial undertaking which involved the time and energies of many people, perhaps the energies of an entire tribe. But of the people, apart from the road itself silently stretching across Co. Longford peatland, there is no trace. We must assume that the remains of the homes and the settlements of the builders exist under the sod of surrounding regions, but these have yet to be found.

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\(^{55}\) Fowler, *Farming in the First Millennium AD…, op.cit.*, p. 30

\(^{56}\) *Ibid.*, p. 28

\(^{57}\) John H. Williams, *The Archaeology of Kent to AD 800* (Woodbridge UK 2007), p. 207

\(^{58}\) Della Hooke, Simon Burnell, *Landscape and Settlement in Britain, AD 400—1066* (Exeter UK 1995), p. 18
“The Corlea roadway thus tellingly illustrates the deficiencies of the contemporary archaeological record, which have hampered our ability to reconstruct the character of everyday life in Iron Age Ireland. The secular habitations of the ordinary people are almost totally unknown...Archaeology is therefore at an acute disadvantage. For in seeking to present a picture of Iron Age society at all levels in Ireland we must lean heavily on the scattered artifacts now in our museum collections which are generally devoid of content and, more often than not, even of provenance. The majority are arguably high-status items, clearly the trappings of an elite stratum of society. Many of these are of considerable technical and artistic quality...we may well question, however, the extent to which such splendid objects are relevant to an understanding of Iron Age society as a whole....

“Such artifacts also demonstrate the existence in a land of a powerful upper class under whose patronage this material was produced...but all the indications are that this group was numerically small. In the archaeological record the majority population of Iron Age Ireland is largely under-represented. These people existed but we cannot see them. Thus we may truly describe them as the invisible people.”

We ask the reader to consider what the historians and archaeologists are suggesting is the case regarding the post-Roman Saxons and the Neolithic, Bronze, and Early Iron Age people. At Skara Brae, the farmers were burning peat which left a rose-pink ash, supposedly for 600 years until 2500 B.C., yet peat, according to the established chronology, did not begin to develop in this region for at least another 1000 years later, or 1500 B.C. In terms of the short chronology, this climate change occurred around 800 B.C. Thus Skara Brae cannot be properly dated by radiocarbon dating and it, along with the rest of the Megalithic Age, must be dated after 1500 B.C., or even 700 B.C. There is a great deal of evidence of mounds both round and long with burials of both inhumation and cremation types with large numbers of skeletons in the Neolithic and Bronze Ages, but for the Saxon period, there are practically none. There is clear evidence of farms with houses and fields with lynchet slopes with plows and plow markings in the Neolithic and Bronze Ages, but none after 700 B.C. up to the Roman epoch in Britain, and then none from around A.D. 500 to 900. There are well-known settlement sites in the Neolithic and Bronze Ages which vanish again in the Iron Age and again in post-Roman times. The same exists with pottery being common in the Neolithic and Bronze Ages but being extraordinarily rare after A.D. 500. The La Tène art which died out in the pre-Roman period is revived in Britain some 500 years later. In Ireland there is not a shred of evidence of the Iron Age people.

59 Barry Raftery, *Pagan Celtic Ireland*..., *op.cit.*, p. 112
The older peoples and all they created during the Neolithic and Bronze Ages survived from around 3100 to 2500 B.C. but the later peoples and all they created after 700 B.C. up to Roman times and from post-Roman times ca 450 A.D. to 900 vanish from the earth. What the historians and archaeologists cannot see nor understand is that they have taken away all the materials of the early Iron Age and post-Roman epoch and given these to non-existent prehistoric peoples; they cannot and will not see nor understand that by giving all these materials to the peoples of the post-Roman era all these archaeological problems are resolved.

They submit that all these prehistoric materials survived the ravages of time, only the much later early Iron Age and post-Roman materials, that existed thousands of years closer to the present, did not survive the same ravages. None of this is plausible nor believable. By moving all these prehistoric materials into the Saxon epoch we have the vanished missing population and all their burials and mounds, including their skeletons that were either cremated or inhumated; we have their missing settlements and their round houses, their fields with lynchet slopes, their plows with their ability to cultivate heavy lowland soils, their stone, bronze, and iron tools. In essence we have a well-known people—the Saxons, who lived in Britain after the Roman exodus with all their materials, their mounds, skeletons etc. As Gunnar Heinsohn found this condition for civilizations in the ancient Near East, we have the identical problem with the prehistoric and post-Roman cultures in the British Isles.

However, there is more related to this problem. We have been told that the people of Skara Brae lived at that site for 600 years and carried on agriculture. Accordingly Gordon J. Barclay asks:

“What was being grown in these fields and plots [at Skara Brae and other sites in Neolithic Scotland]? Direct evidence for cultivated cereals is limited for both the earlier and later Neolithic [because these biodegrade in the soil, are eaten by insects, worms etc.]. Evidence for both barley and wheat was recovered from the settlement at Knap of Howar, Orkney…At Balfarg, a carbonized barley grain was found incorporated within an earlier Neolithic pottery sherd…At Boghead, Moray…, naked six-row barley…made up 88% of the cereal grains, and emmer wheat…11%…Hulled barley…was found at Skara Brae, and a gradual replacement of the naked form by the hulled has been noted generally…[A]t Balbridie…[e]mmer wheat made up a large component (almost 80%) of the assemblage, naked barley 18% and bread wheat…2 %. However, in one posthole
the proportion of bread wheat was 76%, showing the difficulties inherent in examining limited samples of cleaned crops…”

The problem with this concept is that these Neolithic and Bronze Age farmers had to remain sedentary on the land they farmed for hundreds to thousands of years. On the advice of my brother, David Ginenthal, I looked into the question of whether Neolithic and Bronze Age peoples could cultivate the same land for such extended periods of time or whether they could have done so in the post-Roman period. The requirement of these farmers to do so for such a lengthy time means they had to have the knowledge to carry on this dry farming for centuries or millennia. Is such a thesis possible or even probable for the Neolithic and Bronze Age?

“Settled” agricultural farming and animal husbandry requires that soils for both maintain their fertility for that entire length of time. Early farming was not of the settled type because these farmers did not know how to maintain the fertility of the land. Frederick R. Troeh and Louis M. Thompson explain:

“Most soils occurring in climates humid enough to grow large crops with irrigation [or dry farming] have acid reactions…The acidity is caused by the leaching of bases [alkalies] by percolating water. The result is a lowering of soil fertility and a less than ideal medium for the growth of most plants. Increased concentrations of cations [the ions of an element or molecule with more protons than electrons giving it a positive charge] such as aluminum (Al³⁺), iron (Fe³⁺) and magnesium (Mn⁴⁺) are likely to cause toxicity problems if the pH [acid/base level of the soil] is more acid than 5.0. Aluminum toxicity can be a problem for most living things—plants, animals, and even microbes—by interfering with DNA replication, cell division, root respiration and nutrient cycling…Raising the soil pH to a reaction near neutral may be highly profitable in spite of the cost involved. This [placing of lime or ash on the soil] procedure is known as liming.

“Liming will help maintain soil fertility if used in a well-managed cropping system; but in a poorly managed cropping system, liming acts as a stimulant producing good crops immediately followed by gradual impoverishment of fertility. An old time-tested German expression illustrated the effect of lime used on soil without supplemental fertilizing practice: ‘Kalk macht die Väter reich aber die Söhne arm’ (lime makes the fathers rich but the sons poor)…In another old saying: ‘Lime and lime without manure, will make both farm and farmer poor.’…

“Liming and fertilizing [with manure] usually go together as complementary practices at least in humid regions [such as northern Europe]. Liming increases the usage of nutrients because more crop is harvested. Microbial activity is also favoured by liming and organic-matter decomposition is accelerated. The accelerated decomposition causes organic content of the soil to decline. [Over-] liming therefore increases the available plant nutrient supply for a time but decreases the supply. That is why yields with liming are first higher but later lower than without liming. The lowering of yields in later years can usually be avoided by using fertilizer as well as lime. The decline in soil organic matter may be averted or at least limited by minimizing the amount of tillage and maximizing the amount of crop residues returned to the soil from the increased production.

“Fertilization often increases the need for lime.”

To maintain settled farming for several centuries or millennia in humid/rainy northern Europe requires a combination of adding lime or ash to the fields and thereafter constant seasonal fertilizing which meant manure in prehistoric times. These early farmers had no such knowledge of these requirements and thus could not maintain themselves in permanent settled communities. Instead they practiced what is known as “slash and burn” agriculture for a very short time, until the fertility of their soil was depleted, whereupon they moved on to new land and repeated the cycle. They were not truly capable of farming one site for extended periods of time. Fowler described this behavior:

“It is difficult to avoid using the word ‘exploit’ in describing these [agricultural] activities yet exploitation can hardly be the basis for the symbiotic relationship… The point is fundamental in assessing the environmental achievement of later prehistoric agrarian communities and involves both a temporal and a topographical dimension. Broadly speaking, in the first [slash and burn] part of our period we see man apparently pursuing an exploitive strategy in relation to his environment. He is taking [from the soil] what he needs and not replacing it; arguably, he is a farmer but not yet a husbandman…Critically, they were exploiting [the soil’s] natural fertility and not replacing it. They had no need to: with low population and plenty of space, decreasing fertility was obviated simply by moving to another exploitable niche in the ecosystem and there repeating the process. But in this shifting, and, from our point of view, shiftless agriculture, anthropogenic interferences seem in places to have critically affected the natural cycle of [soil fertility] regeneration. Exploitation was of the vegetational cover and of the soil structures and chemistry, and it can be argued that the subsequent history and present appearance of much of

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61 Troeh and Thompson, *op.cit.*, p. 165
what is now the Highland Zone can be traced back to what was happening in the later third and second millennia [2300-1300 B.C.].

“In that thousand years or so the first critical steps were taken towards what in today’s terms is considered a scenic amenity and, wrongly, a natural phenomenon of unspoilt beauty; but in pedological [soil] and vegetational terms much of northern and western Britain is a landscape of dereliction...In other words, climate, one of the environmental elements outside man’s control, accentuated the trend he had started. It was in particular its increased wetness [which lead to deterioration in the north and west highlands where] conditions were suitable for more rapid run-off of the increased rainfall. As a result, erosion, leaching podsolization [creating acid soil], and the ponding which produces active bog [and peat] growth, all proceeded, in many cases remorselessly, perhaps inevitably, because man had disturbed the soil structure, removed the tree-cover [which restricted rainfall run-off], and could not control the consequences of his action in a changed environmental situation. As the derelict field systems of the second millennium show over much of highland Britain from Dartmoor to Shetland..., man the farmer was even forced gradually to abandon some of his developed and highly organized attempts at settled agriculture...When he moved away, however, instead [of replacement and renewal of the land by] weeds and scrub and then woodland taking over his empty fields and extensive pastures, their structure altered, their flora reduced, their fertility drained, [and they] supported only heather [for peat], bog growth, and the occasional silver birch. This whole thesis is of course an oversimplification but not, it is hoped, grossly so.”

Fowler makes it quite clear that the climatic change that occurred after 1500 B.C. caused the land across western and northern Britain to be a land of dereliction. The soil was depleted of fertility and could hardly have supported a farming population. This brings us to the problem of the chronology of Skara Brae and all the settlements dated to 3100-1500 B.C. in western and northern Britain. Because the people of Skara Brae were burning peat, they could only have done so after 1500 B.C. But Fowler has informed us that the region around Skara Brae and elsewhere in Scotland had lost its soil’s fertility and farming could not have been carried on there because of this soil depletion. In terms of the established chronology Skara Brae could not have existed prior to 1500 B.C., because there was no peat available to burn to leave red ash. On the other hand, Skara Brae could not have existed just after 1500 B.C., when peat began to develop in Scotland, because the fertility of the soil in these lands was so depleted that the land “supported only heather, bog growth, and the occasional silver birch.” Furthermore, as we were informed earlier, the farming

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population of Britain disappeared after 700 B.C., so the people of Skara Brae and the rest of Scotland and western and northern Britain could not have existed from 700 B.C. to Roman times. In the most basic terms, Skara Brae could only have existed after the Romans left Britain, as our thesis demands. There is no other time frame in pre-Roman Britain that will permit its existence. In order to create a farming environment, Fowler attempts to get around this problem. We will stress in Italic Fowler’s explanations to show these are based on assumptions and presumptions and not on scientific evidence, and in the end he admits this:

“In what we can begin to recognize after c. 1000 BC as a distinctively Lowland Zone, the man/environment relationship worked out differently though not necessarily entirely deliberately. In the fourth and third millennia woodland areas were cleared, pastures were created, and cultivation was practised here too. In the earlier second millennium [B.C.] we see also the landscape architecture of organized and permanent field systems, and of extensive land allotment, and of individual [megalithic] monuments on the grand scale. Yet around 1000 BC we see a quickening of developments, and throughout the first millennium an increasing technological, material, and social progress. The land itself not only remained in good heart but apparently increased its productivity. Here [, for some reason, there] was not pedological degradation, and, while there was certainly vegetational change, its consequences were not irreversible as was the case further west and north. Yet climatic change occurred here too; why the agrarian differences between Highland and Lowland Zones? Fundamentally it was PROBABLY the result of a change in the nature of the relationship between man and his environment, a change in attitude reflected in practice from exploitation to conservation of the environment. There is of course a danger here of projecting backwards present ideas, but by ‘conservation’ in this context is meant a working with [the land to replenish it] rather than a taking from, a harmonization of objectives rather than a conflict, the development of the long-term concern of the husbandman rather than the cash-crop philosophy of the ‘Mesolithic’ [slash and burn] farmer.”

Fowler admits the lowland zone suffered as much as that of the highlands to the west and north, but the farmers of these lowlands for some totally unknown reason began to treat their land differently. How bad was the deterioration of these lowland areas? The very same greatly increased rainfall had to damage these lowland soils as those in the highlands. Jones shows this to be the case:

“In the lowlands, on the heavy and intractable clay soils developed for cereal production, already formidable problems of drainage and tillage must have

63 Ibid., pp. 204-205 (capitalization and emphasis added)
become even more acute [than in the highlands]. Increased rainfall would have adversely affected a number of upland and lowland soils. By accelerating the processes of soil erosion and soil degradation (soil structural changes), increased rainfall would have further lessened soil fertility and significantly reduced agricultural output and potential.”

While the problems with the heavy, intractable clay soils of the lowlands were “more acute,” they, and only they, according to Fowler, were able to be farmed. The reason is that the people “supposedly,” or in terms of Fowler “obviously” applied manure to the land. “The evidence that CAN BE INTERPRETED in this light is archaeological rather than environmental: the extant longevity and lynchettng of the field systems themselves, the crucial evidence of manuring on them, implied control of stock grazing [so animal manuring of the fields could occur]—undramatic, essentially practical matters but the very stuff of agrarian history.”

Pottery sherds have been found across these fields and it is therefore interpreted that these pots were utilized to carry manure from dung piles to fertilize the fields. While Fowler speaks of “the crucial manuring on them” he suggests: “direct animal manuring the existence of which is entirely theoretical but nevertheless at the same time virtually certain, [and] could have been achieved in two ways [animals manured the fields and people carried manure in pots to the fields] and it seems likely “that both were used.” Here Fowler runs amok of his own caution of “projecting backwards present ideas.” This Fowler calls “entirely theoretical, but nevertheless [he says it is] virtually certain.” Yet he says the evidence “can be interpreted” to give this concept. On the other hand, he also admits: “Perhaps some of the late prehistoric pottery…arrived there in the hooves of manuring cattle.” On this point of manuring, Fowler concludes by admitting this is only “speculation.” “Behind all the speculation, the important point is that from some time in the second millennium BC, we can safely assume that, in many areas, and increasingly, a manuring stage occurred in preparation of the seed-bed after clearance or harvest and before ploughing.”

The entire concept of manuring is based on the following arguments: (1) there was “probably…a change in the nature of the relationship between man and his environment; (2) the evidence can be interpreted” to do so; (3) there is an “implied

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64 Jones, The End of Roman Britain, op.cit., p. 220
65 Ibid., p. 205 (capitalization added)
66 Ibid., p. 170
67 Ibid., p. 170-171
68 Ibid., p. 171
control of stock grazing” which allowed farmers to manure their fields; (4) because “direct animal manuring the existence of which is entirely theoretical but nevertheless and virtually certain.” The problem that seems to have eluded his theoretical explanation is: why didn’t the highland people develop the same “change in the nature of the relationship between man and the environment,” and manure the highlands? The only reason why they failed to manure their land is obviously that they could not learn to do so from those lowlanders. This too we can “safely assume” is the case.

Miranda Green speaks of the problem Fowler has so much difficulty admitting:

“The general assumption is that manuring was practiced from the Bronze Age onward, based on abraded sherds of pottery being recovered from field areas. Tantalizingly, however, very little evidence has been found for the presence of middens or manure heaps within [farm] enclosures, though present research into trace evidence of lipids may alter this in the future. The difficulty lies in the organic nature of the material and its rapid dissolution [biodegradation] and disappearance.”

Yet the fields in which these pottery sherds have been found were cultivated long after prehistory, and animal manure was applied to the land and would have gotten into the pottery. Since we maintain these fields were cultivated in post-Roman times, the people of Europe and also Britain would have had contact with Roman villa agriculture and would have learned from the Romans how to manure their fields. The Roman farms did employ this practice and there is a long history of this knowledge among western scholars. For example John Claudius Loudon as early as 1831 reported:

“Manuring was held in such high esteem by the Romans, that immortality was given to Sterculius for the invention. They collected it from every source which has been thought of by the moderns, vegetable, animal, and mineral, territorial, aquatic, and marine. Animal dung was divided into three kinds, that produced by birds, that by men, and that by cattle. Pigeon-dung was preferred to all, and next human ordure and urine. Pigeon-dung was used as a top-dressing; and human dung, mixed with cleanings of the villa, and with urine, was applied to the roots of the vines and the olives. ‘M. Varro,’ says Pliny, ‘extols the dung of thrushes from the aviaries, as food for swine and oxen, and asserts that there is no food that fattens them more quickly.’ Varro prefers it also as a manure; on which Pliny observes, ‘we may have a good opinion of the manners of our times, if our ancestors had such large aviaries, as to procure from them dung for their fields.’ (Natural History, lib. xvii, cap. 9). Dunghills were directed to be placed near

69 Green, The Celtic World, op. cit., p. 183
the villa, their bottoms hollowed out to retain the moisture, and their sides and top defended from the sun by twigs and leaves. Dung usually remained in the heap a year, and was laid on [the land] in autumn and spring, the two sowing seasons. No more was to be spread than could be ploughed in the same day. Crops that were sickly were revived by sowing over them the dust of dung, especially that of birds, that is, by what is now called a top-dressing. Frequent and moderate dungings are recommended as preferable to occasional and very abundant supplies. Green crops, especially lupines [which are nitrogen-fixing plants, as are beans, peas, lentils, etc..] were sown, and before they came into pod ploughed in as manures: they were also cut and buried at the roots of fruit trees for the same purpose. Trees, twigs, stubble, &c., were burned for manure [and acted like lime to reduce acid in the soil]. Cato says, ‘If you cannot sell wood and twigs, and have no stone that will burn into lime, make charcoal of the wood, and burn in the corn fields the twigs and small branches that remain.’ Palladius says that ‘lands which have been manured by ashes of trees will not require manure for five years.’ (Lib. i.6.). [After harvest, s]tubble was…generally burned, as it was also among the Jews. Lime was used as a manure, especially for vines and olives. Cato gives particular directions how to form the kiln and burn it. He prefers a [kiln shaped as a] truncated cone, ten feet in diameter at the bottom, twenty feet high, and three feet in diameter at the top. The grate covers the whole bottom [and] there is a pit below for the ashes, and two furnace-doors, one for drawing out the burnt stone, and the other for admitting air to the fire…

Marl was used for manure according to Varro in Gaul where he saw “fields manured with white fossil clay.”

It is quite clear that all the peoples of Europe in contact with Rome would have known how to use manure and ash to fertilize their soils. This knowledge would have remained in their possession. Yet if, as Fowler suggests, this knowledge had been known in pre-Roman Britain, then when Caesar came to the island these processes would have been prevalent. Although Caesar’s accounts are often discounted by historians who assume his reports sent to Rome were entirely propaganda, to show the barbaric nature of the people he conquered, such as their use of sacrifice, he had no reason to falsify their agricultural behavior. It this respect Loudon reports:

“…In Britain, according to Caesar, agriculture was introduced by colonies from Belgium which took shelter from the encroachment of the Belgae from Germany, about B.C. 150. These colonies began to cultivate the sea coasts; but the

71 Ibid.
natives of the inland parts lived on roots, berries, flesh, and milk, and it appears from Dionysius that they never tasted fish. Pliny mentions the use of marl as being known to the Britons; and Diodorus Siculus describes their method of preserving corn, by laying it up in the ear in caves or granaries [which occurred in later Roman times].

“...But the general spread of agriculture in Britain was no doubt effected by the Romans. The tribute of a certain quantity of corn, which they imposed on every part of the country, as it fell under their dominion, obliged the inhabitants to practice tillage; and from the example of the [Roman conquerors’ ability to carry on agriculture], and the richness of the soil, they soon not only produced a sufficient quantity of corn for their own use and that of the Roman troops [garrisoned throughout the country], but afforded every year a very great surplus for exportation.”

There is no scientific evidence whatsoever to prove Fowler’s thesis valid, that around 1000 B.C., the Britons were carrying out manuring practices to regenerate their soils that had supposedly been greatly depleted by farming and climate change.

There is, however, clear evidence that the animals eaten and pastured at Skara Brae were of the types that were eaten and pastured in medieval Europe in post-Roman times. The midden surrounding Skara Brae contained bones of cattle, sheep, pig, etc. A great many of these were young animals, but there were also a sufficient number of full-grown adults, and their sizes give us an indication as to the period this village existed. With respect to the size of prehistoric domesticated cattle, Graeme Barker informs us:

“It has often been argued that cattle grew smaller under domestication, for cattle bones from early postglacial contexts in Europe and the Near East are as large as those of the late Pleistocene, whereas most cattle bones from neolithic and later prehistoric settlements were considerably smaller...However, the primary reason for diminution was probably ecological...At the same time human control could well have affected cattle size, as a result of restricted grazing and poor winter feed (rather than, as sometimes suggested, from the deliberate selection of smaller, more easily controllable, animals)...
“It is clear from both historical and archaeological evidence that cattle in Roman and medieval Europe were normally extremely small...Prehistoric cattle from neolithic, bronze age and iron age sites seem generally to have been as small.”\textsuperscript{73}

This is not quite the case. The Romans, because of their husbandry practices, were able to grow somewhat larger cattle, etc., but these were still relatively small compared to those earlier ones living in the wild. On this Ward-Perkins writes:

“But we do, from the West, now have some very revealing evidence for the changing size of stock animals. The careful recording of animal leg bones, recovered from datable excavated contexts, has allowed zoologists to estimate the changing size of livestock in different periods. The results are striking. Cattle and, to a lesser extent, other domesticated animals show a marked rise in average size in Roman times...These larger animals, like modern cattle, carried a much greater weight of meat than their pre-Roman ancestors. But to put on this weight they required intensive feeding on good quality pasture and, probably, winter fodder. These conditions could be achieved in an economic environment, like that of Roman times, that encouraged some specialization in the use of land and in the deployment of labor. But it appears to have been impossible to sustain this improvement in the more basic [impoverished] conditions of the post-Roman centuries. Cattle size fell back to prehistoric levels.”\textsuperscript{74}

Based on our thesis, prehistoric animals at Skara Brae and elsewhere should be quite stunted. With the collapse of Rome, conditions throughout most of the Roman empire were greatly impoverished [to be discussed in the next chapter]. But indeed the cattle at Skara Brae are inordinately large and do not reflect this impoverishment in this cold northern region of Scotland. Piggott and Thirsk explain:

“Jewell pointed out that few of the statements on prehistoric cattle made by earlier authors were based on adequate measurements and no description or measurements of the Windmill Hill ox had ever been published. He thought that on the available evidence there was no reason to believe that the Skara Brae...cattle were larger than other Neolithic cattle, as had been claimed. He re-examined and measured the Windmill Hill material and made comparisons with other measurements. From this he was able to state that Neolithic cattle were

\textsuperscript{73} Graeme Barker, \textit{Prehistoric Farming in Europe} (Cambridge UK 1985), pp. 30-31
\textsuperscript{74} Bryan Ward-Perkins, \textit{The Fall of Rome: And the End of Civilization} (Oxford 2005), p. 145
bigger than Bronze Age, Iron Age, and most medieval cattle. They were in fact as large as the bigger Roman cattle and modern breeds of medium size.”

This means that these cattle were grown at a time when agricultural development was as advanced as in the later medieval period. Therefore, they would be closely related to the more modern breeds and this, in fact, is the case, not only for cattle but also for sheep. Barker tell us at Skara Brae: “The sheep and cattle in the faunal samples were...slender; similarities between the Neolithic sheep of Orkney and the modern fine-limbed Soay [goat-horned] sheep of west-Scotland have been noted by several archaeologists... and in the historic period too—the most stunted animals in the whole of Great Britain...”

In a certain sense it can be argued that in both prehistoric and post-Roman Britain the animals were quite small, yet this proves nothing. We were however also told that the Neolithic sheep of Orkney exhibit similarities with modern “Soay sheep of west-Scotland” today and that the cattle are very large, suggesting they are much more modern. In fact one investigator, looking at the cattle, according to Robert Trow-Smith offered the proposition that they were post-Roman: “Prof. Watson, it should be noted, also adds the warning that he is ‘inclined to think that the Skara Brae cattle are post-Roman, but such a belief rests upon [archaeological] evidence far too slender to stand against the definite [interpretations of] archaeological data’ revealed by the excavations.”

A type of bovid was unearthed at Skara Brae that created a problem for its chronology similar to that of peat-ash found there a thousand years before peat grew in that area. A polled or hornless type of cattle skull was also found in the midden pile at Skara Brae, and this was a great surprise according to Grahame Clark and Stuart Piggott: “Domesticated animals included a polled breed of cattle without parallels elsewhere but clearly the result of careful selective breeding.” The contradiction for the established chronology is that polled cattle were only bred and became common, not in the Neolithic or Bronze Ages, but in the Iron Age, as reported by M.L. Ryder:

“Although there is a suggestion of a polled animal at Skara Brae, the first authentic hornless ox skull is from the Iron Age site of All Cannings Cross. Trow-

76 Barker, Prehistoric Farming in Europe, op. cit., p. 209
78 Grahame Clark and Stuart Piggott, Prehistoric Societies (NY 1965), p. 289
Smith likened this polled skull to the Aberdeen Angus [cattle type] rather than to the Galloway breed. Harcourt has found polled cattle skulls at Iron Age sites quite close to one another in Wessex. But many centuries were to elapse before polled cattle were common.”

In spite of the fact that the mutation for polled/hornless cattle is dominant, they still require “careful selective breeding.” But if these cattle were bred in post-Roman times, as we suggest, they would exhibit features quite similar to modern breeds that were more recently derived from these medieval ones. Trow-Smith discusses Celtic bronze handle mounts on buckets and cauldrons that depict hornless cattle. But then he rejects this evidence:

“This is an immensely tantalizing discovery [at Skara Brae]. No remains of polled cattle of earlier date have been found in Britain, and many centuries had to pass before the ‘hummelled’ [hornless] beast is encountered widely. It is possible—and the degree of probability can certainly be put no higher—that a hornless strain ran through one of these early breeds of Britain, but that the accidents of survival and accurate dating have concealed most other evidence of this…. [That is, the breed was somehow bred but then somehow lost. Yet no such evidence exists to prove this, only this highly conjectural probability.]

“It was the fashion of Celtic art of this period to cast the bronze handle-mounts of buckets and cauldrons in the form of ox heads; and, in fact, the fashion persisted into the brief revival of Celtic craftsmanship in the Dark Ages of post-Roman Britain. Such [polled cattle] heads are one of the common ornaments of this naturalistic culture… It is equally impossible to be certain that a lifelike casting, if such could be detected, was in fact the product of a British craftsman and copied from a British model [of polled cattle]. These qualifications greatly reduce the value of the evidence of these bucket-handle heads, but may not put it completely out of court.”

Although the idea that these cattle were bred for a time in the Neolithic Age and that their heads were depicted by the Celts in the post-Roman period would seem to suggest that one was derived from the other, this problem was then dropped. Nevertheless, Trow-Smith presents clear evidence that suggests just that, by showing that there were polled cattle heads on these bucket-handles and cauldrons

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80 Trow-Smith, *op. cit.*, pp. 32-33
'...whose presence in Britain at this time [that] has been amply confirmed by excavation...in...finds of bronze ox heads gives rise to a supposition that cattle of this kind were also found in Britain at this time, and that, like the now well-authenticated polled beast of [Iron Age] All Cannings...they still await discovery [for others in Iron Age Britain]. In fact, two types of polled oxen have been found on sites of this period, but [only] the All Cannings discovery has been widely quoted while that made by Ewart among the Newstead Fort remains have been ignored. Ewart found not only many of the *b. longifrons* type and crossbred variants of it, but also both the types of polled beast most common to-day...The neglect of these reliably dated remains from a site comparatively close to the modern homes of the Galloway and Angus [where these two breeds became popular] is remarkable in view of the speculation that has raged upon the origins of these two polled breeds of Scotland. Furthermore, Ewart detected a likeness between the polled Angus [breed] of Roman times and the Celtic shorthorn found with it which would suggest that the poll [breed] derived from a hornless strain of *bos longifrons*.'

What the evidence indicates is a continuity of cattle breeding from Celtic times with short-horned cattle into Roman times where the polled Angus breed forerunner existed, into medieval times of Skara Brae when the two early forms of polled breeds developed, giving rise to the two most famous forms, the Aberdeen Angus and Galloway polled breeds. Using the established chronology, what we have is a hodge-podge chronology wherein polled cattle are “carefully bred” in the Neolithic period at Skara Brae or elsewhere, then disappearing for several hundred years and not being encountered again until Roman times, then rebred in the late Middle Ages to become the modern breeds. There is no continuity, no understanding of how this development occurred. Everything is disconnected.

What historians and archaeologists have done is retroject their concepts of the forms of the animals they expected to find in the Neolithic Age onto Skara Brae fauna and therefore interpreted these forms to be exclusive to that place and time, when these forms belong to post-Roman/Saxon times. One theory holds that the Danes or “Baltic invaders introduced their polled cattle [in the 8th century A.D.]. [However,] Danish settlements in Ireland—lie above skulls of polled cattle that had been deposited long before the Vikings arrived. This suggests a preference for

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hornless cattle well back into the era when the Gauls (Celts) dominated…”

Obviously polled cattle existed in post-Roman Saxon times and were not imported.

The point of all this discussion is to show that Skara Brae could not have existed for about 600 years as a permanent community unless it could carry on forms of agriculture that allowed for this, namely liming and manuring. But it is evident that as a post-Roman village these agriculturalists would have known of these forms of cultivation.

Prior to radiocarbon dating creating these problems, the concept that Skara Brae was post-Roman enjoyed a period where it was considered a likely possibility. Childe et al. had concluded “…and so Skara Brae unfolds a picture of Stone Age life in the British Isles…The actual occupation of the village may certainly be comparatively recent, perhaps indeed post-Roman in date.” Tim Murray tells us “…It has been vociferously argued that the site [of Skara Brae] was post-Roman [by J.G.] (Callander 1931)” In his paper “Skara Brae: Its Culture and its Period” Callander summarized the evidence that existed in post-Roman Europe with the evidence discovered at Neolithic Skara Brae and found several common elements in both:

“The arguments for an Iron Age culture [at Skara Brae] are the occurrences of carved stone balls, generally admitted to belong to that time and of special types of beads and perforated pointed tools of bone that seem to have been fashioned by instruments of metal, and the burnished pieces of hematite [iron ore stones], stone pot lids, small thin blades of bone, large and small vessels from hollowed stones and vertebrae of whales…all of which belong to the Iron Age.”

**ANCIENT MODERNITY**

Each and everyone of these elements found at Skara Brae were common in early medieval Britain and Scotland. Thus, the living conditions at Skara Brae should have been noted in the literature as being immensely sophisticated for a Neolithic village and actually should clearly reflect medieval times. Statements regarding this modernity are fairly common. For example, Castleden specifically

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83 Childe et al., *Skara Brae…*, op.cit., p. 208
85 J.G. Callander, “Skara Brae: Its Culture and its Period,” *Proceedings of the Society of Antiquities of Scotland*, vol. 65 (December 8, 1930), p. 113
states: “Living conditions for ordinary people were apparently at least as they were in medieval Britain over four thousand years later as they were at Skara Brae, probably rather better.”

Why would people living in Neolithic Age Skara Brae live better than those of the early medieval period? They could only have done so, we suggest, if they lived in the late medieval period when developments permitted the people to do just that, live in a more advanced sophisticated manner.

Douglas Palmer with Paul G. Bahn and Dr. Joyce Tyldesley call the overall layout of Skara Brae “quite sophisticated” for the Neolithic Age. The word “sophisticated” appears repeatedly with respect to Skara Brae.

Giulio Magli encapsulates what happened:

“In the winter of 1850, a singularly violent storm slammed into the Orkneys and the wind stripped away enough earth to reveal a group of structures buried in a hill known as Skara Brae. A summary excavation was conducted in 1868, but it was not until the 1920s that a more complete study [by Vere Gordon Childe] brought to light a human settlement that, for lack of a better idea, we tentatively called a ‘village.’ Obviously [a well-settled village in the Neolithic times seemed impossible and] everyone at the time thought they were looking at an Iron Age site, and this assumption remained unchallenged until the early 1970s, when [bones used for radio]carbon dating blew it quite literally back to the Stone Age.”

What, indeed, were the sophisticated elements uncovered at Skara Brae that make it so modern? In Antiquity, vol. 5 (1931), page 53 we read…“one interesting conclusion to be drawn from the foregoing study of Skara Brae may be called ‘the antiquity of modernity.’” Blinded by radiocarbon, investigators have simply noted the sophisticated elements of modernity and then averted their gaze and mind to the immense implications these had for a supposedly backward people, newly arrived from primitive conditions. Knight and Lomas introduce us to the first of many such sophisticated elements:

86 Castleden, Stonehenge People, op. cit., p. 51
87 Douglas Palmer, Paul G. Bahn, Joyce Tyldesley, Unearthing the Past: The Great Archaeological Discoveries that have changed History (Guildford UK 2005), p. 68
89 Giulio Magli, Mysteries and Discoveries: Archaeoastronomy from Giza to Easter Island (NY 2009), p. 27
“At first sight the eight best preserved of the...apartments at Skara Brae look like a set from the *Flintstones* movie. They appear to be a Stone Age version of a modern housing estate, with a formalized regularity of layout. Some prehistoric architect appears to have planned the whole development so that stone versions of modern conveniences were provided inside each apartment...”

In essence this village complex exhibited something like a modern motel, a standardized structure with standardized conveniences throughout. One would generally conclude that people in the Neolithic built their houses from scratch and would not have developed a uniform architecture. But this may, nevertheless, have been possible and even probable. However, it is the conveniences that, to put it bluntly, are thousands of years out of place, and these are totally anomalous and unique for that early age, but not to that of the medieval period.

The first modern, sophisticated convenience was the use of bathrooms with toilets, connected to drains which carried human wastes via sewers away to the sea. Knight and Lomas explain:

“Surely, there had to be a very good reason indeed to choose to live on this island...Both the thoroughness of design and the quality of building at this site are simply breathtaking...Skara Brae was built complete with an underground sewage system. When one of the Skara Brae guides pointed...to a modern metal sewage cover[at the site] and asked Robert [Lomas] to lift it if he wanted to see the sewers, he did wonder if he was about to become the subject of a...practical joke. But this modern cap was protecting a 5,000-year-old, stone-built drainage channel which connected the house to an outfall at the sea edge. The drains were made of stone and had originally been lined with tree bark to make them water-tight. It was indeed a remarkably sophisticated system for its time.”

They go on to show that “when the contents of the drains were examined they were found to contain high levels of ancient human excrement...” Peter James and Nick Thorpe in their book *Ancient Inventions* report on this most remarkable convenience: “Sewers.”

“Almost as soon as people started living in towns...it must have become obvious that, unless something was done, they were going to be knee deep in their own filth. Early attempts to deal with this unwanted material presumably just involved carting it

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90 Knight and Lomas, *Uriel’s Machine...*, op.cit., p. 162
91 Ibid., p. 164
92 Ibid.
away and dumping it in the fields [for fertilizer], but this was a problem that, as cities became even more crowded, could not always be dealt with more simply.

“Nevertheless the honor of building the world’s earliest sewage system was not claimed by one the great urban centers of the ancient world; instead it belongs to the Neolithic villages of the Orkney Islands off the north coast of Scotland. At the beginning of the third millennium B.C., the inhabitants of sites such as Skara Brae had built drains fourteen to twenty-four inches high lined with stone slabs, running from toilets in separate small rooms within their houses. It seems as though the drains ran away under the settlement where they discharged their contents into the sea.\footnote{Peter James and I.J. Thorpe, \textit{Ancient Inventions} (NY 1995), p. 359}

Of these other sites, Spencer Hornsey tells us: “Similar drainage systems have been found at the Braes of Rinyo, on Rousay, Orkney…and at the settlement complex at Barnhouse.”\footnote{Ian Spencer Hornsey, \textit{A History of Beer and Brewing} (Cambridge UK 2003), p. 197} Ritchie in discussing another site at Gurness claims “each unit has a…lavatory recognizable by its stone seat.”\footnote{Ritchie, \textit{Prehistoric Orkney}, \textit{op.cit.}, pp. 105-106}

Skara Brae was clearly not a large settlement and its people could easily have left their filth in the nearby fields to act as fertilizer. With a population estimated from between 50 to 80 people, they had to go out of their way to construct at great cost and effort a sewer system attached to what were apparently separate toilet rooms, thousands of years before such conveniences had become common in the first millennium cities and towns in the ancient world. Scotland was relatively close to Hadrian’s Wall which had Roman garrisons housed along it, and these had toilets and sewers as pointed out by Fiona Reynoldson who comments on “a modern artist’s reconstruction of toilets on Hadrian’s Wall. Water ran through a channel under the seats to wash away the sewage.”\footnote{Fiona Reynoldson, \textit{Medicine Through Time} (Oxford UK 2002), p. 33} People who lived in Roman Britain or in Scotland would have encountered these toilets and sewers and would have known of them. The question that never seems to occur to those imbued with the established chronology is: why didn’t these conveniences continue through to the Bronze Age and down to and before the Romans invaded Britain? Surely such an important accomplishment would have spread all across Britain in pre-Roman times. It simply didn’t. Toilets with sewers do not come out of nothing, then flourish and disappear into nothing. Like so much else the chronology of this modern convenience is an anachronism.
We now turn to the construction of the houses at Skara Brae; since there was little or no wood available, stone was employed. Quarried flagstones were used, but, interestingly, these were laid like bricks to form corbelled walls that tapered near to the roof which was covered by either driftwood or whalebone and was then sheathed with material or, as Michael Reed writes: “The houses were built of stone and corbelled upwards for about 10 feet and then roofed over.”

Allison Lee Palmer further explains: “The most famous masonry structures from this time are found in the village of Skara Brae…they reveal some of the earliest corbelled walls…Corbelling is also found in the extremely sophisticated burial mounds from this era…”

Furthermore, the houses at Skara Brae were inordinately similar to one another and to others in the region as reported by Knight and Lomas: “Anna Ritchie assessed the commonality of design and construction between houses of Skara Brae and other houses uncovered at Rinyo, in the Isle of Rousay and at Barnhouse. She reasoned that the idea of identical houses built to a common plan is a modern idea normally associated with saving costs, but that this could not be accepted as an explanation of the similarity between the houses…although she did conclude that there may have been a specialist class of masons.” The point is that a class of masons makes no sense in the Neolithic Age. People were primarily farmers who could not support a class/group of specialists who spent their time constructing uniform buildings. This form of specialization could only exist at a time when food production was not that of subsistence agriculture, which is what Neolithic farming suggests. Building uniform constructions is also a clear indication of the very same cultural development and would exist in the later medieval period, say around A.D. 800 to 1100.

Beyond this, not only were the houses made of quarried flagstones, they were plastered over on the outer walls with clay for insulation. Peter Neal Peregrine and Melvin Ember explain that the houses at Skara Brae were “plastered with clay on the outer face.” Michael Aston, Mick Aston and Tim Taylor add: “The walls [of Skara Brae] were…plastered with clay…, some fragments of which survive to make them more insulated…” The people of Skara Brae, before they built up a midden

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97 Michael Reed, *The Landscape of Britain* (London 1990), p. 43
99 Knight and Lomas, *Uriel’s Machine*, op.cit., p. 165
pile of ashes and rubbish around their houses to further insulate them, plastered the outside walls, and this supposedly during the Neolithic Age by builder specialists.

To enter their houses they had doors with holes in the adjacent walls in which to insert a wooden board or long bone to bolt it from the inside against intruders and for privacy. According to Knight and Lomas: “Each of the houses had a large room with a single entrance fitted with a bolt-securing hole cut into the stone to lock a door from the inside—except house seven which, rather curiously, has a door that is designed to be bolted from the outside.”¹⁰³ This type of door-locking bolt was used throughout Skara Brae. On top of having doors with bolts, doors elsewhere were found that were actually held in place by hinges. As Ritchie *et al.* show, “An exciting discovery in recent years was a large rock-cut pit and sand fiord…within sight of Skara Brae. The pit contained a large box built into the flag stone on one side of which was ingeniously ‘hinged’ to allow easy access…”¹⁰⁴ What is quite unusual is that Ritchie tells us: “The cells of house 1 are particularly interesting as on the left entrance is a cell from which the bar for the door was operated and which had a ‘peep-hole’ into the passage outside.”¹⁰⁵ With respect to doors, Castleden shows one house had a door above the lower entrance: “the walls of the splendidly preserved House 7 survive almost to roof height at 3 metres [10 feet]. There is a doorway above the [lower] entrance and [with it] an upper level passageway to provide access to it, so there is every reason to suppose that this house had an upper floor [second story].”¹⁰⁶ That one house at Skara Brae was a two storied building in Neolithic time!

Once the inhabitants passed through these doors they walked into a room with flagstone flooring throughout. Childe describes this condition: “The floor was composed of laid clay, supplemented by paving slabs of slatey flag[stone].”¹⁰⁷ All the architectural refinements, we are being asked to believe, were built in the Neolithic Age in Scotland. The interior furnishing and appliances of these houses were no less modern. Knight and Lomas give us the following list showing that

“…like the Flintstone residence, all [the Skara Brae houses] were equipped with the following set of desirable ‘mod[ern] con[venience]s:

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¹⁰³ Knight, Lomas, *op.cit.*, p. 164
¹⁰⁴ Anna Ritchie *et al.*, *The Ancient Monuments of Orkney*, *op.cit.*, p. 20
¹⁰⁵ Ritchie, *Prehistoric Orkney*, *op.cit.*, p. 33
¹⁰⁶ Castleden, *The Stonehenge People*, *op.cit.*, p. 33
¹⁰⁷ Vere Gordon Childe, *Scotland Before the Scots* (London 1944), p. 28
“A [large oblong] central stone-hearth with a [low] kerb [stone wall around it] to retain the fire • A large stone dresser with two shelves supported on three large stone legs • A rectangular stone chair • Two stone bedsteads, with large end stones, which Professor Childe has suggested would have supported a canopy [to provide privacy and keep in heat] – A Stone Age four-poster bed, no less! • A stone water tank with its seams packed with clay [to keep bait fish or perhaps wash clothing or utensils in] • Storage space[s] consisting of small stone boxes and cells let into the floor and walls.”

In House 8 with its door-bolt on the outside there was “a kiln.” Merryn Dineley calls this an “oven.” Nor was this oven unique to Skara Brae. Clarke and Maguire state that “small ovens have [also] been found at Rinyo,…a settlement similar in date and character to Skara Brae…”

There is also evidence that the people of Skara Brae played games of dice. Childe writes: “They played games with dice and knuckle-bones.” While it is suggested these may be beads, Childe et al. say “all have different groups of dots on two, four or even six faces…Our objects may be rather cubical beads.” Elizabeth Gaynor et al. further explain that these were six-sided dice or cubes: “Among the artifacts archaeologists found at Skara Brae were two bone cubes with markings like modern dice.”

As for the roofs of these houses, nothing has survived, nor have roof slates at Skara Brae been found that collapsed into them. This means that organic materials were used to roof these dwellings. It is assumed that either driftwood or whale bones, or both, were laid across the roof opening, tied together with rope, and covered with skins. Only evidence of rope has been recovered according to Burl. “Two twisted skeins of heather and of cranberry stems from the famous Neolithic village of Skara Brae [were found] being very like the ropes known as ‘simmons’ used until quite recently in northern Scotland.” That is, the type of rope found at Skara Brae is “very like rope” used “quite recently.” Surely, over thousands of years ropes would have been greatly improved in Scotland and found thereafter everywhere.

108 Knight, Lomas, op.cit., pp. 164-165
109 Hornsey, A History of Beer and Brewing, loc.cit.
110 Merryn Dineley, Barley, Malt and Ale in the Neolithic (Oxford 2004), p. 58
111 Clarke and Maguire, op.cit. p. 22
112 Childe, Scotland Before the Scots, op.cit., p.33
113 Childe et al., Skara Brae..., op.cit., p.167
114 Elizabeth Gaynor et al., World History: Connections to Today (Upper Saddle River NJ 1997), p. 11
115 Burl, Prehistoric Avebury, op.cit., p. 159
It is also quite possible that the roofs there were thatched with straw or, more probably, heather. Heather Duncan tells us:

“Heather moors cover a vast amount of the Scottish countryside, approximately two to three million acres in the east and only slightly fewer in the south and west. It is without doubt one of Scotland’s most prolific and abundant plants…”

“Heather has always played an important role in the traditional construction of buildings, particularly in areas such as the Hebridean Islands where it was used [among other things for] roofs…”116

It is thus probable that the roofing at Skara Brae was thatch made of heather. Of course, thatch of straw was a common component of house roofs in the Middle Ages. C.R. Wickham-Jones states that beside dice at Skara Brae there was found a variety of jewelry, “including different types of bone and shell beads, tusk pendants and decorative bone pins.”117 The most remarkable aspect of these, according to Anna Ritchie, were “bone necklaces from Skara Brae that [show] the stringing was modern.”118 Lastly Caroline and Arthur Arnold report “Hundreds of tools have been found at Skara Brae. They include axes for chopping, mattocks for hoeing, knives for cutting, and scrapers for cleaning skin.”119

Since Skara Brae, in the remote northern region of Scotland, built these conveniences in stone which have survived, we can gauge that in regions to the south in Britain and Ireland, where wood was available, these very same conveniences were constructed from wood. That is, throughout this vast area the supposed Neolithic people in this part of the world quite clearly lived in a most advanced technological state compared to all others of the prehistoric world and then, around 1500 B.C., they vanished into oblivion. All in all the inhabitants enjoyed a standard of living that Castleden admitted, above, “living conditions for ordinary people were apparently at least as they were in Britain over four thousands years later as the they were at Skara Brae, probably rather better.”

Childe also seems to have discovered a sociological aspect of the houses at Skara Brae that is also reflected in modern times, related to the place in the home

118 Ritchie, *Prehistoric Orkney, op.cit.*, p. 36
119 Caroline and Arthur Arnold, *Stone Age Farmers...,op.cit.,* p. 21
of men and women. E. Estyn Evans describes the nature of this space relationship of men and women to the house:

“At Skara Brae...Professor Childe found evidence that the women occupied the left side of the fire and the men the right. In the Hebridean black-houses the left-hand side of the fire was the woman’s quarter. Similarly in the old turf houses of the Highlands, ‘the women were invariably ranged around the central fire on one side, the men on the other...In the humblest Irish farms, which lack sanitary conveniences of any kind, it is customary for the women to use the byre and the men the stable [as a latrine].”

But this sociological condition died out when the Megalithic Age ended and the people disappeared. Yet we are expected to believe that this organization of the house between men and women was revived about 1500 years later out of nothing. Such continuity indicates that these practices have a much shorter history than that proposed by proponents of the established chronology.

Altogether their substantial stone houses were plastered on the outside with clay to keep out wind, cold, dampness, insects, etc. They entered their houses probably through wooden hinged doors, possibly with peep-holes, and had bolts on the inside to lock out intruders to maintain safety and privacy. When they entered these large rooms they did not walk on earthen floors, but on flagstone pavements with underlying clay. Their homes were heated by large rectangular central hearths with kerb stones about them to keep the ashes from spilling out. Around their fires they sat in chairs, probably covered by materials for their comfort, and slept on beds raised above the floor, clearly covered by materials that acted as mattresses and blankets. They cooked in pottery vessels which they kept in cupboards and placed their household possessions in dressers or boxes in the walls or on the floors. In the house they had a water tank whose purpose is still unknown. When they needed to evacuate their wastes they could go to a side room with a toilet attached to a drain system which carried away their wastes. For leisure they probably played games with dice. They ornamented themselves with beads that exhibit “modern stringing” and other materials. For tools they had axes, mattocks, knives, and scrapers. According to J.G. Callander, above, most of these tools “belong to the Iron Age.”

Agriculturally, they raised polled cattle that are common long after the Neolithic Age. Their cattle and sheep were closely related to modern Scottish

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breeds. Because they were rather close to modern breeds, they were in fact “bigger than Bronze Age, Iron Age, and most medieval cattle.” They clearly were using manure to fertilize their fields, and had heavy plows to cultivate the heavy lowland soils where they lived. In other words, their agricultural technology was sophisticated and as highly advanced as their houses and living materials. And all this, too, without a scintilla of a cause, simply vanished instead of maturing into an even more advanced culture. In addition they were carving hard stone balls with some kind of metal tool that didn’t exist in the Neolithic Age, and were burning peat that also didn’t grow there in the Neolithic Age, leaving a reddish ash. But there is clearly no doubt that such amenities and technologies existed in medieval times, especially from after A.D. 600 to 1000. Socially, men and women seem to have been separated to different sides of the house, as in modern times.

However, before correlating these technologies with the medieval period, we wish to discuss another aspect of the lives of these people which clearly belongs to the Middle Ages, namely the possibility that they were brewing beer, mead, or other intoxicating drinks which they drank from their beaker mugs and kept in their larger clay pots. No doubt many of these small beakers were used for drinking purposes. Max Nelson outlines this evidence:

“The supposition that the discovery of beer was made independently in prehistoric Europe seems to be confirmed by recent archaeological finds. Beginning in the third millennium BC clay vessels, so-called Baden and globular amphoras, corded ware, and bell beakers, were to be found throughout all of Europe, usually in sets. This is our first evidence for some sort of European drinking tradition which was spread far and wide. It is usually thought that this was a secular drinking tradition involving beer or mead, but chemical, botanical, and pollen analysis of vessels has gradually been forming a more complex picture of the beverages being consumed at this time.

“A good amount of evidence has been surfacing from various Neolithic and Bronze Age sites in Scotland from the early second millennium BC on…On fragments of pottery that may be as old as about 3000 BC at the site of stone circles at Machrie Moor on the Isle of Arran in Scotland traces of cereals and honey among other organic materials were found, which may suggest that they were used as containers for some type of beer. Other finds point to a variety of ingredients used for fermentation. Thus on fragments of grooved ware from Balfarg, Glenrothes, Fife, Scotland, dating to the third millennium BC there was found the residue of cereals, pollen, and meadowsweet, as well as henbane. This seems to point to a type of flavoured honey beer [i.e. mead] with potentially
dangerously \[sic\] effects. Meadowsweet, a fragrant wild perennial plant with creamy white flowers and dark green leaves, common in meadows and damp areas in Europe, was perhaps used more for its preservative effects (as it has been used in brewing in more recent times) than its taste. It is perhaps mentioned as an additive to wine by later authors, but nowhere is it connected to beer in the ancient literary sources. On the other hand, henbane is a biennial plant found in dry soil, which is narcotic and can lead to convulsions, insanity, and death.”¹²¹

As for Scotland, Nelson adds…

“Henbane…from a much later period (around 600 to 400 BC) has…been thought to have been perhaps used as a beer additive; otherwise, mixed drinks of cereals, honey, and meadowsweet (with other ingredients) are well attested from elsewhere in Scotland as well as in Denmark. At Kinloch, on the Isle of Rhum…in Scotland, Neolithic pot sherds from around 2000 BC were found to contain the residue of mashed cereal straw, cereal-type pollen, meadowsweet, types of heather…and royal fern, thus most likely having come from a type of beer. In a cist burial (from around 1600 to 1500 BC)...in Scotland a beaker was found to contain traces of cereals along with meadowsweet. Similarly, in another Bronze Age cist burial [in] Fife, Scotland, remains of lime and meadowsweet pollen found in a beaker have been interpreted as having come from a mead or a honey beer made from lime honey and flavoured with flowers of meadowsweet. A similar find was made in a Bronze Age grave…in…Denmark. A birch bark bucket was discovered containing traces of lime, meadowsweet and white clover pollen, wheat grains, sweet gale, cowberry, and cranberry. This was probably a beverage which included fermented honey, fermented wheat, and fermented berries.

“This evidence shows us how complex early prehistoric beverages were in northern Europe, since various sources of sugar were apparently fermented at the same time, with no regard to such neat categories as wine (fermented fruit drink), mead (fermented honey drink), or beer (fermented malted cereal drink) as we might have today, and even with the addition of such potentially harmful ingredients as henbane…For now it seems quite certain from the early date of the finds as well as the general paucity of evidence for such mixed beverages in the east that the fabrication of these sorts of beverages was not passed on from the east but developed independently in Europe.”¹²²

Burl describes what a brewer requires to make beer:

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¹²² Ibid., pp. 12-13; see also Hornsey, A History of Beer and Brewing, op.cit., pp. 209-211
“All a Neolithic brewer needed was barley for cakes to be mixed with water and yeast to be fermented into beer…With capacities up to six gallons [24 litres] the pottery has been found all over Britain. On Orkney [the site of Skara Brae] the vessels were ‘heavily gritted with volcanic rock, which would have allowed them to withstand the constant heating and cooling necessary for a successful brew.’ Adding meadowsweet extended the drinkable condition of the beer for several weeks. The weed was plentiful on Orkney.”

The researchers Merryn Dineley and Graham Dineley investigated Skara Brae and especially Hut 8 which was obviously a workshop without beds or dressers and a door bolt that locked it from the outside with an inside kiln; their conclusion was:

“The inhabitants of Skara Brae certainly had the equipment and suitable buildings for making malt and ale from the barley grain that they grew. In the building that Childe named ‘Hut 8’ they had a well-crafted and versatile Grain Barn for dry storage, threshing, winnowing and malting as well as a kiln for drying the malt. They had large pots with lids, suitable as fermentation buckets, and a drainage system. Other Neolithic sites on Orkney were also suitably equipped. The conversion of bere barley into sweet malts and ale was an important aspect of Orcadian life, and of the British Isles during the Neolithic.”

While this concept of beer making is not proven, in terms of the short chronology it does reflect the beer drinking culture of post-Roman Britain and Europe. All these developments existed in medieval times. With respect to this concept, Jeffrey R. Wigelsworth tells us:

“Beer has been made and drunk since the earliest civilizations in Mesopotamia [where grain was cultivated]. Beer was a major component of the medieval diet; it was drunk at most meals, including breakfast…

“Medieval beer production took place mostly at the household level. This often meant that the women of the house made beer as part of their everyday chores. Of these operations, the largest were those carried out in monasteries during the birth of the monastic movement around the eighth century C.E.”

123 Burl, *The Stone Circles of Britain, Ireland and Brittany*, op.cit., p. 74
125 Jeffrey R. Wigelsworth, *Science and Technology in Medieval Europe* (Westport CT 2006), p. 17
William A. Hardwick further reports:

“It appears that much of early Europe drank beer instead of water and the benefits of beer were widely written about during medieval times. Countless thousands of people must have survived harsh times and poor food because beer was available...[Beer makes] significant contributions to the nutrition of both young and old by providing protein supplementation and B vitamins.”

While beer made with hops became an innovation in Europe around 1200-1600, earlier beers in the medieval period were not brewed with this ingredient.

Let us now turn to Skara Brae as it is a clear reflection of the medieval period which had the same construction properties and furnishings. Before doing so, it must be pointed out that when dealing with Skara Brea on Orkney we have an area generally so devoid of timber that they used stone materials for construction while others elsewhere of course used wood. Of further importance is the fact that this region was much more isolated from the larger population centers of medieval Britain. The people living in this outer region would tend to be somewhat more backward and cruder. What we must also keep in mind is that the strata given to the Neolithic, Bronze, and Early Iron Ages, we maintain, belong to the post-Roman/Saxon period for which, as we have shown above, nothing or very little is archaeologically known, especially from 500 to 900 AD. What we are thus forced to do is use the later part of the medieval and retroject that evidence to this earlier period in order to show the close correlations between these periods.

To begin with, it is assumed that during the Neolithic and Bronze Age there was no manorial system with lords and serfs and that the houses constructed would not reflect such a class division; houses should be small and never of a size that would be the household of a lord. But unique evidence of such a house did survive in Ireland that contradicted this belief as Andrew Hayes points out:

“One Neolithic house for which there is sufficient evidence to allow a reconstruction was excavated at Ballynagilly (Co. Tyrone [Ireland]). It was roughly square, 6 by 6.5 m (20 by 21 ft), about the same size as a traditional peasant house [like those at Skara Brae]...Until recently it was assumed that all houses of the early farmers were as humble. Then, in 1977, the remains of a large

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126 William A. Hardwick, History and Antecedents of Brewing (NY 1995), pp. 42-43
timber hall [that somehow survived in the earth] measuring 26 by 13 m (85 by 43 ft) were excavated at Balbridie…From aerial photographs the excavator was confident that he was uncovering a hall belonging to the Dark Ages [when such manorial buildings were constructed], but then sherds of unmistakable Neolithic pottery were discovered together with organic material that was dated to c.3500 bc. Rather than housing a Dark Age lord and his retinue, the Balbridie hall may have sheltered an extended family of the first farmers.”

Had that house not contained so-called Neolithic pottery, the excavator would have published this find as a medieval manor of some great lord. It is impossible for archaeologists to accept that this is actually just such a manorial building, even though its dimensions are so extraordinary. Yet the excavator believed its dimensions indicated that “he was uncovering a hall belonging to the Dark Ages.” This wooden building somehow survived deterioration in the earth. Perhaps in time others may be found that again indicate the same lord–peasant relationship of the Middle Ages.

Let us recall the layout of Skara Brae, which was that of a small village of single room cottages with squarish rooms that have corbelled rounded corners, containing a large central, open, rectangular hearth with kerb stones around it and furnished with beds, cupboards, a dresser, and toilets with drains. This being the case, we should expect to find that medieval houses or cottages largely reflected just this type of arrangement, and that is just what we find. According to David Iredale and John Barrett “The medieval house was a rectangular hall—a single room in which the whole family ate, slept, lived, and died. This was a single-pile structure, one room deep from front to rear. Throughout the year a fire of logs or peat [as was burned at Skara Brae] burned in an open hearth in the centre of a floor of beaten earth, cobbles or flagstones [such as those flagstone floors at Skara Brae].”

R.J. Brown echoes this: “The excavations of deserted medieval settlements have produced some evidence as to the construction of these cots. Generally they were of one room, although some of the larger ones had two rooms; the most common was about 16 by 12 feet…They were [like Skara Brae] generally filled with smoke from a central open hearth.” With regard to these central medieval hearths, Brown further reports: “The medieval house, whether a manor house or a simple cottage, never had a fireplace or chimney-stack. In all cases the fire was placed centrally…with the smoke escaping as best it could through the roof and window…Brick or stone chimneys were exceptional until the fifteenth

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Françoise Piponnier further shows with respect to Mediterranean peasant houses of the late medieval era:

“Some [medieval] cottages had more elaborate hearths, which were designed to confine the fire within a well-defined area. These might be dug down into the earth or raised slightly above it or somewhat like that [as at Skara Brae]; they might be lined with bits of flagstone driven into the ground at an angle. Peasant hearths were generally small. The hearths [unlike those at Skara Brae] were nearly always placed against a wall or close to a door.

“The…home of a minor noble…demonstrated the stark contrast between the life of the peasant and that of the lord. It contained [as at Skara Brae] a large rectangular hearth nearly two metres (more than six and a half feet) on one side lined with bricks. This hearth [like those at Skara Brae] was located in the centre of the room.”

Although very little of wooden materials survives in the earth, excavations carried out at Durrington Walls, near Stonehenge, between 2003 and 2007 by Mike Parker-Pearson found evidence of a cluster of wooden houses quite like those at Skara Brae with similar furnishings, as presented by Marc Aronson and Mike Parker-Pearson. These “had similar furniture in wood whose only surviving traces were long straight slots to hold [wooden] beams that formed the edges of beds and dressers.” Thus we can see that house types like those at Skara Brae were being constructed in wood throughout Britain. Timothy C. Darvill describes the construction form and furnishings of these houses and the segregation of men and women there as well:

“In southern Britain, clusters of houses similar to those at Skara Brae but made of wood rather than stone have been found. Excavations here in 2003-2007 by Mike Parker-Pearson revealed six houses [as compared to eight at Skara Brae]. All were square in plan…Dating to around 2500 B.C., the walls were marked by lines of stake holes suggesting a wattle and daub construction. All had a central hearth and chalk plaster floor…, while internal slots and stake holes suggest furniture and

131 Ibid., p. 236
fittings. Analysis of the finds shows that cooking took place on the [women’s] south side while flint tools were mainly found in the [men’s] north-east corner.\textsuperscript{134}

Skara Brae and this cluster of cottages at Durrington Walls which survived the ravages of time present us with an excellent set of furnishings to compare with those of the later medieval period for which we have evidence. We have shown above that at Skara Brae “two bedsteads were found which Professor Childe had suggested would have supported a canopy [to provide privacy and keep in heat]—a Stone Age four-poster bed, no less.” This type of bed was common in the medieval period as explained by Paul B. Newman, an authority on the Middle Ages, in his book \textit{Daily Life in the Middle Ages}:

“Beds in the households of the middle class and wealthy appear to have most often been canopy beds with heavy wooden frames. These bedframes sometimes had large posts at each corner or [like those at Skara Brae] had solid head- and footboards that reached from the floor to near the ceiling like small walls. In some instances, the frames supporting the canopies appear to have been attached directly to the ceiling and were structurally independent of the bed itself. Some canopy beds of this type were made with headboards but without footboards [which allowed for a slanting canopy over the bed]. Canopies on these beds could cover the entire bed but often only covered the upper half of the bed...The drapes of the canopies served two functions: In cold weather, they created sort of a cozy ‘room within a room,’ protecting the bed’s occupants from drafts. Year round, the drapes provided privacy. Needing privacy in one’s own bedroom may sound odd but bedrooms in the Middle Ages had many occupants. These other occupants routinely included children of the occupants of the bed as well as a servant or two sleeping in smaller beds or pallets on the floor though, unless the occupants of the bed were engaging in intimate activities that required seclusion, small children often slept in the same bed as their parents. This practice appears to have been especially common in middle and lower class households in which there was typically only one bed for the entire family. In these families, as the children grew older, they likely moved to pallets on the floor.”\textsuperscript{135}

Related to this bed arrangement was the segregation of beds in certain houses to those of women and those of men as shown by Hayes for Skara Brae: “On either side of the door were stone boxes that once, filled with bracken and heather, served as beds. That on the right-hand side was larger than on the left hand side. Similar sleeping arrangements, the larger bed being used by the master of the house, the

\textsuperscript{134} Timothy C. Darvill, \textit{Prehistoric Britain} (London 2010), p. 143
\textsuperscript{135} Paul B. Newman, \textit{Daily Life in the Middle Ages} (Jefferson NC 2001), pp. 65-67
smaller by his spouse, were to be found on the crofts of the Hebrides [of Scotland] into modern times.”  

What we have is a clear connection of the sleeping arrangements at Skara Brae, to those of the Middle Ages and to those of Scottish crofters into modern times. When we turn to toilet and sanitation systems, there is much in common between Skara Brae and the medieval period, especially with those found in monasteries. Again according to Newman:

“Even in the earliest centuries of the Middle Ages, monasteries were constructed with both sewer and water systems...[T]he monks had a driving need for these amenities because they were relatively large communities, numbering from dozens to hundreds of people living in the same quarters year in and year out. To keep these quarters habitable and disease-free, they had to have reliable water supplies for cleaning themselves and their surroundings and flushing away the waste.

“Like most toilet facilities throughout the Middle Ages, monastic sewer systems were often very basic, with latrines that emptied into shafts through which the waste flowed into a cesspit or, more desirably, into the nearest river to be carried downstream. In fact, a good supply of running water for this and other purposes appears to have been a key factor in selecting sites for many monasteries. At least one monastery on a coastline [just like that at Skara Brae] refined this idea further and had the latrines empty out into an area that was flushed out twice a day by the rise and fall of the tides.”

Of course this was not the only form of removing sewage during the Middle Ages, but it is extraordinarily similar to that found at Skara Brae. For a larger description of these medieval toilet and drainage systems for the medieval period see Paul B. Newman’s Daily Life in the Middle Ages.

At Skara Brae we have shown that the outer walls of the buildings were originally plastered with clay to keep out drafts and dampness. With the passage of time, peat ash and other materials were piled up around these walls to act in the same way. This caulking, too, was a common building practice in the Middle Ages. Jeffrey L. Singman in his book Daily Life in Medieval Europe reports:

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136 Hayes, op.cit., p. 43  
137 Newman, op.cit., p. 139  
138 Newman, op.cit., pp. 139-143
“Sometimes the vertical [wall] timbers were set into postholes in the ground, which improved the building’s stability [compared with those that were built on top of the surface], but reduced its lifespan by allowing moisture from the ground to soak up into the timbers, causing them to rot over time."

“The materials used to fill [the openings] in the walls depended on local resources. Wood was generally too valuable for this purpose, except in heavily forested areas...In some areas, clay was used as wall filling, in others, slabs of turf.”

We well know that peat was the fuel used at Skara Brae and so, too, was it employed as fuel in medieval times, as shown by Nancy Edwards: “Peat remains a major source of fuel and was also exploited in the early middle ages, together with wood. Traces of [red] peat ash have been recovered from...ring-fort[s]...” In the later Middle Ages, as towns grew larger and larger, peat became the major source of fuel for many houses, rather than more valuable wood. In fact “Peat was widely used as a fuel, to the extent that large areas of the Norfolk Broads [where it was mined and still is even now] consist of flooded medieval peat workings...” During the Industrial Revolution, and even earlier, coal was mined and largely replaced peat in the great towns and industrial factories of Europe. At Skara Brae and elsewhere, peat was available and its use is obvious. The chronology of peat as the fuel for Skara Brae, discussed above, is in clear contradiction to the established chronology.

With regard to the household furnishings at Skara Brae, we were told, only the stone elements survived, the others of wood, basketry, or cloth obviously were completely destroyed. However, in terms of their relation to later furnishings, Olive Cook and Edwin Smith say: “This Skara Brae furniture is perhaps closer in feeling to the slate shelves, mantle pieces and lavatory seats of some Welsh houses...” Clearly Knight and Lomas found Skara Brae to be best described as the homes of the Flintstone people, which reflected its more modern features. As for the storage features in medieval times, Singman tells us: “Storage was provided by simple wooden chests, caskets, cupboards, straw baskets, and coarse fabric sacks...” Bernard Cotton writes that at Skara Brae “...in each of the seven huts [there existed] an imposing structure consisting of two tiers of deep stone

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143 Singman, *op.cit.*, p. 85
shelves...In the 19th century crofts on Orkney [also had] recesses with stone shelves for storage of kitchen utensils and provisions.”

Then there is the workhouse, not attached directly to the village, without beds, but with a kiln and locked from the outside. It was clearly a work place, probably for making beer. This workshop building was common in medieval villages, as described by Singman:

“A craftsman such as a smith would have a workshop. Well-to-do peasants might have other buildings as well. If the wife did any brewing, additional buildings were needed for this purpose: a malt kiln to sprout and dry the grain, and a brewhouse to boil the water and wash it through the malt. She might also have a bakehouse to hold an oven.”

As for the doors barred by a wooden beam fitted into a slot on either side of the door, Castleden shows that “for a long time in prehistory and later Europe the only real security for homes was a substantial bar fitting into iron brackets on the door jambs [or into slots cut into the walls]. This method was almost universal, for the dry stone Neolithic houses of Skara Brae [and] to the doors [and gates] of great medieval castles.” This type of lock was used up to the present as Brown reports: “The age-old wooden bar...placed across the door was the most common lock [in the Middle Ages] and continued to be used in many [English] cottages this century, and probably still is in use in many country areas.”

He further tells us that, like the doors at Skara Brae, “early doors had no frames, the actual doors being hung directly into the opening, whether timber or stone.”

From floors to clay-insulated walls, to single-roomed cottages, to paved floors, to wooden bar-bolted doors, to rectangular central open hearths with kerb stones to keep in ashes fueled by peat, to canopied beds, to toilets with drains, to work houses for making beer or carrying out other chores, to segregation of the sexes, to the very small size of these villages where Skara Brae had eight houses, Durrington Walls six houses, to the types of furnishings: Skara Brae was clearly a medieval village. It is a magnificent anachronism/anomaly to what would be expected for the Neolithic Age. Its accoutrements correlate with its agricultural

145 Singman, *op.cit.*, p.86
147 Brown, *The English Country Cottage, op.cit.*, p. 251
development and also reflect a medieval village. Childe et al., looking at the evidence they excavated there, drew the correct conclusion that this village was post-Roman. Only by turning a blind eye to the evidence of these numerous medieval characteristics on so many levels can one suggest Skara Brae is of the Neolithic period. Furthermore, all its developments show a clear evolution into later times, even into the present. Taken altogether this development and evolution is hardly fortuitous. The excavation of a manorial house of great dimensions dated to the same period as Skara Brae is additional evidence that the people assigned to the Neolithic, Bronze and early Iron Ages belong to the medieval period when such manorial houses were constructed. Add to this the agricultural and domesticated animal evidence, we have very broad and extensive evidence that fits and supports the short chronology. The proponents of the established chronology have nothing of the kind! In fact E. Bickerman finds that the contrasts between the different sets of chronologies indicate “chaos reigning in the medieval dating.”

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CHAPTER 11
THE FALL OF ROME, THE PLAGUE, CATASTROPHE AND DARK AGES

The picture we have painted of the post-Roman period running roughly from around A.D. 500 to 900 is one of a people living in near-Neolithic conditions. The question raised by this thesis is: why and how did advanced ancient peoples go back to the Stone Age after having experienced the high standard of living of Roman civilization? The explanation for this immense change has puzzled historians for centuries. What we have is a highly civilized/advanced society enjoying a high standard of living, being relatively literate, building fine cities, roads, aqueducts, using iron, bronze, copper, carrying on trade over vast regions, employing the medical arts and enjoying the goods of the world, descending into being a backward society that was almost entirely illiterate, with very few centers, mostly made up of tiny villages, with few if any roads, no aqueducts, very little metal, no medical arts, and living on the brink of survival in subsistence conditions. This descent into the Dark Ages at around the end of the Roman empire left its people in a state of stark poverty, a poverty so great that it by contrast shows that we are essentially dealing with two worlds so different in their development, economic and cultural make up, as to be totally unrelated to one another. On what he terms “the Medieval Problem,” historian John J. O’Neill states:

“The era after the collapse of Rome was originally described as medieval, or dark, because so little seemed to be known of it in comparison with the period of the Roman Empire and Classical antiquity; epochs of the past which appeared much richer in documentation and monuments[, a]lthough these terms did not originally have negative connotations...Indeed, examination of the history of the Middle Ages could easily lead to the conclusion that this was a barbarous and ignorant epoch; and that is certainly the view which now prevails...Whether or not the Middle Ages were culturally inferior or barbarous is a question of great complexity...For now, we content ourselves with noting that these ‘medieval’ centuries were characterized by some features which were inferior (and I refer here to the fields of the arts, sciences, and political organization), to the world of classical antiquity. Thus, among other things, the medieval world was a time of...[d]ecline of urban life, [a] (possible) decline in population;...[d]ecline in trade
and general prosperity,…[p]olitical fragmentation and feudalism;…[and a d]ecline in literacy and learning…”¹⁵⁰

Although our discussion of this catastrophic decline in the following pages will concentrate in large measure on the British Isles, continental Europe, and the Near East, we maintain that the very same conditions apply to North Africa, the islands of the Mediterranean, Persia, India, China, Korea, and Japan. That is, the catastrophe that befell the Roman Empire affected the entire Afro-Eurasian world. Ward-Perkins echoes O’Neill and cites William Robertson, a contemporary of Edward Gibbons:

“‘In less than a century after the barbarian nations settled in their new conquests, almost all the effects of the knowledge and civility, which the Romans had spread through Europe, disappeared. Not only the arts of elegance, which minister to luxury, and are supported by it, but many of the useful arts, without which life can scarcely be contemplated as comfortable, were neglected or lost.’”¹⁵¹

Historians have attempted to see this catastrophic change through uniformitarian lenses, a position which Ward-Perkins challenges:

“It is currently deeply unfashionable to state anything like a ‘crisis’ or a ‘decline’ occurred at the end of the Roman Empire, let alone that a ‘civilization’ collapsed and a ‘dark age’ ensued. The new orthodoxy is that the Roman world, in both East and West, was slowly, and essentially painlessly, ‘transformed’ into a medieval form. However, there is an insuperable problem with this new view: it does not fit the mass of archaeological evidence now available, which shows a startling decline in western standards of living during the fifth to seventh centuries. This was a change that affected everyone, from peasants to kings…It was no mere transformation—it was decline on a scale that can reasonably be described as ‘the end of a civilization.’”¹⁵²

It was a change so staggering in its effects that many historians cannot even contemplate it. Florin Diacu, citing Tudor Ratiu on just how unimaginable this change appears to be, states: “‘One thing I have always found hard to accept is that there was no progress in the Middle Ages for almost a thousand years. To me, this contradicts the questing of the human spirit. Can you believe that [almost] nobody wrote books, created art, or developed science in Europe from the fall of Rome

¹⁵¹ Ward-Perkins, *The Fall of Rome..., op.cit.*, p. 2
¹⁵² Ibid., p. 87
until the Renaissance?153 While historians have understood that civilization disappeared in Europe, and that they cannot find the strata of the survivors of the Roman Empire in Europe from around 500 to 900 AD, they have never tied this to the impoverished people of the Neolithic, Bronze, and early Iron Ages who aptly fit into the description of the post-Roman world.

The historians have provided a number of explanations for this collapse which have not generally been accepted. Among them is that the barbarian incursions into and across the Roman world so devastated the cities, killed off the population and interrupted trade, that it destroyed civilization. Yet even during the 6th century, when these incursions had generally ceased, there were cities, towns etc., still flourishing. Another option is that trade with the East was cut-off by the Moslems which led to the decline that is the thesis of John J. O’Neill. Nevertheless, when the Moslems took Constantinople in 1453, no such massive collapse ensued. One thesis is that the climate became colder as evidenced by tree-rings dated to around A.D. 540 and that this cold climate destroyed what was left of Roman society. Nevertheless, Mike Baillie, who agrees in part with astronomers Victor Clube and Bill Napier, after a lengthy study of the evidence for a climatic downturn around A.D. 540, such as volcanic eruption, or cometary Tunguska event, states that this simply does not show up in A.D. 540 ice cores from Greenland but in A.D. 530 and admits: “There can be no doubt that some momentous event took place in the early to mid-sixth century A.D., but we do not know definitely the cause (or causes).”154 Even if there was a downturn, we still have the problem of the Little Ice Age. The Little Ice Age that lasted from around A.D. 1400 to 1800 exhibited nothing resembling the events involved in the fall of Rome. That is, from around the 1300s through the 1600s, Europe was involved in the Black Death plague that destroyed much of its population; there were internecine wars like the Hundred Years War and the Thirty Years War comparable to the barbarian wars across the Roman Empire and a long great cold spell—the Little Ice Age—that all happened around the same period and trade was cut off by the Muslims, but all these factors taken together failed to destroy European civilization between A.D. 1300 and 1700. Historians, failing to understand this, have argued that either plague, or warfare, or climate, or the cutting-off of trade with the East brought about this decline.

153 Florin Diacu: The Lost Millennium: History’s Timetables Under Siege (Toronto 2006), pp. 5-6
154 Mike Baillie, Exodus to Arthur, Catastrophic Encounters with Comets (London 1999), p. 88
There are several proponents for the cold climate hypothesis in the Society for Interdisciplinary Studies in England, but they have never explained why this same form of climate change, accompanied by endless wars, recurrent episodes of the Black Death together with the collapse of trade with the East failed to do the same during the early Renaissance into the 1600s. Sadly Baillie has not addressed our criticisms of tree ring dates in volume II. The cause of this collapse of civilization, we maintain, is related to the very advanced nature of Roman economic, industrial, and political society. We are not discussing the fall of a relatively advanced, integrated culture but of one that was close to that of our own industrial society, say in the 1800s. Ward-Perkins describes the nature of this industrial capitalist development in the Roman empire:

“The Romans produced goods, including mundane items, to a very high quality, and in huge quantities; and then spread them widely, through all levels of society. Because so little detailed written evidence survives for these humble aspects of daily life, it used to be assumed that few goods moved far from home, and that economic complexity in the Roman period was essentially there to satisfy the need of the state and the whims of the elite, with little impact on the broad mass of society. However, painstaking work by archaeologists has slowly transformed this picture, through the excavation of hundreds of sites, and the systematic documentation and study of the artefacts found on them. This research has revealed a sophisticated world, in which a north-Italian peasant of the Roman period might eat off tableware from the area near Naples, store liquids in an amphora from North Africa, and sleep under a tiled roof. Almost all archaeologists, and most historians, now believe that the Roman economy was characterized, not only by an impressive luxury market, but also by a very substantial middle and lower [class] market for high-quality functional products.

“By far the fullest and most telling evidence comes from the study of the different types of pottery found in…abundance on Roman sites…We can tell when and where pots were made, from their shape and fabric, and assess the levels of expertise that went into their manufacture; and we can tell how far they travelled and the status of the consumers who used them…Furthermore, the picture we can build up for pottery also provides an insight into the production and exchange of other goods, for which much less archaeological evidence survives…

“Three features of Roman pottery are remarkable, and not to be found again for many centuries in the West; [they are (1)] its excellent quality and considerable standardization; [2] the massive quantities in which it was produced; and [3] its widespread diffusion, not only geographically (sometimes being transported over many hundreds of miles), but also socially [so that it reached not only the upper
strata of society but the ordinary citizen and farmer]…[T]his level of sophistication is not seen again until perhaps the fourteenth century, some 800 years later.\textsuperscript{155}

Ward-Perkins further informs us:

“This was a society with similarities to our own—moving goods on a gigantic scale, manufacturing high-quality containers to do so, and occasionally, as here [in modern times], even discarding them on delivery. Like us, the Romans enjoy the dubious distinction of creating a mountain of good-quality rubbish…

“In all but the remotest regions of the empire, Roman pottery of a high standard is common on the sites of humble villages and isolated farmsteads.”\textsuperscript{156}

He adds:

“Excavation has confirmed the impression from surface finds that in ancient Italy even humble structures often had tiled roofs [such as] farmstead[s]…Even buildings intended only for storage or for animals may well often have been tiled [such as a ] structure…thought to have been only a barn or stable.”\textsuperscript{157}

This provides us with a good picture of the economic structure of Roman society which capitalistic and industrial in nature with far-flung trade routes. It even had factories somewhat like those of the early 1800s in which massive amounts of goods were manufactured. John Michael Greer describes one of these and what would happen to society when production ceases:

“The centralized production and distribution of goods that defines…any industrial economy is vulnerable to breakdown…An example from the past shows where the danger lies.

“In the Roman Empire ceramics were a central technology and the Roman pottery industry was huge, capable and highly centralized, churning out tableware, storage vessels, roof tiles, and other goods in such quantities that archaeologists across Roman Europe struggle to cope with the [vast number of] fragments [found] today. The pottery works at La Graufesenque in southern Gaul, for instance, shipped exquisite products throughout the western empire and beyond it. Ceramics bearing the Graufesenque stamp have been found in Denmark and eastern Germany, hundreds of miles past the Roman frontiers. Good pottery [and

\textsuperscript{155} Ward-Perkins, \textit{op.cit.}, pp. 87-88
\textsuperscript{156} \textit{Ibid.} p. 92
\textsuperscript{157} \textit{Ibid.}, pp. 95-96
many others] was so cheap and widely available that even rural farm families could afford elegant table ware, sturdy cooking pots, and water tight roof tiles.

“All this ended when Rome fell. When archaeologists opened the grave of a sixth century king at Sutton Hoo in Eastern Britain, the pottery they found told a stark tale of technological collapse. Had it been made in fourth century Britain…Sutton Hoo pottery would have been unusually crude for a peasant farm house; two centuries later, it sat on the table of a king. What’s more, most of it had to be imported because the potter’s wheel dropped entirely out of use in Britain—one of the many technologies lost in a cascading collapse that took the island down to levels of impoverishment more extreme than anything since the subsistence crises of the [collapse of civilization supposedly around 1200 B.C. in the] Middle Bronze Age more than a thousand years before. Cooking vessels, food containers and roofing that keeps out the rain are basic to any form of settled life. An agrarian society that cannot produce them is impoverished by any definition; an agrarian society that had the ability to produce them, and then loses that ability, has undergone an appalling decline.”

Greer further explains:

“What makes this example relevant…is that the post-Roman collapse had its roots in the sophistication and specialization that made the Roman economy so efficient. Huge pottery factories [and others that produced a wide variety of goods] like the one at La Graufesenque, which used specialist labor to turn out quality goods in volume, could make a profit only by marketing their wares across much of a continent, using far-flung networks of transport and exchange to get products to consumers who wanted pottery and had denarii [Roman coins] to spend. The Roman world was rich and stable enough to support such networks—but the post-Roman world was not.

“The…implosion of the Roman empire thus turned an economic advantage into a fatal vulnerability. As transport and exchange networks came apart, the Roman economy went down with it, and that economy had relied on centralized production and specialized labor for so long that no one knew how to replace it with local resources. During the Roman Empire’s heyday, people in towns and villas near Sutton Hoo could buy pottery from local merchants who shipped them in by land from southern Britain and by sea from elsewhere in the Empire. They had no need of local pottery factories, and so nobody there knew how to make good pottery [or a host of other necessary commodities]. This meant that their descendents very nearly ended up with no pottery [or other such manufactured goods] at all.

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“Even where Roman pottery factories still existed, they were geared toward mass production of specialized types of pottery, not small-scale manufacture of the whole range needed by communities. Worse, as population levels plunged during the Roman collapse, the salvage economy that always springs up in the wake of collapse made use of the [remaining] abundant stock of pottery on hand, removing any market for new production. A few generations [of scavenging of these last materials and] of economic contraction, social chaos and depopulation thus erased the craft traditions of Roman pottery-making [with a variety of other products] leaving the descendants…with no knowledge of how to make [them]…

“Trace any other economic specialty [from metallurgy through transport, fabric production, modes of farming, construction of buildings, etc.,] through the trajectory of the post-Roman world and the same pattern appears. Economic specialization and centralized production, the core of Roman economic success, left Rome’s successor states in a world where local needs had to be met [only] by local production, most provinces of the former empire entered the Dark Age more impoverished than they had been before the Roman economy evolved in the first place.”

With regard to this form of specialization Ward-Perkins states:

“...the end of the ancient economy, and the timing of its collapse, were closely linked to the demise of the Roman empire. However, to understand the full and unexpected scale of the decline—turning sophisticated regions into underdeveloped backwaters—we need to appreciate that economic sophistication has a negative side. If the ancient economy had consisted of a series of simple and essentially autonomous local units, with little specialization of labour within them, and very little exchange between them, then parts of it would certainly have survived the troubles of post-Roman times—dented perhaps, but in an essentially recognizable form. However, because the ancient economy was in fact a complicated and interlocked system, its very sophistication rendered it fragile and less adaptable to change.

“For bulk high-quality production to flourish in the way that it did in Roman times, a very large number of people had to be involved in more-or-less specialized capacities. First, there had to be skilled manufacturers, able to make goods to a high standard, and in a sufficient quantity to ensure a low unit-cost. Secondly, a sophisticated network of transport and commerce had to exist, in order to distribute these goods efficiently and widely. Finally, a large (and therefore generally scattered) market of consumers was essential, with cash to spend and an inclination to spend it. Furthermore, all this complexity depended on the labour of…hundreds

159 Ibid., pp. 136-137
of other people who oiled the wheels of manufacture and commerce by maintaining an infrastructure of coins, roads, boats, wagons, wayside hostelries, and so on.

“Economic complexity [like that of the present day] made mass-produced goods available, but it also made people dependent on specialists or semi-specialists—sometimes working hundreds of miles away—for many of their material needs. This worked very well in stable times, but it rendered consumers extremely vulnerable if for any reason the networks of production and distribution were disrupted, or if they themselves could not longer afford to purchase from a specialist. If specialized production failed [on a massive scale throughout the Roman Empire] it was not possible to fall back immediately on effective self-help.”

This cascading economic implosion reverberated throughout the Roman world over a relatively short period of time. Its most devastating effects were felt and exacerbated in towns and large urban centers most dependent on this flow of goods, grain supplies and other necessities of civilized life which disappeared or became inordinately scarce. Greer in the following citation discusses what would happen to a modern-day economy which clearly applies to that of the Roman Empire, and we request the reader translate these effects to that ancient epoch.

“Nearly all workers in the industrial economy perform highly specialized niche jobs, most of which depend on an equally centralized…economy…If the structure comes apart, access to even basic goods and services would become a challenge very quickly.

“Food is the obvious example. Only a small number of people in any industrialized nation know how to grow their own food and not all of these have access to the land, tools, and seed stock to give it a try. The same principle holds, however, for every other necessity of life and countless other things that would be good to have in [a] deindustrialized dark age…Consider the skills [, supplies of material, equipment etc.], needed to find and process clothing. Few people have any of these skills, much less the entire set; the tools needed to do most of them are hardly household items.”

Of what value is literacy when one is starving, or any of the other non-agricultural skills when food supplies cannot be obtained? When the Roman world was faced with the inability to produce and obtain the necessities of the very fabric of life, it became a struggle for survival and those best suited to survive under such circumstances were not writers, artists, actors, statesmen, and a host of other specialists, but those who could farm or raise animals. We do not wish to suggest that this implosion occurred

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160 Ward-Perkins, *op.cit.*, pp. 136-137
161 Greer, *op.cit.*, p. 37
everywhere at the same time, but it was the vast experience of nearly all the people of that age caught up in the ruins of a dead industrialized empire without a surviving comparable replacement culture. Not knowing how to make pottery, construct buildings, etc., and lacking the necessary tools to do so, would necessarily lead to the production of more primitive forms of pottery like that of the Megalithic Age, and buildings of primitive form etc., prevalent in the post-Roman world. Ward-Perkins describes how this implosion affected Britain and Italy:

“For instance in Britain—as ever an extreme case—every one of the building crafts introduced by the Romans, the mundane as well as the luxury ones, disappeared completely during the fifth century. There is no evidence whatsoever of the continued quarrying of building stone, nor of the preparation of mortar, nor the manufacture and use of bricks and tiles. All new building in the fifth and sixth centuries, whether in Anglo-Saxon or unconquered British areas, were either of wood and other perishable material or of dry stone walling [as at Skara Brae and elsewhere on Orkney], and all were roofed in wood or thatch.

“In Northumbria at the very end of the seventh century….abbot Benedict Biscop wished to build churches ‘in the Roman manner’…In order to reintroduce this technology he brought in artisans from Gaul…The resulting buildings, which survive in part, are tiny by Roman or later medieval standards…

“In the Mediterranean region, the decline in building techniques and quality was not quite so dramatic—what we witness here, as with the history of pottery production, is a dramatic shrinkage, rather than a complete disappearance. Domestic housing in post-Roman Italy, whether in town or countryside, seems to have been almost exclusively of perishable materials. Houses, which in the Roman period had been primarily of stone and brick, disappeared, to be replaced by settlements constructed almost entirely of wood. Even the dwellings of the landed aristocracy became much more ephemeral and far less comfortable: archaeologists, despite considerable efforts, have so far failed to find any continuity into the late sixth and seventh centuries of the impressive rural and urban houses that had been a ubiquitous feature of the Roman period…At present it seems that in Italy only kings and bishops continued to live in such Roman-style comfort.”

That is, nearly everyone else except the king or a few bishops lived in abject impoverished conditions. This is also in accord with the lack of these buildings being found by archaeologists in that post-Roman age. What little survived has been primitive and was therefore assigned to the prehistoric Megalithic Age.

Humanity, having fallen back almost to the Stone Age, would not recover for several centuries. Emerging from that Dark Age was no overnight event. All that had been attained and lost had to be completely relearned and there was no one to guide these medieval people back into civilized light and development but themselves. In a few or many generations all the knowledge and connection with the Roman past was undone and then mostly forgotten, lost to all except a few isolated literate clergy attached to monastery farming communities. Nearly all vestiges of the ancient world’s crafts were also lost and the survivors would have to begin from almost nothing to reinvent civilization. This was far different from the great civilizations of the ancient Near East that conquered each other; they did not destroy the entire fabric of life of the peoples they subjugated, but included them in their empire, taking as tribute a part of their wealth, but leaving the subjugated society much as they found it, and also bringing new crafts, materials, domesticated animals etc., to these subject peoples, as the Assyrian/Akkadians/Hyksos did in Egypt. Similarly, the Persians conquered a vast empire, subjugating the peoples of Assyria, Babylonia, Anatolia, Egypt, Palestine, India, etc., and also created a wide network of trade and development that advanced even beyond that of the Assyrians. The Greeks built their empire on that of the Persians and Hellenized that world with even greater advances. Rome, which came last, was the most advanced, incorporating the skills, crafts, resources and developments of all these nations into it. Each empire was, or in turn became, more advanced, technologically and commercially than the previous ones, leading, as we pointed out in the previous volumes of *Pillars of the Past*, to the creation of an ancient Industrial Revolution from about 750 B.C. through 300 B.C. Rome, in this sense being the last, was the most advanced on all levels of development and might well be considered for its time as an ancient post-industrialized society. As Greer and Ward-Perkins pointed out, its very advanced concentration of wealth, resources, skills, trade, etc., into a political, industrial, economic, *gestalt*, left a vacuum when it collapsed that could not be filled because there existed no other similarly advanced state to replace and rebuild what had previously existed. This is not to suggest that what had befallen Britain occurred everywhere else with the same degree of collapse. But inexorably the implosion of Roman civilization continued nearly everywhere it existed. Ward-Perkins claims that the collapse led to post-Roman people going back to a “level of economic simplicity similar to that of the Bronze Age:”

“There is general agreement that Roman Britain’s sophisticated economy disappeared remarkably quickly and remarkably early. There may already have been considerable decline in the later fourth century, but, if so, this was a recession, rather than a complete collapse; new coins were still in widespread use
[to be left at megalithic sites as offerings] and a number of sophisticated industries still active. In the early fifth century all this disappeared…Britain reverted to a level of economic simplicity similar to that of the Bronze Age, with no coinage, and only hand-shaped pots and wooden buildings.

“Further south, in the provinces of the western Mediterranean, the change was much slower and more gradual, and is consequently difficult to chart in detail. But it would be reasonable to summarize the change in both Italy and North Africa as a slow decline, starting in the fifth century (possibly earlier in Italy), and continuing on a steady downward path into the seventh…

“Turning to the eastern Mediterranean, we find a very different [timetable] story…By contrast [for a time] almost the whole of the eastern empire [the fourth, fifth and part of the sixth centuries], from central Greece to Egypt [experienced] a period of remarkable expansion.

“In the Aegean, this prosperity came to a sudden and very dramatic end in the years around AD 600. Great cities such as Corinth, Athens, Ephesus, and Aphrodisias which had dominated the region…shrank to a fraction of their former size—the recent excavations at Aphrodisias suggest that the greater part of the city became in the early seventh century an abandoned ghost town…But even Constantinople shrank dramatically in both wealth and population from a booming centre of perhaps half-a-million people of the years around A.D. 500.”

We submit that the collapse was even greater than just given. The nature of this massive shift from great abundance and prosperity to one of bare subsistence and poverty demands a cause that will so deeply disrupt the Roman world and more significantly overwhelm its population that people would be forced to give up their trades and move out of the towns and cities, returning to the land to scratch out a living in order to survive. We maintain that a catastrophe occurred greater than the barbarian pillaging and climatic downturn and cut-off trade. The only catastrophe where we have specific references to the population of the Roman world being devastated and depopulated, where people abandoned their towns and cities, and where the agricultural lands turned back to wilderness, is that of the great “Justinian plagues.” These plagues were greater than any others that devastated the world. This was the only one that reduced the European/Afro-Asian Roman highly advanced civilization to one in which the whole population was reduced to living at the level of subsistence of the Neolithic and Bronze Ages in Europe. As we read the following accounts of this catastrophe, we will be constantly informed that

163 Ibid., pp. 123-125
entire territories were depopulated, or cities became ghost-towns, being abandoned or engulfed by pestilence while whole populations disappeared. To repeat, no other plague in the civilized world ever left its population living back in the Neolithic and Bronze Ages. We will further show that this plague encompassed the world from the British Isles to Japan, driving their populations back to almost prehistoric levels. It was the first pandemic to strike humanity on so great a scale, and attacked a population with little or no immunity to it. In a certain sense it was like the pandemic that Europeans brought to the Americas that destroyed the native populations which had no immunity to these diseases. This was presented by David Brog, among many others, but he encapsulates the tragedy in the fewest words:

“Of all the crimes perpetrated by man, genocide is the most evil. And of all the genocides perpetrated by man, the largest in absolute terms occurred neither in Europe nor in Africa, but right here in America. Only in America were so many entire nations completely wiped out. And only in America were so many others reduced to tiny, dispossessed remnants. Like so many things in America, our genocide was of truly continental proportions.

“In contrast to conventional wisdom, pre-Columbian America was not a sparsely populated land waiting forlornly to be ‘discovered’ by Europeans. To the contrary; the America to which Columbus first sailed had a population which exceeded that of Europe. Recent studies have estimated that the population of the Americas on the eve of Columbus’s arrival was somewhere between seventy-five and one-hundred million while Europe’s population was between sixty and seventy million. The population of central Mexico alone—approximately twenty-five million souls—was seven times larger than the population of England.

“Fifty years later, there were only ten million American Indians left. The other tens of millions had been killed off by war [but mostly by disease]…”

Russell Thornton, after examining the various estimates by demographers, succinctly puts it this way: “I estimated…a total population of 72+ million American Indians in the Western Hemisphere in 1492. This 72+ million declined in a few centuries to perhaps 4 to 4.5 million (Dobyns, 1966: 415; Thornton and

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Marsh-Thornton, 1981: 48). This was a population about 6 percent its former size. It represents a tremendous population decline over the centuries.”

As we will see below, the plagues that struck the African, Asian, and European continents worked their will repeatedly over about 250 years; they were perhaps as destructive as the holocaust that swept the American continent. Their severity was immense. Never before had such a pandemic ravaged humanity in the Old World as had that of the Justinian Plagues, which suggests the peoples of these vast regions lacked immunity, and thus, being the first pandemic, it was the worst. Lester K. Little describes the Justinian Plagues thus:

“In the summer of 541 AD a deadly infectious disease broke out in the Egyptian port city of Pelusium, located on the eastern edge of the Nile delta. It quickly spread eastward along the coast to Gaza and westward to Alexandria. By the following spring it had found its way to Constantinople, capital of the Roman Empire. Syria, Anatolia, Greece, Italy, Gaul, Iberia, and North Africa: none of the lands bordering the Mediterranean escaped it. Here and there, it followed river valleys or overland routes and thus penetrated far into the interior, reaching, for example, as far east as Persia or as far north, after another sea crossing, as the British Isles.

“THE DISEASE REMAINED VIRULENT IN THESE LANDS FOR SLIGHTLY MORE THAN TWO CENTURIES, although it never settled anywhere for long. Instead, it came and went, and…its appearances were unannounced. Overall, there was not a decade in the course of those two centuries when it was not inflicting death somewhere in the Mediterranean region. In THOSE PLACES WHERE IT APPEARED SEVERAL TIMES, THE INTERVALS BETWEEN RECURRING RANGED FROM ABOUT SIX TO TWENTY YEARS. And then, in the middle of the eighth century, it vanished with as little ceremony as when it first arrived.”

The recurrences had a devastating effect on the size of the population in the Roman Empire. Those who survived had weakened immune systems and were therefore subject to other diseases which increased mortality levels. These recurrences of the plague were well described by Evagrius Scholasticus, as reported by Ward-Perkins:

165 Russell Thornton, American Indian Holocaust and Survival: A Population History Since 1492 (Norman OK 1987), p. 42
"The historian Evagrius, a resident of Antioch in Syria, interrupted the flow of his narrative to give an account of how the disease affected his own family. As a boy, on the plague’s first appearance in the empire, he himself had been struck down, but was lucky enough to survive. Later, however, on its return, it was to kill his first wife and several of his children, as well as other members of his wider family. Just two years before writing, when the plague visited Antioch for a fourth time, he lost both his daughter and her son. There is little reason to doubt that this occurrence and recurrence of disease were not just a personal tragedy, for people like Evagrius, but also a substantial demographic blow to the population of the empire."\textsuperscript{167}

Gregory of Tours reflects on the recurrences of the plague at Marseilles, France:

"The city of Marseilles was suffering from a most serious epidemic of swelling. I want to tell you exactly how it came about...a ship from Spain put into port with the usual kind of cargo, unfortunately also bringing with it the source of infection. Quite a few of the townsfolk purchased objects from the cargo and in less than no time a house in which eight people lived was left completely deserted, all the inhabitants having caught the disease. The infection did not spread through the residential quarter immediately. Some time passed [for the disease to incubate in the host population] and then, like a cornfield set alight, the entire town was suddenly ablaze with pestilence. For all that Bishop Theodore came back and took up residence in Saint Victor’s church, together with several poor folk who remained at his side. There he stayed throughout the whole of the catastrophe which assailed his city, giving all his time to prayers and vigils, and imploring God in his mercy to put an end to the slaughter and to allow the people some peace and quiet. At the end of two months the plague burned itself out. The population [that had abandoned the city] returned to Marseilles, thinking themselves safe. [These people became infected.] Then the disease started again and all who had come back died. On several occasions later on Marseilles suffered from an epidemic of this sort."\textsuperscript{168}

For a list of the recurrences of these plagues see Stathakopoulos:

"In a total of 209 years from 541 to 750 there were about 18 outbreaks of the plague amounting to a supposed average of an outbreak around every 11.6 years. This seems to apply to the first six plague waves for which we can compute the inter-epidemic intervals for Constantinople. These range approximately from 11 to 17 years, with an average of 14.2 years....The situation seemingly changed during the seventh and eighth centuries...From the 11\textsuperscript{th} to the 18\textsuperscript{th} wave Syria was reportedly hit..."\textsuperscript{167}

\textsuperscript{167} Ward-Perkins, \textit{op.cit.}, pp. 133-134
\textsuperscript{168} Gregory of Tours in Barbara Hanawait, \textit{The European World 400-1450} (Oxford 2005), p. 45
on all eight waves with inter-epidemic periods ranging from five to eleven
years…”¹⁶⁹

Little goes on to describe the devastation of the first wave of the plague as it
swept into Asia from Egypt as reported by John of Ephesus who…

“[u]pon his return trip [from Alexandria to Constantinople] overland through
Palestine, Syria, and Asia Minor…found himself keeping abreast of the parallel
movement of the disease as he traveled. In Palestine he saw entire town
populations wiped out. ‘During the tumult and intensity of the pestilence,’ he
wrote, ‘we journeyed from Syria to the capital. Day after day we, too, used to
knock at the door of the grave along with everyone else. We used to think that if
there would evening, death would come upon us suddenly in the night. Although
the next morning would come, we used to face the grave during the whole day as
we looked at the devastated and moaning villages in these regions, and at corpses
lying on the ground with no one to gather them.’ According to John, some people
carried corpses all day [to be buried], while others spent the day digging graves.
Houses and farms were abandoned. Animals forgot their domestication. ‘Crops of
wheat in fertile fields located in all the regions through which we passed from
Syria up through Thrace, were white [with grain] and standing but there was no
one to reap them and store the wheat. Vineyards, whose picking season came and
went, shed their leaves, since winter was severe, but kept their fruits hanging on
their vines, and there was no one to pick them or press them.’ In his Lives of the
Eastern Saints, John reported on one monastery that buried eighty-four of its
members who had died of the plague. Other Syriac writings contain details of later
outbreaks in Iraq, Egypt, Syria, and Palestine, including the Chronicle of Zuqnin,
whose monastic author, in recounting the epidemic of 743-745, specified that the
victims had swellings in the groin, the armpit, or the neck [in the nodes of the
lymphatic system, possibly indicative of bubonic plague].”¹⁷⁰

The fact of the matter is that no one is absolutely sure of the nature of this
disease, and this might have been an unknown pathogen that struck the world during
that period of time. What we encountered above is that this plague depopulated
entire regions including the farming populations and must have contributed to the
death toll by making food scarce or even unavailable to town folk, forcing them to
return to the land to scavenge what crops were available and then, as best they could,

¹⁶⁹ Dionysios Ch. Stathakopoulos, Famine and Pestilence in the Late Roman and Early
Byzantine Empire: A Systematic Survey of Subsistence Crises and Epidemics (Aldershot UK
2004), p. 124
¹⁷⁰ Little, op.cit., p. 7 (emphasis added)
to eke out an agricultural living. The size of the depopulation across the Roman Empire is reported by David Keys, for Constantinople citing John of Ephesus:

“When this scourge weighed heavy upon this city, it eagerly began to assault the class of the poor [who after dying] lay in the streets.

“It happened that 5,000 and 7,000, or even 12,000 and as many as 16,000 of them departed in a single day. Since thus far it was [only] the beginning, men [i.e., government officials] were standing by the harbours, at the cross-roads and at the [city] gates counting the dead [being disposed of].

“Thus they [the people of the city] reached the point of disappearing with only a few remaining. Whereas [of] those only who had died on the streets—if anybody wants us to name their number…over 300,000 were taken off the streets. Those who counted having reached [the number of 230,000 and seeing that the dead] were innumerable, gave up and from then on [the corpses] were brought out without being counted.”

Burial space for the victims of the plague so overwhelmed the ability to bury the dead that the officials turned to disposing of the bodies in the sea from vessels:

“There boats were filled with them and during each sailing, they were thrown overboard and the ships returned to take other [bodies].

“Standing on the seashore one could see litters colliding with each other and coming back to carry and throw upon earth two or three [corpses] and go back again…Others carried [the dead] on boards and carrying poles, bringing and piling [them] up one upon the other. For other corpses, since they had rotted and putrefied, matting was sewn together [for them to be carried away]…

“Thousands of corpses were ‘piled upon the entire seashore like flotsam on great rivers and the pus flowed, discharging itself into the sea.’ Even with the ships busy carrying their macabre cargoes at sea it was proving impossible to clear the backlog of dead bodies.”

Things grew so bad that Justinian had workers dig mass graves into which innumerable corpses were flung. In order to get workers to do these tasks they had to be plied

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172 *Ibid.*, pp. 11-12
“…with gifts to carry and bring [the dead] giving five, six and even seven to ten dinars [payment] for each load. While men stood below, deep as in an abyss, and others above, the latter dragged and threw down [the bodies], like stones…and the former grabbed and threw them one on top of another…

“Because of the scarcity [of burial space] both men and women were trodden upon, young people and children were pressed together, trodden upon by feet and trampled like spoiled grapes. Then again from above [others] were thrown head downwards and went down and split asunder beneath, noble men and women, old men and women, youths and virgins, young girls and babies.

“Whole peoples and kingdoms, territories and regions were seized.”

Keys further adds: “But the full social and political impact of the disease lay in its remorseless habit of returning to claim the lives of those it had previously spared.”

Little turns to describe the plague’s destructiveness in Italy and Gaul:

“…Gregory of Tours from 573 to 594. In his History of the Franks and also in his Lives of the Fathers, he gives testimony to the first appearance of the plague in Gaul, which took place in the Rhone Valley in 543. The context was his telling of the saintly life of…Bishop Gallus of Clermont, in whose time, he says, ‘that illness called inguinal raged in many regions and most notably it depopulated the province of Arles.’…

“Things went differently at Clermont in 571 [when the plague returned] under Bishop Cautinus, who scurried from one place to another to avoid the plague. ‘So many people were killed off in the whole region and the dead bodies were so numerous [as at Constantinople] that it was not even possible to count them. There was such a shortage of coffins and tombstones that ten or more bodies were buried in the same grave. In Saint Peter’s church alone on a single Sunday three hundred dead bodies were counted.’…Bishop Cautinus…got the infection, and died on Good Friday, ‘on the same day and at the same hour as his cousin…’ Lyons, Bourges, Chalon-sur-Saône, and Dijon were [likewise] decimated by this plague.”

In Italy, which was densely populated, the plague was also devastating, according to Little:

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173 Ibid., pp. 12-13
174 Ibid., p. 13
175 Little, op.cit., p. 10
“The final epidemic written up by Gregory was that of the year 590 in Rome, as reported to him by a cleric named Agiulf [who] told of [a flood] as a result [of which] there followed an epidemic…One of the first victims was Pope Pelagius II, and many others followed…The Romans were being carried off not one by one but in droves…‘Homes are left empty, parents are forced to attend the funerals of their children…”\textsuperscript{176}

As for the rest of Italy, Little tells us:

“Of [this] plague as conveyed by Paul the Deacon, a Lombard scholar who lived and wrote two full centuries later than Gregory of Tours in his History of Lombardy, there is mention of four separate outbreaks. The first of these occurred in Liguria in 565…Paul does not tell us anything specific about this outbreak, but he followed with a dramatic description of its effects upon individuals, families and whole communities. ‘The dwellings were left deserted by their inhabitants, and the dogs only kept house. The flocks remained alone in the pasture with no shepherd at hand. You might see villas or fortified places lately filled with crowds of men, and on the next day, all had departed [to escape the plague or to seek sources of food] and everything was in utter silence. Some fled, leaving the corpses of their parents unburied; parents forgetful of their duty abandoned their children in raging fever…’ The common perception that the plague had little impact on the countryside is contradicted by his comments: ‘You might see the world brought back to its ancient silence: no voice in the fields; no whistling of shepherds; no harm to domestic fowls. The crops, outliving the time of the harvest, awaited the reaper untouched; the vineyard with its fallen leaves and its shining grapes remained undisturbed while winter came on;…pastoral places had been turned into sepulchers of men, and human habitation had become the refuge for wild beasts.”\textsuperscript{177}

We mentioned earlier Eusebius, cited in Little’s Plague and the End of Antiquity, that “houses and farms were abandoned. Animals forgot their domestication. Crops of wheat…were white and standing but there was no one to reap them and store the wheat. Vineyards whose picking season came and went shed their leaves since winter was severe…and there was no one to pick them or press them.” This represents an additional catastrophe since people need food to survive. Mike Baillie, who suggests that an extreme cold snap was responsible for the collapse of Roman society, but who has failed to explain why the Little Ice Age didn’t drive people back to the Stone Age, tells us:

\textsuperscript{176} Ibid., pp. 10-11
\textsuperscript{177} Ibid., p. 12
“Any significant downturn in climate [or, as we suggest, plague destruction of farmers] leading to crop failures, or merely to reduced yields, then leaves the population vulnerable to starvation and disease. In extreme cases there would be near-total collapse. This would coincide with those earlier observations in the dry American Southwest, where reliable harvests allowed Anasazi populations to boom and pueblos to expand. Sudden reversals in those areas with regularly failing harvests led to starvation or abandonment.”

Here we have exactly what Baillie claims, except, as the ancient sources say, the cause of the failure of crop harvests was plague and not a climatic downturn. The results were the same: near total population collapse and cities, towns, and villages abandoned.

These descriptions are not reflected in the Black Death that overwhelmed Europe in the 14th through 17th centuries. In that case the people did not revert to living in Neolithic nor Bronze Age conditions. According to Baillie there was over a year of darkness occurring around 540 AD, described as a “dry fog” with extremely narrow tree-rings in the Irish dendrochronological record, and he cites Procopius who reported this, as well as John Lydus and the late chronicler Michael of Syria who described this ‘dry fog.’ He does not, however, report that this dry fog described by these writers in anyway affected the population. They, in fact, never suggest anything destroyed the population. The contradiction Baillie has failed to address regarding the supposed catastrophic effects of this darkness or dry fog is that civilization continued for over 150 years from ca. 540 to ca. 700 AD when there was no record of this happening. Why did the dry fog of ca. 540 AD take over 150 years to affect the population of Europe, Byzantium and elsewhere? While we have direct and numerous statements from records from this period that plague was destroying the people in vast numbers, Baillie never explains this lack of evidence, for him and those proponents of this thesis, nor why it took over 150 years to affect civilization.

According to Victoria R. Rumble, “The Middle Ages were a time of discovery and rediscovery. After the fall of the Roman Empire many citizens lived in abject poverty in the shadow of unbelievably advanced technology such as Roman aqueducts. Roads and sewers fell into disrepair as people lived in mere hovels and food [production] reverted for a time almost to the stage it had been when man

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178 Baillie, op.cit., p. 76
179 Baillie, op.cit., p. 85
lived on wild grains and seeds.”  

This description echoes through the literature. Robert Browning states: “Italy was devastated as it had never been before. Its olive groves and vineyards had been cut down and burnt [for fire]. Its roads lay neglected, its bridges demolished. Of its cities, many were in ruins, some actually abandoned by their citizens, all reduced to a fraction of their population and despoiled of their treasures. War and plague had decimated the people, and the survivors sheltered like wild beasts in the ruins of their former prosperity…” 

Aleksandr Aleksandrovich Vasil’ev writes “The mortality was enormous, cities and villages were abandoned, agriculture stopped…” The same perspective comes from J.N. Larned: “The plague depopulated entire cities and districts, so that forests sprang up in places before inhabited.” 

This catastrophic destruction not only occurred in Italy but throughout the Roman Empire. According to Warren T. Treadgold:

“The cities had always attracted settlers from the countryside, and would have been especially attractive after an outbreak of plague had made urban labor scarce and housing abundant. Nonetheless, migration from the country probably did no more than reduce the net population loss of the cities to about the average for the empire, because the diminished rural population could hardly have supported undiminished cities. Accordingly the population of Constantinople in 610 could scarcely have been more than a quarter million, and was probably closer to two hundred thousand [or even much less]. After the disasters [of recurrent plagues] at Antioch the [eastern] empire must have been left with just two cities, Constantinople and Alexandria, of over a hundred thousand people. Antioch, in its ravaged condition, might have been about half that size [and this was only after the first third of plagues had occurred.]

“The other real cities in the empire, those with significantly more than ten thousand people, surely decreased in number…, perhaps from over thirty cities to under twenty in all. The plague left regions that had previously had somewhat anemic urban life with just one metropolis of any size. Probably the only city over ten thousand remaining in Greece was Thessalonica, while Alexandria may have been the

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180 Victoria R. Rumble, foreword by Sandra Oliver, *Soup through the Ages, A Culinary History With Period Recipes* (Jefferson NC 2009), p. 30
only city in Egypt. By 610 Carthage may well have been the lone city in the whole territory retaken by Justinian with more than ten thousand people…Even in Anatolia and Syria cities declined, with a few exceptions in the inland part of Syria…"

This was the situation after about 80 years of the plague which was to continue destroying populations in the empire for about another 120 years.

Wendy Orent further observes, while assuming the “Justinian Plague did not reach as far as the Black Death…and must have killed far fewer people,”

“…it has affected our world in a way we still do not fully understand. Some writers have suggested that the plague’s depopulation of the Levant remains apparent to this day. Much former Byzantine farmland is still unsettled and monasteries can still be seen in the pasture lands of Syria. The Negev Desert, once home of many carefully nurtured monastic settlements, reverted to a wasteland inhabited only by nomadic shepherders until the mid-nineteenth century. The farmlands of Africa dried up as irrigation systems fell into disrepair and the Sahara began its inexorable advance into the fields that had fed the empire. The population itself did not recover for fifteen centuries, and even then the rural countryside remained sparsely settled.”

In all probability it brought about the end of the Garamantes civilization which is assumed to have fallen to the Moslems. We maintain that the destruction of the population was greater than it is generally estimated to be. These plagues ravaged the hinterlands of Europe and elsewhere as much as they did the Mediterranean coastal regions. J.F.D. Shrewsbury describes the plague that swept into Britain which was no longer part of the empire and separated by the English Channel from the Continent:

“No hint of the approach of plague nearer to the British Isles than the south of France occurs in any sixth-century records of the disease; but there was a great outburst of epidemic disease in the islands [first in] the sixth and [later in the] fourteenth centuries…Under the date of 534 the Annals of Boyle record ‘a great mortality…’ and eleven years later the Annals of Ulster record what was in all probability a recurrence under the name of ‘blefed.’ From that date, 545, until 667 there are numerous notices of this epidemic disease…”

“The next century it was England’s turn….In 664…a great pestilence ravaged the country…Bede, who recorded…events, observed that it first devastated the southern regions of Britain and thence spread north into the Kingdom of Northumbria. It raged without intermission over a large part of the country and caused an enormous mortality among the English people. ‘Moreover,’ Bede continues, ‘this affliction pressed sore on the island of Ireland with a like destruction’, and several of the Irish Annals record its presence in 664.”

Keys shows how, as elsewhere, these plagues depopulated the British Isles and left villages, towns and cities abandoned or reduced to a fraction of their former size.

“After arriving in Britain through one or more entry points, the plague almost certainly proceeded to devastate vast areas of the southwest and Wales. Archaeology has revealed that, at the very time the plague would have been raging, many settlements became totally depopulated, presumably as a direct result of the epidemic…

“In Cornwall, a mile from the Atlantic coast, a settlement now known as Chûn, with its twenty-foot-thick, twelve-foot-high stone defenses became a ghost town in the mid sixth century…

“…in the sixth century [a] somewhat…fortified Cornish settlement, Killibury, with a population of perhaps two hundred to three hundred, appears…to have become depopulated at the same time, as did the village at the site now called Gamble near St. Ives.

“In Devon High Peak—a small fortified town, now a deserted series of earthworks on a windblown cliff top—ceased to exist after flourishing for more than seven hundred years. Another Devon coastal settlement, Mothecombe, also appears to have fallen into oblivion in the mid-sixth century. Dozens of other settlements almost certainly suffered the same fate, but have not yet been detected archaeologically.

“As well as those settlements that appear to have vanished into oblivion at the time of the plagues, several other major archaeological sites have yielded fascinating evidence of rapid change or drastic population reduction at that time. The most important is the ancient Roman city of Viroconium (Wroxeter), which appears to have suffered a major drop in population followed by a complete reordering of the city’s property boundaries.”

“The evidence from the site suggests that in the mid-sixth century the city’s major market fell into disuse because of a reduction in trade and number of customers. Within a few decades this was followed by a complete redesign of the city’s property boundaries with the much reduced urban area, and on the site of the market place a local magnate decided to build a large private house. This disrespect for previous property boundaries and former public property boundaries strongly indicates a substantial demographic discontinuity occurring at around the time of the plague.

“On the eve of the plague the city probably boasted several thousand inhabitants spread over nearly two hundred acres…but a few decades after the plague…it had shrunk to around twenty-five acres [or one eighth its original size]!187

Keys further argues:

“It is likely that the plague recurred several times…and that the cumulative result of the disease (together with the famine which preceded it) was a population reduction…No figures survive (and probably none were ever gathered) for the number of people who died of plague during that time…Normal life virtually collapsed, much agricultural land went out of use, and—as the archaeological record testifies—many towns and villages became depopulated and deserted.”188

Of plague in Ireland Keys tells us:

“Then in the early or mid-540s, a terrible epidemic broke out. Irish sources provide two conflicting dates for this disaster…More likely is the date provided by the *Annals of Ulster*, 545. In that year both France and Spain were already infected with plague, and it is probable that it spread to Ireland from either of these areas or directly from the Mediterranean [via trading vessels]….Some Irish population centers (Lough Shinney, near Dublin and the royal fortress of Garranes near Cork) are known, from archaeology, to have ceased to function in or immediately after the mid-sixth century…

“But worse was to come, for in 550 Ireland was engulfed by a second epidemic…Referred to in the annals as the *Mortalitas Magna* (Great Death), the disease must have wiped out a substantial portion of the population, including a large element of the relatively small literate governing elite.

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187 Keys, *op. cit.*, pp. 110-111
188 *Ibid.*, p. 112
“The Annals of Ulster actually record the deaths of five prominent victims—senior churchmen…The dead clerics probably represented the loss of 20 to 30 percent of the top tier of churchmen in this one outbreak alone.”

This surely is an excellent description of the state of the Roman Empire, in which life virtually collapsed and cities, towns, and villages became depopulated and deserted by the few who survived. It explains why the population reverted to living at Neolithic and Bronze Age levels and also why they seem to have vanished from the archaeological record and why, because they lived in such a primitive state, historians and archaeologists assigned them to living in prehistoric times. This reversion to such a low economic level naturally caused what they built thereafter in wood to leave little that could be recovered by the archaeologists. Ward-Perkins draws the picture of what ensued:

“At first sight this evidence seems to point clearly and unequivocally to a massive drop in population in the post-Roman centuries, to half or perhaps even a quarter [or even less] of Roman levels. However, as so often, the picture is not quite as clear as it at first appears. Archaeologists can find people of the past only if they left behind them durable material remains. If these people belonged to a culture like that of Roman times, which produced large quantities of solid [stone] building materials and shiny pottery, then their settlements show up very clearly in the modern plough soil, as readily identifiable concentrations of broken tile, fragments of mortar, and potsherds. But, unfortunately, the same is not true of settlements from periods with very few durable objects; and, as we have seen, this is exactly what the post-Roman centuries were like. Wooden houses and thatched roofs predominated, which left no tile and mortar fragments, while early medieval pottery is not only much scarcer than its Roman equivalent, but it is also generally a dull brown or grey in colour, and therefore difficult to spot in plough soil. Post-Roman sites, and thus post-Roman people, are often very difficult to find.”

What historians have failed to recognize is that they cannot accept that any form of catastrophic event played havoc in historic times, such as those of an extraterrestrial nature as proposed by Velikovsky, but even those of terrestrial origin—so in this case they claim that plague occurred. Little, having studied this first pandemic, states:

“What is utterly astonishing is the lack of attention shown to the first pandemic by the numerous experts…The Black Death as solid a part of the historical cannon…has

189 Ibid., pp. 18-19
190 Ward-Perkins, op.cit., p. 139
long continued to attract historical investigations and yet those most interested in it have shown little curiosity about whether it had a precedent.”

Here then is a summary of the Black Death of the 1300s, provided by Samuel Kline Cohn:

“That system, marked by saturated use of resources and stagnant outputs, might have persisted indefinitely. The plague broke the deadlock, and allowed Europeans to rebuild their demographic and economic systems in ways more admissive of further development. Culturally, the plague thinned the cadres of the skilled and learned and reduced their years of service; it weakened schools and universities; and it compromised the quality of cultural traditions. But it also prepared the road to renewal.”

We ask the reader to note that the Black Plague only “thinned the cadres,” not nearly wiped them out. And in its wake “prepared the road to renewal” of civilized development which did not follow closely on the heels of the “plagues of Justinian.” Europeans did not revert to living in near Stone Age conditions. They created the Renaissance with a rebirth of learning, great works of architecture, literature, art, advances in mathematics, astronomy, medicine—just the opposite of what follow the Justinian Plagues.

We maintain that this pandemic not only destroyed and impoverished the Roman Empire but that it traveled across Asia to Persia, India, China, Korea, Japan, and that it encompassed the entire civilized world which was carrying on a vast network of trade. Whole populations with different disease immunities came into contact and were afflicted with a disease against which they had little or no immunity or passed on the disease to those others that infected them. This interaction in Europe between the Romans and the so-called barbarian peoples of Europe has been noted by Karen S. Cook:

“The Roman Mediterranean populations contracted from the third century on and collapsed after the Justinian plague in the late fifth century. The Germanic barbarians invaded the Western Roman Empire, or more precisely, wandered in to take over vacant land. This occupation made a bad situation worse, because as Roman and Germanic ‘masses began to commingle, living as close neighbors and

191 Little, Plague ..., op.cit., p. 16
intermarrying, disparate ethnic groups were exposed to the ravages of alien diseases for which they lacked effective antibody defense.”\textsuperscript{193}

As with H.G. Wells’s science fiction novel, \textit{The War of the Worlds}, once foreign peoples met and interacted with one another, the bacteria and viruses to which these groups lacked immunity almost entirely destroyed them, as Eli Heckscher, citing Procopius, points out, with which we concur: “During these times there was a pestilence by which the whole human race came near to being annihilated,…It did not come in a part of the world, nor upset [only] certain men, nor did confine itself to any season of the year…it embraced the whole of the world, and blighted the lives of all men, though differing from one another in the most marked degree, respecting neither sex nor age.”\textsuperscript{194}

What we have is a population crash that is quite common among animals and is described by René Jules Dubos:

“In many species, the numbers of animals increase continuously from year to year until a maximum population density is reached; then suddenly an enormous mortality descends. This phenomenon, known as ‘population crash,’ has long been assumed to be created by epidemics corresponding to those which have been so destructive in the course of human history…Indeed, several different kinds of pathogens have been found to attack animal populations at the time of the crash. Pasteurellae and salmonellae are among the bacterial organisms that used to be most commonly incriminated; two decades ago a particular strain of \textit{Mycobacterium muris} (the vole bacillus), isolated from field mice in England, was thought for a while to be responsible for the population crashes in these rodents. Now that viruses have taken the limelight from bacteria, they in turn have been made responsible for occurrences of widespread mortality in several animal species.

“It has become apparent, however, that the relation between population crashes and microbial diseases is far less clear than was once thought. On the one hand, several different types of pathogen can be associated with crashes in a given animal species. On the other hand, there are certain crashes for which no pathogen has been found to account for the pathological picture.”\textsuperscript{195}

\textsuperscript{193} Karen S. Cook, \textit{Trust in Society} (NY 2003), p. 270
\textsuperscript{195} René Jules Dubos, \textit{Man Adapting} (New Haven CT 1980), pp. 101-102
We are not discussing the regular periodic population crashes of hunting animals whose population grows and consumes the prey animals until there are so few left that the predator animals starve, leaving very low numbers so that their prey then grows abundant and the cycle repeats itself. We are discussing animal population crashes based on pathogens. The human race at the height of the Roman Empire was huge and open to a pathogen or pathogens that would do to it what happens to animal populations. This is generally denied, as for example David S. Kleinman suggests that although animal populations crash, “the fact is that humans are a different and unique species [not subject to such crashes].”

What historians and even certain scientists assume is that the human race, prior to the advent of our understanding of pathogens and the ability to defend against them, was not subject to what many other species experience. They have, in a sense, made our species exempt from the natural world’s calamities. Humanity was deprived of technology and nearly all that civilization provided, therefore it seemed to disappear from the archaeological record. O’Neill outlines this assumed absence of archaeology throughout the post-Roman world as it had in Britain, Ireland, Scotland, and the rest of Europe. This is the time which we have filled with the megalithic people:

“For those academics who wish to abolish the term Dark Ages there remains, however, the problem of archaeology: or rather, the lack of it. The intensive archaeological investigation which has characterized the last hundred years has added virtually nothing to our knowledge of the three hundred years which mark the peak of the ‘Dark Ages,’ namely the three centuries between the early seventh century and the early tenth. Three hundred hears, it would seem, have left almost no trace in the ground. Truly, it would appear, these years were indeed dark. Not only did men forget how to build in stone, they seem to have lost the capacity even of creating pottery; and the centuries in England that are generally designated Anglo-Saxon have left little or nothing even of this necessary domestic art. Pottery-making does appear again in the tenth century, but what did the Anglo-Saxons use in the ages that preceded it?”

Those who have followed the thesis and evidence above will understand that these people used grooved ware, beakers, and all the pottery types misdated to the Neolithic, Bronze, and early Iron Ages. The people did not disappear, nor did their archaeological remains. In a recent article Heribert Illig argues:

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197 O’Neill, op. cit., pp. 227-228
“Documents are not usually written in the open countryside, but in solid houses, and, therefore, numerous settlements have to be shown to have existed, yet all over the world there is a shortage of buildings and settlement traces for the period in question. Here is the latest example: until recently Prof. Ferdinand Opll, of Vienna, was of the opinion that in this originally Roman settlement only one cemetery church had maintained settlement continuity, but in August 2010 he finally admitted: ‘For more than 300 years, Old Vindobona [Vienna] was deserted…Wolves were searching the ruins for prey.

“This is not surprising, since Prof. Karl Brunner of the same department has for years insisted that the entire Danube valley between Linz and the Danube Bend near Vienna was uninhabitable for three centuries…—this in a particularly fertile region of all places!

“Similar cross-checks have been carried out for many places, towns, regions, and entire countries—such as all 70,000 square km of Bavaria, or for Hungary—and have always confirmed the thesis that the early Middle Ages were not just a dark age, but no part of the timeline at all.”

O’Neill goes on to show this archaeological problem exists throughout the rest of the post-Roman world:

“Yet the absence of buildings and of archaeology throughout Europe, a situation normally blamed on the depredations of the Germanic Barbarians, is matched in the Arab and Byzantines [sic] worlds—two regions never conquered by the Barbarians and therefore two regions which should not have a ‘Dark Age.’ Yet a Dark Age there most assuredly is.

“The lack of Arab and Byzantine archaeology for this period has only recently come to the attention of the scholarly community, and has caused something of a sensation…

“In the Byzantine lands archaeologists discovered an unbroken line of development from the foundation of Constantinople through the fifth and sixth centuries. But then, about forty years after the death of Justinian the Great, from the first quarter of the seventh century, there is a total and absolute break. Hardly a house, a church, or artifact of any kind has been recovered from the next three centuries. Cities were abandoned and urban life came to an end. There is no sign of revival until the middle of the tenth century. In his Byzantium: The Empire of

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New Rome, Cyril Mango describes this epoch as a ‘dark age,’ and in a chapter: ‘The Disappearance and Revival of Cities,’ he looks at the vexed question of causes. ‘The evidence,’ he notes, ‘for the collapse of the cities [in the seventh century and after] is largely archaeological.’ This veritable disappearing act is found throughout the Byzantine realms, both in the Balkans and in Asia. Sirmium, he notes, was ‘completely deserted after its surrender to the Avars in 582.’ South of Sirmium, in Stobi, we find ‘considerable evidence of building’ throughout the fifth century, but ‘no building activity whatever after the sixth century.’ Town after town throughout the region ‘ceased to exist’ shortly after circa 600. ‘The same panorama of abandonment,’ he notes, ‘is visible in Greece.’ This is the case at Athens, Corinth and elsewhere. In Boeotian Thebes there is no sign of any urban life between the sixth century and the second half of the ninth. ‘We may add,’ he says, ‘that, with the exception of Thessalonica and the island of Paros, not a single Early Christian church remained standing in all of Greece, and there is no evidence of any building activity between about 600 and the early years of the ninth century.”

“There seems then to have been a general and complete abandonment throughout all the urban centres of south-eastern Europe [just as there was in Britain.]”

Of Italy, as reported in the SIS Chronology and Catastrophism Workshop of February, 2011, p. 23, we are told of that period, citing Current World Archaeology, issue 42, Aug/Sept 2010, pp. 28–36 and The Cambridge Alumni Magazine, Issue 61, p. 16,

“…at the end of the Roman Empire, starting in the 6th century AD, Rome’s control of the Tiber Valley was severely disrupted following the Lombard invasions at the end of the 6th century [AD]. This period saw the start of transfer of occupation to hilltop towns and villages, but, although this occurred as early as the 6th century on the eastern bank of the Tiber, documentary evidence for this only comes from the 10th century. On the opposite side of the Tiber, the archaeological evidence suggests ‘a virtual depopulation of the rural landscape from the 5th to 6th centuries until the late 8th to 9th centuries,’ and elsewhere this ‘rural wasteland’ endured until the 10th to 11th centuries…Here we have yet another example of a 300 year archaeological AD ‘dark age.’…Over in Britain, the archaeology indicates that it was quite a common occurrence for Roman masonry to have lain around for centuries after the departure of the Romans only to be ‘mined’ as building materials in Medieval times.”

Getting back to Byzantium O’Neill states:

199 O’Neill, op.cit., pp. 230-231
“…from the mid-seventh century onwards, the Eastern Empire suffered its own Dark Age: Byzantium experienced three centuries during which…almost all her cities were abandoned, populations plummeted and high culture came to an end. So great was the destruction that even bronze coinage…disappeared. And when archaeology again appears, in the middle of the tenth century, the civilization it reveals has been radically altered: The old Byzantium of Late Antiquity is gone, and we find an impoverished and semi-literate rump; a Medieval Byzantium strikingly like…Medieval France, Germany and Italy…”

Describing Europe Sing C. Chew, writes

“Deurbanization became a trend that was very evident (Wells 1999). In Roman Europe, urban centers declined in size, and there was an end to building programs, which caused rural settlements to return to patterns of [the] pre-Iron Age. Settlements in rural Italy were marked by a further decline from the third century onward (Randsborg 1991). Theaters, public baths, aqueducts, and all city life decayed (Katz 1961). Villas that were the centers of socioeconomic life were not being built in Italy or the provinces. Old buildings were being reused. …

“In the western provinces of the empire, deurbanization and decay were most prominent during the fifth and sixth centuries (Drummond and Nelson 1994). For the Netherlands, urbanization was at a high level until the last half of the third century A.D., and following this it went into a dramatic decline. In lower Saxony, the number of settlements went down starting about A.D. 200 and never reached the heights achieved at A.D. 100 (Randsborg 1991). In the Cologne area, the height of urbanization occurred in the last half of the second century, and from the third century onward, decline followed and was the most drastic between A.D. 400 and 500. In Gaul, the second and third centuries were periods of villa building with decline starting about the beginning of the fourth century. This was also repeated in the Alsace-Lorraine region and west of Mainz. However, in Britain, villa settlement increased in the fourth century but declined at the start of the fifth century A.D., which is the time when the imperial military system started to decline. In southeastern Europe, villa complexes vanished as well by A.D. 400. In northwestern Europe, a large number of towns ceased to exist after A.D. 400…”

This similarity between what was found in the British Isles and the Byzantine Empire after several recurrences of plague shows that humanity was almost totally wiped out. We submit it points to a level of depopulation unheard of in the annals of

200 O’Neill, op.cit., p. 5
world history and moreover points to the situation when people in Britain and the Byzantine Empire were forced to revert to living at a level comparable to the Neolithic and Bronze Ages. The historians, and particularly the archaeologists, coming on the scene and unearthing peoples living in almost Neolithic conditions, then placed these people into that chronological context which seemed to make archaeological sense—finding people living at Neolithic levels of existence ipso facto meant they belonged to the Neolithic, later or even earlier stages of human development.

What is particularly significant regarding this issue is how it correlates with Velikovsky’s thesis as that is incorporated into the chronology of Gunnar Heinsohn, Lynn E. Rose and Emmet J. Sweeney. According to that hypothesis, mankind did not and could not have developed and cultivated civilized life in cities, had agriculture or pastoralism on a major scale prior to around 1500 B.C. In terms of Velikovsky’s hypotheses, mankind was living alongside the megafauna, and, although he undoubtedly was a hunter and gatherer, he was also prey to the great carnivores of that time. Therefore, it would have been impossible for man to have the capacity—in the daunting conditions in which he lived—to permit time to be devoted to building large permanent settlements. His life was one of bare survival with high mortality and constant movement to follow the animals he hunted. It was only with the sudden catastrophic destruction of the megafauna that man found himself in a new ecological niche that was unthreatening which he quickly filled and began to build civilization with all its wondrous achievements. Therefore, when historians and archaeologists find early sites of civilized society prior to 1500 B.C., these are chronologically anachronistic and belong either to the period just after the first Velikovskian catastrophe or, in other cases, to the post-Roman epoch where mankind was forced to live at Neolithic or Bronze Age levels. Hence, we should expect to find other anomalous sites somewhat like Skara Brae in prehistoric times that reflect this dramatic step backward.

This regression will be seen in finding Skara Brae-like prehistoric settlements and/or megalithic monuments also dated to these earlier times. These sites will, like Skara Brae, be exceptional in their development; they will contain advances that were not to come into existence for thousands of years, such as those we found like sewage, toilets, doors with hinges, canopy beds or others. These will mark these sites as post-Roman but they, like Skara Brae, will be very few in number and exceptional because they were constructed of materials that were unable to withstand the ravages of time. Other sites, built of perishable materials, which were the vast majority in the British Isles, will not be found. But the Skara-Brae-like sites will show us how the plague survivors lived in these various regions. These sites, though layered, will be
relatively short-lived from, say, 300 to 600 years although interpreted as up to 1000 years. In the words of Kathleen Kenyon, as she applied them to Jericho:

“We may therefore envisage [these sites] as a culture with all the attributes of civilization, except that of a written language. The town must have been almost modern, or at least medieval in appearance, and it must have been surrounded by fertile fields. In the neighbourhood we can assume that there were other similar towns [and] we can assume trade with and relations between neighbouring districts.”

These settlements with all their advances will come into being out of nowhere and nothing and disappear, as did the megalithic world, into nowhere and nothing.

**TURKEY AND SYRIA**

Of particular interest are sites in Turkey (Anatolia), Iraq, and Syria that we suggest defy historical development as it was long understood. In order to have a large city with specialized trades such as builders, weavers, potters, and the rest, there has to be a base of farmers to produce surpluses of food to free these people from tending the land or herding flocks to carry on their trades. As Sir Leonard Woolley explains,

“If hitherto change had been painfully slow, it was because Neolithic man was hard put to it to live. All his efforts were necessarily devoted to getting sufficient food for his family…; only when the struggle for existence ceased to absorb all his time and energy could he find leisure for the amenities of life. The first requisite for civilization was a wide extent of rich soil easily worked…

“Only where soil and climate alike are favourable can man produce in excess of his actual needs and yet have leisure to enjoy the surplus; and hitherto [during the Neolithic Age] he had found no region which satisfied both conditions…

“[Only in] a land where agriculture was rewarded without stint and little risk of failure [could civilization develop:] what was no less important, the new territories were so extensive that they could support such a [large] population as under the conditions of the Neolithic Age had been impossible.”

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That is, a relatively widespread and advanced agricultural development precedes the building of cities. John Noble Wilford echoes this:

“Scholars had long ago established that the first cities anywhere arose about 5000 years ago [in the short chronology about 3000 years ago] in the lower valley of the Tigris and Euphrates rivers...Cities were presumably an outgrowth of an increasingly productive agriculture. Crop surpluses supported expanding long-distance trade and freed people to specialize in such crafts as textiles and ceramics. As the number of merchants and artisans grew, farm villages evolved into cities.

“By the middle of the third millennium B.C., it now appears, the first experiments in city living were such a resounding success that people were flocking to new cities over the entire region, in what is now Syria, northern Iraq, and Turkey.”

However in Anatolia, a region which also contains megalithic monuments, there have been unearthed two cities; one dating to 2500 B.C., the other to about 7000–6000 B.C. The first dates to around the time of Skara Brae and will be dealt with. In 1991, Dr. Guillermo Algaze of the University of California at San Diego and his team excavated a city on the upper Euphrates River valley in southeastern Turkey named Titriş Höyük. According to Wilford

“...at the center once stood a palace and other public buildings on high ground. Out from there, streets ran through residential neighborhoods. Beyond city walls lay a cemetery and scattered suburbs.

“On closer examination, however, archaeologists have found surprises. The streets and terrace walls appear to have been laid out and built before the houses. And the houses [just like those at Skara Brae] seem to follow a master plan, some larger than others but all of the same design.

“Archaeologists are thus drawn to the conclusion that Titriş Hoyuk, population 10,000 in its heyday, represents a striking example of urban planning in antiquity. Built in about 2500 B.C., this was a kind of Levittown [a suburban town built with very similarly designed homes on Long Island, New York].”

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205 Ibid.
Strikingly, Titriş Höyük existed for a very brief time—300 years—equal to the missing archaeology of the Byzantine Empire:

“The vagaries of history also appear to have simplified the [archaeological] task. Titriş was ultimately a failure as a city, rising and falling over a 300 year period, never again to be reoccupied. So, in contrast to most ancient cities [buried beneath mounds with one period of development built above the other], the ruins of Titriş are not obscured by layer upon layer of debris; they lie less than three feet beneath the surface.”

This city cannot be as old as assumed; had that ancient date been the case, earth worms would have buried it several meters/yards beneath the surface. The three foot level is the same as the Roman ruins and the bluestone chips at Stonehenge in Britain.

While the streets were paved with “cobbles and crushed pot sherds” they found “fieldstone foundations for several houses that were more or less identical in design. Walls rising from the foundations [like houses of the Middle Ages] were made of mud bricks, which have now disintegrated…with roofs…of thatch.”

As for the life in this city:

“[The] people typically entered the houses through a door off the street that led to an antechamber and then to a second small room that opened on the main house…

“The houses were probably occupied by extended families, for each one contained several cooking areas.

“The houses were places of work as well as residence. In the ruins, archaeologists found several raised oval basins lined with plaster. High concentrations of tartaric acid in the plaster indicated the basins were used in wine production though some of the basins may have served several purposes.

“Since some basins drained into the streets, Dr. Virginia Badler, an archaeologist at the University of Toronto, said they would not have been used for wine but perhaps in washing and processing wool or fleece. Among the artifacts in several houses were stone loom weights, no doubt associated with weaving.…

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206 Ibid.
207 Ibid. p. 59
“Each house excavated so far had a family crypt, usually in the central courtyard. Though partly subterranean, the tops of the crypts were visible above the floor foundation.

“Each of the tombs contained skeletons of seven to nine people [such a small number suggests these houses were not lived in for a very long period, or that others were buried in a cemetery elsewhere], as well as the remains of food, weapons and other artifacts…

“Dr. Harvey Weiss, an archaeologist at Yale University, said that similar evidence of planned cities had been showing up at other sites in northern Syria and Iraq. ‘We don’t understand why,’ he said, ‘but this sudden urbanization and…urban planning, are becoming a very remarkable story of how civilization spread.’”

Here we have a thriving civilization that comes into being, with a system of streets, houses with doors, drains leading out of the houses, with uniform construction, flagstone bases, mud brick walls, probably wine vats, cloth, thatched roofs. This is, on a very large scale, comparable to what we found at Skara Brae. Why then did all this sudden efflorescence disappear? This we are not told. However, when the cities of the Byzantine Empire were abandoned, entire groups of people, we suggest, settled elsewhere and attempted to recreate, with whatever skills they still possessed, habitations that reflected their earlier life.

However, there was another major archaeological discovery that was even more anomalous, also found in Anatolia at a site called Çatal Hüyük, dated to 7000–6000 B.C., as reported by Knight and Lomas:

“In the Turkish province of Anatolia at a site called Çatal Hüyük, archaeologist James Mellaart excavated a city which extended over [many] acres. Mellaart estimated that it once supported a population of more than 7000 people. It was a sophisticated city with squar[ish]-built terraced houses featuring timber-supported flat roofs, with access holes that enabled them to be used as roof top verandas. On the wall of one of the buildings he found a detailed map [urban plan] of the town in its prime, showing terraced housing, and behind it a picture of a twin-peaked active volcano, which had long since become extinct. The importance of this volcano was as a source of obsidian, a variety of black volcanic glass highly prized for making stone tools and mirrors. Indeed, Mellaart investigated the economy of the city and found that it seemed to have been an important trading centre for obsidian and artefacts made from it. The city also had an agricultural infrastructure based on the

\[^{208}\text{Ibid., pp. 59-6}\]
farming of barley, wheat and cattle. It was clear from a detailed study of the rubbish heaps that the people of Çatal Hüyük also ate the meat of wild animals.

“The level of workmanship demonstrated in the artefacts he dug up—woven fabrics, jewelry, tools, pottery and obsidian mirrors—was extremely high. The craftsmen of the city were evidently well skilled in their various professions. They obviously spent most of their time on manufacture to develop such a high level of craftsmanship, which implies specialization of skills. Mirror grinders still have to eat, and so they had to trade their mirrors for food…”

“What is most surprising about this site is its date. It flourished between 7000 and 6000 BC…Mellaart noted of the site…”

“‘Neolithic civilization revealed at Çatal Hüyük shines like a supernova among the rather dim galaxy of contemporary peasant culture…[It represents] a link between the remote hunters of the Upper Paleolithic and the new order of food production that was the basis of all civilization.’”

The problem outlined at the start of this discussion of agriculture being the basis for the development now rears its head. Çatal Hüyük was supposedly built well before such advanced agricultural and pastoral development would have existed. In order to overcome this obvious contradiction that cities had come before agriculture, Jane Jacobs in *The Economy of Cities* (1969) reversed the paradigm. She well outlines the paradigm we presented earlier.

“The dogma of agricultural primacy says: agriculture first, cities later. Behind the dogma lies the notion that in pre-Neolithic times, hunting men lived only in small, economically self-sufficient groups, finding their own food, making their own weapons, tools and other manufactured goods. Not until some of the primitive groups learned to cultivate grain and raise livestock, it is thought, did stable villages emerge, and not until after the villages were built did complex divisions of labour, large economic projects, and intricate social organization become possible. These advances, coupled with a surplus of agricultural food, are supposed to have made cities possible.”

The problem Jacobs faced was how to explain and demonstrate some mechanism in order to have Çatal Hüyük, a large city for 7000–6000 B.C., to be constructed before

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209 Knight and Lomas, *Uriel’s Machine, op.cit.*, pp. 419-420
there was an adequate agricultural base to support a large population of specialist craftsmen. What she did was look at Japan after World War II and suggested that

“…it created rural productivity upon a foundation of city productivity.

“Modern productive agriculture has been reinvented [in modern times] by the grace of hundreds of innovations that were exported from the cities to the countryside, transplanted to the countryside or imitated in the countryside…The agricultural revolution occurred first in cities and later in agriculture…There is no way to increase rural productivity first and city productivity later…Because we are so used to thinking of farming as a rural activity we are especially apt to overlook the fact that new kinds of farming come out of cities.

“In very ancient times too, cities were engaged in developing agriculture and animal husbandry. In the Egyptian cities of the Old Kingdom, for example, many experiments with animal domestication were tried: records of the efforts have been left in pictures…during Old Kingdom times hyenas were tied up and force-fed until fat enough for slaughter. Pelicans were kept to lay eggs, mongooses tamed to kill rats and mice…The ass and the common house cat were domesticated in the ancient cities of the Nile; they are city animals distributed into the rural world.”

She then went on to use Çatal Hüyük as an example to advance her thesis, for surely here was a classic experiment that proved it. It was a fairly large city of thousands that supposedly existed well before the agricultural revolution was underway. She argued that the city artisans traded goods to the distant countryside farmers. As the town and countryside grew, new products developed in the town could then be traded to obtain greater supplies of food, even wild game. Much of the material found in the middens at Çatal Hüyük was of such game. Thus the town and the countryside prospered. She further suggested, according to Knight and Lomas:

“Only non-perishable food is traded because it stands the trip to the settlement best and it can be stored by the settlement. The major types of non-perishable food will be live animals and hard seeds. With a successful settlement, large quantities of live animals [many of them wild] and seeds accumulate in what has now become a small city. The settlement will quickly develop specialized individuals whose job it is to look after the stores of food. Those who look after wild animals will slaughter the most dangerous animals first, and the more docile species that can eat grass will be stored until they are needed for food, and will probably end up breeding…

\[211\] Ibid. , p. 421
“In this way, sheep, goats and cattle become domesticated [in the cities] over many generations.”

A similar but somewhat different development applies to the domestication of wild grains to domesticated ones. All in all, Jacobs appears to have shown that Çatal Hüyük is a prime example that proves cities produce agriculture first and pastoralism second, and that such a city does not defy historical/ecological development. However, what she failed to understand is that she had to feed a population of about 5000 people by this economic trade system and that to do so requires a great deal of food. This food could not be transported unless by wagons or by asses, horses or other such conveyances as these did not exist then. The wild animals had to be captured live and somehow controlled by ropes or placed in cages to be carried by hand to the cities as well. Marc Van de Mieroop specifically dealt with Jacobs’s theory in his book *The Ancient Mesopotamian City*, stating:

“A…theory emphasizes the role of long-distance trade in the development of cities. Its most extreme advocate is the urban theorist, Jane Jacobs, who has earned an almost legendary status in the USA for her ‘visionary’ ideas and is extensively quoted in anthropological and sociological literature. However, she is almost entirely unknown (or ignored) among ancient Near Eastern specialists. In her book *The Economy of Cities* (1969), she attacked the ‘dogma’ that agriculture preceded cities, and replaced it with a theory that the existence of cities led to agriculture, and that cities first originated because of long-distance trade in raw materials. The empirical basis for this theory is the existence of such eighth- to sixth-millennium settlements in the Levant and Anatolia as Jericho and Çatal Hüyük, interpreted by some scholars to have been cities. But, whereas the latter scholars acknowledge that an agricultural base was needed for these settlements, Jacobs hypothesized the existence of pre-agricultural cities antedating these excavated sites. She described an imaginary city, New Obsidian, which was [like Çatal Hüyük] involved in the long-distance trade in obsidian without being located near its sources. Food for the inhabitants would have been obtained partly through barter with nearby hunter-gatherers, but mainly through imports from foreign hunting territories. Because meat would have spoiled during its transport, live animals were driven to New Obsidian, where soon a selection of the tamest animals was made for breeding. Seeds and nuts were brought into town as they preserved better than fruits and vegetables, and when mixed together in bins and partly sown in wild patches, they were accidentally cross-bred and became ‘better’ than their wild progenitors.

Ibid., pp. 422-423

Ibid., pp. 423-424
the city began to grow most of its own food [and this spread to the countryside.]
‘Cities First—Rural Development Later.’”\(^\text{214}\)

Van de Mieroop then turns to examine the underlying fallacy of this hypothesis:

“Jacobs’s theory is based entirely on false premisses. It relies heavily on an unconvincing parallel with the modern world, where the most advanced urban areas are supposed to have the most advanced agriculture. And, when dealing with prehistory, Jacobs ignores altogether what [Paul] Bairoch has called ‘the tyranny of distance.’ The transport of food products is very expensive [without some form of transport system that can carry large amounts of food quickly over long distances], as [in prehistoric times] the transporter consumes part of his or her load. A man carrying [on foot] the entire load can daily transport 35-40 kilograms [72-88 pounds] over 30-35 kilometres [18-22 miles]. Every day he has to consume about one kilogram [2.2 pounds], so he eats his entire load in about seventeen days when he has marched at the most 600 kilometres [360 miles] one way, taking into account that he has to eat on his way back. Of course, to be able to barter the food he cannot consume all of it. An example makes this problem obvious. If a man comes a distance of 100 kilometres [62 miles] to obtain obsidian at New Obsidian, he will start out with 40 kilograms [88 pounds] of food. In three days he will have reached his destination with 37 kilograms, and he will be able to barter a maximum of 34 kilograms [65 pounds] for obsidian. Those 34 kilograms will feed one inhabitant of the city for slightly more than one month. For the 5,000 inhabitants of Çatal Hüyük to be supported for one year, 60,000 trips of this type would be needed from a radius not surpassing 100 kilometres [62 miles] from the site. That area is only 157 square kilometres [90 square miles], and considering the extremely low population density in pre-agricultural times, there were not enough people in that region to provide all the food needed in New Obsidian. Moreover, all these calculations are based on the assumption that circumstances were optimal. It seems impossible to walk 33 kilometres a day in the region around Çatal Hüyük, while rest-stops on the road would have increased the consumption by the transporter. How long would it have taken to drive live undomesticated animals over this distance? The problem could not be alleviated by using draft animals, as Jacobs states that all this activity took place before the domestication of animals. Nor does river transport provide an alternative, as rivers are only navigable in one direction in the area of Çatal Hüyük [and therefore food can only be brought from a relatively narrow region to the town]. Jacobs’s theory is thus entirely unacceptable.”\(^\text{215}\)

\(^{214}\) Marc Van de Mieroop, *The Ancient Mesopotamian City* (Oxford UK 1999), pp. 24-25

\(^{215}\) *Ibid.*, pp. 25-26
Understanding this, the archaeologists and historians suggest that agriculture and pastoralism around the site were able to feed the population of perhaps 5000 people for 1100 years from around 6700 to 5600 B.C.  

This means that the area around Çatal Hüyük had to farm and domesticate animals raised to do just that. Ian A. Todd admits on the basis of all information: “We would…suggest a population of between 5,000 and 10,000 with preference being given to the lower end of the scale [in order] to understand the relationship of the population size to the carrying capacity of the region around Çatal Hüyük.” He goes on to add:

“In order to define the carrying capacity of the Çatal Hüyük area, we analyze the territory with a 5 km [3.125 mile] radius of the site. Various works suggest that the exploitation of areas at a distance greater [than this] would not be economically feasible. The exploitation territory of Çatal Hüyük should perhaps be more accurately limited as Chisholm suggests…by walking time rather than by an artificial 5 km [3.125] limit, but for the purpose of this study such a limit will suffice.”

A farmer in Çatal Hüyük must be able to walk to his field, work there for part of the day and return home that day. The time it will take him to get there cannot be more than a few hours because the time it takes the farmer to reach his fields and return to Çatal Hüyük before dark limits the amount of useful work he can spend on maintaining the crops and protecting them. If, for example, it takes eight hours to reach the fields and eight hours to return, the farmer will have spent all or nearly all the daylight hours going to and coming back to Çatal Hüyük. Therefore the 5 kilometer—3.125 mile radius was chosen as the most feasible distance from the site that could be cultivated and pasture cattle, sheep and goats to feed this town. Todd then goes on to show how this area around the town worked to feed its population.

“The radius around the site encompasses two main types of soil…The back swamp soils, unsuitable for irrigation;…melons, cereals, and sugar beets are grown on them nowadays; and a few saline areas are used for grazing…The Carsamba fen soils are classified in three types, all with adequate drainage. Irrigation is possible, but dry farming also occurs. The main crops are wheat, barley, sugar beet, melon, oats and alfalfa…Of the 7850 hectares (ha) within the 5-km radius, the majority of the area would have been suitable for cereals, while some small poorly drained areas would probably have been utilized only for grazing. If we accept [the possibility] that some fallowing system must have been used to maintain soil fertility (one year of

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216 Ian A. Todd, Çatal Hüyük in Perspective (Menlo Park CA 1976), p. 105, figure 53
217 Ibid., p. 123
218 Ibid.
cropping followed by one year of lying fallow?) [a concept never proved to have been employed for that period], the total land area available each year would approximate 3750 ha. Allan suggests a figure of approximately 600 kg [1,320 pounds] of grain per hectare as the general level...if the 600 kg figure is retained, a total yield of 2,250,000 kg [4,950,000 pounds] of grain might be postulated annually. An annual grain requirement of 300 kg [660 pounds] per head of the population suggests that the exploitation zone of Çatal Hüyük could have supported a population of 7500. If the calculation is based on a figure of 1.5 ha of cultivable land per head, the population of the Çatal Hüyük region would be approximately 5000 people.”

All this makes theoretical sense but not chronological sense because Todd had to say, above, “If we accept that some kind of fallowing system must have been used to maintain soil fertility.” This flies in the face of what we were told earlier about agriculture during the Neolithic Age in our discussion of agriculture regarding Skara Brae. People at this very early time cleared the land by the slash and burn method and then cultivated it for a very short period until the soil lost its fertility, the people then moving on to new land to repeat this process. Todd was forced to suggest the fallow process because no other one was available to maintain the relatively large population at Çatal Hüyük. But the Greeks and Romans were well aware of fallowing and employed it, as George Edwin Fussell points out: “...the Greeks followed a crop and fallow method, whereas, as time went on, some Romans recognized the advantages of a three-course rotation (of a kind), which allowed the land to be cropped for two years in succession and to be fallow the following year or permitted a catch crop of legumes [which nitrifies the soil], or the like, to be raised after grain harvest was gathered, and the land to be fallow in the spring.”

In the Byzantine/Anatolian region this required constant care of the fields by either plowing or hoeing out weeds throughout the year, as shown by Alan Harvey:

“The most difficult problem confronting Byzantine farmers was to preserve the fertility of the soil in the climatic conditions of the eastern Mediterranean, characterised by winter rains and long summer droughts. Dry farming consists essentially of making the most of a limited supply of water. It can be done by using crops which have their main period of growth during the rainy season, by weeding thoroughly to eliminate unnecessary waste of water [taken up by weeds], and by storing rain by means of stubble fallow [leaving the unneeded part of the grain crop to fertilize the soil]. It was a laborious process because the fallow had to

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219 Ibid., pp. 123-124
be ploughed [or hoed] regularly to keep it free of weeds and to ensure that the soil was receptive when the rains came.”

When we place Çatal Hüyük in post-Roman Anatolia we do not have to suggest: “If we accept that some kind of fallowing system must have been used…” because it was being used and the 5000 or more people at Çatal Hüyük could live on the land adjacent to their town. Given these conditions of hard work with an almost subsistence lifestyle, anemia would have been fairly endemic to that population especially since the area appears to be near a swamp. According to Todd:

“Cohen has also suggested that there may have been more moisture present in the soil in the Cumra area at the time of the occupation of Çatal Hüyük than is the case today. It is notable that the site was founded on the edge of the area surrounding the village of Kucukkoy termed ‘former swamp’…an area with moderate-to-fairly-poor drainage…A further possible indication of moisture in the region is provided by the evidence of porotic hyperostosis, which J.L. Angel takes to imply the occurrence of endemic faliciparum malaria amongst the population of Çatal Hüyük, resulting from close contact with anopheline mosquitoes. The source of the mosquitoes may have been the back swamp areas to the north and west of the site.”

It is “presumed” that malaria “affected 41% of the 143 [skeletons]” noted, but anemia would surely be responsible for this and it is admitted that anemia was prevalent due to “dietary deficiencies” as shown by Robert L. O’Connell who writes: “The skeletal evidence is also revealing in another regard. In spite of the town’s obvious material wealth and capacity to grow crops—something like fourteen food plants were under cultivation—there are clear indications of anemia and other problems that could be attributed to widespread dietary deficiencies.”

This is particularly seen in the infant mortality rate. Children are more susceptible to death from anemia as pointed out by Christopher Willis: “The people of Çatal Hüyük suffered appalling infant mortality and none of them made it into what we would call old age [as during the Middle Ages].”

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222 Todd, *op. cit.*, pp. 112-113
223 *Ibid.*, p. 73
225 Christopher Willis, *Yellow Fever, op.cit.*, p. 40
This aspect of malnutrition and high infant mortality well describes conditions that existed in medieval times and this condition applies to the entire Neolithic population as pointed out by Melvin Harris and Eric B. Ross. While there was a short-lived improvement in the quality of life at the beginning of the Neolithic Age, probably because the smaller population consumed whatever food had been stored, they tell us that

“...these authors also suggest that chronic malnutrition was first evidenced more generally in the Neolithic in the form of thinned cortical layers of long bones and they note that rates of enamel hypoplasia are higher for neolithic groups [probably because they also ate larger amounts of grains] than for earlier ones. In Rathbun’s data from Iran, moreover, the Neolithic appears to be a period of peak stress in terms of rates of infection, arthritis, and at least one indicator of malnutrition, porotic hyperostosis. In Rathbun’s figures, later [civilized] populations seem to have adjusted more successfully to farming than did the Neolithic populations. Angel’s figures for the Mediterranean suggest a similar pattern. Several indicators of malnutrition in this study suggest that nutrition declined into and through the Neolithic and that a rebound in nutrition occurred only beginning with the early Bronze Age. In Europe, Meiklejohn et al. note that the decline in stature that began in the Mesolithic extended into the Neolithic, with rebound beginning only late in the Neolithic. Martin et al. suggest that early farming populations were healthier than later farmers, but they offer no pre-farming groups for comparison...

“Equally common is a pattern in which early farmers [of the Neolithic period] display poor health in comparison with [hunter-gatherer and highly agrarian] populations both before and after them.”

In this respect the Neolithic exhibits a clear correlation to the Middle Ages, just as we observed with so many other aspects of Neolithic life. Two more points about the population indicate that the people of Çatal Hüyük were not native to that site. If, as we suggest, there was much abandonment of the great cities because of the plagues, then this should show up in the demographic growth of Çatal Hüyük, namely its numbers should show sudden influxes of people emigrating from the pestilential cities to the countryside. Paul K. Wason reports:

“If Angel (1971) is even roughly correct in his assessment of demographic trends, Çatal’s population grew much faster than did the settlement on which Mellaart postulates extensive emigration. ‘Many towns and villages [around the

site] would owe their origin to Çatal Hüyük’s population explosion…” [Though] this is obviously speculative, yet there is evidence of cultural traits common to these [nearby] sites, and we might reasonably assume that innovations most often moved out from Çatal [to other sites]…Mellaart observes that: ‘its culture covers the entire Konya plain as well as a number of outlying areas.’[227]

Furthermore, if the population of Çatal Hüyük came from some of the major cities and towns of the Byzantine Empire in post-Roman times their racial characteristics should reflect the cosmopolitan nature of the population of that period. The Byzantine Empire, of course, was trading with Africa, Europe, and the Near East, and clearly people from these regions would have settled in Constantinople and its other great cities. According to Todd:

“Preliminary publication of the study of the morphology of the population of Çatal Hüyük by Denise Frembach…indicates the existence of two races: Proto-Mediterranean (mainly Dolichocephalic [longheaded people]) and Alpine (Brachycephalic [round headed people]). Both races occur in all levels from which skeletal material was examined.”[228]

As time went on, this number was enlarged to three types as shown by Kris Jeter: “The residents of Catal Huyuk consisted of three races. Sixty percent were hardy Eurafrican, twenty-four percent were circular-headed Alpines, and seventeen percent were thin, [long headed] delicate-boned Mediterraneans. This community readily welcomed emigrants [to the town].”[229] Carl J. Becker enlarges on this and its ramifications:

“The first human town of any size seems to have been created in Anatolia, or modern-day Turkey, around 7000 B.C. Çatal Hüyük was a town that could have housed 8,000 people...A study of [about ] 300 skeletons uncovered at Çatal Hüyük reveals a multiplicity of physical [racial] types from around the Mediterranean. There are long-headed Eurafricans—Ham?, long–headed proto-Mediterraneans—Shem?, and short [round]-headed Alpine types—Japheth?...The most numerous group resembles Upper Paleolithic Combe-Chapelle humans from southern France...Mellaart emphasizes that these are still very close to their Upper

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228 Todd, op.cit., p. 73
229 Kris Jeter, “Partnership Cultures,” in Katja Boh et al., Cross-Cultural Perspectives on Families, Work and Change (Binghamton NY 1990), p. 282
Paleolithic ancestry. Çatal Hüyük represents a community of Mediterranean peoples the likes of which would never occur again down to the present day.”

Tom Hayes put the case for the cosmopolitan nature of Çatal Hüyük thus:

“The first cosmopolitan city-state, Catal Huyuk started out humbly…, a few huts located near a clean water source. Soon, more structures went up and more people passing through opted to stay. Over time, people far and wide…sought it out. Gradually, a new form took shape, a city. While typical [Stone Age] settlements of the time may have had a dozen or so residents, Catal Huyuk became home to…thousand[s of] citizens…This unprecedented degree of human scale and density required people to learn new ways….

“The ‘urban revolution’ that began at Catal Huyuk spread…”

Robert M. Schoch with Robert Aquinas McNally write: “In its day Çatal Hüyük was like the imperial Rome of the Caesars; a cosmopolitan center…” They write elsewhere: “Consider…the city of Çatal Hüyük in what is now Turkey. This bustling city of the seventh millennium BC was home to some 7000 residents and was a cosmopolitan melting pot of Asian, European and African immigrants.”

In this respect it must be acknowledged that when this city or town was built, it developed advanced crafts and arts around 7000–6000 B.C. or about 1000 years or so after the Ice Age ended and the extinction of the mammoths and other megafauna had supposedly occurred. Lucy Wyatt understood the enigma of such a town existing at that early time: “One should not forget that the time of Çatal Hüyük was relatively soon after the end of the Ice Age and the disappearance of the woolly mammoth. Life was still fairly primitive: we were not long out of the cave…”

We do not disagree that civilization can develop rather quickly, but that after developing around 7000–6000 B.C., it should have become a permanent part of the Near Eastern landscape. That is, there is a hiatus of five millennia that separates

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Çatal Hüyük from the first early cities of Mesopotamia based on the established chronology and almost seven millennia based on the short chronology.

If our hypothesis is correct Çatal Hüyük will, like Skara Brae, have many firsts in terms of civilization that do not come into existence as part of urban life until some 5000 years later and can have counterparts in the post-Roman epoch. Wyatt asks about these unique developments for that time:

“Yet the people of Çatal Hüyük had access to an anachronistic sophistication—such as the beads they had in their possession which were too fine for modern needles, implying a technical ability that does not re-occur until ancient Egypt five thousand years later. How does one make sense of Çatal Hüyük’s apparently contradictory combination of sophistication and the primitive? If the people of Çatal Hüyük did not develop their skills of their own accord, then from whom and where did they acquire their knowledge?”

Our answer is that the sophistication of these people comes from their having lived in the Byzantine Empire and some of them had some knowledge of these arts and crafts. The nature of their primitive development stems from the fact that when the great plagues overwhelmed the Byzantine population, the people abandoned their cities and towns and, with whatever knowledge they possessed, moved away to certain places and began to rebuild civilization as it were almost from scratch. This is precisely what we found at Skara Brae and will now analyze in just this light. Its sophistication is riddled with anachronistic enigmas when placed in the eighth-seventh millennium B.C., but not when placed in the seventh-tenth centuries AD. In the words of Peter N. Stearns et al.: “Çatal Hüyük was the most advanced human center of the Neolithic Age.”236 Let us attempt to “make sense” of all this.

The houses there were made of sun-dried rectangular bricks as Todd explains:

“The buildings at Çatal Hüyük were constructed of sun-dried mud brick, and no use was made of stone. The bricks were formed in a mold and usually included much straw; they measure up to 95 cm [37.4 inches] in length and range in thickness from 8 to 10 cm [3 to 4 inches]. Mortar was frequently employed in thick layers between courses. Wooden beams are also a feature of the

architecture…In order to minimize the span of the roof [like Skara Brae], a form of corbelling was employed.”

The question of course is: from whence did these people learn to make sun-dried [adobe] rectangular bricks in moulds about 7000-6000 BC simply out of nowhere? Since we maintain these people came from the post-Roman Byzantine world we would expect that in this empire mud brick was employed. In fact, there were Byzantine laws guaranteeing the quality of mud brick construction as pointed out by Cyril Mango who writes that Byzantine “[b]uildings of mud-brick must endure six years, and if within these six years the work collapses because of the craftsman’s incompetence, he shall renew it without charge.”

Not only was the town constructed of mud bricks about three lengths of the human hand and the width of one hand which indicates a brick craftsperson or -person came to the site, but surprisingly, at a certain point, someone actually made a map, the first urban map, showing the structure of Çatal Hüyük. According to Jeremy Harwood and A. Sarah Bendall:

“One of the earliest surviving maps dates from around 6200 B.C.E.…It comes from [a wall of] the prehistoric site of Çatal Hüyük…The map is the oldest known town plan in existence today. It is not a measured or surveyed plan but a picture map, with some parts represented in plan (as if seen from a viewpoint vertically above the town) and some parts represented in profile (as if seen from ground level). The mixture of profile and plan is still characteristic of many modern maps.…

“The map found…is a 275-cm-long (9-ft) wall painting (the original may have been even bigger, as only fragments have survived)…. The plan shows some 80 buildings arranged on rising terraces…

“The Çatal Hüyük map is a sophisticated representation of this Neolithic settlement. What makes it even more remarkable is the fact that, as confirmed by radiocarbon dating, the map was created 2,700 years before cuneiform writing was invented…."

237 Todd, op. cit., pp. 29-30
238 Cyril A. Mango, The Art of the Byzantine Empire, 312-1453 (Toronto 1986), p. 207
239 Jeremy Harwood, introduction by A. Sarah Bendall, To the Ends of the Earth: 100 Maps That Changed the World (Cincinnati OH 2006), pp. 11-12
Above and beyond this remarkable finding, the very order of the town, like Skara Brae, was haphazard and without any particular arrangement. This is, in fact, much like the haphazard arrangements that characterize the later medieval towns and cities. Mark Gelernter presents just this point in his discussion of architectural layout in the Middle Ages:

“The revival of cities after the millennium [A.D. 1000] led to a variety of town designs. Some new towns were rigidly laid out according to the old Roman grid system, while others softened or bent their grids to accommodate local geographical conditions like curving rivers or hilltops [etc.]…Existing towns built on an old Roman grid still showed hints of the grid, but over centuries of destruction and reconstruction the street layouts usually altered to what we now typically think of as a medieval street pattern. Streets curved to follow the lay of the land, or to avoid obstacles; they might abruptly change direction altogether after someone constructed a building directly on the old street line. This haphazard growth harks back to the casual arrangements of archaic cultures like Çatal Hüyük, where no strong central authority imposed a plan onto the town.”

Although the earlier levels of the town did not have streets, according to Todd, “only in the latest levels (III-I) are any spaces which resemble streets found between houses.” This haphazard arrangement well resembles Skara Brae.

It is assumed that these layers of buildings represent around 1000 to 1500 years of habitation, as Todd notes: “The number of building levels and the unusually good state of preservation of the walls account for the accumulation of possibly 18 m[eters or 60 feet] or more of occupation debris within an estimated span of 1000 to 1500 years.” The problem with this estimate is that it is based on the way the stratigraphical levels in Mesopotamia were dated where mud brick was also a building material. As we have shown repeatedly in the previous volumes, based on scientific technological and other grounds, the chronology of that region must be shortened by about two thirds. This gives evidence, the 1000- to 1500-year occupation of Çatal Hüyük must also be shortened by about two-thirds, or somewhere between 300 and 500 years. This is well in accord with the chronology we uphold. Given that southwestern Anatolia in Neolithic times received greater rainfall than Mesopotamia in most of historical times, the length of the occupation

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241 Todd, *op. cit.*, p. 24
242 Ibid., p. 20
of Çatal Hüyük must be shortened even further, again, in accord with the chronology we uphold. Marion Valerie Cutting estimates rainfall there at around 500 mm or 20 inches. What archaeologists are doing is using the same stratigraphical measure that we showed has failed throughout the Near East, to gauge the chronology of Çatal Hüyük so that it accords with radiocarbon, another chronometric process we have shown has also failed.

As with Skara Brae, the furnishings of the houses at Çatal Hüyük include built-in furniture, ovens, benches, rectangular or square hearths with a kerb around them, cupboards or recesses in the wall, and querns sunk into the floor. Todd describes these furnishings:

“Internal features in houses and storerooms vary considerably, especially in the earliest levels, but a certain degree of standardization is also apparent. The ladder leading up to the roof always seems to have been located against the south wall, in association with the hearths and ovens which were probably placed there to take advantage of the ventilation provided by the opening in the roof. Benches and lower platforms [beds and sofas] occur against the walls in all houses…Each house usually contains a single rectangular or square hearth with raised edges, positioned close to the south wall; however, circular hearths are found in the lower [earlier] levels. Ovens, often several in the same house, are characteristically oval, flat-topped, and partially set into the south wall. Other internal features found in some houses include small storage areas or cupboards recessed into the walls and querns sunk into the floor. Plastered bins for grain and boxes for tools are commonly found in the storerooms attached to the houses.”

Interestingly, there appears to have been the same sort of distinction in sleeping arrangements for men and women. According to Riane Tennenhaus Eissler, “…in Çatal Hüyük the sleeping platform where the woman’s personal possessions and her bed or divan were located is always found in the same place, on the east side of the living quarters. That of the man shifts and is also somewhat smaller.” These larger beds may have been to accommodate the mother and her children. There can be no doubt that these houses in many respects were far from what one would expect from people who had no former historical development. Even more significantly, Sandra

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244 Todd, *op. cit.*, pp. 27-29
Kynes tells us: “In Roman homes the altar was frequently placed near the hearth—a common practice in earlier Neolithic settlements such as Çatal Hüyük…”

When we turn to technologies at Çatal Hüyük we find that these were highly advanced, well ahead of what one would consider Neolithic as Harun Yahya, a religious Moslem, argues, first citing James Mellaart:

“‘The amount of technological specialization at Catal Huyuk is one of the striking features in this highly developed society which was obviously in the vanguard of Neolithic progress...How for example, did they polish a mirror of obsidian, a hard volcanic glass, without scratching it and how did they drill holes through stone beads (including obsidian), holes so small that no fine modern needle can penetrate. Where and when did they learn to smelt copper and lead...?’

“These findings showed that the inhabitants of Catal Huyuk possessed an understanding of urban life, were capable of planning, design and calculation, and that their artistic understanding was far more advanced than had been thought. Professor Ian Hodder, current leader of the excavation team [there], states that these findings totally invalidate evolutionist [historical theoretical] claims [for the gradual development of civilization]. He says that they have unearthed an astonishing art whose origins were unclear and notes that it was very difficult to account for the geographical position of Catal Huyuk—which, according to Hodder, has no direct geographical link to areas known to be settled at the time. The frescoes discovered are very advanced for the period. He says that...the real question is how the group of people achieved such a stunning cultural success. So far as we know, he says, there was no evolution in the cultural development achieved at Catal Huyuk, where such major works of art [and, we add, technological advances] emerged spontaneously and from nothing.”

In terms of weaving Eric Broudy reports: “In the early 1960s fragments of plainwoven cloth with up to 30-x-38 threads per inch—as fine as today’s lightweight wools—were found at Çatal Hüyük...The fiber might have been flax or possibly wool, but the threads were smooth and well prepared for weaving. The presence of a heading cord on some fragments suggests that they were woven on a

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246 Sandra Kynes, *Your Altar: Creating a Sacred Space for Prayer and Meditation* (Woodbury MN 2007), p. 166
warp-weighted loom…but the horizontal ground loom…was an equal possibility.”

Of this development Todd states:

“Despite the dampness of the strata below the surface of the site, certain perishable materials were preserved in carbonized form in the burials below Level-VI buildings. These include human flesh, textiles, skins, and fur in addition to wooden vessels…Published analyses of the textiles…variously identify the fibers as wool or flax (Burnham, 1965 and Ryder, 1965), but there seem to be objections to both identifications, and the question must remain open. The preserved fragments indicate considerable competence in textile manufacture, and several different techniques were employed. Burnham mentions two qualities of tabby woven cloth in addition to netlike twined fabrics. Heading cords and one example of a selvage [unfraying edges] were found.”

Rosemary Radford Ruether further reports “Many of these wall paintings seem to portray woven wall hangings, indicating the extensive development of textiles. Some of the designs are still seen in woven rugs in the area today.” In New Scientist we find a discussion of “the earliest known textiles,” which shows “Turkish carpet-making, it seems, is a very ancient industry; wall paintings show gaily patterned designs not very different from the rugs of today.” While there was no color left in the textiles found at Çatal Hüyük because the materials were carbonized (burnt), there is clear evidence to this effect as pointed out by Robert Chenciner: “The earliest indication of coloured thread may be from Çatal Hüyük…At this…site a group of beads were found with traces of red inside the string holes, which suggests that the now missing thread was originally coloured red.” With respect to the wall pictures, Bruno Barbatti tells us: “In Çatal Hüyük burial chambers were found with wall pictures which are assumed to be painted carpets or wall hangings. These would be [if actually found] the absolutely oldest evidence of the existence of carpets known to us up to the present. The archaeologists Mellaart and Hirsch have already pointed out the affinity between the oldest Neolithic evidence of textile art in Anatolia and present-day Anatolian

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249 Todd, op. cit., p. 95
250 Rosemary Radford Ruether, Goddess and the Divine Feminine: A Western Religious History (Berkeley CA 2005), p. 28
251 “Notes and Comments,” New Scientist (February 21, 1963), p. 389
kilims [rugs].” 253 One is led to ask how similar designs used in the Neolithic Age 7000–6000 B.C., which disappeared with that culture, should reappear on Turkish rugs made in more modern and present-day times? Surely if these designs were used in late post-Roman times it is much more feasible that some of them were copied into the present. The idea that these designs were miraculously preserved for about 9000 years defies reason.

Also, as with pottery found in Megalithic Age Europe, the pottery sherds found at Çatal Hüyük are not wheel-turned. Speaking of this site, Charles Gates states: “Pottery, always handmade without recourse to a potter’s wheel, occurs from the earliest levels…”254 According to Cynthia Stokes Brown: “People in Çatal Hüyük made coil-based pottery, not your wheel-turned variety.”255 And there are indications that wine was also made at that time. As noted by Elsie Boulding: “Hackberry seeds occur in great quantity [at Çatal Hüyük] suggesting the producing [malting] of hackberry wine.”256

As for metallurgy, Robert Raymond reports: “The particular significance of Çatal Hüyük…lies in the discovery, among the thousands of artifacts, of a handful of small objects made of copper and lead, together with fragments of mineral ores of copper, lead, iron, and mercury. Some copper pieces which were strung on the fringe of garments show signs of being hammered…”257 He adds: “A solid copper mace head found at Can Hason, not far from Çatal Hüyük, and dated to about 5000 BC, was certainly a significant advance for this region…because it is the earliest indisputable example yet found of metal cast in a mold.”258

Edward W. Soja, citing Mellaart, summarizes all this development thus:

“The wealth of material produced by Çatal Hüyük is unrivaled by any other Neolithic site. Moreover, not being a village but a town or a city, its products have a definitely metropolitan air. Çatal Hüyük could afford luxuries: obsidian mirrors,
ceremonial daggers, and trinkets of metal beyond the reach of most of its known contemporaries. Copper and lead were smelted and worked into beads, tubes and possibly small tools, thus taking the beginning of metallurgy back into the seventh millennium [B.C.]. Its stone industry in local obsidian and imported flint is the most elegant in the period. Its wooden vessels are varied and sophisticated. Its woolen textile industry fully developed...Funeral gifts though not rich by later standards are less sparing than among other contemporary cultures. Trade was well established [and] there is evidence for Neolithic religion in the form of numerous shrines, artistically decorated with reliefs in plaster...or with wall paintings in one or more colours.”

Elsewhere Mellaart speaks of:

“...the weavers and basketmakers, the matmakers; the carpenters and joiners; the men who made the polished tools (axes and adzes, polishers and grinders, chisels, mace heads and palettes); the bead makers who drilled in stone beads ‘holes that no modern steel needle’ can penetrate and who carved pendants and used stone inlays; the makers of shell beads from dentalium, cowrie and fossil oyster, the flint and obsidian knappers who produced the pressure-flaked daggers, spearheads, lance heads, arrowheads, knives, sickle blades, scrapers and borers; the merchants of skin, leather and fur; the workers in bone, who made the borers, awls, punches, knives, scrapers, ladles, spoons, bowls, scoops, spatulas, bodkins, belt hooks, antler toggles, pins and cosmetic sticks; the carvers of wooden bowls, the mirror makers, the bow makers; the men who hammered native copper into sheets and worked it into beads, pendants, rings and other trinkets; the builders; the merchants and trades who obtained all the raw materials; and finally the artists—carvers of statuettes and painters.”

Soja adds: “Notably absent from the list, in part because Mellaart downplays the heavy emphasis given to it by most archaeologists, is pottery-making. But even here there are some pathbreaking achievements. Although it was rather simply made, easily breakable and mostly undecorated, the world’s earliest pottery can also be traced to Çatal Hüyük and the nearby region of northern Syria.” The development of any site will depend on who survived the plagues and what skills they brought with them. In most cases, because finding food for survival was a paramount concern, most crafts would be lost. Therefore, the sophistication or

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260 Ibid., Mellaart, p. 99 and Soja, pp. 34-40
261 op.cit., Soja, p. 42
primitive character of these sites will vary greatly but the simplicity of their construction will largely remain similar.

The domestication of plants and animals found at Çatal Hüyük presents another major problem because we know of no precursor developments from that time. Here Todd shows with regard to grain domestication:

“Discussion of the prehistoric flora of the Konya Plain must also include a brief examination of the difficulties posed by the natural habitat zone of the various cereals. Several forms of wheat and barley have been found at Çatal Hüyük, and Helbaek has indicated that the plant husbandry of the site must have a long prehistory [of plant domestication] in another area, since at least some of the wild progenitors would not be native to an area such as the Konya Plain. Helbaek’s distribution maps for wild wheat and barley do, however, show that central Anatolia [far from Çatal Hüyük in the southwest] as a whole forms part of the native habitat zone for...(the ancestor of domesticated barley) and...(the ancestor of einkorn [wild wheat])...and the origin of the Çatal Hüyük cereals might therefore lie too far from the site. The later maps published by Harlan and Zohary do not include [or show] central Anatolia and the Konya Plain in the area of distribution of wild wheat or barley, suggesting that the prehistory of the Çatal Hüyük plants must lie at a[n even] greater distance from the site. Cohen is correct in stating that it is unwise to base discussion of the wild habitat zones of plants on the modern ecological aspect of an area such as the Konya Plain, but the balance of the evidence [the lack of these pollens in central Turkey for that earlier time] indicates they were not part of the prehistoric landscape of the Konya Plain or surrounding regions.”262

The evidence indicates that the wild forms of barley and wheat did not grow in Anatolia during or prior to the supposed construction of Çatal Hüyük. This means that in order to grow the domesticated forms of these cereals, these had to be domesticated far from the Konya Plain and then brought to Çatal Hüyük after it was founded. In essence, these grains had to be domesticated by some unknown earlier culture, which apparently never built a town, and then this unknown earlier culture taught or brought this knowledge to Anatolia. But what, when, how and where this earlier culture was that developed these different domesticated grains and who it was we apparently don’t know nor are likely to know. However, when we place Çatal Hüyük and the various sites adjacent to it on the Konya Plain in post-Roman times, then of course we know these people had such grains. They had been used in the pre-Roman Near East by the peoples of Mesopotamia. We know

262 Todd, op.cit., pp. 115-116
what grains had been used, where they were used, when they had been used, and why. There is no ambiguity or problem with the people of Çatal Hüyük growing these crops and doing so using the fallow method of cultivation.

However, one aspect of the art at Çatal Hüyük may be significant. Since these people came from the Christian Byzantine empire they might have incorporated some iconographic aspect of that earlier experience into their art, although most of these connections must have been lost. Yet, according to Eisler:

“One of the most fascinating works of art excavated at Çatal Hüyük is a carved relief of a woman and a man embracing, and next to them, the woman with a child in her arms. As Mellaart writes, ‘This may be one of the earliest representations of the hieros gamos, the ‘sacred marriage’—a rite of sacred sexual union that… survived as an important mythical theme well into historical times.

“Even more interesting is the second part of this Neolithic plaque [with the mother holding the child which] may also be an ancient precursor of a major theme in later historic times: the thousands of virgin and child figures in European Christian art. However [, based on its Neolithic date], it would have had a very different meaning.”

But if placed in post-Roman times this icon may indeed reflect the vestiges of Christian iconography.

Having achieved all this, one would expect that out of this development the people of Çatal Hüyük and those they traded with would go on and flower into an effulgent civilization, yet that did not happen. Like the megalithic peoples of Europe, for no apparent reason this supernova glow fades away into darkness as Michael Rice tells us: “After a few centuries of richly productive life, [Çatal Hüyük and the rest of] the Anatolian settlements disappeared: a long night descended until a second dawn heralded the first appearance of those civilizations [Mesopotamia and Egypt] which were to set the pattern for all the ages which followed them.”

It never dawns on the historians that these settlements were pagan and were either converted or killed off by later Christians and Muslims after A.D. 900. Note that Rice mentioned other “Anatolian settlements,” that exhibited advanced characteristics but then vanished. These we encountered all across

263 Riane Tennenhaus Eisler, Sacred Pleasure: Sex, Myth and the Politics of the Body (San Francisco CA 1995), pp. 62-63
Europe and now will find the same throughout the Near East, Africa, and later in Persia, India, China, Korea, and Japan. Burra Gautam Sidharth reports:

“The most dramatic archaeological finding of recent times has been the relatively unknown excavations at Nevalı Çori in Anatolia. This site dates back to around 7500 B.C. Current excavations there reveal even older underlying structures…at least several centuries older…[T]he Nevalı Çori civilization is unique in that it represents an already developed civilization with Megalithic elements…In fact this archaeological site contradicts straightaway the theory that civilization began in Egypt and Sumeria around 3000 B.C.

“The very remarkable feature of Nevalı Çori is that in civilizational terms it is [like Çatal Hüyük] an isolated oasis within the framework of present-day knowledge. It does not relate to any civilization or culture of its period. There is a gap of some five thousand years before we come to a similar civilization. With one exception [Çatal Hüyük].

“Its echoes can be found again in the fairly recent excavation at Mehrgarh in the Baluchistan area of the Indian subcontinent. The Mehrgarh civilization dates back to between 7000 B.C. and 6000 B.C. and parallels Nevalı Çori in terms of economy, agriculture, domestication of animals, and the planning and layout of large settlements (Professor Hauptmann, the excavator of Nevalı Çori, subscribes to this view).

“The buildings at Mehrgarh were constructed of mud bricks. Several rooms were used for habitation and storage of food. Stone tools were used for harvesting cereal grasses…Copper metallurgy was also present… [Later] pottery was made using potter’s wheels.”

Steven Mithen reports of Nevalı Çori:

“Here, no more than three kilometres [1.9 miles] south of the Euphrates [one] finds the village of Nevalı Çori…There are about twenty-five abandoned buildings—all single story, rectangular in shape, and built from limestone blocks bonded with mud-mortar…

“Several houses had been aligned in a terrace with narrow passages [like those at Skara Brae]…Most have thick plaster floors; where these are decayed, stone drainage channels…are exposed [like those at Skara Brae].

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265 Burra Gautam Sidharth, The Celestial Key to the Vedas (Rochester VT 1999), p. 93
“Within [one] finds clay and stone figurines that had fallen from wooden shelves…

“Animal pens have collapsed, while clusters of grinding stones sit amidst husks and chaff [of grain]. …

“A stone bench runs around the walls, partitioned into sections by ten stone pillars. There are further slab-like pillars in the middle of the room. These have T-shaped capitals (\( \text{T} \)) and…when [one] looks closely [one] sees a pair of human arms carved in low relief…From the steps [one] looks towards a niche in the opposite wall. It contains a human head upon which a snake has come to rest—both head and snake carved in stone. The surrounding walls had once been thickly plastered and covered in exotic murals painted in red and black.…

“[One can also find] more sculptures, some free-standing, some built into the walls and pillars…

“[The] inhabitants had been farmers with domesticated wheat and herds of sheep and goat, although hunting and gathering had also been pursued.”266

Rather tragically the construction of the Atatürk Dam has flooded this site, and further evidence of its development cannot be obtained. However, there is a similar site not far away which exhibits similar carvings and materials, known as Göbekli Tepe. According to Harald Haarmann and Joan Marler:

“One of the early sites that challenges the traditional definition of Neolithic life [of primitive people living at the subsistence level] is Göbekli Tepe, in the Urfa region of southeast Anatolia. This settlement, which began during the 10th millennium BC, was a community of fully sedentary hunter-gatherers showing no signs of farming or plant and animal domestication [nearby]. The inhabitants created elaborate ritual buildings with monumental sculpture emphasizing a range of wild animals that were significant to ‘a vanishing society of archaic hunters’ (Hauptmann, 2003:270).

“‘The decorated pillars and sculptures of Göbekli Tepe possibly represent a materialisation of the imaginative world and spiritual beliefs of this…society…’ (Hauptmann 2003:264).

Charles Ginenthal, *Pillars of the Past*, vol. IV

“The site of Göbekli Tepe rivals Çatalhöyük in terms of its animal symbolism and monumental art. The carved relief[s]...engraved on a stone slab within a sanctuary resemble the plaster wall reliefs at Çatal [Hüyük].”

Steven Mithen describes the site thus:

“Göbekli Tepe is entirely unique in the Early Neolithic archaeology of western Asia and is requiring archaeologists to reconsider their understanding of Neolithic religion and the origin of agriculture. Although excavated since 1996...the nature of the site has only revealed itself in the most recent field seasons of work. It has become apparent that several semisubterranean circular structures were constructed into the summit of a limestone hill...most likely around 9500 BCE ([Klaus] Schmidt, personal communication). These [circles] were up to ten meters [33 feet] in diameter and had stone-built walls. In the center of each structure, two massive stone pillars were erected, each weighing around seven tons. Additional pillars were erected at regular intervals around the walls and separated by benches [between them]. The pillars had been quarried from local limestone. Their surfaces [as at Stonehenge] were carefully prepared [smoothed] and then carved with wild animals...Various abstract symbols were also engraved upon their surfaces.

“There are no traces of domestic activity at the site, leading the excavator to describe it as a Neolithic hilltop religious sanctuary—an assessment that cannot be challenged on present evidence. Its construction [like Stonehenge and other megalithic sites in Europe] required a vast amount of time and effort. The ambition of those who undertook the work is evident from one of the stone pillars that was not completely removed from the bedrock; it would have weighed at least thirty tons and stood seven meters [22 feet high].”

In his book, *After the Ice*, Mithen shows

“The slabs were assumed to be the remains of a cemetery, probably of Byzantine date, which accorded with a few shards of medieval pottery found by the survey [similar to Roman, megalithic and medieval pottery found at sites in megalithic Britain and Brittany]...In the 1960s the idea of an Early Neolithic site perched upon a hilltop was simply inconceivable...Klaus Schmidt of the German Archaeological Institute...in 1994 recognized the...artifacts as Neolithic...Excavations have continued...to reveal a truly spectacular and unique Neolithic site. When Klaus

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267 Harald Haarmann and Joan Marler, *Introducing the Mythological Crescent: Ancient Beliefs and Imagery connecting Eurasia with Anatolia* (Wiesbaden, Germany 2008), p. 128
showed me around…at the end of his 2002 excavation season I felt completely overwhelmed by what he had discovered and the grandeur of the setting.

“Very soon after 9600 BC…, people had come to Göbekli and carved the massive ‘T-shaped’ pillars out of the limestone bedrock…These were erected within circular structures that had been sunk into the hill to create what looked like cellars in the earth….The face of one pillar had been carved to depict a human arm and the pillars themselves resembled massive human torsos.

“Four adjacent buildings of this type had been exposed when I made my visit and they simply took my breath away. Schmidt suspects there are several more still deeply buried below the surface of the hill. When the site had been abandoned, the Early Neolithic people [like those at Newgrange and other passage graves] deliberately buried their ritual buildings and pillars beneath several tons of soil.

“The time and effort [as with the construction of megalithic monuments in Europe] involved in quarrying, carving, transporting and erecting such pillars by people equipped with no more than flint tools is staggering to consider. And even the 7-ton pillars had not entirely satisfied their needs. When Klaus showed me the quarries located up to 100 metres [320 feet] from the buildings he pointed to an unfinished ‘T-shaped’ pillar still partly connected to the bedrock—if removed it would have been no less than 20 feet long and 50 tons in weight.”

It is assumed at this early date, ca 9500 B.C., that hunter-gatherers who lived a subsistence life, finding game and edible plants much like nomadic North American Indians, could devote thousands upon thousands of man hours to digging semi-subterranean chambers, quarrying dozens of huge stone pillars, dragging them the lengths of football fields and erecting circular temples with them. In Britain archaeologists understand that a fairly advanced level of both cultivation agriculture and a supply of domesticated animals are required to provide the nutrition in such abundance as to allow for just such an enormous undertaking. At that 9600 B.C. time, the Neolithic people were supposedly living in small scattered groups moving from place to place, following the herds, etc. Yet we are being asked to believe that somehow a group of such people were able to hunt and gather sufficient food to store, without any means of preserving these foods for long periods of time, and take thousands upon thousands of man hours out of their hunting and gathering occupation to erect these monuments at Göbekli Tepe. To get around this problem Roger Matthews tells us:

269 Mithen, After the Ice, op. cit., pp. 65-66
“[Klaus] Schmidt has argued that, as some of the megaliths used in construction weigh up to fifty tons, there must have been regional assemblies of hunter-gatherer communities from a wide area around Göbekli Tepe in order to provide the necessary labour, and that prolonged occupation of communities at the site, during periods of construction and use of the buildings, may have stimulated incipient cultivation of the locally available wild cereals.”  

Note that he claims without proof that these Neolithic people formed assemblies and came to Göbekli Tepe and stayed there for a period long enough to erect some of these monuments. But, while traveling to this site, they had to eat and therefore had to hunt and gather food along the way. Then, while there, these assemblies had to go through the countryside to gather even more food. But this implies that the animals around the region, scenting their predators, stayed there rather than migrating to safer areas farther from the site. What Schmidt has offered as his solution is simply a hypothesis divorced from any evidence. Matthews turns to another researcher who also invented an entire series of unproven explanations to explain this problem away:

“As Brian Hayden has articulately summarized, there is an emerging consensus [not evidence or proof] in many approaches to the Neolithic revolution (Hayden 1995). For the revolution to happen at all, specific elements had to be in the right place at the right time, including at least partially sedentary societies [that possessed] the use of storage facilities, population densities of a certain critical mass, high resource diversity, technology for processing and harvesting plants, and, finally, plants and animals with the potential for domestication.”

In order to build Göbekli Tepe the archaeologists have created one specific element on top of another, on top of another, that all came together at the right time and place by coincidence. But William E. Stiebing, Jr., an Egyptologist disagreeing with Velikovsky’s theory, has argued: “Scientists [and, we add, historians and archaeologists] are used to evaluating explanations of data in terms of probability rather than possibility, so the evidence would have to be clear and unambiguous to convince them that a theory…is valid.”  

Not only are we being presented with a highly improbable set of conditions falling into place at just the right time; we are given absolutely no evidence or proof to support that improbable set of conditions; yet

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272 William E. Stiebing Jr., “Cosmic Catastrophism” *AEON*, vol. II, no. 6 (May, 1992), p. 60
that is the consensus. It is based on high improbability and no proofs, which, according to Stiebing, strongly indicates, it is not ‘valid.’ Nigel Spivey put the problem thus:

“Given that the mound itself is manmade, built of thousands of tones of earth and rock brought up from the plain below, we are bound to speculate [which is all the archaeologists have done] that the measure of human organization required to build Göbekli Tepe was in some way towards that required to build the Egyptian pyramids. Göbekli Tepe, in the words of its excavators, therefore stands at ‘the dawn of a new world with powerful rulers and a complex, stratified hierarchical society.’

“So this is a major revelation from current archaeology…It causes wonder, not disbelief. And for those who like to regard art as an optional luxury in life, a pastime to be indulged in only when the necessary business of survival and subsistence has been completed, Göbekli Tepe offers a particular challenge…”273

These circles of T-shaped stones seven feet (over two meters) high and 33 feet (10 meters across) were built, according to Spivey, “much earlier than Stonehenge—by 7000 years. It is also much different in one key respect. The well trimmed pillars of Göbekli Tepe are not just megaliths (big stones). They are decorated with images either engraved on the surface or else picked out in shallow relief…”274

Repeatedly we were told these sites in Anatolia contradict the concept of how sedentary life originated. People there were building sophisticated towns, employing advanced agriculture, erecting huge megaliths etc., just after mankind had emerged from the Ice Age. Wherever archaeologists uncovered these Anatolian sites with a combination of sophisticated technologies and primitive life styles, they have opted to accept that primitive life styles determined the chronological placement of these people in the Neolithic rather than accept that the sophisticated technologies and often life styles place these people in much later times. The finding of “a few shards of medieval pottery” at Göbekli Tepe never alerted them as a clear indication that this site was built in post-Roman times. This concept disagreed with their hypothesis and thus they were left to create an entire societal construct without evidence or proof to accord with that chronological hypothesis. The question raised by Braidwood and Howe in 1960 as cited by Matthews: “How are we to understand the great changes in mankind’s way of life which attended the first appearance of settled village farming communities?” still stands.

274 Ibid., p. 47
O’Neill has told us, above, that “in the Byzantine lands archaeologists discovered an unbroken line of development from the foundation of Constantinople through the fifth and sixth centuries. But then… from the first quarter of the seventh century, there is a total and absolute break. Hardly a house, a church or artifact of any kind has been recovered from the next three centuries. Cities were abandoned and urban life came to an end. There is no sign of revival until the middle of the tenth century [etc.].” While there is practically nothing that survived during this period of the Middle Ages, the historians cannot understand that all the sites we discussed and others surrounding them throughout Anatolia display both the sophistication and primitive features that existed for a people, bereft of most of their knowledge following the great plagues, who settled in these regions. As the megalithic and Beaker peoples throughout Europe left clear evidence that we place in post-Roman times, so also the supposed Neolithic peoples of Anatolia left clear evidence that places them chronologically into the Dark Age of Byzantium.

These sites and their advanced state of development arise out of nowhere, flourish for a time and then “disappear,” as Michael Rice told us above, and “a long night descended until a second dawn heralded the first appearance of those civilizations which were to set the pattern for all the ages which followed them.” These settlements have been pressed into various chronological schemes from the tenth millennium B.C. in Anatolia to the third and second millennium B.C. elsewhere. They all enter the stage with no past and exit with no future.

Speaking of the regions adjacent to the Byzantine Empire, Peter M.M.G. Akkermans and Glenn M. Schwartz describe similar architectural developments as those at Skara Brae:

“The[se] freestanding rectangular buildings had been repeatedly renewed, suggestive of some permanence of occupation, although there seem to have been less durable shelters as well, attested by numerous postholes [as also seen in Britain]. Small storage structures stood in the courtyards. They had a grill-plan foundation, built of low parallel stone walls at close intervals (15-20 cm) [6 to 9 inches], probably supporting wooden beams on which the plaster floors were laid. In this way, the floors were elevated and air circulated below, which helped to keep the buildings dry during the rainy winter months. This kind of architecture is unusual in Syria and the Levant but common at sites in the Taurus piedmont in south eastern Anatolia such as Çayönü and Nevalı Çori…

“The larger eighth-millennium settlements on the Euphrates are characterized by regularity [as seen at Skara Brae] and order in the pattern of house construction,
indicative of careful planning and organization of occupation. Compartmentalization of the houses is another main characteristic..., and may have involved a desire for privacy or the need for space allotted for storage of harvested crops. These were recurrent, long-lasting features. In the lowest levels at Bouqras [a site] in eastern Syria were segments of rectangular mudbrick compounds, with a large courtyard in one of the corners, surrounded by the L-shaped quarters for living. The rooms often had small ovens or shallow bins sunk in the hard, white-plastered floors, or niches in the walls...At Abu Hureyra and Halula, the houses between about 46 and 82 sq. m [55 and 98 sq. yards] in area stood tightly together, with small courts and narrow alleys 0.6–1.8 m [20 inches to 5 feet] wide in between [them]. They were built with a single story and were rectangular in plan, although some had additional rooms...Some buildings were short-lived and were used as quarries for mudbrick after abandonment, but others had been frequently rebuilt in the same place and on the same alignment... suggestive of a long, continuous use of space over many generations by, perhaps, one family. Comprising the buildings were three to five rooms accessible through doorways and narrow portholes with high sills. The central main room measured between 20 and 25 sq. m [24 and 30 sq. yards] (side rooms measured 4 to 8 sq. m [4.8 to 9.6 sq. yards]), often including a small rectangular hearth for heating or cooking in the middle. Another oven built of mudbricks stood upon a low rectangular platform against one of the walls. Other common features in the houses were plaster-lined storage bins set against the walls, niches or recesses in the walls themselves, and low platforms [probably beds] at one end of the rooms. 275

The authors go on to describe one site after another across Syria for this early period (ca. 7000–6000 B.C.), but they cannot tie this development to the actual development of cities and towns that supposedly occur in the “mid-third millennium BC, [when] Syria experienced one of the most important transformations in its history—the full fledged adoption of urban life and its associated institutions.”276 This urban development of course we date to after 1200 B.C. as described in the earlier volumes of this series. That is, between around 6000 and 2500 B.C., based on the established chronology, there was no evolution from the first efflorescence of urban development to the later age when civilization came into being. We do not place all such settlements in this time frame; those that have clear evidence of later civilizations in the strata above them we place after 1500 B.C. Those that clearly lack the overlying strata of later civilizations we maintain are most probably post-Roman, especially those with advanced/ sophisticated developments.

275 Peter M.M.G. Akkermans and Glenn M. Schwartz, The Archaeology of Syria: From Complex Hunter-Gatherers to Early Urban Societies (ca. 16,000—300 BC) (Cambridge UK 2003), pp. 62-63
276 Ibid., p. 233
OTHER POST-ROMAN NEOLITHIC SITES

Thus far we have covered Britain, the Byzantine Empire into Syria, and the Levant. But the plagues reached Egypt and crossed north Africa, and there, too, civilization vanished. When it comes to the Islamic world, O’Neill shows that it also exhibits the same absence or paucity of archaeological materials:

“It is precisely the same with the Islamic world. Here of course the impact of Islam cannot be blamed for the poverty of remains. The seventh to tenth centuries, after all, were reputedly Islam’s Golden Age, when the Muslim world stood at the head of civilization. Yet…the Islamic regions are as devoid of material remains in this epoch as the Christian. Egypt was the largest and most populous Islamic country during the Middle Ages. The Muslim conquest of the country occurred in 638, and we should expect the invaders to have begun almost immediately, using the wealth of the country to begin building numerous and splendid places of worship—but they didn’t. Only two mosques in the whole of Egypt, both in Cairo, are said to date from before the eleventh century [one dated to] AD 641 and the [other to] AD 878. However, the latter building has many features found only in mosques of the eleventh century, so its date of 878 is controversial. Why should the Muslims wait over 300 years before building themselves places of worship? And it is the same throughout the Islamic world. No matter where we go, from Spain to Iran, there is virtually nothing between circa 650 and 950. Spain, for example, is supposed to have witnessed a flowering of Islamic culture and civilization in the two centuries after the Arab conquest of 711; and the city of Cordoba is said to have grown to a sophisticated metropolis of half-a-million people or more. Yet according to the Oxford Archaeological Guide, the city has revealed, after exhaustive excavations over the past half-century: (a) The south-western portion of the city wall, which was ‘presumably’ of the ninth century; (b) A small bath complex, of the 9th/10th century; and (c) A ‘part’ of the Umayyad (8th/9th century) mosque. The poverty of these remains, from a reputedly half-million strong metropolis, is striking; and we can only conclude that the evidence shows such a metropolis never existed. It would appear that we have a ‘dark age’ in [these Muslim] regions where there was no collapse of civilization—where no dark age should exist.”

As we pointed out earlier, the Beaker culture incorporated not only northern Europe but central Europe as well as Spain and much of northwestern Africa. While we have concentrated on the British Isles, Brittany, Byzantium, and Syria

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which experienced this contraction, let us turn now to the rest of Europe. Grahame Clark speaks of this culture as the “Battle-axe and Beaker peoples.”

“The close of the Stone Age was marked over large parts of temperate Europe by the rapid movement of ethnic groups, which through their impact on the static peasantries and through their blazing of new routes helped to prepare the way for the spread of metallurgy. East of the Rhine [supposed] warrior groups, armed with stone battle-axes, spread widely, burying their dead everywhere in single-graves. They [supposedly] issued from the lands between the Vistula, the Baltic and the Dnieper, from which the Funnel-neck Beaker folk had emerged in a previous period, but the custom of barrow burial, which not all of them adopted, came in all probability from the steppe region further east. There were [supposedly] spreads in several directions and each of the main regional groups that emerged was distinguished by differences of style both in battle-axes and pottery. To the north they [seem to have] spread across the Baltic to Sweden and Finland and through the East Baltic lands to the Gulf of Riga [Estonia]; and in the forests of the Oka-Volga watershed east of Moscow…To the south others moved into central Europe and some few penetrated as far as Greece…Another main drive was to the west: some turned north into Jutland, where they contrasted notably with the megalith-builders, who practiced mixed farming on the richer soils of the drift; and others proliferated in Saxo-Thuringia and spread into the Rhineland on a broad front from the Alps to the Low Countries. In the Rhenish area they came into contact with powerfully built, broad-headed bowmen, having…bell-shaped beaker pottery…These Bell-Beaker folk seem to have spread immediately from Iberia, following the megalithic sea-routes, but also finding their way into central Europe and in small numbers into Denmark, as well as into the Rhine Valley and thence, as well probably as from the south, to the British Isles.”

This Beaker culture, which was already in place, and which we date to post-Roman times, came into being out of nowhere, existed for a period and vanished from history as mysteriously as they entered it. They also are found on the islands of the Mediterranean as Brad Olsen shows: “To add further mystery, the Beaker People also constructed [megalithic] monuments hundreds of miles away on the Mediterranean islands of Malta and Gozo. No traces of these people have been found anywhere else in the world.” Barbara Ann Kipfer tells us: “Their origins are

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278 Grahame Clark, *World Prehistory: A New Outline* (Cambridge UK 1979), pp. 143-144
279 Olsen, *Sacred Places Europe, op.cit.*, p. 205
uncertain, with theories of their being the Battle Ax people from south Russia, the Spanish Megalithic people from Almeria, or people from Portugal and Hungary.”

All the people and all their materials that John J. O’Neill has told us do not exist or barely exist in the stratigraphic record, we maintain have been found but misappropriated in chronology to prehistory. However, the plague was carried via the great trade routes to the East. The great ancient Industrial Revolution had spread across the Eurasian-African landmass encompassing the civilized world. This brought foreign peoples into close contact with one another through trade and allowed the plagues to travel to the ends of the civilized world. Foster Stockwell describes this trading connection:

“The trade between Rome and China during the Augustan Age (27 B.C.–A.D. 14) was such that silk from China had become a quite familiar, though costly, commodity in Italy. The Roman writer Pliny complained that China, India and Arabia drained 100 million sesterces [Roman dollars] from the Roman Empire each year…

“During the Tang dynasty (A.D. 618–907) commerce over the northern Silk Road reached perhaps its greatest height. The imperial capital at Xi’an experienced an almost constant flow of foreign merchants coming and going from the city…By 742 more than 5,000 persons from countries as varied as Turkey and Japan were living in that city, and according to statistics from the early Tang dynasty, more than 10,000 households in the city of Luoyang contained envoys, students, and merchants from Southeast Asia, the…states [of Korea and Japan], Central Asia, and even Persia and the Roman empire.”

These cosmopolitan connections were clearly the outgrowth of intercontinental trade. Stockwell adds:

“By the seventh century A.D. some 200,000 Persians, Arabs, Indians, Malays, and others were living in the [Chinese] port city of Guangzhou alone, working as traders, artisans, and metal workers. Further north along the coast, in the city of Quanzhou, foreign merchants from Arabia, Persia, Syria, India, Italy, and Morocco engaged in extensive business relations with the Chinese. Here tens of thousands of foreign nationals married local residents. When such foreigners died,

they were buried south of the city as shown by the tombstones and other carved stones with their names on them that have been uncovered there.”282

Lesley Adkins and Roy A. Adkins show evidence of the money exchanged between the Roman Empire at its acme and these other parts of the world:

“The [Roman] trade routes stretched from China and Scandinavia to the Atlantic, and included the various routes of the Silk Road terminating at Loyang.

“The two most conspicuous routes were those used for Rome’s trade in the Indian Ocean…The *Periplus Maris Erythraei* gave much information about trade to and from east Africa and India (as far as the Ganges), but the [Roman] author was aware of lands farther east. Included is information about what could be bought and sold en route. Chinese records indicate that some Roman merchants reached Malaya, Java, Vietnam and the border of China.

“The Romans paid for the imported goods largely in gold and silver, and many Roman coins of gold and silver (not bronze) have been found in places such as east Africa, Afghanistan, India and Indochina. In the Indian area about 6,000 gold and silver coins have been found, mainly dating to the 1st and 2nd centuries, but continuing to the 5th century and into the 6th from the eastern empire [at Byzantium].”283

Interestingly, we can see that trade continued to flourish up and into the 6th century A.D. and apparently came to a halt just after the outbreak of the Justinian plague. Importantly in many of these regions dolmens and menhirs were also built in post-Roman times, but significantly none of these exhibit (except perhaps one we know of) graffiti written in Latin, Greek nor any of the languages of the pre-Roman age. According to Claude Reignier Conder:

“The discovery of so many dolmens and menhirs within the limits of the survey is the more remarkable because they are not found in Western Palestine, except in a few cases in Galilee. They occur in Gilead, north of the survey, and Bushan. In 1882 I found a group near the source of the Jordan. They are known in Asia Minor, in Cyprus, in Phoenicia, in Arabia, in the Sinaitic desert and on the south shore of the Mediterranean, as well as in Europe, Persia and India…

“The monuments found include cairns, stone circles, menhirs (or single standing stones), dolmens (or stone structures with a capstone supported on upright stones)…”

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“Another peculiarity of these dolmen-fields in Moab consists in the great number of the monuments discovered at each of the centers. In some cases the dolmens nearly touched each other. In England such monuments are found singly or scattered. In Algiers the dolmens are, perhaps, as numerous as in the country beyond the Jordan…

“The question of the origin of these constructions is treated at some length…Some antiquaries regard dolmens as tombs, though the use of dolmens not covered by a mound for such purposes is unproven, while the mounds of prehistoric tumuli existing in so many countries show how improbable is the theory that such mounds may be washed away. The position of many dolmens…is inconsistent with the idea that they were once covered with mounds…”

Therefore the plague which depopulated Europe had spread eastward. On this William J. Bernstein reports:

“Within a few generations…the plague of Justinian…had also spread eastward from India to China’s seaports. Convincing Chinese descriptions of the disease appear by the early seventh century, and although confirmatory demographic data are few and far between, it seems likely that the plague devastated the Tang at least as much as it did Byzantium. One observer reported that in AD 762 half of the province of Shantung succumbed; between AD 2 and 742, the Chinese population appears to have decreased by about one-fourth.”

Of the Arabian lands Bernstein suggests: “The hot, dry, and largely uninhabited Arabian Peninsula offered some protection against the disease,” and explains: “first and foremost, plague is a disease of trade.” Thus, when trade ceased, the plague would burn itself out over centuries in the various populations it infected. This trade, especially at cities along these routes, would have carried the plague as it did elsewhere. Regarding Arabia, Keys tells us:

“Evidence from Ibn Ishaq, the Koran and an inscription suggests that in the 540s, Yemen and possibly other areas of the Arabian Peninsula were racked by depredations of bubonic plague. The identification of the disease is made more secure by the date of the 540s…the exact period [when the Justinian plague started

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and] when one would have expected the plague to affect Yemen having arrived there en route from East Africa and the Mediterranean.

“An early inference of what appears to have been plague in Yemen is from one of the earliest parts of the Koran itself. Viewing the plague as an affliction from on high and the dark skin boils as ‘baked clay’ dropped by God-sent ‘flying creatures,’ the Koran describes what befell an army from Yemen that was threatening Mecca around 550.

“‘Hath thou not seen how thy Lord dealt with the [enemy]? Did He not bring their stratagem to naught…and made them like green crops devoured?’

“Ibn Ishaq expanded on this by recording that as the army withdrew ‘they were continually falling by the wayside, dying miserably by every water hole.’

“‘[The enemy leader] was smitten in his body, and as they took him away his fingers fell off one by one. Where the fingers had been, there arose an evil sore exuding pus and blood…’

“And lastly, an inscription discovered by archaeologists at Marib records that work on the dam there had to be delayed in the 540s because the work force had been decimated by plague…

“Yemen had been the principal indigenous power within Arabia for at least a thousand years. Indeed, its people had comprised half the population of the entire Arabian Peninsula, and its fall must have created a huge power vacuum there.”

William McNeill in his historic Plagues and Peoples outlines these events as they relate to China:

“The earliest description of such an illness traceable in Chinese medical writings…A.D. 281-361…has been translated as follows:”

“‘Recently there have been persons suffering from epidemic sores which attack the head, face and trunk. In a short time these sores spread all over the body. They have the appearance of hot boils containing some white matter. While some of these pustules are drying up a fresh crop appears. If not treated early the patients usually die. Those who recover are disfigured by purplish scars which do not fade until after a year.’

288 Keys, Catastrophe…, op.cit., pp. 63-64
“This seems like a clear description of smallpox (or measles), but there are difficulties, since the passage continues:

"...The people say that in the fourth year of Yung-hui (A.D. 653) [about one century after the onset of the Justinian plague], this pox spread from west to east and spread far into the seas. If the people boiled edible mallows, mixed with garlic and ate the concoction the epidemic would stop. If when first contracting the disease, one ate the concoction with a small amount of rice to help it down, this too would effect a cure....When Chinese armies attacked the barbarians at Nan-Yang, it was given the name of ‘Barbarian pox’…”

"What one can conclude even from this fragmentary and imperfect data is that some time between A.D. 37 and A.D. 653, diseases like smallpox and measles arrived in China. Coming overland [along the Silk Road] from the northwest, they acted like new infections, breaking in upon a virgin population. Demographic consequences must have been similar to those the Roman world was experiencing at the same time.

"As for bubonic plague, the earliest Chinese description of this disease dates from A.D. 610. In 642 another writer again mentions it and observed, significantly, that plague was common in Kwangtung (i.e., the [southern] province in which Canton is located) but rare in the interior provinces. On the strength of these references it seems reasonable to believe that bubonic plague came to China via the seaways, arriving early in the seventh century, i.e., about two generations after the disease had penetrated the Mediterranean in 542.”

That is, the Tang Dynasty, with its great population, suffered a plague during the height of its development which afflicted its southern provinces much more than those to the north. This is exhibited by a sudden break between the first part of the Tang and the second. Apparently this cut off trade, as Gary G. Hamilton shows, considering “this striking contrast in the use of imports between the first half of the imperial period and the last half the analyst faces a dilemma.” Why did imports practically cease unless a great event/plague stopped trade?

This is also reflected in the downturn in art between the earlier and later Tang as admitted by Wenjie Duan and Chung Tan: “On the whole, the art of the second half of the Tang Dynasty has shown characteristics which are different from the earlier period. The works of the first half of the Tang Dynasty are clearly superior to those of the second half in their depiction of power and health, vividness and

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289 William McNeill, Plagues and People (NY 1977), pp. 118-119
vitality.” History of the second half of the Tang is no longer found in written form according to Patricia Eichenbaum Karetzky who reports “during the late Tang...histories were no longer written by contemporary annalists.”

Deurbanization followed in the wake of the recurrent plagues, much as it did in Europe, Byzantium, Syria, the Levant, and north Africa. Chye Kiang Heng describes this collapse thus: “The city structure, already loosened during the second half of the Tang period, saw an even greater erosion during [that second half] of political instability, incessant rebellions, and warfares. The great cities destroyed at the end of the Tang period were rebuilt in haste and were only shadows of their former selves. The careful planning and forethought that preceded the building of [the] Tang [cities of] Chang’an and Luoyang were a distant memory.” Charles D. Benn concludes: “The [Tang] era ended on a sad note. Due to floods, droughts, locust plagues and epidemics, famine broke out in the two capitals in 682. The corpses of the dead lined the streets, and citizens resorted to cannibalism in order to survive.”

The final point regarding this downturn of the Tang Dynasty is that the number of eclipses toward the end of that period appears to be only two, and many of the others may have been retrocalculated as shown by Alvin P. Cohen and Robert R. Newton:

“[Of] the early data (there are only two eclipses) recorded after 848, until the end of the dynasty...since the Tang dynasty suffered severe disastrous large-scale insurrections that included the sacking of the capital in 755 and 880, it is quite possible that the astronomical bureau suffered damage involving the loss of some of its records and eventually the curtailment of its observational activities. This is suggested by the unevenness of the eclipse data in THY and in later sources. In particular, the material for the first two centuries of the dynasty is more extensive (that is the ratio of recorded to visible eclipses is greater than the material for the final century.”

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291 Wenjie Duan and Chung Tan, Dunhuang Art: Through the Eyes of Duan Wenjie (New Delhi 1999), p. 183
292 Patricia Eichenbaum Karetzky, Court Art of the Tang (London 1996), p. XII
293 Chye Kiang Heng, Cities of Aristocrats and Bureaucrats: The Development of Medieval China (Singapore 1999), p. 90
The authors further tell us “Before we turn to the ‘Canon’ of Tang eclipses… that forms the database of this study, it seems imperative to elaborate on the problem of the filiation of our sources, lest there be confusion about ‘original’ as opposed to ‘dependent’ source materials. Let us reiterate that there are no ‘original’ sources extant; all available documents are based on, or copies from, earlier ones that are no longer known to exist.” In this case we cannot employ the Tang eclipse data as it relates to this period since much of it may actually be retrocalculated rather than observed. The implication is that at some time the Tang Dynasty stopped observing eclipses toward the second half of its existence because of warfare and plague. However, it does seem that as a very weakened kingdom, the Tang in some measure survived these devastations.

The entire society did not completely disappear but was left at a low state of development, and clearly its people did not revert to the Stone Age, but China did enter its own medieval epoch. Since the plague was far worse in southern China during the second half of the Tang dynasty, this region with its rich rice paddies would have been desirable for development in the period of the five dynasties that followed, and especially that of the Song with an enormous rebound in the population. According to Asaf Moshe Goldschmidt:

“One of the most striking features of Song China was the sharp increase in population. Between the eighth and eleventh centuries [A.D.], China’s population nearly doubled…This demographic explosion was not only a phenomenon of magnitude but also of location. During the Northern Song, the main concentration of China’s population shifted from the North—the traditional heartland of Chinese civilization—to the South, where, by the end of the Northern Song period, 1102 CE to be specific, approximately 67 per cent of the population lived. In other words, the population of South China [devastated by the plague] at the end of the Northern Song dynasty eclipsed in number that of the Tang dynasty [at its most populous]….

“The changes in population began during the second half of the Tang and the Five Dynasties era in a process of migration from the north, torn by war and disorders, to the south. Rice cultivation, which provided abundance of food in the south, fueled the migration process further….The most important change probably was the shift of the economic center of vitality to the South. It should be noted that the term ‘South China’ itself was changed. Prior to the Song, the population in the

\footnote{Ibid., p. 351}
South concentrated predominantly in the lower Yangzi valley. During the eleventh century, the ‘South’ extended to much further regions…”

However, the plagues did not stop at China but were carried by armies and traders to Korea and Japan. This is outlined by William Wayne Farris:

“When Tang armies invaded Korea in the mid-seventh century, however, they carried smallpox and other infections into the peninsula directly, setting off plagues among the numerous susceptible hosts there. By 698, the Japanese government had begun to complain frequently about pestilence. The reason for these repeated epidemics was that both Japan and Korea suffered from a similar dilemma: their populations were dense enough to sustain major die-offs concentrated in a few years, but not large and compact enough to allow the microbes to survive indefinitely…The result was that in both areas an epidemic would rage for a year or two, killing off large numbers of persons of every age, and then run out of the most susceptible members of the population, only to reappear from abroad a decade or two later, claiming another round of victims. This demographic cycle—a lethal pestilential visitation, followed by gradual recovery, only to lead to another bout with the same infection among a whole new generation with no immunities—obtained in Korea until at least 940, and in Japan until about 1150.

“Between 698 and 800, there were at least thirty-six years of plagues in Japan, or about one every three years. The most well-documented epidemic—and to judge by the mortality and its social, economic, and political effects, the most significant—was a smallpox outbreak during 735–737. It started in northern Kyushu, a certain sign of its foreign origin, but by 737 the virus had spread up the Inland Sea and on to eastern Honshu, aided, ironically enough, by the improved network of roads linking the capital and provinces. To its credit, the court tried to apply pragmatic principles to treat the symptoms of the disease, but to little effect. Statistics from various provinces scattered from northern Kyushu to eastern Honshu suggest that mortality was about twenty-five percent, meaning that a million or more persons may have succumbed. As a result of the depopulation, an entire layer of village administration was abolished. Another irony was that the death rate among the…aristocracy—living crowded together in the capital at Nara—was even higher, a full thirty-nine percent. At the end of 737, chroniclers wrote, ‘Through the summer and fall, people…from aristocrats on down have died one after another in countless numbers. In recent times, there has been nothing like this.”

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The human race, it appears, at its apex of ancient development was in places driven to the edge of extinction, and in other regions reduced to numbers that made advances in the civilized arts next to impossible. Jack London, the American writer who exalted the strength of people in adversity, in his description of the plague in China in modern times captures the essence of what had befallen the civilized human race in the wake of the Justinian plague.

“Had the reader...been in Peking six weeks later, he would have looked in vain for the eleven million inhabitants. Some few of them he would have found, a few hundred thousand, perhaps, their carcasses festering in the houses and in the deserted streets, and piled high on the abandoned death-waggons. But for the rest he would have had to seek along the highways and byways of the Empire. And not all would have been found fleeing from plague-stricken Peking, for behind them, by hundreds of thousands of unburied corpses by the wayside, he could have marked their flight. And as it was with Peking, so was it with all the cities, towns, and villages of the Empire. The plague smote them all. Nor was it one plague, nor two plagues; it was a score of plagues. Every virulent form of infectious death stalked through the land. Too late the Chinese government apprehended the meaning... They could not stop the eleven million plague-stricken wretches, fleeing from the one city of Peking to spread disease through all the land. The physicians and health officers died at their posts...and the Emperor, hidden away in the Summer Palace, died in the fourth week.

“Had there been one plague, China [the Roman empire in Europe, North Africa, and the Byzantine eastern empire, the Near East, Persia, Arabia, India, Korea and Japan] might have coped with it. But from a score of plagues no creature was immune. The man who escaped small pox went down before scarlet fever. The man who was immune to yellow fever was carried away by cholera; and if he were immune to that too, the Black Death...swept him away....

“All organization vanished. The government crumbled away. Decrees and proclamations were useless when the men who made them and signed them one moment were dead the next...The farms were ravaged for food, and no more crops were planted, while the crops already in were left unattended and never came to harvest....

“Such was the unparalleled invasion of [the great Afro-Asian-European landmass]. For that billion of people there was no hope. Pent in their vast and festering charnel house, all organization and cohesion lost, they could do naught but die.”

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One of the major questions that these horrific events explain is why the human race, poised on the verge of entering the modern Industrial Revolution, failed to do just that. The ravages of the plagues apparently stopped this leap into modernity and delayed it for well over a millennium.

When it comes to India, although there are no reports of the plague there until later times, we nevertheless find another site that reflects what was happening throughout the post-Roman epoch. As we were told above by Burra Gautam Sidharth that at Nevalı Çori, much like Çatal Hüyük, “echoes can be found…at Mehrgarh…[which] parallels Nevalı Çori in terms of economy, domestication of animals and the planning and layout of large settlements.”

Georg Feuerstein, Subhash Kak and David Frawley report:

“At the Mehrgarh site, the earliest level of inhabitation is composed of rectangular buildings fashioned from mud bricks…and contains between four and six symmetrical rooms each….

“No pottery has been found at the oldest level of occupation…When pottery first made its appearance…, it was rather rough. However, before long it made way for glossy red wares of a finer quality [followed by] simple geometric patterns [and later] pottery showed complex animal decorations and was expertly thrown on the wheel.”

According to Gregory L. Possehl, “ovens and hearths were usually found in the corners of rooms, and signs of their use can be seen…” According to the Pakistan Year Book (Karachi 1981), page 69, these “circular oven[s] look like a tandoor” which is an oven existing today. The question of course arises, why did people living in ca. 7000 B.C., build ovens that just happen to resemble modern-day ones. Like so much else, we suggest, that because this city was built in the post-Roman epoch this type of oven naturally evolved into the present-day tandoor one. Furthermore, as we pointed out above for Çatal Hüyük, its population was multiracial like that which was found in Byzantium because of the great trade connections with the rest of the world. So the same multiracial mixture at Mehrgarh is also found, as Feuerstein, Kak and Frawley tell us: “The populations

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300 Georg Feuerstein, Subhash Kak, David Frawley, In Search of the Cradle of Civilization: New Light on Ancient India (Delhi 2005), p. 148
301 Gregory L. Possehl, The Indus Civilization: A Contemporary Perspective (Walnut Creek CA 2002), p. 25
of many of the early Neolithic sites, such as Mehrgarh or Çatal Hüyük in Anatolia, were surprisingly multiracial."\footnote{302}

One characteristic of the multiracial population was discovered by examining the teeth in the burial sites. At Mehrgarh, as pointed out by Peter Neal Peregrine and Melvin Ember: “Burials provide evidence of all age groups...Dental analysis has shown the Neolithic population [at Mehrgarh] to have distinct shovel-shaped incisor and canine teeth and other characteristics of...South and South East Asia.”\footnote{303} Not only were Indian-type racial characteristics found in the population of Mehrgarh, but distinctive racial characteristics from as far east as China and Indo-China. This, of course, is clearly to be expected in a center that was one of the hubs of international trade with China and Indo-China. The reason this population is called “surprisingly multiracial” is that it is hardly to be expected that Neolithic peoples thousands of miles distant from this site should be found in large numbers. Why would Neolithic populations in India exhibit strong dental characteristics that belong only to East and Southeast Asia and not at all to India?\footnote{304} According to Jonathan Mark Kenoyer, the shape of these teeth, “do[es] not have strong morphological relationships to known Neolithic populations. On the contrary, their dental morphology associates them with a distinctive Asian gene pool.\footnote{305} The answer, we submit, is that these were traders from China and Indo-China living in India during the great trading period of the Roman Empire. We are further told that “The people of Mehrgarh had domesticated cattle in addition to sheep, goats, and possibly water buffalo.”\footnote{306} They carried on trade.\footnote{307} The population of this town was quite large:

“Mehrgarh: The Largest Town of Early Antiquity?...Estimated to cover an area of over 500 acres, Mehrgarh appears to be an assemblage of villages from different periods. The Mehrgarh of the fifth millennium B.C. covered roughly 168 acres. This is still five times larger than the contemporaneous site of Çatal Hüyük...which has been called ‘the largest Neolithic site hitherto known in the Near East.’ The population of Çatal Hüyük has been estimated at around four thousand individuals. Employing similar standards, early Mehrgarh’s population

\footnote{302}{Feuerstein, Kak, Frawley, \textit{op. cit.}, p.150}
\footnote{304}{John R. Lukacs, \textit{Current Anthropology, loc.cit.}}
\footnote{305}{Jonathan Mark Kenoyer, \textit{Ancient Cities of the Indus Valley Civilization} (Oxford UK 1998), p. 38}
\footnote{306}{Feuerstein, Kak, Frawley, \textit{op. cit.}, p. 148}
\footnote{307}{\textit{Ibid.}, p. 150}
was at least twenty thousand individuals—the size of Tyre, capital of the Phoenician empire, and of the modern university town of Stanford, California.”  

Possehl makes this provocative comment about Mehrgarh:

“While Mehrgarh['s first period] is undoubtedly an early village farming community, there is also the sense that the excavations there have documented the beginnings of food production and domestication in the region. It is certainly nothing like a terminal hunting-gathering site with the intensive collection of cereals and pulses of sophisticated hunting. These people were already farmers.”

Possehl goes further to show:

“What we see at Mehrgarh is a sequence of events that seems to document the domestication of animals. The sheep, goats, and cattle start out looking wild and were manipulated…Over time the potential domesticates came to look like domesticated animals (smaller with the osteological hallmarks of domesticated beasts)... The contribution of domestic or ‘pre-domestic’ stock to the faunal assemblages came [over time] to surpass that of other animals in the aceramic [earlier period].”

That is the nub of the problem. Instead of humanity having accomplished feeding relatively large populations living in urban settings 9000–6000 B.C., these capacities do not lead to full urban development for millennia! What caused Çatal Hüyük, and all the other sites near it, to stop developing into civilizations? This question, we suggest, is never properly addressed, because it contains a basic contradiction. Unless something destroyed these early sites, they were on the verge of creating civilization, but they did not, and there is no evidence that they were destroyed by outside forces or peoples. Because of the entrenched established chronology we have two beginnings of civilization in the Near East. The first around 9000–6000 B.C. developed through agriculture, domesticated animals into towns, then stopped; the second, in southern Mesopotamia, China, India, and Egypt also developed through agriculture, domesticated animals into towns, but the people there went on to build civilization. The reason why the first development did not achieve civilization is never explained with anything resembling testable evidence. The first experiment failed because that first experiment failed. Q.E.D. With our chronology there is only one period of time when mankind moved from

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308 Ibid., pp. 145-146
310 Ibid., pp. 459-460
agriculture, domestication of animals and towns on to full-fledged civilization, not two. Lucy Wyatt puts the case thus:

“Furthermore, one of the peculiarities of prehistoric farming was its two distinct stages of development thousands of years apart, with no evidence of experimentation in between. The first stage, a sort of proto-farming that fits our image of early farming, coincided with the end of the Ice Age…and the second phase with the fifth millennium B.C. [in the established chronology].”

She adds:

“This [second] key change in the domestication of farm animals, transforming agriculture, did not happen until the fifth millennium BC [based on the established chronology], curiously, about the same time that cities appeared in Mesopotamia. This next phase in farming is understandably termed by archaeologists the ‘Secondary Products Revolution.’ Only now, more than eight thousand years later after the end of the Ice Age, all of a sudden people were able to take milk from a cow, wool from a sheep, ride a horse, use a light plough, plant vines and so on—that is, carry out a form of ‘civilized’ farming that has not changed in essence until the present day.”

The further question also unexplained is, why did Çatal Hüyük and Mehrgarh have multiracial populations during the Neolithic? With regard to Mehrgarh, Possehl raises the question of where and when its earlier predecessors existed.

“Almost nothing is known of the time between the late Glacial Age at circa 15,000 BC and the beginning of Mehrgarh at circa 7000 BC…The first period at Mehrgarh has fully developed domestic architecture based on mud brick…So while Mehrgarh…is undoubtedly an early village farming community…[i]t is certainly nothing like a terminal hunting-gathering site…These people are already farmers.”

That is, Mehrgarh supposedly arrives out of nowhere as a fully developed town with agriculture and domesticated animals. How can that be? Of course this development had to have a long tradition leading up to it which is explicable in terms of our chronology. Being built in the aftermath of the great Justinian plagues all these developments makes sense. The problem of how these developments evolved cannot be solved in terms of the established chronology.

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311 Wyatt, *op.cit.*, p. 44
312 *Ibid.*, pp. 51-52
313 Possehl, *Indus Age, op.cit.*, p. 440
One question must be addressed before proceeding: why did these post-Roman peoples employ domesticated plants and animals that appear somewhat primitive in form? After all, they supposedly had all the well-defined domesticated animals that existed in Roman times but in the post-Roman epoch these became more primitive. Juliet Clutton-Brock explains this change from the domesticated form as it reverts to the wild form. “Domesticated animals that return to living in the wild, that is they become feral, will usually revert by natural selection to a physical form that is closer to the wild species…”

We ask the reader to recall that in various citations above we were told that with the fall of the Roman Empire and the Justinian plagues, the cattle and other farm animals were left unattended, and that great areas in which they lived became wilderness. This fact that domesticated forms revert closer to the wild forms was well known. In a review of J.D. Caton’s book, *Reversion of Domesticated Animals to the Wild State*, in the *Popular Science Monthly*, vol. 20 (New York, 1881-1882), page 116, we find:

“The Hon. J.D. Caton has been taking some notes during a sojourn in the Sandwich Islands on the tendency of domesticated animals, when left to go wild, to revert to the habits, forms and colors of their wild ancestors, ‘with the exception of the goose and the duck, nearly all the animals which have been introduced into the islands, as well as those which were held in domestication, have reverted to the wild state. Among them are the ox, the horse, the goat, the sheep, the hog, the dog, the cat, the turkey, the peacock, and the barn-yard fowl. The greatest physical degeneracy was observed in the wild horse and the wild sheep. The latter are small, gaunt, and long-legged, with a scant and coarse pelage [hairy covering] instead of thick wool.”

Therefore, archaeologists, when they dug up these various post-Roman sites discussed above, finding what were clearly wild forms of animals, assumed these only existed in prehistoric times and drew the chronological conclusion, as with stone axes, primitive pottery etc., that this was confirmation that these animals belonged in the Neolithic Age. It fit their preconception and therefore they accepted it without question. They then fit all the other evidence into this framework. Thus they created the two stages of agricultural development separated by millennia.

Moving on to Iraq, we come to the village of Jarmo, excavated by R.J. Braidwood and B. Howe. Charles Keith Maisels summarizes the nature of this supposedly prehistoric village dated to ca 7,000–6000 BC and claims: “The radiocarbon dates are generally what Braidwood…calls ‘whimsical,’ but he is

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convinced that Jarmo flourished for around 300 years...” This 300-year duration for Jarmo appears to fit the missing years discussed above. Maisels continues:

“Jarmo was a permanent village establishment with perhaps twenty or more houses and of rather long duration. Its people possessed at least the domestic goat, two kinds of wheat and barley, and a variety of artifacts adapted to the cultivation, storage, and processing of vegetable foods....’ (Braidwood and Howe, 1960: 183)

“The wheats recognized were both wild and domesticated emmer and einkorn, plus what seems, from the tough rachis [that holds the cereal head], to be two-row barley...in the process of domestication....Sickle are certainly present, but non-cereal gathering was still important, as seen in remains of pistachio nuts and great quantities of snails...Further evidence of the transitional nature of Jarmo is the fact that in the lower levels wild, and thus hunted, animals, including red deer..., cattle..., onager, and gazelle..., are of equal importance (49 percent) to domestic sheep and goats....However, even in the later levels domesticated animals, which ...include pigs (but not cattle) amounted to only 60 percent of identified mammal bones...

“Braidwood and Howe...estimate the human population at not more than 150, in a maximum of 25 houses of rectilinear form, at all levels constructed of tauft (mud moulded, not poured, in courses on the wall under construction)....

“Early levels are aceramic, but employ stone vessels instead, such that:

“although the making of stone vessels was normal concomitant of early village life throughout the Near East, there was at Jarmo a rare cultural emphasis on this industry that found expression not only in volume but also in quality of output. Many of the wide variety of shapes that are present are aesthetically very fine, and the regularity of form, the high polish, and the extreme thinness that were frequently achieved reflect a high degree of craftsmanship. (Adams 1983b:209)”

It seems evident that a stone mason or even a few had survived the plague in Iraq; they had come to Jarmo to practice their craft and may have taught it to...

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316 Ibid., pp. 104-105
others. "The materials used [to make these fine stone pots] include limestone, marble, and, to a lesser degree, sandstone, all locally occurring."\textsuperscript{317}

In terms of pottery, instead of a gradual development from crude forms to more and more refined, it arises as a fully accomplished craft.

"Pottery tended to selectively supplement stoneware at Jarmo, where it seems to be introduced as a technique from without. Adams...makes a most interesting suggestion [to account for this sudden refinement] connecting the obsidian present, the availability of which increased during the life of the village..., with the introduction of pottery; namely that ‘its early practitioners at Jarmo may have been women from some [unknown] distant village, perhaps brought back as wives by men trading for obsidian.’ Thus when pottery appears it does so [out of nowhere], like the handling of tauf, as an accomplished practice, not as ‘the fumbling beginnings of a new craft’..."\textsuperscript{318}

What we encounter at these varied and distant sites are a variety of crafts at different levels of craftsmanship which we suggest reflect the survivors of the plagues that migrated to these sites at different times. The same applies to the plants and animals they utilized. They were able to capture certain former domesticated animals that had reverted to their wild forms and used whatever grains or other plants were available, while depending in large measure on hunted forms of wildlife.

There are indeed other supposed Neolithic sites in Iraq which are briefly outlined by Graham Hancock:

"Hassuna [is found] in northern Iraq (35 kilometers south of Mosul). The first settlement here has the appearance of a more primitive Neolithic farming community living in huts or tents. Overlying this layer, archaeologists found six layers of houses progressively larger and better built.

"[At] Umm Dubaghiya—about 8,000 years old: more sophisticated features including beautiful murals and floors made out of large clay slabs carefully plastered with gypsum and frequently painted red...

"The Samara period—named after a widespread pottery style created by what Roux describes as ‘a hitherto unsuspected culture which flourished in the Middle

\textsuperscript{317} Ibid., p. 105
\textsuperscript{318} Ibid., pp. 105
Tigris valley during the second half of the sixth millennium BC, i.e., approximately 7,500 years ago. The geneticist Luca Cavalli Sforza suggests that his date should be pushed back to ‘about 8,000 years ago.’ There is evidence that this culture used irrigation techniques, grew large surpluses of wheat, barley and linseed, and built spacious houses out of mud-brick.”

Again, why all this disappeared at the end of the Neolithic Age we are never told; for the British Isles, Aubrey Burl tries to explain this collapse. We ask the reader to compare and contrast his explanations with what we have presented about the collapse of civilization in the post-Roman era, to see how archaeologists are groping toward a thesis similar to our own. We suggest this thesis be carried to all the lands and sites we have examined:

“The years between about 3250 and 3000 BC constitute a ‘Dark Age’ in… prehistory…During these empty centuries the slow creeping back of the forests, the grass encroaching upon cultivated land turning it into prairie, the absence of new ceremonial centres, the silence of the landscape show how critically Neolithic society had been affected…

“Evidence obtained from articles that can [supposedly] be securely dated by the C-14 process graphically illustrates this decline. The previous steadily growing activity…from the earliest Neolithic onwards, around 4,450 BC, suddenly and dramatically declines before recovering its steady rise as population and output began once again to increase.

“Towards the end of the fourth millennium BC there were decades, perhaps centuries, when a deterioration in established ways of life resulted in a loss of faith. Traditional customs were set aside. Pollen from previously cultivated areas shows scrub and weeds spreading across deserted fields. Forests regenerated. Chambered tombs were blocked up [probably by the Christian church] and abandoned [by the converted pagan peoples]…

“Former causewayed enclosures were converted into defended [Medieval] villages with heavy gateways and walls. They were attacked [in feudal wars]. At Carn Brea in Cornwall, Hambledon Hill in Dorset, and Crickley Hill in Gloucestershire burnt-down entrances and savage masses of flint arrowheads are proof of bitter conflict. Explanations for the troubles have been many. Overuse of

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land; failure of crops; famine, plague, an expanding [or plague destruction of the] population…Or a more insidious agent, a worsening of the weather.”

The evidence clearly shows that all across Europe, North Africa, Byzantium, Syria, Iraq, and the central Babylonian plain to India, etc., humanity suddenly returned to the Stone Age and remained in a fairly primitive state for centuries.

The very same development of advanced culture is found in Neolithic Japan, which also suffered from plagues. In this respect Graham Hancock reports on “the mystery of the pots:"

“Nor were these the only surprises that the Jōmon [period of prehistoric Japan] had in store…As we’ve already noted, what is truly outstanding and unexplained about these ‘primitive hunter-gatherers’ is that they were the first people in the world to invent pottery—one of the great leaps forward in human culture which, in their case, took place not just hundreds but thousands of years before anybody else. As recently as 1998 most scholars believed that the oldest Jōmon pottery was made about 12,500 years ago [10,500 B.C.]—itself a staggeringly early date—but so rapid is the pace of new discovery in this field that the origins of Jōmon civilization have had to be continuously revised backwards.

“In May 2000…I held in the palm of my hand four fragments of a broken Jōmon pot 16,000 years old [14,000 B.C.]. Excavated at a site known as Odaya Mamaoto No. 1 Iseki, the potsherds had been dated using state-of-the-art AMS [accelerator mass spectrometry] technology.

“It is still a little known fact that the Jōmon of Japan is the world’s oldest pottery-making culture. But even less well known is the extent to which this prehistoric people maintained a distinct identity as a single, homogeneous group. According to Dr. Yasuhiro Okada…Chief Archaeologist at Sannai-Muryama, ‘they were one culture, from beginning to end.’

“Imagine that—one culture, probably one language, probably one religion, staying intact for more than 14,000 years.”

Unlike Hancock, who is a proponent of past civilizations, not Atlantis, that existed many thousands of years ago, we cannot “imagine” that a culture existing 14,000 years ago, during the Ice Age, produced the world’s first pottery. But when

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320 Burl, Prehistoric Avebury, op.cit., pp. 121-122
321 Hancock, op.cit., p. 556
one gives credence to radiocarbon dating, then one must accept that before the Ice Age ended, the human race that was supposedly living in caves, was able to make clay into pottery forms and put these into some kind of kiln for several hours to bake all the water from them. The sheer incomprehensibility of accepting this concept as evidence shows what we hold to be the completely irresponsible nature of radiocarbon dating at its worst. Yet that is what this process demands from its supporters, namely, that there was some kind of semi-civilized culture at the end of the Ice Age that was making pottery.

This concept that, several thousand years before anyone else, they invented pottery staggers the imagination. Hancock goes on to describe this pottery.

“Whatever the source of the original inspiration, there is no doubt that Jōmon pottery is very distinctive [comparable to the varied Beaker pottery of Neolithic Europe]. Its most characteristic decoration is the cord-mark (indeed, Jōmon means ‘cord-mark’ in Japanese and is another name given by archaeologists;...). This decorative technique requires the potter to press lengths of knotted twine down into the clay before firing and sometimes to roll the cords to produce additional effects...

“Jōmon pottery is scattered geographically throughout Japan from the far south, including the Ryu Kyu archipelago, to the far north including Hokkaido, and on the other hand is spread out in time connecting the world of relatively recent and comprehensible history (2000 years ago) with the world of remote prehistory 16,500 years ago, when the Ice Age went into meltdown.”

Above and beyond this Hancock describes a settlement and a nearby buried mound that has a pyramid made up of seven terraces of stone. He tells us:

“In 1998...I visited the Jōmon site of Sannai-Muryama in Aomori Prefecture and was surprised to discover how large and how well-organized the ancient settlement had been at its peak 4500 years ago—the same epoch as ancient Egypt’s pyramid age.’ Sannai-Muryama with its spacious public buildings, wide streets and planned sanitation, was not at all what I had expected of primitive hunter-gatherers. These were the obvious signs of permanent settlement, stability, ordered organization and economic success. And they were accompanied by equally clear indications of a society with evolved spiritual ideas. In particular, the use of grave goods by the ancient inhabitants...”

322 Ibid., p. 558
323 Ibid., pp. 553-554
Atsuyuki Okabe describes the site thus:

“The Sannai-Maruyama site…was settled from the early to late stages of the Jomon period…Many archaeological artifacts, including a great quantity of Jomon pottery, stone lithic articles and ornaments, clay figurines, pit dwellings, storage chambers, clay-mining pits, and graves, were unearthed…Radiocarbon dating showed that people lived at this site for 1700 years between 5900 to 4200 B.P. [3900–2200 B.C.]. The nature of the settlement that can be discerned following excavation at this site is different in two respects from that which archaeologists imagined before excavation. First, the number of dwellings in a typical prehistoric village is considerably larger than we imagined. We had considered that 5-10 houses, with 20-50 inhabitants would be normal. However, 50 to 100 houses were discovered to represent one archaeological phase, suggesting that 200-400 people lived together. Second, the life span of the villages in the site was much larger than we originally thought…maintaining such a large settlement [for 1700 years] must have been difficult for a…society whose survival basis was subsistence hunting and gathering.

“The following questions arise:

“What kind of subsistence strategies supported the size and duration of the Sannai-Maruyama site?

“What kind of environmental factors allowed people to live with such a lifestyle?”

When primitive people hunt and gather food in an area, in a relatively short period they drive off the animals they hunt, and the smell of their refuse alerts and drives other animals out of the area. Therefore, hunter-gatherer societies have to move from place to place to reestablish themselves in areas with fresh game, like the American Indians. But this population of 200 to 400 people did not do this, supposedly for 1700 years. All sorts of hypotheses were suggested to permit this to occur. But if, as we maintain, this site was used for only 300 or so years, the people could have moved seasonally to other sites and then back to this central site. In fact, according to David Young and Michiko Young: “So far archaeologists have uncovered the remains of 800 pit-dwellings, 120 post-and-beam structures…”

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324 Atsuyuki Okabe, *GIS-Based Studies in the Humanities and Social Sciences* (Boca Raton FL 2006), pp. 176-177
problem if this 800 was the size of the village. They go on to say: “The findings at Sannai Maruyama have forced scholars to change their ideas about Jōmon communities. Contrary to earlier beliefs that Jōmon people had a primitive lifestyle based upon hunting wild animals, the residents of Sannai Maruyama settled in one place for an extended period of time, cultivated some food such as chestnuts, imported goods by boat from different parts of Japan, buried their dead, and lived at peace with their neighbors.”326 But chestnuts will not feed so many people throughout the year. Some other form of nourishment that made up for this had to be utilized. Nevertheless, we have once again found a largish village that does not fit into the scope of a hunter-gatherer society but could have existed for a shorter period in the post-Roman era when craftsmen were employed to produce goods. Richard Rudgley points out: “The diversity of the items produced at Sannai-Maruyama also points to the existence of craft specialization…”327 Like Çatal Hüyük, Sannai-Maruyama actually had craftsmen producing goods, something not often, if ever, found in hunter-gatherer societies where people produced all their own needs by themselves. That craftsmen existed at Sannai-Maruyama clearly implies this culture had an earlier one where craftsmen existed and which was emulated by these later people. Anthony Rausch puts the standard of living for these people thus: “The site has yielded a massive number of [craft made] artifacts, proving that a large community existed in this area during the early to middle stages of the Jomon era….The site provides evidence that the people of the Jomon lived in far more affluent circumstances that [sic] generally believed, a far cry from the traditional image of a barbaric, impoverished people living a hand-to-mouth existence.”328

Jonathan Turk reports on one point:

“The prehistoric Jomon constructed the largest monument built by any hunter-gatherer culture in the world. At San’nai Maruyama, archaeologists have uncovered six foundation posts for a tower estimated to be sixty feet tall [the height of a six-story building]. This structure must have been a shrine, watchtower, sundial, or monument built by people who had the time and resources for community and spiritual pursuits.”329

326 Ibid.
327 Richard Rudgley, The Lost Civilization of the Stone Age (NY 2000), p. 32
328 Anthony S. Rausch, A Year with the Local Newspaper: Understanding the Times in Aomori Japan, 1999 (Lanham MD 2001), p. 62
329 Jonathan Turk, In the Wake of the Jōmon: Stone Age Mariners and a Voyage across the Pacific (Camden ME 2005), pp. 71-72
Hunter-gatherers do not generally undertake large construction projects. Nevertheless, at a site not very distant from Sannai Maruyama called Kuromata Yama there is a large mound that Hancock describes:

“In Akita Prefecture…two hours’ drive from Aomori, I climbed the cedar-covered slopes of an 80 metre [265 feet] high mound which juts emphatically out of the surrounding plains. Its name is Kuromata Yama (Mount Kuromata) and according to local legend it is ‘a pyramid built by an ancient people.’ Geologists remained skeptical until a multi-disciplinary team of scientists from the Japan-Pacific Rim Studies Association led by Professor Takashi Kato of Tohoku Gakuin University produced detailed radar maps of Kuromata Yama in the 1990s. The maps show that the interior of the mound… ‘consists of seven terraces with stones laid out on each terrace. This is a clear indication that it was shaped by man and is certainly very different from a natural mountain formed by volcanic eruptions or natural weathering.’

“The experts concluded that a natural hill had indeed once stood on the site but that it had been deliberately quarried, sculpted and reinforced with stone blocks to create a pyramidal core with seven terraces that was covered with ramped earth and then overgrown by vegetation…Since work of such ambition and scale has never previously been associated with the Jomon, it was at first assumed that the construction work was unlikely to be very old—perhaps no older than the eleventh century AD.”

The question of course is how did a hunting and gathering society have the time and resources (mainly food and tools) to erect such a great monument, the height of a 25 or 26 story building. Ultimately they had to be able to take time away from subsistence living to devote it to this construction which strongly suggests some form of agriculture. In a subchapter titled: “The rice bombshell,” Hancock further reports:

“During a seven-week journey through Japan in April and May 2000…, I heard hints of agriculture. At Ofunato Iseki on Hokkaido, the chief archaeologist, Chihara Abe, told me he was convinced the Jomon had ‘farmed’ chestnut trees: ‘…To all extents and purposes they were doing agriculture.’

“Another intriguing recent discovery is that as far back as 8000 years ago the Jomon were cultivating a non-indigenous plant, the bottle-gourd, which paleobiological studies indicate must have been imported from Africa…

“Since it was for a long while more or less automatically assumed that the Yayoi [who followed the Jomon] brought rice cultivation to Japan, it is also highly

330 Hancock, op.cit., pp. 554-555
significant that archaeologists have now found undisputed evidence of rice cultivation by the Jomon at Itazuke on the island of Kyushu. This evidence has been dated to around 3200 years ago and is thus older than the Yayoi period by several hundreds of years. Mitsuo Tsukada of the Quaternary Ecology Laboratory of the University of Washington summed up the finding this way:

“The oldest evidence of rice pollen [in Japan] comes from the well-known Itazuke site...which dates to about 3200 BP [1200 B.C.]. Since the plant is not a Japanese native, its presence provides definite evidence that rice cultivation began in Late or Latest Jomon in Kyushu. Phylothic [plant] studies also support the fact that rice cultivation began at this time. It has been clear for some time that the notion that its cultivation appeared in Japan at the beginning of the Yayoi is dated. Yet this idea persists in the writings of many specialists in East Asian archaeology!”

Sahara Makoto, the Director-General of the National Museum of Japanese History, according to Hancock said:

“There was other evidence...tiny particles of rice had somehow got into the potters’ clay before firing. Known to Jomon scholars for a decade, this evidence concerned several different pieces of pottery and several different sites, all of them in the range from 5000 to 3000 years old [3000 to 1000 B.C.]. Some archaeologists have gone to great lengths to underplay the significance of these finds, even arguing that the rice fragments had been brought over from China on the wind, or on the feet of grasshoppers—any logical contortion would be worthwhile, it would seem, rather than question the central paradigm of the Jomon as ‘simple hunter-gatherers.’

“Yet the more I looked into these matters the more obvious it became that increasing numbers of Japanese archaeologists are abandoning the ‘hunter-gatherer’ paradigm and are moving toward a new view of the Jomon as a sophisticated and very ancient culture.”

If, as we maintain, the Jōmon existed in post-Roman times then we might expect to find with the Jōmon technologies or other forms of evidence that also date to between A.D. 600–900, and that is just what we find. According to Hancock:

“At Sakuramachi Iseki, near Oyabe City in western Honshu, archaeologists have recently excavated examples of 4000 year-old Jomon carpentry using
complex joints, dovetails and corners of a type not previously thought to have been introduced into Japan before AD 700.

“Another example of historians radically misdating and misattributing inventions, ideas and icons concerns the classical curved jewel of the Japanese nobility—the comma-shaped (or foetus-shaped?) magatama, often carved from jade. References to magatama in Japan’s national epic, the Nihon Shoki, which was compiled at the end of the seventh century AD, and the frequent finds of magatama in archaeological sites of that period, have led most Japanese to an unquestioned assumption that the magatama is an invention of the so-called ‘Yayoi’ and ‘Kofun’ periods from 300 BC to AD 800. Yet on my travels through Japan archaeologists showed me dozens of beautiful magatama from times some of them more than 8000 years old.”

Carmen Blacker in this respect states: “The earliest Magatama of the Jōmon period are simply claws and teeth perforated at one end, but by the end of the Jōmon period they are found carved from stone.” What we have is very similar to the entasis of the sarsens at Stonehenge. The Jōmon people invented complex joints, dovetails, and corners for construction around 2000 B.C. that do not exist in Japan until after A.D. 700. They invent Magatama thousands or hundreds of years before this form is used by the Japanese nobility in late Roman to post-Roman times. But in-between, this technological advance and this symbol of nobility disappear. With our chronology, these developments are not anachronisms but fit into the period when we place the Jōmon after A.D. 700. Another question is how the bottle gourd from Africa came to Japan before 1000 B.C. Junko Habu fully admits: “Bottle gourd seeds have been recovered from many Jomon sites, including the Early Jomon…[Bottle] gourds are not indigenous [to Japan].” How did an African domesticated plant come to Japan before 1000 B.C., when it had absolutely no contact with Africa? But in our chronology this use of an African domesticated plant is clearly explained by the great trade that existed in Roman times across the Afro-Eurasian landmass.

When it comes to Korea we can connect the chronology of the Korean peninsula to that of Jōmon Japan through well-known artifacts of both cultures. Finding either Korean artifacts at Jōmon sites or Jōmon artifacts at Korean sites

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333 Ibid., p. 562
335 Junko Habu, Ancient Jōmon of Japan (Cambridge UK 2004), p. 118
connects them chronologically to the same period. Because the sea between Japan and Korea is narrow, this exchange can occur. Okazaki Takashi explains:

“The narrow stretch of ocean between the southern tip of the Korean peninsula and the northern coast of Kyushu was a natural passageway for traffic between Japan and the continent…Archaeological findings indicate that the peoples of Kyushu and Korea were in contact some five thousand years ago.…

“Findings at Jōmon sites dating around 3000 B.C. suggest that there was some contact between the peoples of southern Korea and northern Kyushu, Japan’s southernmost island and the region closest to the [Asian] continent. Pottery found at the Neolithic Chûlmun (comb-pattern) site at the Tongsam-dong shell mound in Pusan [Korea] has been compared with northern Kyushu’s Sobata-style pottery, an early Jōmon type. Their shapes and markings are similar, and both types were made by mixing asbestos and talc with clay, leading some scholars to argue that Sobata pottery was derived from a Korean prototype. Contact between Korea and Japan continued throughout the Jōmon age, and pottery of middle and late Jōmon types has also been discovered at Tongsam-dong. The absence of Korean pottery at middle and late Jōmon sites, however, indicates that Korean cultures exerted little influence over Japan during these periods…”

Since the Jōmon period is dated to A.D. 700–900, so, too, this epoch in Korea is also dated to that time and indicates there was a great impoverishment from former times because of the relatively low development of the Koreans.

Not every empire fully collapsed and we suggest that in China the Tang Dynasty suffered but did not disintegrate. Nevertheless, we have shown that from Britain on the Atlantic to Japan in the Pacific, most of the great cultures were destroyed by plagues and were forced to revert to Stone Age or near Stone Age conditions. However, with Heinsohn’s method, we have shown that these centuries were not empty but filled with people striving to support themselves with whatever knowledge they still possessed. Therefore, there are anachronistic developments contained in these societies that reflect here and there those earlier advanced technologies that had somehow survived with certain individuals who fled the plague-ridden cities to forge a new life. These we have outlined throughout this book and will not rehash for the reader. Each of these supposed Neolithic Age sites had elements that indicated that they were in one way or another too advanced for

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that primitive time. A typical example of this advanced development can be seen on the island of Malta which contains an immense megalithic temple. This is described by Hancock:

“The site of a huge semi-subterranean stone circle—is…the ‘Giant’s Tower’ of Gigantija, the greatest and the oldest of the megalithic temples of the Maltese archipelago, reckoned to be built around 3600 BC…”

“…—an outer retaining wall of cyclopean blocks, some up to 5 metres [16.5 feet] high and many in the range of 15 tonnes or more, set out in a series of expansive, graceful curves to enclose an irregular space…The inner space (of the temple) contains a series of altars, shrines and large apsidal rooms interconnected by axial passageways, all of which are also lined with huge megaliths…”

“Orthodox scholarly opinion holds that the islands of the Maltese archipelago remained entirely uninhabited until 5200 BC—7200 years ago—when they were settled by Neolithic agriculturalists from nearby Sicily.

“Orthodox scholarly opinion dates Gigantija to 3600 BC—5600 years ago.

“The time lapse between settlement 7200 years ago and the construction of Gigantija 5600 years ago is 1600 years. And while there is evidence of small-scale construction and the hewing out of rock tombs in the Maltese islands during this period there is nothing from the excavation record that archaeologists are able to show us which in any way seriously charts the evolution of the temple-building phase.”

Why would an isolated, island people, out of nothing, build a huge temple of massive stones without having some connection to another culture that built great monuments? Hancock continues:

“On the contrary:

‘‘The temple builders did not begin with small-scale structures. Gigantija…is a tremendous work of architectural design and of engineering built a thousand years before the date usually given for the great pyramids.’

‘To this Colin Renfrew, Professor of Archaeology at the University of Cambridge, adds:

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337 Hancock, op.cit., pp. 322-324
“The façade [of Gigantija], perhaps the earliest architecturally conceived exterior in the world, is memorably imposing. Large slabs of coralline limestone, set alternately end-on and sideways on, rise to a height of eight metres [26 feet]; these slabs are up to four metres [13 feet] high for the first course, and above this six courses of megalithic blocks still survive. A small temple model of the period suggests that originally the façade may have been as high as 16 metres [52 feet]!”

“Cyclopean walls 16 metres high”? At first sight, admits Renfrew,

“it seems inconceivable that such monuments could be built without the organization and the advanced technology of a truly urban civilization…Yet according to the radiocarbon chronology the temples are the earliest free-standing monuments of stone in the world. In the Near East at about this time, 3000 BC and perhaps even earlier, the mud-brick temples of the ‘proto-literate period’ of [Mesopotamian] civilization were evolving impressive monuments in themselves but something very different from the Maltese structures.’

“[Hancock asks] How are we to explain the fact that the oldest free-standing stone monuments in the world, which by virtue of their size and sophistication unambiguously declare themselves to have been built by a people who had already accumulated long experience in the science of megalithic construction, appear on the archaeological scene on a group of small islands—the Maltese archipelago—that had not even been inhabited by human beings until 1600 years previously? Isn’t this counter-intuitive? Wouldn’t one expect a ‘civilization history’ to show up in the Maltese archaeological record documenting even more sophisticated, construction techniques…?

“Dr. Anton Mifsud, President of the Prehistoric Society of Malta…offers this succinct summary of the problem: ‘Malta is presently too small in size to have sustained the earliest architectural civilization.’”

Likewise all the various megalithic or Neolithic societies we encountered had developments that are “counter-intuitive.” Each of them exhibits advanced developments one would expect from a culture with a long ‘civilization history’ that ‘declare themselves to have been built by people[s] who had already accumulated long experience’ in various technologies. As we said earlier and reiterate again here: all these cultures arrive on the scene out of nowhere and have technologies that do not come into existence for thousands or several hundreds of years, but all these advancements do not lead to sustained civilized society; rather

338 Ibid., pp. 324 - 325
they disappear with the societies that supposedly invented them. With our chronology, every one of these societies had a long history of civilized development. When overcome by recurrent episodes of plague, these societies disappeared, but the survivors migrated from their centers to establish cultures with both Neolithic and more modern elements. The advanced elements repeatedly reflect either Roman or post-Roman times in their respective parts of the world.

It should be pointed out that, in some parts of the world that we discussed, areas may have succumbed to plague even earlier. For example, in Italy the Antonine Plague dated roughly to A.D. 165 brought on a crisis. Although there is much dispute as to its effects, some villages, towns and even cities would have been depopulated, but not all. For an overall picture of the debate see Christer Bruun, “The Antonine Plague and the Third Century Crisis,” Crises and the Roman Empire: Proceedings of the Seventh Workshop (Leiden, the Netherlands 2007), pp. 201-218. At other places the plague came later. In some areas it was immensely virulent, in others not so. Therefore, some sites in Italy and/or elsewhere would have been abandoned earlier than those of the Justinian plague, but, again, these will be fewer in number. The Roman Empire continued on for centuries after this earlier plague, showing it was not as destructive as the latter.

With the discovery of these prehistoric civilized cultures we were told, above, that their existence contradicts the fundamental understanding of historians and archaeologists. Dr. Harvey Weiss discussing Titriş Höyük, above, said: “We don’t understand why” this civilization existed. Lucy Wyatt said: “Çatal Hüyük…clearly contradicts the idea that farming was the precursor of urbanization” and “It makes no sense that it is nearly five thousand years later, after the first appearance of agriculture in the Golden Crescent in the tenth millennium BC, that cities arise…in Mesopotamia,” that “the people of Çatal Hüyük had access to an anachronistic sophistication.” Todd, above, said the environment of Catal Hüyük posed “difficulties;” of Nevalı Çori, Burra Gautam Sidharth, above, said “it is an isolated oasis within the framework of present-day knowledge;” of Göbekli Tepe, Harald Haarmann and Joan Marler, above, said the site “challenges the traditional definition of Neolithic life…” They have “unique” developments that became “common” 5000 years later, such as drainage/sewer systems at some, or town planning, maps, cloth, beads with holes too small to be threaded by modern

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339 Wyatt, op.cit., p. 45
340 Ibid.
341 Ibid., p. 54
needles such as those found in Egypt, cosmopolitan populations, sophisticated construction techniques, pottery as old as the end of the Ice Age, huge stones and earthen constructions, carpentry techniques that only come into existence at the end of the seventh century A.D., or as in Jōmon Japan the people grew African bottle gourds, to name several. We are being asked to accept contradictions to the historical orderly development of mankind for theses that time after time exhibit anachronistic developments that are accepted as not anachronistic at all.

This does not preclude that there were prehistoric sites that developed aspects of civilization soon after the Ice Age ended, in terms of Velikovsky’s chronology around 1500 B.C. One example is Dolni Vestonice in central Europe, dated to 24,000 B.C., based on radio carbon dating which we date to just after 1500 B.C.\textsuperscript{342}

We further maintain that based on all the previous evidence in volumes I, II and III of \textit{Pillars of the Past} and Velikovsky’s hypothesis, that the Stone Age in Europe and the rest of the world was not the time of highly civilized societies or, if they existed, they were quite small and very rudimentary because this was the time of the megafauna. By placing the Beaker peoples and all the others discussed above into post-Roman times, all the missing people across the Afro-Eurasian landmass are not missing and their strata are known.

As further evidence for this we turn to the domestication of the cat. In volume III of this series, pages 149–156, we outlined the origin and spread of the domestic cat from Egypt. Therefore any remains of cats found in any of these presumed prehistoric sites would, we maintain, have to have existed after dynastic Egyptian times. Present-day prehistorians, on the other hand, suggest that cats were domesticated well back into prehistoric times because they have unearthed them in certain sites that they dated via the radiocarbon method, but with no other scientific method to corroborate that chronology. It must first be pointed out that finding the remains of cats is rare, as explained by Donald W. Engels:

“Nevertheless, it has been frequently noted that cat remains are relatively scanty compared to other animals, such as goats, cattle, sheep and dogs. To understand this a few salient facts must be considered. First the nature of the bones themselves. Unlike large animals, cats have small and fragile bones that do not always survive in acidic or other unfavorable soil conditions. Often it is difficult to tell whether a small bone fragment belongs to a cat, a rabbit, or other small mammal. Furthermore, the large size of early domestic cats often reaches 42 inches (1.07 m) in length and

\textsuperscript{342} Knight, Lomas, \textit{Uriel’s Machine, op.cit.}, pp. 11-13
was comparable to the contemporary *sylvestris* and *libyca* [genres], making it difficult to differentiate the domestic from the wild varieties.

“We must also understand the nature of cats’ deaths in order to understand the circumstances for the survival of their bones. To understand their deaths we must understand their lives. Most cats live solitary lives in barns, villages, or as feral cats in the fields. Upon their deaths they are unlikely to have been interred in gardens or adjoining fields or discarded in refuse pits. It has often been noted...that when death is near, even house cats will often quietly disappear to hidden places outdoors to die. Under such circumstances their bodies were probably scavenged by their ancient enemies. Thus their bones may often have been completely gnawed away or scattered in small fragments in fields. Most cats died alone and unmourned. Hence it is only through chance that such remains are found.”

Therefore, if one finds the remains of a domesticated cat it is quite probable that there were others living in that vicinity, because cats like other mammals need their mothers’ milk and, to sire kittens, a tom cat must exist. Once cats cohabitate in the same environment with humans that raise and store grain which attracts mice or other vermin, they will naturally continue the symbiotic relationship. As we know from volume III, the Egyptian domesticated cat, once it adopted mankind, never disowned that connection. They remained with or near man from their first mutually beneficial contacts in ancient Egypt up to the present. There are no cat Dark Ages after Egyptian times. However, prehistorians must posit Dark Ages for the cat from prehistoric sites and times. That is, the cat is found in a prehistoric burial context but is not found again for thousands of years in any other nearby site.

In this respect, in terms of our chronology, where cats’ bones are found at presumably prehistoric digs, we maintain that these were actually post-Roman sites and this is in line with all the previous evidence presented above. Therefore, our chronology has no cat Dark Ages. Juliet Clutton-Brock reports:

“It is still not known when cats were first domesticated. Zeuner (1963) believed that it was comparatively late, although in an earlier publication (1958) he clearly liked the idea of suggesting that the remains of cats from the pre-pottery Neolithic site of Jericho (c. 7000 BC) were from animals that had some sort of association with humans. As with so many other of his ideas Zeuner may have been correct in this view because in 1983 the well-preserved mandible of a cat was found at the Neolithic site Khirokitia in Cyprus, dating to around 6000 B.C."

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343 Donald W. Engels, *Classical Cats: The Rise and Fall of the Sacred Cat* (London 1999), pp. 176-177
(Davis, 1987). And as there is no fossil evidence for cat on Cyprus this animal must have been taken to the island by humans.”

According to Simon Davis, this cat “belongs to the species known to have been domesticated, and in view of the absence of wild cats from Cyprus, it [the jaw] probably belonged to a domestic cat…” He adds, “the measurements of the Khirokitia specimen (carnassial tooth length, and tooth row length)…suggest that it is Felis silvestris—ancestor of domestic cat, perhaps even a domestic cat.”

According to Graeme Barker “a cat was found carefully buried with its owner and other grave-goods.” But then the domestic cat vanishes from the island for thousands of years. “The Felis sylvestris libyca [is] the direct ancestor of all domestic cats.” That is, the remains of cats now found at Cyprus suggest the cat was domesticated on the mainland and in Asia or Africa for some time and then was brought by boat some 60 miles or more to Cyprus. The problem is the intervening cat Dark Age that follows this discovery. According to Laura Stone, Paul F. Lurquin and Luigi Luca Cavalli-Sforza:

“Until very recently it was thought that cats were first domesticated in Egypt about 4000 years ago at the earliest. However, a recent archaeological find in Cyprus pushes that date back to at least 9500 years ago…This burial seems to be intentional. Since an even more ancient cat figurine has also been found in the same Neolithic village, this suggests that cats had already been domesticated FOR A WHILE in Cyprus.”

Note that they tell us the cat was domesticated 9500 years ago but then deserted man for no apparent reason. This is exactly the same as man’s prehistoric domestication of plants and animals in the very early times on a fairly large scale but then humanity failed to create civilization. Meg Daley Olmert states: “But it would be another four thousand years before the cat really found its best friend. During that long time cats and humans seem to have been content to keep their

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344 Juliet Clutton-Brock, *op.cit.*, p. 138
348 Engels, *op.cit.*, p. 18
distance until the Egyptians invited cats into their homes."

This is pure speculation. Why would cats, once living closely with humans and deriving the benefits of shelter and food, in the form of mice and other vermin living with man, decide to give up this beneficial connection? The reason given for this occurrence is that it is the only way to maintain man was carrying on civilized agricultural and pastoralist existence on a relatively large scale thousands of years before civilization began. In terms of the short chronology the cat was not domesticated over 6000 years before civilization developed. However, given all the previous evidence for dating this period into the post-Roman era, cat domestication correlates, corroborates and converges with all that evidence in a congruent manner to be dated to after the fall of Rome and the descent of man back to Stone Age development.

As with the other finds of civilized development in assumed prehistoric sites “archaeologists were surprised to find the remains of a male cat carefully laid to rest with a human in a grave on…Cyprus.”

Others were also found at Jericho, dated “from about 9000 years ago, and …from Harappa [cats paw marks were found in clay]…dating from 4000 years ago. However, it is thought [not proved] that these were probably from wild cats…killed for their pelts or, possibly, for meat.” But according to Linda S. Braidwood who evaluated these finds: “For the present, therefore, we must continue to assume that cats became domesticated in dynastic Egypt. The Jarmo excavations [however] yielded four lower jaws and the proximal end of an ulna that could have been from a domestic cat.” However, Alfred Haldar suggests regarding Jarmo, “the goat, the dog, and possibly the cat were with fair certainty domesticated.”

In assumed Neolithic Britain Stuart Piggott reports: “The Neolithic wild cat (Felis sylvestris) [was found] at Windmill Hill.” To get around this seeming contradiction Colin Renfrew, Judson T. Chesterman and Martin Jim Aitken present the fact that cat remains in an assumed Neolithic site in Scotland tell us: “The bones and teeth are small [unlike those of wild cats that are larger than

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350 Meg Daley Olmert, Made for Each Other: The Biology of the Human-Animal Bond (Cambridge UK 2009), p. 160
351 Ibid.
352 Josephine Wills and Ian Robinson, Bond for Life: Emotions Shared by People and Their Pets (Minocqua WI 2000), p. 16
353 Linda S. Braidwood, Prehistoric Archaeology along the Zagros Flanks (Chicago IL 1983), p. 448
354 Alfred Haldar, Who Were the Amorites? Vol. 35 (Leiden, the Netherlands 1971), p. 38
domesticated ones] which suggest [it had somehow crawled into the site in more recent times rather] than [that they are] from a neolithic wild cat. It is also unlikely that wild cats have been part of Orkney fauna [at that period].”

As with everything else described above about these sites, the domesticated cat is an anomaly with the established chronology but not with what we have presented. That there should be so many forms of evidence that are anachronistic with the established chronology and none or extremely few with the short chronology points ineluctably to the validity of the short chronology. When one places any known people in prehistoric times then of course their stratigraphy disappears.

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CHAPTER 12

OTHER EXPLANATIONS FOR DARK AGES

At this point we are brought to other revisionist chronologies regarding these times. Heribert Illig hypothesizes that three centuries, A.D. 614–911 never existed. Anatoly Fomenko’s theory holds that historical chronology does not come into being until around A.D. 1100–1200. Both Heinsohn and Sweeney are proponents of Illig’s missing three centuries, while Sweeney places the building of Stonehenge and the Megalithic Age in the early first millennium B.C. Laurence Dixon and many members of the Society for Interdisciplinary Studies have hypothesized that during the Middle Ages there were catastrophic events that affected the polar tilt and rotation period of the Earth, which makes any astronomical retrocalculation of events prior to these incorrect. In all instances their theories challenge and call into question the ability of archaeo-astronomy to correctly retrocalculate dates and the celestial positions of the Sun, Moon, planets, and stars as described in ancient documents to a precise day. We, on the other hand, accept the accuracy of archaeo-astronomy after the 8th century B.C. and therefore are obliged to show that these aspects of their revisionist chronologies are invalid.

EMMET J. SWEENEY

Sweeney in his book, The Lost History of Ireland: An Enquiry into the pre-Christian History of the Gaels, has dated the Megalithic Age in Europe to the latter part of the first millennium B.C., prior to the catastrophe of the 8th century B.C. In this way he has placed Velikovsky’s Venusian near-collision with the Earth in historic times. He cites Robert Graves’s translation of Diodorus Siculus respecting the placement of Stonehenge on “an island in the ocean, not smaller than Sicily, lying in the North” where “the inhabitants venerate Apollo more than any other god” in “a remarkable temple of a round form [sic].”¹

Apparently Sweeney, whose revisions in very large measure we accept, has failed to take into account the fact that the “round” temple should have been translated as “spherical.” He has further failed to take into account the work of James Ferguson who showed that in Diodorus’s work Stonehenge is described in

¹ Sweeney, op.cit., p. 57
his book covering Asia, where Stonehenge does not exist, and that in his book which describes Britain, he makes no mention whatsoever of a spherical or even a round temple. While Sweeney cites “the words of Julius Caesar, “ he has failed to note that, as Algernon Herbert had shown in 1849, there is not one Greek or Roman writer who has mentioned the megaliths in their writings. Given all the other evidence we have presented regarding the period of the Megalithic Age in the earlier chapters of this book, Sweeney’s thesis fails especially because of the fact that Stonehenge’s alignment, as will be shown by Lynn E. Rose in the Appendix, must be dated to between about A.D 500 and 700. Because Sweeney also accepts Heribert Illig’s thesis that there is an empty or non-existent period between A.D 614 and 911, his objection to this criticism based on astro-archaeology stands only so long as Illig’s hypothesis stands. Our criticism of Illig is therefore crucial.

**HERIBERT ILLIG**

Dr. Heribert Illig, with his associate Dr. Hans-Urich Niemitz, rather than attributing this Dark Age to barbarian invasions, radical climate shifts or recurrent plagues which depopulated these regions and caused the disintegration of the Roman industrial-commercial empire, maintains this period of time simply never existed. They maintain that the year after A.D. 614 was not 615 but was in reality A.D. 911, and furthermore, since this time period, which they term “phantom centuries,” never existed, then everything including the documents of this time are either forgeries or false extrapolations. O’Neill, a proponent of Illig’s thesis, gives the best description of it and the evidence for it in the English language. He has presented Illig’s thesis, much of which will now be cited, to a wider English speaking audience:

“A Radical Solution”

“The lack of archaeology during this epoch has long been an enigma, but it is an enigma for which a novel solution has been proposed. Since the early 1990s, German writer Heribert Illig has been suggesting that the years between 600 and 900, or, more precisely, between 614 and 911 [a total of 297 years], never actually existed, and that almost three phantom centuries were inserted into the calendar. It was this chronological error, more than anything else, that gave rise to the notion of the Dark Age.

“The present writer has been aware of Illig’s thesis now for over a decade, and researched it thoroughly before coming out in favour. I was initially attracted to
the idea because it seemed to solve many of the riddles and enigmas surrounding the Dark Age.”²

Nevertheless, there are many existing documents from different lands during these phantom centuries that correlate with and corroborate one another by direct linkage to events and by the specific people named in both sets of documents. And in these cases there are not just a few of them but a great many. Although this is not scientific evidence, it is, however, powerful historical evidence and therefore needs to be accounted for by Illig and/or any of his proponents. O’Neill described this evidence for us:

“Copious records, in the form of chronicles and annals, are known to exist from the Dark Age; and these documents cover the period between 600 and 900 in detail. In addition, they appear to be internally consistent. The Anglo-Saxon Chronicle, for example, and [the Venerable] Bede…mention the visit of an Anglo-Saxon king to France in a certain year, and the corresponding chronicles of medieval France will confirm the visit. Furthermore, if three hundred years were added to the calendar, how could this error have been transmitted to the Byzantine and Islamic world? Do not their records agree in detail with the western calendar? To argue that all these documents are false, we would apparently need to assume that they [all] are in some sense fraudulent and that there was thus a vast conspiracy that somehow took in all the nations of Europe and the Middle East. Such a proposition seemed utterly improbable.”³

We will go into this below when we discuss the Irish *Annals of Ulster* which correlates directly with *The Antiphonary of Bangor*. However, not only does this conspiracy encompass Europe and the Byzantine Empire, it must also encompass China’s interactions with Islam which also occur in these supposed phantom centuries. A battle between the Tang Dynasty of China and Islam was fought in eastern central Asia in A.D. 751, known as the Battle of Talas. The Arabic forces evidently won and Chinese prisoners were taken who appear to have brought the ancient Chinese art of paper making to the Islamic world. Furthermore, this battle led to the cutting off of trade via the Silk Road between China and the West. The germane point is that the sources of documented evidence for this battle are found in both Arabic and Chinese records.⁴ Based on these accounts, in which the Muslim and Tang

² O’Neill, *op.cit.*, p. 233
⁴ Mohamed Taher, *Encyclopedic Survey of Islamic Culture* (New Delhi 2003), p. 113; and Dionisius A. Agius, *Classic Ships of Islam: From Mesopotamia to the Indian Ocean* (Leiden, the Netherlands 2008), p. 77 (n. 67)
dynasty records agree with each other and with the western calendar, Illig must argue that these documents too are false or fraudulent and that a vast conspiracy to present false and fraudulent or forged documents stretched from Britain to China. The dimensions of this conspiracy as O’Neill stated above seem “utterly improbable.”

Still it is Illig’s contention that all these annals, chronicles, records to the last one, must be fraudulent or forgeries, which he has only proved of certain ones in Europe. Yet he has failed to prove this for all of them. Of those Illig has shown to be forgeries O’Neill shows:

“There can be no doubt that the chronicles of Western Europe do provide a wealth of detail about events during the dark centuries; and the details provided in the various manuscripts are indeed internally consistent. According to Illig, all of these documents were composed in the eleventh, twelfth and thirteenth centuries, and none of them date from the periods they claim. Now, there is no question that the high Middle Ages was a period noted for documentary forgery. The best-known example for this was the so-called Donation of Constantine, supposedly written in the eighth century, but now widely recognized as originating at a later date…

“Another famous, or rather infamous, example of this genre is the so-called Pseudo-Isidorean Decretals. These constitute the most extensive and influential set of forgeries in medieval Canon Law. Some collections of them included, for good measure, copies of the Donation of Constantine. These works, supposedly produced during the mid-ninth century…in north eastern France, have been universally recognized as forgeries for well over a century. We should note that ‘Immense labor and erudition went into creating this work, and a wide range of genuine sources were employed’…

“Documentary forgery then was something of an industry during the Middle Ages…

“Aside from those recognized forgeries Illig and his colleague [Hans Ulrich] Nimitz [sic] have noted that a great many of the Early Medieval documents which are still regarded as genuine have an ‘anticipatory’ nature. In other words, they framed laws which, at the supposed time of writing, was [sic] useless or redundant, but which later during the twelfth, thirteenth and fourteenth centuries, became very useful indeed to the temporal and ecclesiastical authorities…The Councils of Toledo, of the seventh and eighth centuries [are] prime examples of these. These texts set legal and moral precedents for the treatment of Jews, which [only] during the thirteenth, fourteenth and fifteenth centuries, were in fact enacted against that people…
“These, according to Illig’s thesis, had to be created in the years after Otto III, since, following his calendar reform, there existed on paper three centuries which never existed in fact, and which had, therefore, no history [and] had to be filled with something…He has drawn attention to the fact that modern textual criticism and forensic science has proven and is in the process of proving more and more of these ‘Dark Age’ documents to be forgeries…”

Because these three centuries did not exist according to Illig, O’Neill notes:

“…neither…did the characters said to have lived in them…, such as Charlemagne and Alfred the Great…Illig has now modified this somewhat extreme position and has suggested that these persons probably did exist; only they didn’t live when the chronicles said they did. A king Alfred of Wessex probably did fight the Danes, but he would have done so in the early seventh century, not the ninth. In the same way, it could be that the entire Carolingian Dynasty [of Charlemagne], of the seventh, eighth and ninth centuries, is little more than a replication of the Merovingian dynasty (both dynasties were Frankish) of the fifth, sixth and seventh centuries…If Charlemagne is identical to Clovis, this would explain why he is never credited with fighting the Muslims; Islam didn’t exist in his time.”

Illig’s contention is that these forgeries were created only because these phantom centuries supposedly never existed. But if they did and the vast number of people lived in tiny villages surrounded by forest and were illiterate, having houses of wood and wattle, then that, too, would produce a vast illiterate Dark Age. The forgers of the later centuries attempting to fill these same three or more centuries would have a very good motive to fill them in with their own religious and/or political concepts of what they believed should have existed in these times. To paraphrase O’Neill, because these centuries existed with almost nothing documentary for vast regions, it appeared they had no history, and therefore had to be filled in with something. This was not a time when scrupulous historical canons of ethics existed to restrain writers. Like Manetho and Eusebius, they fabricated a historical past with documents, political figures, and events that gave their countries an exalted history that in most instances never happened.

In this regard we are again following the approach to a short historical chronology for the Megalithic Age as did Heinsohn for the ancient Near East. Heinsohn claimed that the records of Manetho, Berossus and perhaps Josephus were written to fill in centuries with little or no evidence for their existence. And

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5 O’Neill, op. cit., pp. 243-244
6 Ibid., p. 245
they filled these centuries with dynasties and civilizations that did not exist. This is also exactly what modern scholarship has done for the Megalithic Age, having the very same situation and incentives. As Manetho and Berossus filled in these supposed ‘phantom centuries’ with historical events and characters that may only have been part of folklore and tradition, so too did the later medieval scholars of the Megalithic Age. A perfect example of this is the myth of King Arthur and perhaps others as well. These later medieval antiquaries would have known almost nothing of these post-Roman centuries; like Manetho and Berossus they gave free reign to what they thought had transpired and along with this they invented history and fraudulent documentation for it.

Our historical interpretation is as valid a thesis as that presented by Illig. Let us recall that Manetho wrote about the 7th–10th Egyptian Dynasties that some modern historians doubt existed; others have filled in this period with their own interpretations of what existed in them. These modern historians are highly trained and attempt to be scrupulous in their handling of evidence, but nevertheless have created a non-existent period with a history and then interpreted the archaeology to fit this interpretation, when, as we have shown in volume III, these dynasties belong to the Hyksos period and, therefore, the archaeological evidence also belongs to a different period, that of the Hyksos. The writers of the later periods after the Dark Age were under no such compunction to refrain from producing well-organized, highly fraudulent explanations and forgeries to fill up these centuries. This is our interpretation, which in no way discredits Illig’s evidence but does undercut the thesis upon which Illig has interpreted these forgeries and fraudulent documents.

For example, Gunnar Heinsohn has presented confirmatory evidence to support Illig’s contention that these phantom centuries never existed and argues:

“Nearly fifteen years ago, when I first pondered the viability of Heribert Illig’s identification of Europe’s proverbial Dark Age in its Early Medieval period (AD 600–AD 900) as an additional phantom period, I drew on Simon Dubnow’s history of the Jewish people. In the early 20th century, this giant of a scholar had already noticed an awkward textual silence of the ‘People of the Book’ for that very AD 600–900 period. Later, he sternly warned his colleagues to abstain from ruthlessly inventing ‘paper structures to bridge this chronological abyss.’ Meanwhile, although
no other nation in the entire world has left us more Medieval first-hand texts than Jewry, in fact, no Jewish texts from AD 600-AD 900 have yet been found.”\(^7\)

But neither Heinsohn nor Illig have considered other possible interpretations in terms of the established chronology to explain this. There are three reasons based on evidence that can account well for this silence, and perhaps others that we have not discovered or considered. The first is that the Jews of this period, like everyone else, were not immune to the great plagues that were ravishing Europe and the rest of the Afro-Eurasian continent. They also undoubtedly lost most of their population to these recurrent pandemics. As Salo Wittmayer Baron explains of the Justinian plagues, “Together with the rest of the population Jews undoubtedly suffered from the high mortality occasioned by the plague[s].”\(^8\) Furthermore, the Jews, like the rest of the population, would have been reduced to a level of poverty and thus had to devote all their attention and efforts to staying alive. Scholarship, at that time, would have had to be neglected to do just that.

Stephen Tomkins describes a second cause for their silence, namely the persecution inaugurated by Justinian, the kind of persecution that followed Jewry throughout history. In the Byzantine Empire, even prior to the plagues “As defender of orthodoxy, he brutally victimized Jews…, forbade Jews to build synagogues, read Hebrew scripture or testify against Christians…‘In the monstrousness of his actions one sees the power of the devil,’ said the civil servant Procopius, ‘No one but God I believe could count those [Jews] he murdered’.”\(^9\)

O’Neill enlarges on this persecution:

“In 532, just five years after his coronation ‘…the emperor issued a decree to the effect that in cases of dispute the Jews could act as witnesses against one another, but not against Christians…*(Corp. Juris* i, 5, 21). The synagogues of the Samaritans shall be destroyed, and if they dare to build others, they shall be punished. They may have no testamentary or other legal heirs except Orthodox Christians’ *(ib. i, 5, 17; comp. i, 5, 18-19). They were forbidden to leave legacies or presents.”\(^10\)

O’Neill goes on to show how destructive these laws were to the Jews in this empire. In the later *Novella* Laws enacted around 537, Jews enjoyed no privileges

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\(^9\) Stephen Tomkins, *A Short History of Christianity* (Grand Rapids MI 2005), p. 69

\(^10\) O’Neill, *op.cit.*, p. 76
if they took office. They could not acquire real estate from a church or religious institution; if this happened, their holdings were to be confiscated. They could not erect any new synagogues. When the Jews revolted and joined with Persia, “Heraclius had promised [them] amnesty for their earlier treachery, but...the Emperor reneged on his promise and ordered a general massacre of the Jews.”

Although O’Neill calls upon Jewish and other sources to soften these persecutions, he fails to mention Procopius’s statement about the number of Jews “he murdered,” and attempts to suggest very little of these persecutions actually were carried out.

Typical of Jewish persecutions was the Emperor Hadrian’s repression of the Jewish uprising in Israel which “was quelled only after a frightful massacre and the virtual extinction of the Jewish race [sic; the Jews are a religion, not a race, and this O’Neill should know better] in the land of Israel.”

Justinian’s power extended throughout much of the Roman world and those who followed or enacted such laws may have been, and quite probably were, emulated by local rulers in order to seize Jewish property, just as did the later Inquisition and Nazis as did others. When I was a boy, my father told me some of our neighbors, who were members of the German Bund, were found out to be drawing up a list of Jews in our area whose property they planned to seize when Germany won World War II. The Justinian predation of the Jews prior to the onset of the plagues would have had a further devastating effect on the Jewish population’s ability to produce documents.

There is yet a third reason for the silence on the part of the People of the Book; not only did the persecutions under Justinian murder so many Jews that “no one but God I believe could count [them]”, but then the plagues played havoc with the rest over the succeeding centuries, so that they had to return to the countryside to eke out an existence, where literacy was no help. Moreover, they needed material upon which to write—in this case paper/papyrus, and thereafter parchment. When trade across the Mediterranean halted, it brought a stop to the importation of papyrus to Europe, Byzantium, Greece, etc. O’Neill explains:

“Worst of all, perhaps, from the perspective of culture and learning, the importation of papyrus from Egypt ceased. This material, which had been shipped into Western Europe in vast quantities since the time of the Roman Republic, was

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11 Ibid., p. 77
12 Ibid., p. 75
absolutely essential for a thousand purposes in a literate and mercantile civilization; and the ending of the supply had an immediate and catastrophic effect on levels of literacy. These dropped, almost overnight, to levels perhaps equivalent to those in pre-Roman times.”"\(^\text{13}\)

O’Neill goes on to cite the Belgian historian Henri Pirenne:

“‘Papyrus,’ he says, ‘was the first [item of trade] to disappear. All the works written in the West on papyrus of which we have knowledge are of the 6\(^{th}\) or 7\(^{th}\) century. Until 659–677 nothing but papyrus was used in the royal Merovingian chancellery. Then parchment made its appearance.’ And it is hard to quarrel with Pirenne on this point. The Middle Ages, by definition, were the age of parchment, on which was [\textit{sic}] executed the famous illuminated manuscripts of the period.”"\(^\text{14}\)

A further reason that so very few papyrus documents were discovered from this period, according to Pirenne, was that “the fragility of papyrus in the northern climate explains why so little is left; but we must be under no illusion as to the quantity which was formerly employed,” to which O’Neill adds “The loss of papyrus supplies…had a devastating effect…”\(^\text{15}\)

The manufacture of parchment furthermore required a great deal of time and effort to make; it was also far from inexpensive as outlined by Anatoly T. Fomenko:

“Just how available had parchment been?

“The manufacture of one sheet of parchment requires the following…:

“1) skinning a young calf no older than 6 weeks or a young lamb;

“2) macerating the skin in running water up to 6 days;

“3) scrubbing the membrane off with a special scrubber;

“4) loosening the wool via scouring the skin in a damp pit and subjecting it to ash and lime for 12-20 days;

“5) scraping off the loosened wool;

\(^\text{13}\) O’Neill, \textit{op. cit.}, p. 3
\(^\text{14}\) \textit{Ibid.}, pp. 99-100
\(^\text{15}\) \textit{Ibid.}, p. 98
“6) fermenting the clear skin in oat or wheat bran in order to remove excessive lime;

“7) tanning the skin with special extracts to make it soft after drying;

“8) eliminating the roughness by pumicing the chalked skin.

“This is the procedure required for the manufacture of every leaf of parchment.”\(^{16}\)

These processes required someone with the skills to prepare parchment and a buyer with sufficient funds or trade goods to pay for a sheet of it. Nearly all the people of the early and even middle of the Middle Ages, including the Jews, did not have the means to pay for it. And further, what independent parchment-maker or one attached to a monastery would be willing to supply Jews with it so they could advance Hebrew causes? The Middle Ages were not a time that Jews were all that welcome in Christendom. Abundant supplies of parchment came relatively late in Medieval Europe according to Christopher De Hamel:

“Parchment is made from the skin of an animal. The process of transforming the animal skin into clean white material suitable for writing was the task of the *percamenarius*, the ‘parchment-maker’ or parchmenter. Such professions existed throughout the Gothic period and probably back into the Romanesque and Carolingian ages [of the earlier part of the Middle Ages]”\(^{17}\)

Beyond this, “parchment was used again and again, with ancient texts frequently erased and lost forever to scholarship. We cannot doubt that the disappearance from Western Europe by the eleventh century of many of the Greek and Latin masterpieces was due entirely to this cause.”\(^{18}\) Even if the Jews had parchment, during the ensuing period, pogroms and other attacks on the Jewish community would have also caused such writing to be lost to posterity. It was only in the Gothic period, as we were told above, that parchment was widely available and that period came after 911 AD—after the end of Illig’s phantom centuries. Only then its widespread availability allowed Jews to purchase it from a more commercial environment and write.

This developmental aspect of the early to somewhat later medieval period has, we believe, generally been a dark spot in the vision of some historians and revisionists. They assumed that the impoverishment that followed the collapse of

\(^{16}\) Fomenko, *op.cit.*, p. 383

\(^{17}\) Christopher De Hamel, *Scribes and Illuminators* (Toronto 1992), p. 8

\(^{18}\) O’Neill, *op.cit.*, pp. 98-99
Roman civilization was quickly followed by various developments such as metals, stone buildings, stone castles, cathedrals and monasteries, when the case appears to be that these buildings, like those of the peasants, were constructed of wood or that no trade networks to supply the population with goods existed. Instead, this was a time when self-sufficiency was the order of the day. As noted above, people lived in tiny isolated villages surrounded by vast woodlands that were not cleared until around the 11th century. Only in the great wooden monasteries did communities with a few monks and other churchmen carry on literacy and of these literate materials many, if not most, documents have been lost.

Therefore, not only does our short chronology explain these matters but it exhibits a logical and orderly development of growth from primitive, almost Neolithic conditions, that commenced with the destruction of civilized life by the eighth century which then took Europe 400 to 500 years to begin to revive. This implies that some of the people Illig claims never existed may have done so, such as King Alfred whom we will discuss below.

It must be pointed out at this juncture, that we have not done complete justice to Illig because his works are nearly all in German which is beyond our linguistic abilities. If he has dealt with the criticisms we have raised, we will be happy to have his response provided to us in English.

Nevertheless, in order to determine the validity of Illig’s thesis that these phantom centuries did not exist or our thesis that they did exist, we must turn as far as possible to science. Above we have already shown that at Stonehenge, the first glint of the Sun appeared in the avenue as seen from the center of the monument between the Heel Stone and its partner stone on the summer solstice from about A.D. 500 to 700. This evidence shows that Illig’s first phantom century existed. However, this must be backed up by other forms of scientific evidence to confirm and corroborate our thesis. If there is direct scientific evidence that proves that these phantom centuries were real, that is, that they existed, then no matter what Illig has concluded from fraudulent and forged documents, his thesis will be discredited. One final point should be mentioned before proceeding. One of the strongest points of his thesis is that there is not any stratigraphical evidence in the earth for his period and therefore no people existed then. However, we have found the missing stratum, maintaining that it is that of the Neolithic/Bronze Age peoples etc., who were erroneously assigned to prehistoric times, and we have given various and numerous forms of evidence which places them in the post-Roman era.
and also into the three centuries Illig denies existed. With our chronology we have the people and the stratum that Illig argues does not exist.

What then are the scientific forms of evidence that support or refute Illig’s thesis? If the scientific facts repeatedly uphold Illig, his case will be validated; if the scientific facts repeatedly contradict Illig his case will thus be falsified. Hence what is that evidence and what does it prove?

The first form of evidence we turn to is the very nature of the recurrent plagues that began in the East in 541 and in western Europe in 542-543 and later. These recurrent plagues did not last a very short time; they came back repeatedly for about 200 years. This is quite similar to the Black Death plague of 1330 that lasted into the 1600s and lingered on in France into the 1700s. That is, whenever these later great pandemics broke out over large, highly populated regions, they did not come and recur for only a few decades; rather they lasted for centuries. But if Illig’s phantom centuries existed, these recurrent plagues that began in A.D. 541 either ended in 614 (a period of only 74 years), or they recurred after 911 for about 140 years. That is the overall length such pandemic plagues last—200 to 300 years. This suggests that Illig’s plague period of 74 years is a complete anomaly. As we cited Gottfried above: “after 599-600 successive epidemics of the first plague pandemic were less virulent, but about as frequent. Large parts of Mediterranean Europe were afflicted in 608, 628, 640, 654, 684-86, 694-700, 718 and 740-750.”

Here then is the problem: the writers of the fraudulent or forged documents of the 11th, 12th, and 13th centuries, who would have been totally unaware of the pandemics that would occur in the future, long after they were dead, somehow not only falsified their plague records, but somehow could know that in the future there would be a pandemic that lasted for centuries, and used these as their model to fabricate history. Had there been non-existent phantom centuries, then the plagues would have continued on into the 10th and 11th centuries. Since 614 AD was followed by 911 AD, this is an ineluctable outcome. But there are no reports of recurrent plagues at this later time. Why they invented recurrent pandemic plagues that just happen to fit the normal time span of ones is not explained nor does it make scientific sense.

However, Illig and his supporters can turn to, and have turned to, what appears to be the very same type of evidence we have consistently relied on, namely astronomical/calendrical evidence. It is maintained that Illig’s phantom century hypothesis is fully correlated and corroborated by science! Therefore this scientific evidence which is his strongest form of chronological proof must, they can then
argue, override any contention to the contrary upon our part. He is employing the very same evidence that we hold in the highest regard.

Illig has presented this scientific/calendrical evidence in *Vorzeit-Frühzeit-Gegenwart* 1/91 in a paper titled “The Christian Era is Too Long.” O’Neill summarized it as follows:

“…we would be remiss if we proceeded without mentioning the fact that his thesis has a very powerful astronomical, or rather calendrical, support. This concerns the Gregorian calendar and the circumstances surrounding its introduction in 1582. The latter was intended to replace the old Julian Calendar, introduced by Julius Caesar in 45 BC, a necessary reform owing to the inaccuracy of the Julian system. The Julian calendar treated the year as exactly 365.25 days long—an extra day was added every fourth year, or Leap Year. But the year is not exactly 365.25 days; it is more precisely 365.2422 days, which means that following the Julian system…eleven minutes [and 14 seconds] are added every year; and this, in the 1,627 years that had apparently elapsed between Caesar’s reform and Pope Gregory’s, should have produced an error of roughly thirteen [lost] days. [But i]n fact, the astronomers and mathematicians working with the Pope found that the civil calendar needed to be adjusted by only ten days, and it thus appears that the calendar counted roughly three centuries which never existed. The normal explanation for this discrepancy is that the Julian Calendar must have been ‘updated’ [to correct the day of the year of the vernal (spring) equinox] and two or three days [were] removed at the Council of Nicaea in AD 325 when the date for Easter was set. Yet there is no evidence in documents dealing with the Council that this occurred.”

The Julian Calendar loses one day from the calendar every 128.2 years. As Illig has presented, by setting the date of the vernal equinox on March 21 in 45 B.C., with the inauguration of the Julian Calendar, and allowing it to run to A.D. 1582, the date of the Gregorian reform, the number of years between the two vernal equinoxes totals 1,627 years. When we divide 128.2 into 1,627 we lose 12.7 days rather than the 10 days Gregory added to the calendar. This means that about 345 extra years had accumulated in the calendar since the time of Caesar. This Illig maintains is rather strong scientific evidence that solidly upholds his phantom centuries thesis. Since there is clearly no evidence from the Council of Nicaea in A.D. 325 that the date of the vernal equinox had been reset to allow it at that time to fall on March 21, then it appears obvious that the original dating of the vernal equinox to March 21 began 45 B.C. It follows that there should have been 12.7 days or a full 13 days added to the

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19 O’Neill, *op.cit.*, p. 242
calendar in 1582, but only 10 days had been added. Therefore in the Gregorian Reform there are approximately 345 superfluous years that never existed in history between 45 BC and A.D. 1582. This roughly agrees with Illig’s revision and thus it appears that astronomical/calendrical dating evidence—a scientific proof—fully correlates, corroborates and is congruent with Illig’s concept of phantom centuries and unquestionably validates it. It thus appears that Illig has beaten the protagonists of the established chronology as well as those of us who say these centuries existed, such as Lynn E. Rose and myself and any proponents of our revisionist history. He seems to have beaten us at our own game with our own tools and processes!

We say “seems to have beaten us” because, in reality he has done no such thing. The basis upon which Illig has dated the vernal equinox to March 21 he explains thus:

“Is it possible that the Ancient Romans were unable to accurately determine the equinoxes, i.e., the East-West direction? The question sounds absurd since the required knowledge is said to have been available for more than 2000 years at the time of Caesar. Its visible proof is the Pyramid of Cheops, the incredibly precise orientation of whose base line accord[s] to the cardinal points [which] has always been admired. Even if…the 6th century is considered to be the time of its construction (Heinsohn/Illig, 1990, p. 115), the Romans would have had 500 years to learn Egyptian measuring methods. That they did in fact learn this lesson in good time can be shown on the inimitable sundial of [Emperor] Augustus.

“On the Field of Mars in Rome, the first Emperor ordered [to be] erect[ed] a subtly calculated combination of victory monument, birthday memorial, mausoleum, peace altar and sundial…

“Augustus wanted to document his victory over the Egyptians by setting up an obelisk, the first one to have been brought all the way from Egypt. In the year -12…the 50-year-old-emperor decided to use it as a gnomon, nearly 30 m[eters] high, the pin of a sundial whose network of lines, made of marble, was to cover [the surrounding] surface…

“The Emperor himself had been born…at the moment of the autumnal equinox [September 23]. ‘On the emperor’s birthday […] the shadow [of the obelisk] wanders from morning to evening…along the dead straight equinoctial line, precisely to the middle of the Ara Pacis [the Peace Altar]; there is, thus, a direct line from this man’s birth to Peace which visibly demonstrates that he was “natus ad pacem” [born for peace]’ (Buchner 1982, 37).
“In order for this phenomenon to occur, the equinoctial line must run perfectly straight from West to East.

“If it was possible, at that time, to orient the course of the shadow so accurately according to the equinoxes, then we can assume, with very high probability, that one generation earlier, at the time of Caesar, the determination of the equinoxes would not have caused any difficulties either. This means, however, that the vernal equinox in Rome fell on the same day it [did in the time of Augustus–March 21]”

Illeg repeated these very same claims as late as 2011 in his article, “Calendar Reforms of Caesar and Gregory XIII.”

If Illeg’s analysis is correct, then there can be no doubt that the date that this obelisk shadow moved directly in a west to east line was the time of Augustus. It is this almost precise framework upon which Illeg’s astronomical/calendrical evidence rests. The problem is that on this point Illeg is seriously in error. He unfortunately wrote this piece, cited above, in 1991, relying on the evidence presented by Buchner who wrote in 1982, to maintain both the accuracy and the original date for the construction of the sundial. Although the date for the construction is correct, the time its shadow moved directly from west to east along a straight line is not known. This site was changed in the reign of the Emperor Domitian about 134 years later. Jonathan Edmondson in 2007 makes it quite clear that what first Buchner and then Illeg took to be positive proof regarding Augustus’s sundial is seriously erroneous:

“In…Rome, archaeological probes conducted between 1979 and 1981 located elements of the monumental sundial (*horologium*) erected by Augustus in the Campus Martius in 10 BC and discussed by Pliny the Elder (*NH [Natural History]* 36[, pp].72-73), allowing its complex relationship to the *Ara Pacis* to be worked out in close detail, or so it appeared at first. The sundial’s pointer (*gnomon*), an Egyptian obelisk, it was argued, cast its shadow in a straight line right up against the entrance to the altar only on the afternoon of the autumn equinox, 23 September, Augustus’ birthday, while the obelisk was aligned with Augustus’ mausoleum north-north-west of the sundial rather than along the north-south meridian line that was laid out as part of the sundial….This interpretation was enthusiastically championed in many studies of Augustus during the 1980s and 1990s, but it has now been shown that the mathematical calculations that underpinned the conclusions

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21 in *Chronology and Catastrophism Review* 2011, pp. 3-6
about the length of the shadow cast by the sundial on Augustus’ birthday are seriously erroneous, and what in fact has been revealed by excavation were not parts of Augustus’ horologium, but of Domitian’s later reconstruction of it.”

The point is that the sundial did not reflect the time of Augustus but that of Domitian who reigned about 135 years later. The assumption that the sundial was correct in the time of Augustus is “seriously erroneous,” which means the vernal and autumnal equinoxes did not arrive on the dates that Illig has assigned to them. The very foundation—astronomy and calendrics—upon which Illig built this argument for the phantom centuries, based on Augustus’s horologium/sundial has been shown to be “seriously erroneous” and therefore meaningless for determining the correct date of the equinoxes in the year -12.

This supposition that the Romans could precisely determine these equinoctial days is also somewhat questionable. On this point Katharine Volk explains:

“The Autumnal equinox, did not coincide exactly with Augustus’ birthday… but this need not have prevented his contemporaries from making the connection…the exact placement of the tropic points [the cardinal directions] was an object of debate, and the actual occurrence of the solstices and equinoxes may not have always agreed with when people [in Caesar and Augustus’s time] thought they should occur.”

The reason for this can be explained by comparing the orbit of the Earth around the Sun to the swinging of a pendulum. At the solstices, which are the two highest points on either side of a pendulum, both the pendulum, and, in terms of the laws of gravity, the Earth both slow such that, as Ruggles pointed out above, the Sun moves one-thirtieth of a solar diameter for about two weeks. This makes precise dating of these solstitial days difficult to determine. With the equinoxes, on the other hand, these are represented by the lowest point on the swing of the pendulum when it and, in terms of the laws of gravity, the Earth is moving fastest. Unless one has a very precise clock or some other mechanism, throughout the year these equinoctial points occur, the length of daylight being equal to that of night is also extremely difficult to determine. The Romans had clocks of different types but these were never as accurate as needed for such precise time-keeping.

Lynn E. Rose has sent this author material that shows how with a few simple devices, Sir Fred Hoyle has shown that the solstices and, in our case, the equinoxes

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22 Jonathan Edmondson, Augustus (Edinburgh 2007), pp. 11-12
23 Katharine Volk, Manilius and His Intellectual Background (Oxford 2009), p. 152
could be found. This is quite true and may have even been used in the times of Caesar and Augustus.

Beyond all this, although Augustus was supposedly born at the time of the autumnal equinox, even this is questioned by modern scholars. According to Volk:

"The exact date of Augustus’ birthday is subject to debate (cf. Hannah, 2005: 124-5). Suetonius reports it as VIII Kal. Oct., ‘on the ninth day before the Kalends of October’ which (with inclusive reckoning) means 23 September in the Julian calendar. However, Augustus was born before Caesar’s calendar reform, at a time when September was only twenty-nine days [long instead of thirty] and when the ninth day before Kalends of October was thus 22 September. Some scholars therefore believe that the emperor was actually born on 22 September but after the Julian reform changed his birthday to the ‘new’ ninth day before Kalends, i.e., 23 September.”

The most disastrous problem for Illig and his supporters is that when Gregory XIII corrected the calendar by 10 days, he was not doing so by going all the way back to the time of Julius Caesar, i.e. 45 B.C. No, he was going back to A.D. 325, the time of the Council of Nicaea, as Florin Diacu points out:

"…[the] papal bull, issued on February 24, 1582…stated:

"‘Our care was not only to reinstate the equinox in its long ago nominated place from which it has deviated since the Council of Nicaea by approximately ten days…Therefore, to return the equinox to its proper place established by the Church fathers of the Council of Nicaea…that ten days…be deleted.’"

If Gregory XIII had corrected the calendar all the way back to the time of Julius Caesar, he would have deleted 13 days! There are no missing 300 years in the calendar as Illig has told us. His calendrical evidence on this point is totally invalid. See Lynn Rose’s Appendix.

The only way we have out of this dilemma is to find not only a location for this 300-year period where can find the people who lived during this time but the stratigraphical evidence for their existence and, above all, precise archaeo-astronomical evidence as proof of these 300 years from these same people. In this respect let us turn to Ireland. Ward-Perkins has told us above:

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24 Ibid., p. 147, fn. 47.
25 Diacu, op.cit., p. 113
“If these [European] people belonged to a culture like that of Roman times, which produced large quantities of solid [stone] building materials and shiny pottery, then their settlements show up very clearly in modern plough soil…But, unfortunately, the same is not true of settlements from periods with very few durable objects; and, as we have seen, this is exactly what the post-Roman centuries were like….Wooden houses and thatched roofs predominated, which left no tile and mortar fragments…Post-Roman sites…are often very difficult to find.”

While this seems to be the case for Europe, for Ireland, the case was much worse and thus would appear to support Illig. Raftery points out:

“In considering the home in Iron Age Ireland, the most glaring discrepancy is the absence of domestic pottery. This is quite inexplicable. It may be that pottery will one day be found, when [and if ever] the normal settlements are discovered… All the indications are that for some unknown reason the custom of pottery making was abandoned in Ireland at the end of the Bronze Age and was not revived until late in the first Christian millennium [which covers Illig’s phantom centuries].”

However, the supposed Neolithic Irish left pottery in the earth, and they too lived in wooden houses, with perhaps thatched roofs. They nevertheless had churches and monasteries in which literate monks kept records, and most significantly they kept records of lunar and solar eclipses. Therefore, if these phantom centuries never existed, it would be scientifically impossible that they only observed and noted these eclipses before Illig’s missing period, from A.D. 614 to 911, and further that they dated them to a precise year, month and day of the week, but also precisely to the very hour these occurred. We submit that no one before A.D. 1500 had the requisite astronomical knowledge or skill to so precisely retrocalculate eclipses that are connected to one another and to modern retro-calculations for this long interval, and we challenge Illig and any supporters of his hypothesis to prove it could be done and therefore was done! This then, brings us to the Annals of Ulster compiled from monastery-records in the late 1400s by Cathal McGuire. Celtic scholar Peter Berresford Ellis describes them thus:

“Now it so happens that the Annals of Ulster which treat of Ireland and Irish history from about the year 444, but of which the written copy dates only from the fifteenth century, contains from the year 496-884, as many as 18 records of eclipses and comets which agree exactly even to the day and hour with the calculations of modern astronomers. How impossible it is to keep such [precise]
records unless written memoranda are made by eye-witnesses is shown by the fact that Bede...in recording the great eclipse which took place only eleven years before his own birth is yet two days astray in his date, while on the other hand not only the correct day [May 1st] but the correct hour—thus showing that Cathal Maguire, their compiler [of three sets of Irish annals in the 15th century], had either access to the original or to a copy of the original account of an eye witness.”

Actually there were five additional eclipses beyond the 18 discussed above, but these came after A.D. 884, as we are informed by Robert Bartlett:

“Any brief examination...of the annals and chronicles composed in the medieval period will show how deeply interested their composers and compilers were in astronomical phenomena. In fact, the early Irish annals have been singled out for their sustained series of observations, with the Annals of Ulster receiving the accolade as ‘the richest source of observations and the most accurate chronologically.’ Amongst the entries in the Annals of Ulster are records of twelve lunar eclipses and eleven solar eclipses...”

If these eclipses merely were correct to the day, or were off by several hours either way, Illig and his supporters could rightly claim these variations do not reflect observed eclipses and that these annals are not to be trusted. But that is not the case: they happen on the exact hour of the day that was recorded. Such precision means that they were observed and the monks or other observers noted the hour of their occurrence. For example, in the Annals of Ulster edited by S. MacAirt and G. MacNiocaill (Dublin 1983), page 34, we read about an eclipse correctly dated by Bede which occurred “‘Te[ne]brae in kalendis mali in nona hora,’ i.e., 1 May, 3:00 p.m., where the difference in hour from Ireland through England of the eclipse represents the time taken for the totality of the [solar] eclipse to pass across Ireland and thence to Northern England.” We are told that the Annals of Ulster not only gave the hour of the eclipse in Ireland, but it astronomically correlates with the observation made by Bede to the time of day for the eclipse to pass from Ireland and reach England. Such precision does not arise out of mere chance or happenstance, nor could this have been retrocalculated in the 15th century.

Moreover, there were also two eclipses found in the Annals—one of the Sun and one of the Moon in the same month, and both are correct to the hour in the Annals which read as follows, as cited by B. MacCarthy:

28 Peter Berresford Ellis, A Brief History of the Druids (NY 2002), p. 104
“The entry of 878 may be quoted in full. ‘An eclipse of the moon on Oct. 15th, [4:30 a.m.] on Wednesday and an eclipse of the sun on Oct. 29, the 28th of the moon, about the seventh hour of the day [1 p.m.] Wednesday; fifteen solar days intervening.’ A statement of such minute and accurate detail proceeded from no other than an eye-witness.”

In this instance, we have two eclipses occurring in one month precise to the day and hour of each event, as well as the intervening span of time in days and hours. This could hardly be a coincidence. However, there is one aspect of the annals that must be noted, namely that they are all antedated by one full year. Nevertheless, by moving the eclipses forward in time by precisely one year they are correct in all other respects.

In terms of the historical accuracy of the *Annals of Ulster*, Elizabeth Mary O’Shea reports:

“In the few cases also where early foreign or English writers notice Irish affairs, they are always in agreement with the Irish annals. A remarkable instance is Egenhard’s record of the defeat of the Danes in 812. Testimony of this kind might be multiplied almost indefinitely. The names of fifteen abbots of Bangor who died before 691 are given in the Irish annals with the respective years of their deaths. In the ancient service-book known as the Antiphonary of Bangor, which is still preserved on the continent, there is a hymn in which ‘these fifteen abbots are recited in the same order as in the Annals; and this undesigned coincidence is the more interesting because the [two] testimonies are perfectly independent.’

Moreover, the Irish annals are so accurate that even the tides as retrocalculated to a specific day and the times of that day are correct. There was a battle between two provincial Irish Kings and not with the Danes that show this precision.

“A well-known entry in the Irish account of the Battle of Clontarf, fought A.D. 1014, comes under the test of natural phenomena. The author of…the ‘War of the Gaels with the Gauls,’ written early in the eleventh century, soon after the battle, states, in his detailed account that it was fought on Good Friday, the 23rd April; that the battle commenced in the morning at sunrise when the tide was full in; and that it continued the whole day till the tide was again at flood in the evening when

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31 see T.M. Charles-Edwards, *The Chronicle of Ireland*, vol. I (Liverpool 2005), pp. 35-36 where the correction is explained
the foreigners were routed: ‘They…continued in battle array, fighting…from sunrise till evening. This is the same length of…time as that which the tide takes to go and to fall and to…flood again…’ ‘So the Irish record’.\textsuperscript{33}

The Moon’s motion around the Earth on April 23, 1014 was such that it raised the tide around Ireland to high tide on Good Friday morning and allowed it to fall to its lowest level, and return to full tide that evening of the battle, just as described in the \textit{Annals}. All this accuracy is hardly fortuitous.

The precision of the astronomical and historical data in the \textit{Annals of Ulster} speaks volumes about their accuracy. D. McCarthy and A. Breen have written extensively about them and do so in the prestigious \textit{Vistas in Astronomy}:

“\textit{The astronomical entries scattered through the Irish Annals have been examined in a serious astronomical context by R.R. Newton as part of his research into the acceleration of the earth and moon, and by D. Schove and A. Fletcher, as part of the Spectrum of Time project...What emerges is a body of records from 442 to 1153 [covering the entire period Illig claims does not exist] documenting eclipses, comets, aurorae, volcanic dust clouds, and possibly a supernova; from 664 to 1133 all the records are observations made in or near Ireland and most of them are accurate in their chronological and descriptive details. Analysis of the details of these records implies that at least from the seventh to the eleventh centuries, careful and sustained observation and recording of astronomical phenomena were conducted in some Irish monasteries...}”\textsuperscript{34}

Robert R. Newton has also examined these records and found them to be so accurate as astronomical evidence that he has given them the highest possible rating of “5” regarding the precision of eclipse data.\textsuperscript{35}

The question of course is: how could Cathal Maguire, who copied the various Irish chronicles when compiling the \textit{Annals of Ulster}, have somehow fraudulently computed these eclipses to the precise hour they occurred? There are 23 of them that cover the period just before Illig’s phantom centuries and right through them.


\textsuperscript{35} Robert R. Newton, \textit{Medieval Chronicles and the Rotation of the Earth} (Baltimore MD 1972), pp. 181-199
Where did Maguire learn how to retrocalculate precise eclipses, some of which were over a thousand years earlier than the time in which he wrote? This is the challenge that must be met by Illig or his supporters, to present precise scientific evidence to refute this data, not suggestions or allusions to other places, but to Ireland and these annals. As we said above, it only requires one region where these supposed non-existent centuries are scientifically and stratigraphically shown to exist to discredit Illig’s these. One cannot have the centuries between A.D. 614 and 911 exist in Ireland as a fact, but nowhere else on Earth.

The implications of this evidence are clear. Because these centuries do exist, the Justinian plague that broke out in A.D. 541 did not last only about 75 years, as Illig’s thesis requires, but lasted for about two centuries, as did the other pandemics in Europe and elsewhere take to run their course. The writers who wrote about this outbreak after 614 were not clairvoyant and could see into the future that such pandemics took centuries before they subsided.

At this point we will tie in the *Annals of Ulster* with the plague that struck England and Ireland in A.D. 664 and 665. *The Dublin University Magazine* reports:

> “The following historic fact will prove that the Irish literati…recorded…with scrupulous accuracy the time of remarkable eclipses. We have elsewhere stated that the Ulster annals inform us, that in the years 664 and 665 the people of this island were afflicted with a most dreadful pestilence. They further state that there had been an eclipse of the sun on the ninth hour of the calends of May, in the [same] year 664. In the course of the summer the whole heavens appeared on fire; and in August an awful mortality swept off multitudes of the people, so much that two thirds of the inhabitants perished during the pestilence. Now the…Anglo-Saxon historian Bede corroborates every part of this statement except the time of the eclipse [but] he stated that the pestilence which followed the eclipse depopulated the southern parts of Britain and the province of Northumbria…”\(^{36}\)

The annals are correct not only to eclipses but the very time of year when the plague struck their country—50 years after the 614 date of Illig’s “phantom centuries,” or one-sixth of the time Illig claims does not exist. This, of course, correlates with and corroborates the time it took—about 200 years—for the plague to burn itself out. As for Bede’s error in retrocalculating the total solar eclipse of

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664, Faith Wallis reports “Bede dates the eclipse to 3 May ... the Moon on 3 May was two days older in the [Dionysian] tables than it was in the heavens…”\(^{37}\)

In this respect, for dating that eclipse Bede was using an inaccurate table which was then in vogue. The Irish chroniclers used no such table but only reported what was observed.

Because these three centuries exist, Illig’s assumption that the date of the equinoxes in 45 B.C. falls on March 21 is “seriously erroneous.” The calendar reform of Pope Gregory in 1582 which added ten days to the calendar therefore also conforms to the evidence of the *Annals of Ulster*. Lastly the eclipse date in these annals dated to the exact hour of the day on which they fell is a stunning repudiation of Illig’s thesis.

Since we have shown that the three hundred years do exist between the inception of the Julian Calendar in 45 B.C. and the reform of Gregory XIII in A.D. 1582, we must account for the fact that Gregory XIII added ten days to the calendar rather than thirteen. This was explained to me by Lynn Rose (telephone conversation September 27, 2011): Gregory did not retrocalculate back to Caesar’s 45 B.C. reform. According to Rose, Gregory admitted he only retrocalculated back to the Council of Nicaea held in A.D. 325, not to Caesar’s time. In A.D. 325, the vernal equinox fell on March 21 and Gregory wanted the vernal date to also fall on A.D. March 21, 1582. When we subtract 325 from 1582, we obtain 1,257 years between the Council of Nicaea and Gregory XIII’s reform. By dividing this by 128 years, equal to one day change in the Julian Calendar, we obtain 9.8 days lost to that calendar. Thus Gregory could only add 10 days to his reform. The additional three days exist between the establishment of the Julian Calendar in 45 B.C. and the Council of Nicaea in A.D. 325. When we add these two together we get 370 years. Again, by dividing this 370 years by 128 we obtain 2.97 days lost or almost three days. In essence the 13 days are fully accounted for in our chronology. This means the vernal equinox in Caesar’s time was about three days too early in 45 B.C., so by A.D. 325 it fell three days later, on March 21, during the Council of Nicaea. All this will be explained by Lynn Rose in his Appendix.

For now we can say with complete assurance that Illig’s thesis has failed on all grounds. His use of the *horologium* of Augustus was in error. The *Annals of Ulster* prove his “phantom centuries” do exist. The extrapolation/retrocalculation by

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Gregory XIII explains why only ten days were added to the calendar in 1582. We also have the stratigraphy of the people from these centuries, their houses, fields, burials, and artifacts. Thus, we have answered Illig in full.

In April 2011, I called Clark Whelton, a supporter of Illig’s thesis, to alert him to the evidence from these annals and to show him that the missing stratigraphy and people of these centuries are the stratigraphy of the Neolithic people misplaced in the established chronology. I also called Frank Wallace in Canada to do the same. I wanted this evidence to reach those in Illig’s camp to give them time to be ready to answer this evidence. Whelton’s remark was that if these eclipse documents existed, where were the monasteries from which they were observed? This question, at first glance, would seem reasonable, but it must apply to Ireland’s monasteries which is the region where we set out to prove these 300 years existed, based on the evidence given above. As we said, if these 300 years exist in one region, they must exist in every other region on earth.

Buildings constructed of wood do not last for over 1000 years in the damp countries of Europe and especially ones with lots of rainfall. Wooden beams in the earth rot, those above ground either dry out and over time crack, and the wood is eaten by termites. When one travels along a highway and sees an old deserted barn by the roadside, the deterioration of that wooden building is rather obvious. The wood turns gray, the walls lean outward or inward, the roof collapses, and in time the entire structure collapses. Earthworms complete the job of burial of the wood, just as it did with Roman villas, and the wood rots in the damp soil or is eaten away by termites. To maintain say a wooden monastery for over a millennium requires constant care and replacement of damaged timbers. Furthermore, wooden structures readily burn, and in a country that uses fire for light at night and to cook meals, such buildings could easily be, and were, burned to the ground. Lastly, when people began to build with stone, they would replace structures of wood, especially churches, monasteries and forts, and these reconstructions would have occurred only after these medieval people had the wealth and proficiency to build with stone. And even such stone constructions had interiors of wood, so that stone castles, churches and monasteries would catch fire and cause much of the building to collapse. Lastly, many such destroyed stone structures would have their stones removed to be used at other building sites.

On this point Ward-Perkins writes:
Yeavering, the great royal estate centre of the sixth- and seventh-century Northumbrian kings, may well have been used for over a century by more than 100 people, including men and women from the very highest ranks of society. But its buildings were constructed entirely of perishable materials, which left no trace in the topsoil; and its pottery was not only very scarce, but also extremely friable and hence liable to fall into dust under the plough...Even a very thorough archaeological field survey could have walked right over Yeavering without noticing any trace of settlement. The site was...discovered...because the post holes of its timber [became] visible from the air.”

He further states “Precisely because post-Roman buildings were made of perishable materials, we know very little for certain about what they were really like.” All this answers both Whelton and Illig who asked above: “Documents are not usually written in the open countryside but in solid houses, and therefore numerous [buildings] have to be shown to have existed.”

The question is: did Europe, and particularly Ireland—the crux of our studies—build with wood during Illig’s phantom centuries or with stone? Newman in this respect tells us:

“Many wooden castles were later rebuilt and replaced with stone works but many others were maintained as wooden structures for their entire useful lives and remained in service for generations, sometimes even for several centuries, but were later abandoned and disappeared, leaving only post holes and earth works behind. . . .

“...wooden elements [in castles] were vital [but] these features virtually all... have long since vanished.”

For example in Britain, we are told, “Vikings first raided in 795 and the early monastery constructed of wood, mud, wattle and thatch was razed to the ground. The monks rebuilt the monastery but the Vikings came again in 798, 802 and 806 when as well as burning the place, they murdered 68 monks.” With regard to Ireland, Charles Thomas shows: “There is...evidence from early Ireland of...a series of larger wooden churches. None survives, structurally. Recovery would in any case be hampered by the suspicion, or fact, that most of them probably vanished.”

38 Ward-Perkins, op.cit., p. 142
39 Ibid., p. 110
40 Newman, op.cit., p. 73
41 AA Walks Through Britain’s History (NY 2001), p. 1473
underlie later stone churches [built] on the same sites.\textsuperscript{42} This was common throughout Europe. According to John Blair and Nigel Ramsay:

“Æthelric (Egelric, bishop of Durham 1042–56) demolished the [British] wooden church in which St. Cuthbert’s remains had rested in the 10\textsuperscript{th} century, and built a new one of stone. In 1187 the archbishop’s church at Hackington (Kent) was similarly rebuilt in stone, and when the Dean of Salisbury visited the chapel at Earley (Berk.) in 1224 he found a collection of stones ‘as though for building a stone chapel,’ the existing building being a wooden one.

“Not only were there more individual stone buildings…after the Norman Conquest [in 1066], but many of them were considerably larger.”\textsuperscript{43}

They also show that at this period Roman stone construction remains were incorporated into English churches\textsuperscript{44} and that up until the Norman Conquest there were few if any early stone structures. “One of the effects of the Norman conquest was to stimulate new building….The Conqueror faced apparently an underdeveloped stone supply in England.”\textsuperscript{45} It was only after 1066 that good rock quarries were exploited.\textsuperscript{46} In Poland, for example their castles were rebuilt in stone long after Illig’s phantom centuries according to J.E. Kaufmann, H.W. Kaufmann and Robert M. Jurga: “By the 13\textsuperscript{th} and 14\textsuperscript{th} centuries stone fortifications replaced the gród [a fort] in many areas…In the 14\textsuperscript{th} century Casimir the Great converted most of the Polish grody and wooden castles into stone.”\textsuperscript{47} That is, the outer parts of Europe like Poland and Ireland only produced iron tools to quarry stone late in the first millennium of Christian times.

It was only with the advent of trade that produced wealth and the capacity to quarry stone with iron tools that stone castles, churches, and monasteries could be built. Let us recall that in the earlier part of this book we pointed out that Harold of Hastings when buried along with his thanes did not have iron weapons but stone axes! In such a technologically impoverished environment there were little or no iron tools to quarry rock. When the elite of a society have to wield stone axes, the

\textsuperscript{42} Charles Thomas, \textit{Christianity in Britain to AD 500} (Berkeley CA 1981), p. 152
\textsuperscript{43} John Blair and Nigel Ramsay, \textit{English Medieval Industries: Craftsmen, Techniques, Products} (London 2001), p. 20
\textsuperscript{44} \textit{Ibid.}, p. 17
\textsuperscript{45} \textit{Ibid.}
\textsuperscript{46} \textit{Ibid.}
\textsuperscript{47} J.E. Kaufmann, H.W. Kaufmann and Robert M. Jurga, \textit{The Medieval Fortress: Castles, Forts and Walled Cities of the Middle Ages} (Cambridge 2004), p. 126
rest of that society will have almost no iron tools with which to erect castles, churches and monasteries. It was only following the Norman Conquest that trade and sufficient wealth as well as iron production allowed people to build in stone. In terms of Ireland we were told, above, of the “wooden churches. None survives.” Whelton clearly was unfamiliar with this evidence when he raised the question as to where the monasteries were from which the monks or others made astronomical observations of eclipses. To argue that because we discussed castles and churches this does not apply to monasteries is illogical. Why would the Irish build only their monasteries of stone but not their churches and castles? Castles were of course, indispensable for protecting people, and if man could build with stone, these would surely be one structure where stone was used. As E. Estyn Evans and Brian de Breffny state: “ancient Irish monasteries were normally made of wood and wattles…” These were later replaced with stone and many of them had caught fire, had been burned or been rebuilt at later times. It was a slow change as Alfred Dwight Foster Hamlin explains: “The early churches were of wood and the substitution of stone for wood proceeded slowly.”

If Whelton or any other of Illig’s supporters know otherwise, let them explain why in 1066 the richest people of Britain, Harold of Hastings’s nobles, could not obtain iron weapons but somehow others of far lower station—workmen—could obtain iron tools to quarry great numbers of stone blocks with which to build castles, churches, and monasteries. In Ireland, we were told, “monasteries were normally built of wood and wattle.” And the rebuilding with stone of further churches of wood “proceeded slowly.” The people in Illig’s phantom centuries lived in wooden and thatched-roofed houses, worshipped in wooden churches, and protected themselves by building hillforts of earthworks, of wooden and fieldstone stockades, and/or defensive walls.

**DARK EARTH**

Gunnar Heinsohn has recently claimed that in addition to the 297-year phantom centuries posited by Heribert Illig, there is an additional phantom period just prior to that of Illig’s empty age, of 200- to 300-years that are also missing in the strata. One of his proofs of this additional empty age is the layer of “Dark Earth” found in various European Roman and post-Roman cities, towns, and other sites, which may be the

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48 E. Estyn Evans and Brian de Breffny, *The Irish World* (NY 1986), p. 82
material left by a catastrophe. Since we disagree with Heinsohn on this issue, it behooves us to explain this dark earth. Peter Wells argues for occupation of towns for

“…the formation of dark earth…attest[s] to changes in the character of settlement structures…These physical changes are the material manifestations of transformation in the style of living of the residents of the place—from Roman architecture of stone and cement…to buildings of native British character [built of wood, wattle-and-daub]…

“This change from stone architecture of the early Roman period [was] to wattle-and-daub…Many early medieval kings lived in palaces using these architectural techniques…”  

With this analysis we strongly disagree! Paul Goldberg and Richard Macphail, on the other hand, write about walls made of bricks of earth that were used in Roman times and then turn to dark earth with which we strongly agree:

“European dark earth. As urbanization declined in what had been the Roman and Classical world, a number of processes and events were recorded in deposits called dark earth. The interpretation of these dark earth deposits has not been straightforward, however…It has also been argued that dark earth did not develop because of the total abandonment of towns and cities, but rather it resulted from a change in the use of urban space…

“Abandoned buildings constructed of brick earth floors and plaster-coated brick earth walls…began to collapse as biological agencies destroyed supporting wood beams, or structures were robbed [of wood to build or use for fire]. In fact, a wide variety of mechanisms have been identified that could lead to dark earth forming out of construction materials and occupation deposits…

“Dark earth developed as a soil and can reflect the original construction materials of an abandoned building forming in the first decades a very thin humic calcareous soil…The breakdown of earth-based materials such as daub and earthbrick yielded mainly clay and silt. The weathering…of lime-based mortar and plasters is exactly the same as soil formation on limestone…This occurs when calcium carbonate…is removed in solution by rain water containing more or less dissolved CO$_2$—carbonic acid (HCO$_3$)…Weathering in the form of decarbonation occurs and dark earth is thus generally decarbonated. As evidence of this, dark earth that has taken 400-600 years to form, often contains biogenic calcite (e.g. earthworm granules) from biological activity.

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50 Peter Wells, *Barbarians to Angels: The Dark Ages Reconsidered* (NY 2009), pp. 118-119
“The reworked remains of insubstantial buildings are believed to be one of the mechanisms contributing to dark earth and dark earth-like deposits…”\(^{51}\)

All these processes, we maintain, occurred after these towns were abandoned. Goldberg and Macphail also show that dark earth can form in more rural settings.\(^{52}\)

Not only has dark earth formed in early medieval abandoned towns, but this same material has actually been found building up at abandoned sites in post-World-War II Europe, as Goldberg and Macphail show: “Some of these processes have been modeled from World War II blitzed areas of London and Europe.”\(^{53}\) It is, therefore, a natural process and well explains what would occur in post-Roman Europe after cities, towns, and villas were abandoned. Vance T. Holliday nicely summarizes this evidence:

“Another type of anthropogenic soil is the ‘Dark Earth,’ common in cities throughout much of Europe…‘Dark Earth’ is a term applied to dark-colored, seemingly homogeneous urban deposits. In many ways they can be considered anthropogenic sediments rather than soil, but they have undergone surface weathering and are typically considered a soil…and are so considered here. According to Courty et al. (1989, p. 261), ‘they are common to most cities with long histories, especially those in Europe of Medieval and earlier ancestry.’…In Britain these soils are linked to late- or post-Roman, Saxon, Viking, Medieval, and perhaps post-Medieval occupation…General characteristics of Dark Earths include ‘an exceedingly uniform color’ of…a dark grayish brown…dry to very dark gray…moist, mildly alkaline [with] some phosphate, and abundant midden [animal/human waste] debris…

“Macphail…distinguishes two types of Dark Earth: organic-rich, sometimes waterlogged soils containing ‘cess [human waste] material probably representing refuse continuously dumped in a densely occupied urban environment; and a well-drained soil probably deliberately dumped for within-wall cultivation…Moreover, the late-Roman to Medieval periods are poorly represented archaeologically in many cities in Britain, and the Dark Earth soils provide the best clues to human activity during that interval…The Dark Earths can contain exceptionally well-


\(^{52}\) *Ibid.*, p. 272 ff

preserved archaeological materials, but environmental analyses [of these materials] have been ‘exceptionally unrewarding’…”

What is most important in this respect is that dark earth lies well beneath another type of anthropogenic soil, namely “plaggen and related soils.” According to Holliday:

“They vary in color from dark gray or black to brown, probably because of different vegetation mixed with the manure from area to area (e.g. heather sod, grass sod, forest litter, or peat litter). Any one plaggen, however, will be homogeneous in color (the uniform color is believed related to a desire for uniform fertility). The plaggen [soil] horizon typically is sandy, but ultimately its texture is determined by the texture of the subsoil that produced the bedding. A buried soil typically is preserved below the plaggen, and the plaggen itself will be readily distinguishable by being significantly thicker (typically >50 cm [20 inches]) than the surface horizon typically <30 cm) of non-anthropogenic soils in the same area. Chemically they will be relatively high in phosphate and have low pH. Artifacts such as charcoal, brick and pottery fragments, and burned soil are common in plaggen. Recognition of plaggen soils and their geomorphic setting is a useful tool in local archaeological surveys…

“BY ALL ACCOUNTS, PLAGGEN SOILS DEVELOPED IN THE MIDDLE AGES (PROBABLY AROUND THE 10TH CENTURY…”

That is, the “dark earth” was laid down from late Roman into early and through early medieval times, but the more modern plaggen soils were laid down after the 900s A.D. which contain brick and pottery identifiable to post-900 A.D. times. Therefore, we have a soil chronology that indicates quite clearly that with the abandonment throughout Europe of Roman urban centers, natural processes went to work on the materials left behind to create dark earth. Then, in the 10th century, when the Europeans had cleared the land for cultivation, they used the land somewhat differently to produce plaggen soils. “The traditional interpretation of plaggen origins is that they resulted from a need to restore or increase soil fertility as populations expanded in northern Europe…Heidinga (1988) notes, however, that the first appearance of plaggens is coincident with a time of significant drought across

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55 Ibid., p. 315, capitalization added
northern Europe. He argues that the development of the plaggen increased water-holding capacity and also deepened the plow zone, thus minimizing crop failure.”

As we also know, Megalithic Age soils lie at the same level and slightly above Roman town and villa soils. At the top level of ancient Roman urban centers and villas, we have “dark earth,” at about that same level and higher we have Megalithic Age soils, often containing Roman pottery, and from around A.D. 900, after the forest wilderness was removed and the climate became warmer; as populations grew, as we will show below, plaggen soils accumulated.

Both these developments occurred in Europe after A.D. 900; the population rose, and the clearance of the forests may have altered the climate. Furthermore, it is well known that Europe warmed up from around A.D. 800/900 to 1200/1300 but to dryer conditions, as Thomas Gale Moore outlines under the caption “The High Middle Ages and Medieval Warmth”:

“From around A.D. 800 to 1200 or 1300, the world warmed considerably and civilization prospered. The period, called the Little Climate Optimum, generally displays, although less distinctly, many of the same characteristics as the first climate optimum [Hypsithermal] (Lamb 1968, 64). Virtually all of northern Europe, Britain, Ireland,…were considerably warmer than at present. …

“Evidence for the medieval warming comes from contemporaneous reports on weather conditions…from upper [altitude] tree lines in Europe, and from sea level changes. They all point to a more benign, warmer climate with more rainfall; but because of [this greater heat there was] more evaporation, less standing [bodies] of water [such as lakes, ponds, etc. ] … In England…, medieval water mills on streams that today carry too little water to turn them attest to greater rainfall. Although England apparently received more rainfall than in modern times, the warm weather led to more drying out of the land. Support for a more temperate climate in central Europe comes from the period in which German colonists founded villages. As average temperatures rose, people established towns at higher elevations. Early settlements were under 650 feet in altitude, those from a later period were between 1,000 and 1,300 feet high; those built after 1100 A.D. were located above 1,300 feet…”

56 Ibid., p. 315-316
57 Thomas Gale Moore, Climate of Fear: Why We Shouldn’t Worry About Global Warming (Washington DC 1998), pp. 46-47
For a fuller description of the Medieval Climate Optimum with its concomitant population rise, we suggest Wolfgang Behringer’s *A Cultural History of Climate* (Cambridge UK 2007), pages 74 to 83. As for population growth, Behringer writes on pages 80-81:

“Europe’s population grew by leaps and bounds, reaching dimensions it had never seen before…At this time Europe developed its characteristic landscape with high settlement density in which remnants of forest and islands lay amid huge areas of farmland…

“Some 46 million people lived there in 1050, 50 million a century later, 61 million in 1200, and no fewer than 73 million at the end of the expansion around 1300…Although the roots of European urban life lie in antiquity, the present density of distribution goes back to the age of the high medieval climate optimum.”

Not only do the types of soil plaggens and dark earth tell the same chronological history outlined in this book, but so does the climate chronology corroborate and correlate with that history. We patiently await Professor Heinsohn’s response to this evidence.

**ASTRONOMY, CALENDARS, LAURENCE DIXON, AND MANY IN THE SIS**

This evidence also applies to Laurence Dixon and many members of the *Society for Interdisciplinary Studies* in England and elsewhere. According to Dixon,

“…if one believes, as many in our Society do, that regular catastrophes, including polar tilts, have occurred since that date [700 B.C.], then the whole idea of retro-calculation becomes invalid. Four favoured later catastrophes are dated to AD 540 (Keys), AD 1350 (Baillie), AD 1577 (Clark Whelton) and AD 1680 (myself). … Members of the Society (notably Steve Mitchell) are investigating whether the period before AD 1000 may contain a gap. If this turns out to be true, retrocalculation of astronomical dating would lead to incorrect results.”

The crux of Dixon’s statement is “if one believes as many in our society do, that regular catastrophes, including polar tilts, have occurred…, then the whole idea of

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retrocalculation becomes invalid.” What Dixon has put before us is “belief” (“if one believes”), not proof, certainly not scientific proof. Therefore, the Annals of Ulster’s 18 eclipses must somehow be explained away by those “believers.” How is it possible that just by coincidence, in Ireland, we have almost a score of lunar and solar eclipses that, when retrocalculated over a period of about 440 years, are precisely correct to the hour? If, as Dixon and these believers suggest, there were catastrophes during this period up until A.D. 1600, these eclipses would not and could not, by any stretch of the imagination correlate with modern astronomical retrocalculation. That would be equivalent to Cathal Maguire tossing the astronomical dice 23 times and coming up with seven (solar eclipses) and eleven (lunar eclipses), although these 23—twelve and eleven—are not the precise number of solar and lunar eclipses in the Annals applicable. If all these pole tilts occurred, it would take a miracle for Maguire, by chance alone, to just happen to get so many eclipses accurate to the year, month, day of the week, and hour of the day by retrocalculation.

On top of this eclipse evidence running from the 400s A.D. through 1000, we have evidence of two eclipses, one solar dated to A.D. June 16, 364, and one lunar dated to A.D. November 25, 364. These were both described by Theon of Alexandria. We described the solar eclipse, citing John M. Steele, in Pillars of the Past, vol. III, p. 208. According to F. Richard Stephenson, “the timed [solar] eclipse of A.D. 364—is the most reliable.”59 Neil Schlager tells us “Theon’s life can be dated by a number of occurrences, including the solar eclipse he witnessed on June 16, 364 as well as a lunar eclipse on November 25 of that year.”60 Although this lunar eclipse was not precisely timed by Theon, it was, nevertheless, observed in India. Robert Sewall tells us: “Eclipse of November 25, A.D. 364. This began at Lanka [India] 23.8 or 5 h. 8 m. AM, mean time on 26 h. (civil reckoning) + 10 m. for apparent time…5 h. 18 m. AM. It was therefore visible at Lanka for 42 minutes before sunrise. But at Rangoon (long. 96° 17’ diff. [or] + 1 h. 22 m.) … the eclipse was invisible.”61

Here, once again, we have another two eclipses of the Sun and the Moon accurately dated before the period Dixon claims such eclipses are incorrect. Rose has told this author that there are simply far too many such eclipses going back into the distant past to permit any large changes in the Earth’s orbit or polar tilt in the Middle Ages or later. This large number of reliable eclipse events precludes

59 F. Richard Stephenson, Historical Eclipses and Earth’s Rotation (Cambridge UK 2008), p. 365
60 Neil Schlager, 2000 B.C. to A.D. 699 (Detroit MI 2001), p. 222
61 Robert Sewall, Eclipses of the Moon in India (London 1896), p. 6
Dixon’s suggestion that the Earth’s orbit or axial tilt changed in any substantial way. The *Annals of Ulster* completely contradict Dixon’s claim.

How do Dixon, and many of the others in the SIS who accept the thesis presented, *prove* that such a miracle occurred in Ireland? We suspect that, rather than facing this scientific negation of their beliefs, they will change the subject to deal with other times and places, giving non-scientific explanations to get around this dilemma (i.e. contradiction to their beliefs) or ignore it altogether. We maintain they have nothing resembling scientific proof that discredits this evidence!

I have been quite severe and provocative in my critical remarks about Dixon and many others at the Society for Interdisciplinary Studies because this is not the first time I have answered his work which was published in its *Workshop* in May 2009. Five months later, in November, I responded him in the next *Workshop* where I laid out a very similar form of evidence, which I must reiterate here, to try to provoke a response, since Dixon has failed, to this date, to answer that critique, which goes as follows:

“The problem that Dixon and all other critics of [Lynn E.] Rose’s retro-calculations [for dating the 12th Dynasty 1477 years closer to the present] face, is the large number of data points that correlate and mesh with the short chronology. For example, in terms of Sothic dating...Dixon’s suggestion that intervening catastrophes between ancient times and the present day would make these data point ‘fits’ invalid, fails to address the enormity of the number of astronomical fits. If these intervening catastrophes occurred and changed the Earth’s orbit and polar tilt, as Dixon and others suggest, very little, if any, astronomical data would correlate so precisely with the short chronology. Specifically in terms of Rose’s Sothic dating, it would be astronomically, mathematically and historically impossible to fit 34 lunar festival dates out of 36, with 2 being off by one day (due to bad seeing or scribal misinterpretation) with the helical [sic] rising of Sothis/Sirius. It would further be astronomically, mathematically and historically impossible for these lunar festival dates to follow the New Crescent Moon by fixed intervals of days: the dates being 0, 2, 3, 8, 16 or 19 days after the New Crescent. None of these many lunar festival dates would fit so precisely within, and integrate with, the Short Chronology. I must stress that, with Sothic dating all data fit—not [just] some of it [sic]...

“What Dixon and all critics of Rose are trying to explain away by proposing intervening catastrophes, is that such a large number of data points could simply never be retro-calculated into ancient times because of these catastrophes. However, consider the possibility of throwing a pair of dice 36 times in a row, but
having them come up with 7, followed by 11, followed by 2 and repeated 12 times in a row. In effect, this is what Dixon and all those who would deny Rose’s evidence would be forced to propose in order to explain away his Sothic dating—namely, that a miraculous set of circumstances occurred, which just happened to fit Rose’s astronomical evidence. The reason that this evidence fits Rose’s data [as well as the eclipse data in the Annals of Ulster] is that his data is [sic] real and correct and not an impossible coincidence. Intervening catastrophes did not affect the Earth’s orbit.

“As Rose has repeatedly pointed out, the great bulk of reported eclipses and other astronomical events from late ancient, medieval and even early modern times, are as compatible as can be expected with retro-calculation. Consequently, none of the later ‘catastrophes’, or other alleged situations that Dixon and others advocate have had any significant orbital…effects whatsoever. This one fact destroys a multitude of ill-informed theories.

“Rose’s achievement is [in Dixon’s words] a ‘tour de force’ because it integrates a plethora of astronomical data points with precise dates of numerous Egyptian, Neo-Assyrian/Persian and other kings….I don’t believe that critics of the Short Chronology have grasped the enormity of evidence in its depth and breadth…”

I have raised this very same issue of miraculous astronomical coincidence in volume I of Pillars of the Past, pp. 114-115, as well as in volume III, pp. 192-193 and 572-573, the latter volume where both David Rohl and Peter James et al.’s Sothic dating was shown to be “seriously erroneous.” Having raised this coincidental astronomical miracle, now five times, when, if ever, will those proponents of intervening catastrophes answer this evidence with scientific, astronomical evidence? It appears, since they all have failed to do so, that they have no answer! The inconvenient plethora of data points contained in the Annals of Ulster, the Sothic evidence of Rose and Rose’s astronomical dating evidence of the Neo-Assyrians presented in volume II of Pillars of the Past in chapter 2 and its “Appendix”, pp. 597-640, has not been answered scientifically. It seems clear that this evidence must be ignored in order to hold on to Dixon’s and those others’ intervening catastrophes. Why these numerous and miraculously accurate astronomical data points should agree with modern retro-calculation we are never told; will they attempt to explain this?

In July 2011 Dixon wrote yet another article questioning the validity of Delta T. But he never once deals with this particular astronomical probability evidence published in any of the volumes of Pillars of the Past, where this probability argument was raised, nor even my presentation in Chronology and Catastrophism Workshop of November 2009. He failed to confront this probability contradiction to his thesis respecting Rose’s 36 precise solar and lunar festival dates in the El-Lahun papyri. He seems to know nothing of the 23 solar and lunar eclipses dated to the month, week day, and hour as they occurred in the Annals of Ulster, nor of VAT 4956 for the accurate positions of the Moon and the five planets over a year. He continues to return to the same question of the accuracy of Delta T and ignores now three sets of astronomical data that contradict all he has presented. The Delta T curve cannot be so very wrong because of the statistical probability of large sets of astronomical data can agree so well with retrocalculation to the very day and time of day they occurred, unless miracles of coincidence always come into play to make them correlate with such specificity.

Beyond this, Dixon raised the discredited 25-year cycle of Richard Parker, answered by Rose in “Reply to Chappell” as well as our detailed response to Chappell in Pillars of the Past vol. III (2010), pp. 201-204. It is as if Rose nor we never wrote anything that disputed Parker on this point. But repeatedly our criticisms of Chappell’s presentation of Parker remain unanswered and/or unread or ignored. How does one carry on a serious debate when our evidence is not answered?

Dixon makes a further point that exhibits surprising naiveté, namely “More recent calculations have included [in astronomical retrocalculations] the function known as Delta T in order to imitate the slowing of the Earth’s rotation, and this function is estimated by making certain ‘well-established’ ancient eclipses observable from the ‘correct’ place on Earth’s surface. If the dates of the eclipses are altered, as Rose proposes, then the function Delta T would need to be re-calculated, even from a uniformitarian viewpoint, and this would alter the matches.”

Dixon assumes that when Rose moved Sothic dates and certain solar and lunar eclipses closer to the present, these would change the rate of the slowing of the

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64 VAT 4956, a cuneiform document located in the Near Eastern department of the Pergamon Museum in Berlin
Earth’s rotation because they are in a new time frame. This is entirely false. When Rose moved any of these eclipses closer to the present, he did so because they did not fit into the Delta T curve where they had originally been placed, but they did fit into that same curve closer to the present. That is, the Delta T curve did not change to accommodate the closer to the present placement of these eclipses. They ended up on the same curve only where they fit accurately. Since these eclipses fit on the Delta T curve where Rose placed them, but not on the same curve where they were originally set farther back in time, there is absolutely no reason to change that curve to accommodate the new placement of these eclipses. Dixon is under the misapprehension that the point on the Delta T curve where these eclipses originally were placed when changed to a time closer to the present also carries along with it the Delta T part of the curve as well. It emphatically does no such thing; only the erroneously placed earlier eclipse moves forward in time to a different point on that same Delta T curve; the curve does not change!

Dixon in the “Editorial Note” at the end of the article points out that “Michael Reade went to great lengths and wrote several articles dealing with astronomical matters, but is not referenced by Rose.” Dixon in no way tells us if or how these matters have anything to do with Rose’s work, nor if they criticize Rose in any way. In effect he leaves the impression that Rose’s work may be denied by Reade, but never indicates that this was done. But it does leave the impression that Rose is a poor researcher in spite of Dixon calling his work a “tour de force”. If Dixon had any solid evidence form Reade’s work that invalidated Rose’s retrocalculation, he could have presented it. Since he chose not to present Reade’s evidence that may have something to do with invalidating Rose’s “tour de force”, one wonders what, if anything, Dixon’s reference to Reade has to do with Rose? If he had such evidence, why didn’t he present it instead of leaving that impression?

In the final comment of his paper, Dixon adds: “Possibly Rose’s main achievement is that he has conclusively shown that even under uniformitarian assumptions retrocalculation need not give a unique match.” The point that Dixon has evaded when he made this statement is that “under uniformitarian assumptions, erroneously placed eclipses when properly placed do indeed give a unique match” on the Delta T curve. They do not, as Rose has shown, when they are erroneously placed, give unique matches. By failing to properly deal with Rose’s work regarding these matters, Dixon has shown that his understanding of Rose’s

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67 Ibid.
68 Ibid.
analyses is spurious at best. The question is: Will Dixon and the others in his society answer these criticisms with scientific evidence, or will he and they continue to evade that scholarly responsibility? Failure to answer is a tacit admission that these criticisms can’t be answered and are, therefore, correct.

ANATOLY T. FOMENKO

Anatoly T. Fomenko’s work in part was derived from, and influenced by, the Russian polymath Nikolai Aleksandrovich Morozov (1854-1946) who claimed that the chronology of the ancient world was far too long. He began an intensive investigation of the chronology of the ancient world then in vogue. The outcome of this research was his book *Christ: The History of Human Culture from the Standpoint of the Natural Sciences*. He attempted, as do we, to establish the chronology of the ancient world on the basis of science. He also came to the conclusion that the established chronology was much too long. Also, like Heinsohn and Sweeney, he maintained that history exhibited repetitiveness; the same events and characters occurring at different periods inferring that in reality they were one. These events and characters belonged closer to the present. Fomenko also drew the same conclusion.

Morozov’s work covered the chronology of Greece, Rome, Egypt, and China, arguing that the dates assigned to these empires, based on the data of astronomy—eclipses—were deeply in error, just as do we. Florin Diacu, in his informative book, *The Lost Millennium: History’s Timetables under Siege*, which covers and summarizes Fomenko’s theses and also describes criticisms of it, reports:

“Morozov considered the mistake to have been made in a misapprehension of the Saturn-Jupiter cycle. For time reckoning, the early Chinese had used the sixty-year period during which Jupiter and Saturn simultaneously complete the smallest number of full revolutions around the Sun (Jupiter, five; Saturn, two). The Chinese observations of the planets led Morozov to conclude that the starting point of the first recorded cycle [of Jupiter and Saturn] was not [in] the third millennium BC, as history books taught, but AD 1323.”

One of the problems with Fomenko’s work is that he only employs ancient astronomical materials that corroborate his thesis, but omits other astronomical materials that contradict his thesis. As one case in point, we turn to those ancient astronomical observations of a document know as VAT 4956, located in the Near

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69 Diacu, *op. cit.*, p. 47
Eastern department of the Pergamon Museum in Berlin. Abraham Sachs and Hermann Hunger translated and published its contents in 1988. This covers astronomical events as well as meteorological happenings, prices for goods, and even earthquakes.\(^{70}\) The great significance of this document is that it records the positions of the five planets visible to the naked eye as well as the Moon over a period of about one year, during the 37th regnal year of Nebuchadnezzar II and can be precisely retrocalculated to the year between 568 and the first month, Nisan, of 567 B.C. According to Dale W. Wong, the translation shows:

“Year 37 of Nebukadnezar king of Babylon. Month I [Nisan] (the first of which was identical with) the 30th (of the preceding month) the moon became visible behind the Bull of Heaven [constellation]…Saturn was in front of the Swallow [constellation]…[the 11th] or 12th Jupiter’s acronyca [starting at sunset] ringing…month II…Saturn was in front of the Swallow; Mercury, which had set, was not yet visible…The 3rd [day of the month] Mars entered [the constellation of] Praesepe. The 6th [day of the month II] went out (of it)…The 18th [day of the month II] Venus was balanced 1 cubit four fingers A Leonis [a star in the constellation Leo]…Month III (the last of which was identical with the 30th of the preceding month) the moon became visible behind [the constellation] Cancer…At that time Mars and Mercury were 4 cubits in front of […] Leonis] Mercury passed below Mars to the East, Jupiter was [at that time] in the West opposite [the star] Z Leonis.”\(^{71}\)

In essence, we have the positions of all the naked-eye visible planets and the Moon over about a one year period. The accuracy of these positions could be retrocalculated from the present day to a specific period in the past in that ancient Babylonian calendar. Wong explains:

“The location of the planets and the moon are described relative to the stars and the Babylonian zodiac from the perspective of an observer in Babylon…

“We cannot over-emphasize the importance of this discovery. Modern science can duplicate the [astronomical] conditions and determine the calendar period [used by the Babylonians] with good accuracy and precision. Astronomers have found that the opening period of the astronomical diary of VAT 4956 has the beginning and end of Nebuchadnezzar’s 37th year [from the first month and day] Nisan I for 568 and [Nisan I of] 567 respectively.


\(^{71}\) Dale W. Wong, *The Ancient 360-Day Year: What It Was and How It Changed* (Charleston SC 2006), p. 72
“How likely is this correct? The relative locations of the moon and the planets at an instance in time provides a unique description of the motion and opposition of these celestial [planetary] objects that may not occur for thousands of years. Such is the case with the astronomical observations of the 37th year of Nebuchadnezzar’s reign.”\textsuperscript{72}

Carl Olof Jonsson raises this point which, we suggest, is provocative for Fomenko’s chronology:

“How great is the possibility that all these ‘distorted’ observations would fit into one and the same year… Accidental errors of this kind do not ‘cooperate’ to such a great extent. So there is no sound reason to doubt that the original observations have been correctly preserved in our copy.”\textsuperscript{73}

As with the Annals of Ulster and the El-Lahun papyri of the 12th Dynasty, we have an astronomical document for Nebuchadnezzar’s 37th year with the positions of the five planets and the Moon that correlate and corroborate one another when retrocalculated by modern researchers. If, as Fomenko and Diacu suggest, Nebuchadnezzar must be placed around 900 to 1200 AD, they would have to find this identical planetary arrangement for this period. This neither Fomenko nor Diacu have calculated and we maintain there is no place in this late medieval period where it can be made to fit.

In terms of Diacu, this challenge is especially telling because he had read Jonsson’s book \textit{The Gentile Times Reconsidered}, the 1986 edition, which recites the same material regarding VAT 4956 on page 291. He had to know that this astronomical evidence completely negated Fomenko’s thesis, yet he ignored the challenge it represented. Hopefully he or Fomenko will address this problem by proving via retrocalculation that the planetary positions in VAT 4956 fit precisely anywhere in Fomenko’s revision. Fomenko, Diacu informs us, “adheres to a different set of values from that of historians. To him celestial mechanics and astronomy have priority, and historical arguments contradicting scientific results should be dismissed.”\textsuperscript{74} Interestingly, because Fomenko has moved ancient history

\textsuperscript{72} \textit{Ibid.}
\textsuperscript{73} Carl Olof Jonsson, \textit{The Gentile Times Reconsidered}, 4th ed., revised and expanded (Atlanta GA 2004), p. 163
\textsuperscript{74} Diacu, \textit{op.cit.}, p. 191 (2011 ed.)
to the Middle Ages, he has, somewhat like Gunnar Heinsohn, “unknowingly implied that Alexander the Great didn’t exist.”\footnote{Ibid.}

Instead of dealing with these five planets in VAT 4956, Morozov and, possibly, by implication, Fomenko, has turned to the astronomical evidence of these same five planets in a Chinese document. Diacu, in analyzing this Chinese evidence, has pointed to Morozov’s retrocalculation of these planets; but dated according to the established chronology between 2513 and 2439 B.C.:

“As evidence for this radical reordering of the millennia, Morozov mentioned a Chinese emperor traditionally considered to have lived between 2513 and 2436 BC. The use of the [Jupiter-Saturn] sixty-year cycle had begun during the reign of this ruler, whose astronomers recorded the alignment of all visible planets near the stars $\alpha$ and $\beta$ of the constellation Pegasus. But the grouping of planets in a certain region of the sky is a very rare phenomenon, and no such configuration occurred in these years. The only viable alignment took place on AD February 9, 1315.”\footnote{Ibid., p. 47}

Therefore, this unique arrangement of the planets strongly supports the concept that ancient Chinese civilization had begun or existed in the Middle Ages. The positioning of these planets is described by Harish Kohli thus: “Scholars say that, on this day [March 4] thousands of years ago, five planets (Venus, Mercury, Mars, Saturn and Jupiter) were ‘lined up like a string of pearls’ near the constellation Pegasus at dawn on the first day of [ancient China’s] spring. Soon afterwards the sun and new moon also came into alignment.”\footnote{Harish Kohli, \textit{Across the Frozen Himalaya: The Epic Winter Ski Traverse...from Karakoram to Lipu Lekh} (New Delhi 2000), p. 90} The problem with all these assertions is that this citation in \textit{The Annals of the Bamboo Books} never says that the five stars are the five planets. This is an inference that had been drawn from the texts. We must point out that there is no mention whatsoever of Venus, Mercury, Mars, Jupiter, or Saturn in this citation. Henrietta Lo had raised this very alignment as criticism of Velikovsky in “Velikovsky’s Interpretation of the Evidence Offered by China in his Worlds in Collision”, in the \textit{Skeptical Inquirer}, vol. II (spring 1987). Here, then, is the citation as presented by J. Legge, which in Chinese reads:
The interpreter’s note of the text reads:

“Note. When the emperor had been on the throne 70 years, a brilliant star issued from the constellation Yin [Pegasus], and phoenixes appeared in the courtyards of the palace; the pearl grass grew and the admirable grain flourished, sweet dews moistened the ground, and crystal springs issued from the hills; the sun and moon appeared like a pair of gems and the five planets looked like threaded pearls.”

When I requested confirmation of this interpretation of the Chinese from Lorraine Lai, a postgraduate university associate fluent in Mandarin Chinese, to see if the information in the Chinese text was correct and referred to “the five planets,” she denied this interpretation. After having carefully read it, she said it

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was “pure poetry,” “fanciful imagery,” and “had nothing to do with the planets.” Her translation is as follows:

“When the emperor from Yin had reigned 70 years a brilliant star issued from Yih. In the sky stars formed wings phoenix-like which were reflected in the courtyard of the palace on pearly grasses bedewed with drops of water. The reflections appeared as shining gems below the starry phoenix-like sky wings. The light of all these taken together appeared lovely like five stars strung on a thread.”

When I asked another Chinese student about this, she, too, said it was “poetry and had nothing to do with the planets.” What seems to be evident is that the later interpretation by the copyist changed “five stars strung on a thread” to “the five planets looked like threaded pearls.” What the later scribe who added this note changed was the words “five stars” to “five planets.” What the astronomers, including Morozov, and Diacu, and perhaps Fomenko, have done is assume the generally accepted interpretation of this text. If the word “star” in Chinese also means “planet,” we still have a problem. How does one know for certain that in this case the word means “stars” and not “planets.” This places the entire astronomical interpretation of the text in doubt.

Nevertheless, let us for the moment assume that the “five planet alignment” is the correct interpretation of this citation, are there other places in history where these could have occurred while Morozov, according to Diacu, shows that the arrangement of these planets is “only viable [as an] alignment…on AD February 9, 1315.” This is clearly not the only time this alignment could have occurred in history. According to M.G.L. Baillie,

“There are some very elegant interpretations of solar and planetary configurations which can be fixed in time by ‘back computation’. The first notable example is the identification of a five-planet conjunction in the reign of King Yu, the founder of the Hsia dynasty (the semi-mythical dynasty before the Shang). Apparently Chinese records state that ‘at the time of [King] Yu the five planets were strung together like a string of beads.’ [Kevin D.] Pang and his colleagues were able to find that ‘in late February 1953 BC Mercury, Venus, Mars, Jupiter and Saturn came within only 5 degrees of one another. So close [together] that they could be
Charles Ginethal, Pillars of the Past, vol. IV

blocked out by a thumb held at an arm’s length! Furthermore, on 23 February 1953 BC they appeared to be indeed strung up in a nearly straight line…” (Pang 1987)”

Thus, even accepting Morozov’s, or perhaps Fomenko’s translation of “five planets” instead of “five stars”, their theses respecting this alignment are in a hopeless situation. If interpreted as five planets, the alignment can be dated to February 23, 1953 B.C., and not necessarily to A.D. February 9, 1315 where they have placed it. Interpreted as “five stars” the alignment need not necessarily refer to “five planets.” We, however, do not accept the “five planets” interpretation because the planets are not specifically named and “our” translators made it abundantly clear that “five stars” had nothing to do with the then known “five planets.”

While Morozov is not the case in point, but Fomenko is, it behooves us to analyze his thesis which holds that all of the ancient civilizations—Mesopotamian, Egyptian, Anatolian, Greek, Roman, and even the Indian and Chinese, begin in the Middle Ages. Fomenko draws the distinction between his thesis and that of Morozov: “Our conception…is…different from the version of N.A. Morozov…For example, according to N.A. Morozov, the primary Biblical events took place in the III-V century A.D., or about one thousand years later than the [established chronology] suggests. The results of our methods place these events in the XI-XVI century A.D., which is about a millennium later…”

We must examine the basis upon which Fomenko has established his chronology. He maintains, as we cited him above, that the strongest evidence is that of science and in particular astronomy. Therefore, his first endeavor was to prove that the astronomical evidence used to place these ancient civilizations back to 3500 B.C. instead of A.D. 600-700 was entirely in error. His approach in this regard was to show that the Delta T data for the gradual acceleration of the Earth’s rotation, as one goes back in time, is too deeply flawed to be employed for dating lunar and especially solar eclipses. The problem with Delta T is that

“According to R. Newton ‘the most stunning fact…is the drastic drop in D’ [Delta T] that begins with [A.D.] 700 and continues until about [A.D.] 1300 …This drop implies the existence of a ‘square wave’ in the osculating value of D”…Such changes in the behavior of D”, and such rates of these changes [from

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80 Fomenko, op.cit., p. 334
A.D. 700—1300] cannot be explained by modern geophysical theories’…Robert Newton wrote an entire monograph…that was concerned with trying to prove this mysterious gap in the behaviour of $D^*$, which manifested as a leap by an entire numeric order. One has to note that these mysterious non-gravitational forces failed to manifest [themselves at any other time] in any other way at all.

“Having studied the graph that was drawn as a result of these calculations, R. Newton had to mark that ‘between the years (-700) and (+500), the value of $D$’ remains the lowest as compared to the ones that have been observed for any other moment during the last 1000 years’…

“Newton proceeds to tell us that ‘these estimations combined with modern data tell one that $D^*$ may possess amazingly large values, and that it has been subject to drastic and sudden fluctuations over the last 2000 years, to such an extent that its value became inverted around 800 A.D.’”

The implication Fomenko draws from this large change in Delta T, having no clear explanation in gravitational theory, is that it must be an artifact of a false chronology. By moving all these early civilizations into the Medieval period, the Delta T curve remains generally the same, showing a uniform acceleration into the deep past and therefore any retrocalculation to these earlier ancient periods must be false. That is, by the removal of these 1000s of years and fitting the astronomical data of ancient times into Fomenko’s chronology, there is no anomalous problem with Delta T. However, there may be causes that can be known that affect the rotation of the Earth as it relates to the Moon’s position vis-à-vis Delta T. Diacu points out:

“Among the possible nongravitational factors that change the values of D”, Newton proposed the tidal friction between water and sea bottoms, Earth’s magnetic force, the withdrawal of the ice caps, and the growth of Earth’s core. But he couldn’t tell how realistic these proposals were, since there is no way at present to compute their contribution to the Moon’s acceleration. It is far from clear that their influence on $D^*$ could account for the bend [in the Delta T curve].”

Diacu adds that while Newton accepted the eclipse data, Fomenko did not, and suggests that Fomenko’s approach “appears to be more convincing, because he

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81 Ibid., p. 94
82 Diacu, op.cit., p. 98
resorted to no mysterious forces.”83 This, of course, is based on the traditional established chronology. In terms of the short chronology, we have shown that eclipses and the positions of the planets, Sun and Moon fit far better or accurately when retrocalculated, even employing the presently accepted Delta T. If Fomenko is correct, he would have to place all of Lynn Rose’s Sothic data regarding the position of the star Sirius and the lunar festival dates into his own chronology and the positions of the five visible planets and the Moon as well. Neither he nor Diacu have ever shown this to be the case.

Another example in this respect is the eclipse of April 15 in 136 B.C. It is the most accurate and therefore reliable eclipse ever retrocalculated. David H. Kelley, Eugene F. Milone and Anthony F. Aveni explain why this is so:

“All solar eclipse of 15 April 136 B.C., at Babylon.

“The records of the eclipse of 136 B.C. were evaluated by Stephenson and Clark (1978) as the most accurate before the invention of the telescope. Two separate records were found among the clay tablets of the astronomical diaries of Babylon, and these provided precise locations of the Sun and several planets in the sky during totality. On the basis of planet positions and the date of nearby eclipses. Huber was able to determine the date of the eclipse. In addition, a ‘retrocalculation’ by Mitchell (1989/90, p. 10) of the date of a lunar eclipse that occurred 14 days prior to the solar eclipse is also in agreement with the data recorded on one of the tablets [for the April 15, 136 B.C. solar eclipse].”84

The reading of this eclipse on each tablet is as follows:

“The 29th at 24° after sunrise, solar eclipse, when it began on the south and west side, [...Ven]us, Mercury and the normal stars were visible; Jupiter and Mars, which were in their period of invisibility, were visible in its eclipse [...] it threw off (the shadow) from west and south to north and east 35° onset, maximal phase and clearing. [LBAT *1429, Rev 13’-15’; trans[lation] Sachs & Hunger (1996: 185)

83 Ibid.
“Month XII … The 29th. Solar eclipse, when it began on the south-west side [of the Sun] in 18° of daytime in the morning it became total. At 24° after sunrise [LBAT 1285, Rev 24-28; trans[lation] Hunger (personal communication).”\textsuperscript{85}

That is, the hour of the day when the eclipses began to the time of totality is given and also where the shadow of the Moon began to encroach upon the solar disc from the south-west side, and the positions of the planets. Stephenson also describes this eclipse as “probably the most reliable account of a solar eclipse before AD 1500.”\textsuperscript{86}

Here then is a simple way to test whether Fomenko and Diacu’s supposition that the Delta T curve back into ancient times is valid. In this case we have a total eclipse located at a specific site—Babylon—found on two ancient Babylonian diaries, and the specific time of day. Moreover, we have the presence of four planets observed during totality. Of these, Jupiter and Mars were close enough to the Sun that they could not be seen in daylight, but during totality. Furthermore, we have a lunar eclipse that occurred 14 days prior to this eclipse. Lastly, we have the direction from which the Moon’s shadow encroached upon the solar disk and the time of totality from this encroachment. If, as Fomenko and Diacu suggest, the Delta T curve is erroneous, this solar eclipse could never have taken place connected with so many other forms of astronomical data attendant with it:

1. The precise location—Babylon
2. The precise location of the Sun in the sky
3. The approximate location of Jupiter and Mars and the fact that Venus and Mercury were visible
4. The precise encroachment point on the solar disc where the Moon’s shadow began to eclipse the Sun
5. The time of totality
6. The time of a total lunar eclipse that occurred 14 days earlier

Just as with VAT 4956 which gives the positions of the five naked-eye planets and that of the Moon over a long period, we have succinct and distinct astronomical data for this April 15, 136 B.C. total solar eclipse. In order to determine whether Fomenko’s thesis, supported by Diacu, was accurate, I wrote to

\textsuperscript{85} John M. Steele, \textit{Observation and Prediction of Eclipse Times by Early Astronomers} (Dordrecht, the Netherlands 2000), pp. 54-55

Lynn E. Rose sending him a copy of Fomenko’s book for his analysis. In a letter of August 10, 2010 Rose writes:

“Fomenko puts the birth of Jesus Christ in +1152, which was apparently several centuries after the death of Alexander. In between would be the conventional placement of the Seleukids, with the eclipse report from -135 presumably now falling somewhere around +1020. To be generous, let us look at the entire stretch from, +800 to +1200.

“I picked this eclipse because it is widely regarded as one of the best attested total solar eclipses from antiquity. It was reported as total in Babylon during the early morning of day 29, month Adar the second, year 175 of the Seleukid Era, with mid-totality about 42 $uš = 2$ hours and 48 minutes after Sunrise. This was presumably April 15 -135. Both Lange and Swerdlow 3.1 and Starry Night confirm that there was indeed a total solar eclipse in Babylon that morning, with mid-totality about 2 hours and 50-some minutes after Sunrise. (This is about the best that can be expected; remember that the ancients had no mechanical or pendulum clocks.)

“Using both Lange and Swerdlow 3.1 and Starry Night, I have determined that there were no total solar eclipses in the morning in Babylon in either February, March, April, or May between +800 and +1200. That is enough for me. I shall spend no further time on Fomenko until such astronomical difficulties have been resolved to my satisfaction.

Lynn E. Rose

Casa Grande, Arizona”
To back up his claim, Rose added the following attachment to his letter, noting all the total/large solar eclipses at Babylon between 800 and 1200 AD, the time to which Fomenko dates these early civilizations:

<table>
<thead>
<tr>
<th>No.</th>
<th>Month</th>
<th>Day</th>
<th>Year AD</th>
<th>Magnitude %</th>
<th>SN</th>
</tr>
</thead>
<tbody>
<tr>
<td>1.</td>
<td>5</td>
<td>14</td>
<td>812</td>
<td>87</td>
<td></td>
</tr>
<tr>
<td>2.</td>
<td>5</td>
<td>4</td>
<td>813</td>
<td>67</td>
<td></td>
</tr>
<tr>
<td>3.</td>
<td>6</td>
<td>26</td>
<td>819</td>
<td>72</td>
<td></td>
</tr>
<tr>
<td>4.</td>
<td>3</td>
<td>14</td>
<td>834</td>
<td>41</td>
<td></td>
</tr>
<tr>
<td>5.</td>
<td>3</td>
<td>4</td>
<td>862</td>
<td>82</td>
<td></td>
</tr>
<tr>
<td>6.</td>
<td>5</td>
<td>27</td>
<td>876</td>
<td>97</td>
<td>SN large partial rising</td>
</tr>
<tr>
<td>7.</td>
<td>4</td>
<td>26</td>
<td>906</td>
<td>70</td>
<td></td>
</tr>
<tr>
<td>8.</td>
<td>3</td>
<td>6</td>
<td>946</td>
<td>80</td>
<td></td>
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<tr>
<td>9.</td>
<td>7</td>
<td>10</td>
<td>967</td>
<td>77</td>
<td></td>
</tr>
<tr>
<td>10.</td>
<td>3</td>
<td>7</td>
<td>992</td>
<td>54</td>
<td></td>
</tr>
<tr>
<td>11.</td>
<td>8</td>
<td>20</td>
<td>993</td>
<td>97</td>
<td></td>
</tr>
<tr>
<td>12.</td>
<td>4</td>
<td>7</td>
<td>1000</td>
<td>73</td>
<td></td>
</tr>
<tr>
<td>13.</td>
<td>5</td>
<td>19</td>
<td>1007</td>
<td>54</td>
<td></td>
</tr>
<tr>
<td>14.</td>
<td>6</td>
<td>19</td>
<td>1015</td>
<td>83</td>
<td></td>
</tr>
<tr>
<td>15.</td>
<td>4</td>
<td>9</td>
<td>1046</td>
<td>82</td>
<td></td>
</tr>
<tr>
<td>16.</td>
<td>2</td>
<td>15</td>
<td>1059</td>
<td>98</td>
<td>SN large partial ~7:13</td>
</tr>
<tr>
<td>17.</td>
<td>6</td>
<td>20</td>
<td>1061</td>
<td>100</td>
<td>SN total 7:40</td>
</tr>
<tr>
<td>18.</td>
<td>8</td>
<td>1</td>
<td>1087</td>
<td>85</td>
<td></td>
</tr>
<tr>
<td>19.</td>
<td>5</td>
<td>21</td>
<td>1091</td>
<td>78</td>
<td></td>
</tr>
<tr>
<td>20.</td>
<td>3</td>
<td>19</td>
<td>1113</td>
<td>92</td>
<td></td>
</tr>
<tr>
<td>21.</td>
<td>5</td>
<td>11</td>
<td>1119</td>
<td>82</td>
<td></td>
</tr>
<tr>
<td>22.</td>
<td>6</td>
<td>12</td>
<td>1173</td>
<td>89</td>
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<tr>
<td>23.</td>
<td>4</td>
<td>11</td>
<td>1173</td>
<td>87</td>
<td></td>
</tr>
<tr>
<td>24.</td>
<td>9</td>
<td>4</td>
<td>1187</td>
<td>100</td>
<td>SN very nearly total ~3:10 p.m.</td>
</tr>
<tr>
<td>25.</td>
<td>6</td>
<td>23</td>
<td>1191</td>
<td>79</td>
<td></td>
</tr>
<tr>
<td>26.</td>
<td>2</td>
<td>19</td>
<td>1216</td>
<td>91</td>
<td></td>
</tr>
</tbody>
</table>

At this point we must perforce return to Laurence Dixon who has chosen to directly claim that the April 15, 136 B.C. total solar eclipse path did not pass over Babylon, stating:
“Let us illustrate this using one particular eclipse that was recorded on a tablet [actually, there were two tablets], found in the ruins of Babylon and now kept in the British Museum. It records details of an eclipse that…had to be interpreted and it is said to be equivalent to the 15th of April 136 BC in the backward extension of the Julian calendar used by modern astronomers. According to the standard computer programme in use to retro-calculate eclipse dates there was indeed a total eclipse of the Sun on that date, but it was not visible at Babylon. Babylon is at latitude 32.5 degrees north and longitude 44.5 degrees east, but the computed eclipse path crossed that latitude at 4.0 degrees west. It was no better if they included tidal forces in the programme as the path now crossed that latitude at 67.0 degrees E. These paths are shown in [the] Fig. [below].”

In fact, we presented a long discussion of Delta T evidence in volume III of this series in 2010, pages 208-212; this in general explained this question which we will deal with yet again.

The first point regarding Dixon’s source, Thomas Crump never suggested that tidal forces are the only ones that affect the Earth’s rotation and admits:

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87 Dixon, “Retro-calculation and the Delta T function,” op.cit., p. 27
“The right-hand [eastern] curved line shows where [the path of totality] would have been according to modern geophysical theory if the tidal effect described above were the only force affecting the Earth’s rotation. The fact that the actual path of totality to pass over Babylon fell short of this line means other geophysical [non-gravitational] effects must have acted to counteract the tidal friction.”\(^{88}\)

On page 147, Crump gives a few of these other possible forms of evidence that can change the rotation of the Earth and counteract tidal theory only effects.

The second point is that if Dixon is correct, then when solar eclipses occurred, say, 2000 years ago and earlier, none of their paths of totality would have passed over the cities that reported their occurrences all across Greece, Turkey, Mesopotamia, and China. In essence, in all these countries these astronomer/astrologers were reporting to their kings a solar eclipse they never saw. The kings in all these countries were apparently so obtuse that when they were informed that a total eclipse happened but which they could not and did not see, they accepted it as a factual event. Therefore, when the king of Babylon was told that an eclipse turned day into night on April 15, 136 B.C., but there was none, he accepted it. The only reason for a king to accept such an event as true is if he and the entire population experienced such an event. What Dixon’s concept requires is that every astronomer/astrologer across this vast region was lying to his monarch, and that the king and the population were either ignorant or blind and were fooled into believing that day turned into night when it did not.

In this respect Dixon is in error because, besides “tidal effects” there are well-known and well-established other effects that also affect the acceleration of the Earth’s rotation as we go back in time. Here then is what L.V. Morrison and F.R. Stephenson explain:

“While the tidal component of the Earth’s acceleration can be derived from recent high-precision observations, the actual long-term acceleration, which is the sum of the tidal and nontidal components, cannot be measured directly because it is masked by the relatively large decade fluctuations [of these].”\(^{89}\)

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What Morrison and Stephenson are saying is that, even after making highly precise measurements, the same tidal effects as described by Dixon in his article, tidal effects from decade to decade, do not account for the acceleration of the Earth’s rotation. Something else is at work and this is unknown, but it exists as the present measurements clearly indicate. What Dixon’s thesis requires is that these forces/this force do not/does not exist. The only person(s) who seems to believe these nontidal/nongravitational forces don’t exist is Dixon and those who think as he does. All astronomers know of their existence. All archaeo-astronomers take them into account when they retrocalculate eclipses into the deep past, and all agree that the April 15, 136 B.C. total solar eclipse’s path of totality did pass over Babylon, including Crump, from Dixon’s own citation.

Returning to Fomenko, altogether there were 26 large solar eclipses between A.D. 800 and ca. 1200, none of which fit nor could be accommodated with Fomenko’s chronology. None of them in any way incorporated all the astronomical data found in the two cuneiform tablets. This unequivocally proves that Fomenko’s chronology is invalid, but the preciseness with which the data fits the April 15, 136 B.C. total solar eclipse proves the Delta T curve back into ancient times is basically correct. In summary, for the present, we have discussed thus far four unique sets of astronomical evidence that are extraordinarily accurate and that deny that the Delta T curve is seriously erroneous:

1. The 23 eclipses in the *Annals of Ulster* that are accurate to the hour
2. The VAT 4956 astronomical data for the positions of the five naked-eye visible planets and the Moon over a period of one year at Babylon
3. Lynn E. Rose’s Sothic evidence for the rising of the star Sirius with 34 solar or lunar festival dates for the placement of the 12th Dynasty in Egypt.
4. The total solar eclipse of April 15, 136 B.C. as it correlates with the location of Babylon, the time of onset and totality, the direction from which the Moon’s shadow encroached upon the solar disk, the approximate positions of several planets during totality, and the lunar eclipse that occurred 14 days earlier.

The last item “4” definitely does not fit anywhere into Fomenko’s chronology, nor, we dare say, do any of the other three.

Fomenko has argued, “Many ‘ancient astronomical observations’ may have been theoretically calculated by late mediaeval astronomers and then included into
The problem is that VAT 4956 and the tablet on which the 136 B.C. total solar eclipse were written in cuneiform which could not be read by medieval scholars and that the El-Lahun papyri were written in hieratic ancient Egyptian which also could not be read until well after that medieval period. These, as well as others, we know contain astronomical data that fit the short chronology discussed in the previous three volumes, and when retrocalculated correlate with ancient historical times. How could the medieval scholars have anticipated that the astronomical data in documents discovered in the ground centuries after they died, in arcane ancient languages, would be so precise when retrocalculated fit not only with a chronology over a thousand years prior to their time but also correlate and corroborate with each other? How could the medieval scholars know that the Delta T curve recognized in fairly modern times correlated with these ancient documents?

Moreover, we have to account for the unstable quality of Delta T, that is, explain the non-gravitational force or forces that could account for this variation? In this respect the question of the reliability of the Delta T curve was used against Velikovsky, when John Q. Stewart of Harvard University maintained that Velikovskian catastrophes could not have changed the tilt of the Earth’s axis or its revolution and rotational period about 1500 B.C. and 800 B.C. Had these catastrophes occurred, it would have been impossible to date astronomical eclipses and other events prior to 800 B.C. Stewart, however, failed to acknowledge that Delta T values after 800 B.C. are unknown or are highly ambiguous. In his response to Stewart, Velikovsky raised the concept that electro-magnetic interactions between the Sun’s magnetic field variation and that of the Earth were responsible for this Delta T phenomenon:

“Stewart…finds the complexity of lunar motion ‘one of the most imposing demonstrations of the validity of celestial mechanics.’ S. Newcomb, however, on the basis of eclipses from Ptolemy to this century, found ‘disturbing variations’. I quoted Simon Newcomb, the great American mathematical astronomer, on this very problem of lunar motion [related to Delta T] as checked by ancient eclipses:

“I regard these fluctuations as the most enigmatic phenomenon presented by celestial motions, being so difficult to account for by the action of any known causes, that we cannot but suspect them to arise from some action in nature

Fomenko, op.cit., p. 110
hitherto unknown…It would be natural to associate them with the Sun’s varying magnetic activity and the varying magnetism of the Earth.””91

This connection had not been explored or even taken seriously by those who maintain electro-magnetic forces have no place in celestial mechanics or are minute or drowned out by gravity. Nevertheless, Ralph E. Juergens, as long ago as 1977, was able to explain this electro-magnetic interacted in a paper titled “On the Convection of Electric Charge by the Rotating Earth,” showing that solar flares when aimed at the Earth actually slowed its rotation:

“In 1960 [A.] Danjon reported a sudden deceleration of the Earth’s rotation following a solar flare of record intensity [in Comptes rendus des Séances de l’Académie des Sciences, vol. 250 (1960), p. 1399]. According to his observations, the length of the day increased by 0.85 millisecond and thereafter began to decrease at a rate of 3.7 microseconds per day. Eventually the rate of spin stabilized near its pre-flare level.

“This announcement raised quite a few eyebrows. Quite impossible, said the experts. One skeptic pointed out that the phenomenon implied an increase in the Earth’s polar moment of inertia of such magnitude as might only be produced, for example, by instantly lifting the entire Himalayan massif to a considerable height. Danjon, anticipating such objections, argued that ‘it is very likely electro-magnetism alone that will furnish the explanation for these variations…’ But his claim was generally disregarded.

“Then in 1972 it happened again, even more impressively. Danjon was gone (deceased 1967), but Plagemann and Gribbin were on the watch…

“[They] had predicted the phenomenon, had been on the alert for unusual flare activity, and had been rewarded for their vigilance. O’Hora and Penny of the Royal Greenwich Observatory pooh-poohed the whole idea: ‘Indeed if a solar storm of such exceptional magnitude…exerted such little influence on the rate of rotation of the earth, then there are good grounds for believing that changes in the length of the day are induced by some other mechanism.’””92

Juergens then goes on to illustrate the physics that would cause the Earth’s rotation to slow and then speed up by a large electrical charge of a solar flare

directed at the Earth. We have twice discussed this elsewhere, and thus we cite our book *Carl Sagan and Immanuel Velikovsky* (Tempe AZ 1995), p. 148:

“John Gribbin, in *The Death of the Sun*, presents a graph on page 131 wherein he correlates solar [flare] activity with the length of the day...His chart begins around 1820 and extends to about 1980. It shows that when the Sun was more active—produced more electromagnetic radiation—the length of the day increased...When the Sun was less active, the opposite occurred. [The Earth’s rotation speeded up.]

“In 1981, D. Djurovic’s ‘Solar Activity and Earth’s Rotation,’ in *Astron Astrophys*, Vol. 100, pp. 156-158, showed a correlation of the Earth’s rotation with well-known solar Sun spot variation periods of 0.5, 3.3, 6.6, and 11 years.


“It is well known that the Sun oscillates up and down some ten kilometers periodically every 160.1 minutes. G.P. Pil’nik’s ‘Multiple Waves in the Earth’s Diurnal Rotation,’ in *Soviet Astronomy*, Vol. 28, No. 1, pp. 112-114, showed that this solar oscillation of 160.01 mins. was reflected in changes in the rotation of the Earth in 159.56 mins.

“Each of these studies resoundingly illustrates that long and short term variations in the Sun’s activity are reflected in corresponding correlated changes in the Earth’s rotation....

“In fact, James S. Trefil in his book, *Space Time Infinity* [(NY 1985), p. 159], admits ‘To further complicate the problem (of the length of the day) recent research suggests that the solar wind [the Sun’s electromagnetic field] can alter the length of the day’.”

The germane point with respect to Delta T of Gribbin and these other measurements is that the Earth’s rotation speeded up when solar activity was less, and slowed when it was greater. These changes in the rotation of the Earth over
very long periods would add up to changes in Delta T from a straight uniform curve change to one that is non-uniform.

This indicates, although it cannot be proven, that at the period to which Fomenko, R.R. Newton and Simon Newcomb refer with regard to the erratic nature of Delta T, this was caused by a non-gravitational force—electromagnetism. Since we don’t know what was happening on the Sun for this lengthy period, we cannot prove nor exclude this explanation for the Delta T curve.

Nevertheless, as we pointed out above, using four unique sets of ancient astronomical data, these correlate with that Delta T curve and mutually with each other. Neither Fomenko nor anyone else has shown that they can make these fit Fomenko’s chronology. What Fomenko has also done is “attempt” to make a set of three eclipses—two solar and one lunar—reported by Thucydides—fit his thesis. We will show that Fomenko has interpreted this report such that it agrees with his thesis, while another interpretation clearly does not, but fits the time period. Thucydides says: “I was writing down the events [of the Peloponnesian Wars] witnessed by myself as well as what I had heard from others, after as meticulous a study of each fact as circumstances allowed…I have survived the entire war…understood it, and studied it attentively.”

Fomenko summarizes the three eclipses Thucydides reported:

“1) All three eclipses were observed from the square [area] fitting into the following geographical coordinates: longitude between 15 and 30 degrees, latitude between 30 and 42 degrees;

2) The first eclipse is solar;

3) The second eclipse is solar;

4) The third eclipse is lunar;

5) The time interval between the first two eclipses equals 7 years

6) The interval between the second eclipse and the third equals 11 years;

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93 Fomenko, op. cit., p. 98
7) The first eclipse occurs in the summer [Thucydides relates this to two periods, one when warfare can be conducted, i.e. summer, March through October; and winter: November through February];

8) The first solar eclipse is a total eclipse, since one can see the stars…one cannot see any stars during a partial eclipse;

9) The first solar eclipse occurs after midday, local time;

10) The second solar eclipse occurs in the beginning of summer;

11) The lunar eclipse takes place around the end of summer,”

The major problem that exists for this triad of eclipses is that there is a fundamental astronomical contradiction related to the first one as directly stated by Thucydides:

“The same summer, at the beginning of a new lunar month (the only time by the way at which [a solar eclipse] appears possible), the Sun was eclipsed after noon. After it had assumed the form of a crescent…, and some of the stars had come out, it returned to its natural shape.”

Various astronomical programs retrocalculate this eclipse to August 3, 431 B.C. However, they indicate that the eclipse was an annular one, with the outer rim of the Sun still shining around the eclipsing Moon. Thucydides suggests it had a crescent shape. The problem/contradiction is that in either case, an annular or a crescent eclipse, it would be impossible to see the stars. Diacu, who generally follows Fomenko, put his finger on the sore spot of analyzing this eclipse, saying “Perhaps the mentioning of stars was an exaggeration?” This, indeed, is the crucial question. Since one cannot have an annular or crescent-shaped solar eclipse and still see the stars, either the stars were not seen, but reported to have been seen, or the eclipse was total to allow them to appear. In essence, one has a choice to decide which element of Thucydides’ description is false—the stars being seen or not, or the eclipse being annular, crescent or total. Both elements are mutually exclusive.

94 Ibid., p. 99
95 Thucydides, History of the Peloponnesian Wars, cited in Stephenson, op.cit., p. 346
96 Diacu, op.cit. p. 101
And this is the problem: if someone Thucydides took to be honest misinformed him about these two contradictory elements, one is left with a choice of which element to accept and which to reject. If one accepts that the stars were seen during the eclipse, then that eclipse could not have happened at the time indicated by Thucydides. But if one rejects the element that stars were seen, then that eclipse did happen at the time indicated by Thucydides. What Fomenko has done was only accept as true that the stars were observed, and therefore he had to move this triad of eclipses to a different period. That is just what he did, he adjusted the interpretation so the eclipses could then fit into his chronology. Without any proof whatsoever as to the truthfulness of this report he chose to believe that part of it which permitted him to move these eclipses into the Middle Ages. Had he rejected the element that the stars were seen, he had no recourse but to move these eclipses out of the slot given them by Thucydides. In other words, he proved his case based on the supposition that he could tell which parts of Thucydides’ report were true and which were false. That, we suggest, is playing fast and loose with the data. Although we cannot prove which elements are true or false, we can evaluate these eclipses by how well they correlate with VAT 4956, the Sothic data for the 12th Dynasty, the total solar eclipse of 136 B.C. and the Annals of Ulster as well as Rose’s redating of the Neo-Assyrian kings closer to the present by 274 years or thereabouts. The fact of the matter is that Thucydides’ triad of eclipses fully correlates with all these other forms of astronomical data only if we keep them in the ancient time frame assigned to them. They have no such multiple accurate forms of astronomical data that fit Fomenko’s chronology. Only when Fomenko can place all these other forms of data into his chronology, will the Thucydides eclipses fit. While we have also chosen which element of Thucydides’s eclipse data to accept—the Sun was annularly eclipsed—and which we reject—the stars were observed during the eclipse—we nevertheless have an accurate astronomical basis upon which to choose, while Fomenko does not! We most certainly agree with both Fomenko and Diacu’s approach in very great measure except that we have numerous forms of scientific and technological evidence as the foundations upon which our short chronology rests. Fomenko has no multiple forms of science and technology to uphold his chronology. Diacu agrees with our approach when he writes:

“In the end, for historians and reformers of traditional time frames alike, an assumed chronology is the guiding force behind historical interpretation, with the reference frame driving the choices. From Joseph Scaliger and Dionysius Petavius to Isaac Newton to Anatoly Fomenko, every inquirer has fixed a few dates and
tried to connect them to events mentioned in the chronicles. To make sense of those stories, the researchers have dismissed some facts and accepted others.

“The choices one makes depend on one’s values and beliefs…[H]istorians, on the one hand, and reformers, on the other, have their reasons for taking a certain path…”

“What comes first: the astronomical evidence [because it is so precise], the words of the chroniclers, the legends, the sacred texts, the scientific dating methods, or some combination of them all?”

Diacu cites Fomenko to the same effect: “To proceed, the material will certainly require a large variety of methods: purely historical, archaeological, philological, physical, and chemical and mathematical, which…will permit us to look at the problem of chronology from a new angle.” As with Fomenko and Diacu, “We believe that the unprejudiced reader is already convinced that we are moved by the firm logic of scientific research. We have to move further along this path if we want to stay on the ground of common sense and strict scientific principles.”

This approach has been the entire basis for our underpinning of the short chronology which we have earnestly followed throughout our long journey of the four volumes of *Pillars of the Past*. Diacu has nevertheless charged Velikovsky scholars with being essentially incompetent. In his attack on those of us involved in Velikovskian research he argues:

“The circle of Velikovskian supporters continued to grow. They were intelligent, enthusiastic, and faithful. Apprenticeship was short—only as long as it took to read and understand *Worlds in Collision*. They believed in their cause and could contribute ‘scientific’ ideas and articles within weeks or months of joining the club. No time and money needed to be spent in graduate school, in the hunt for academic jobs, or in climbing the academic ranks. And success was guaranteed within the group.”

Diacu has presented no evidence to prove this deplorable assertion. And he should know better that researchers in the ranks of Velikovskian are PhDs and college professors.

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97 Ibid., pp. 241-242  
98 Fomenko, cited in ibid., p. 246  
99 Ibid., p. 164  
100 Ibid., p. 21
To answer Diacu’s accusation about university trained researchers like himself and us supposedly uneducated Velikovskians, we will turn to the accomplishments of Wilbur and Orville Wright and what the great men of science said prior to their first flight. Mark Eppler writes:

“Simon Newcomb was not among those who gathered along the Potomac River on October 7, 1903, to witness Professor Samuel Langley’s first attempt at manned flight. Newcomb, the only American since Benjamin Franklin to be made an associate of the prestigious Institute of France, was one of America’s most gifted and honored scientists. He was a renowned Johns Hopkins mathematician, a savvy physicist, and a giant in the field of celestial mechanics. In his ‘spare time,’ Newcomb was one of the economists responsible for developing the Quantity Theory of Money. Albert Einstein would later acknowledge the importance of Newcomb’s work in the development of his Theory of Relativity.

“It’s surprising that Newcomb, as one of the ‘giants of discovery’ at the dawn of the twentieth century, would pass on the opportunity to witness history in the making. Even more so since Langley was involved. Dr. Langley was secretary of the Smithsonian Institution, the most prestigious scientific post in America. His exalted stature had enabled him to secure the backing of both the Smithsonian and the United States military to fund his effort to invent a heavier-than-air flying machine. Nevertheless, on the day Langley’s machine, called the Great Aerodrome, was put to the test, Simon Newcomb had better things to do. He ‘took a pass’ on the event because he knew, beyond a shadow of doubt, it would fail.

“Several weeks after the Great Aerodrome had ‘fallen like a sack of mortar’ (one reporter’s description) into the Potomac, Newcomb wrote an article for The Independent explaining why it had done so. In it, he not only refuted the possibility of manned flight, he detailed what would happen should anyone succeed in getting a flying machine in the air. With palpable arrogance, Newcomb stated, ‘Once he (the pilot) slackens his sped, he’ll fall out of the sky!’ If such a scientific luminary as Simon Newcomb thought flight was impossible, how could anyone justify taking on the problem? Especially two bicycle merchants from Dayton, Ohio. …

“Read any book on the Wright brothers and it won’t be long before you encounter a question similar to the one Peter Jakab puts forth in Visions of a Flying Machine. Jakab asks:

“‘How were these two [trades]men, working essentially alone, with [as Diacu argues] little formal scientific or technical training, able to solve a problem so
complex and demanding as heavier-than-air flight in only a few short years, when it had defied better known experimenters for centuries?’

“Wright biographers and aviation experts have grappled for decades to find an adequate response to the question. It seems inconceivable that a problem of such complexity could be solved by such unlikely [uneducated] candidates.”\textsuperscript{101}

Sam Howe Verhovek further explains:

“It is much fun for us, knowing the outcome here on our side of history, to look back on just how widespread the skepticism was that man would indeed figure out how to fly. Shortly after…Samuel Langley crashed his ‘Aerodrome’…an eminent astronomer and physicist named Simon Newcomb put his finger on the apparent problem…Newcomb noted the great Langley’s failure…

“ ‘May not our mechanicians [Newcomb wrote] be ultimately forced to admit that aerial flight is one of the great class of problems with which man can never cope, and give up all attempts to grapple with it?’ Newcomb, the founding member and first president of the American Astronomical Society, asked in the October 22, 1903 issue of The Independent.

“Even if a pilot could somehow get off the ground, Newcomb reasoned, it was nigh impossible to figure out how he could defy the forces of gravity for any sustained period of time.

“ ‘It is the speed alone that sustains him,’ Newcomb argued of his hypothetical flyer. ‘How is he ever going to stop? Once he slacken his speed down he begins to fall. He may, indeed, increase the inclination of his aeroplane. Then he increases the resistance to the sustaining force. Once he stops he falls a dead mass. How shall he reach the ground without destroying his delicate machinery? I do not think the most imaginative inventor has yet put upon paper a demonstratively successful way of meeting this difficulty.’

“ ‘The only ray of hope,’ Newcomb said, now a bit more sarcastically, was back with the birds again. ‘Quite likely the most effective flying machine would be one carried by a vast number of humming-birds; if we could combine their forces, they would carry an aerial excursion party of human beings through the air.’

“Not two months after Newcomb’s airy dismissal of the whole idea, the Wright brothers flew—and landed quite safely—at Kitty Hawk. To be fair to him, the

\textsuperscript{101} Mark Eppler, The Wright Way (NY 2004), pp. 27-28
pioneering astronomer did not conclude that manned flight would be impossible always and forever, but Newcomb did posit that the human race was a long way off from figuring out all the angles. And in so arguing, he was just one among legions of [educated] experts who scoffed at the idea that success in flight was just waiting in the wings.”

What is also germane to Diacu’s description of Velikovskians is how the evidence of flight by the Wright brothers was seen by the rest of the scientific and academic communities: Having castigated the heavier than air flight concept, the scientific as well as the journalistic community saw those who were reputed to have flown as cranks, crackpots, or frauds. Velikovsky reported just how such news was greeted by that community:

“…on January 16, 1906, Scientific American printed an editorial comment on the ‘alleged’ flights by a ‘mysterious aeroplane’ that covered a ‘reputed’ distance of 38 kilometers. The brothers Wright were presented as two shadowy persons with fantastic claims, unfounded because unheard of.

“If such sensational and tremendously important experiments are being conducted in a not very remote part of the country, on a subject in which almost everybody feels the most profound interest, is it possible to believe that the enterprising American reporter who, it is well known, comes down the chimney when the door is locked in his face—even if he has to scale a fifteen-storey skyscraper to do so—would not have ascertained all about them and published the broadcast long ago?”

“The Wright brothers [were made to] appear…as two crooks: ‘Why particularly, as is further alleged, should the Wrights desire to sell their invention to the French government for a “million” francs?’”

Because it was inconceivable to the scientists that tradesmen could have solved the great problems of flight that had defied the greatest minds of the age, Scientific American refused to send anyone to see what was happening and instead attacked the Wright brothers rather than concentrate strictly on the scientific possibility.

In terms of calendrics, astronomy, and chronology, Diacu’s attack is aimed at us Velikovskians rather than at our evidence. We hold such an approach to material

102 Sam Howe Verhovek, Jet Age: The Comet, the 707, and the Race to Shrink the World (NY 1020), pp. 48-49
103 Velikovsky, Stargazers..., op.cit., p. 317
evidence to be shoddy in the extreme. If this particular book, as Diacu seems to suggest, is superficial, coming from a proponent of Velikovsky, let him by all means answer the calendrical/astronomical evidence presented in these volumes and especially in this volume that discredits Fomenko’s hypothesis.

GUNNAR HEINSOHN’S TWO PHANTOM CENTURIES

Gunnar Heinsohn’s thesis, with which we are in close agreement, has also presented the concept, much like that of Heribert Illig, that the two centuries between ca. 400-200 B.C. never existed, and that Alexander the Great, like Charlemagne, never existed. He bases this on the missing stratigraphical evidence that is expected to be there but that is missing for this period. If, as Heinsohn maintains, ca. 400 to 200 B.C. never existed, it would be impossible to find a total solar eclipse in a primary cuneiform document that is very accurately described and when retrocalculated occurred in this very same period, like the 18 eclipses that occurred before and during Illig’s phantom centuries.

Stephenson, who discusses the cuneiform document, states:

(1) BC 322 Sep 26 (computed mag. at Babylon = 0.17)

Year 2 of (king) Philip…(month VI)…The 28th, around 3 deg before sunset, solar eclipse…It set eclipsed.

[BM 34093 (= LBAT 212), Rev. 23'; trans. SH I, p. 227; see also Huber, p. 83.]

This primary document shows specifically the latitude and longitude where it occurred—Babylon. It gives the year, month and day it occurred. It gives the time of day—late evening—and, significantly, the fact that this eclipse occurred above the horizon so that the Sun set below the horizon still eclipsed. When this eclipse was retrocalculated, it fit all these conditions accurately. Since a total solar eclipse lasts only a very short time, the document had, like that of the Annals of Ulster to have been written by someone who observed this event. Heinsohn, of course, calls this sort of evidence “pseudo-astronomical” science. But because of his background he has been unable to present precise astronomical evidence that these two centuries never existed. That is, if Heinsohn is correct, like Fomenko, all the

104 Stephenson, Historical Eclipses..., op.cit., p. 131
retrocalculations of astronomical events would fit with his chronology, and he expects that, if Lynn Rose were to retrocalculate these about 500 years closer to the present, which includes his 200 years B.C. and Illig’s 297 years A.D., that this would fit his chronology. Unlike Illig, who attempted to use the equinoctial evidence of Caesar Augustus to astronomically prove that instead of 10 days, the reform of Pope Gregory XIII should have taken out 13 days, Heinsohn has no astronomical data to support his hypothesis. In the closing remarks of his article, “Phantom Periods and Astronomical Retrocalculation,” he has thrown down the gauntlet regarding these matters:

“Astronomical Retrocalculations

“Such retrocalculations face two major problems:

“1. Modern astronomers must be able to precisely translate coordinates and data found in ancient texts into the coordinates used today. This is extremely difficult, as may be illustrated by an admission of one of the leading men in the field: ‘When this [retrocalculative] study was started some four years ago, the interpretation of each kudurru [stela with celestial and planetary imagery] was attempted separately and revised a few times until I was satisfied with the results’ (emphasis added [in original]). When the appearance of a peculiar [particular?] celestial body is described in ancient texts, its identification must be beyond doubt if calculations are to be valid.

“2. If yet-to-be formally identified (i.e. hidden) phantom periods exist between the date of retrocalculation, let us say, 28th June 2005, and an ancient text from, let us say, sometime in the decade 760/750 BC, modern astronomers will be led astray. As long as these assumptions of two fictitious periods—from 400 BC–200 BC (1.) and from AD 600-AD 900 (2.) are not refuted by presenting hard evidence for this combined time-span of 500 years, any retrocalculation which tries to reach further back in time than 900 AD, and simultaneously ignores the possible phantom centuries, will be seriously flawed.”\(^{105}\)

We suggest Heinsohn’s citation about the kudurrus requiring revisions a few times before one of the leading men in the field found a time in which to fit them as evidence against astronomical retrocalculation contains a serious flaw of logic. Since he and we hold that the chronology of the ancient Near East must be greatly shortened while that authority does not, the authority will be working with an erroneous chronological structure. That being the case, such retrocalculations are

\(^{105}\) Heinsohn, “Phantom Periods…”, *op.cit.*, p. 9
bound to be erroneous from the start and the authority will, of course, be unable to
date them easily and accurately because he/she adheres to an erroneous
chronology. Therefore, he/she will have to move the data on the kudurrus around
to find a “satisfying” fit rather than an accurate one. As Lynn Rose has repeatedly
shown, the astronomical data of ancient documents cannot fit the established
chronology. He has shown that the El-Lahun papyri Sothic date for the late 12th
Dynasty, a chronological key to dating Egyptian chronology, must be moved 1477
years closer to the present where it does fit. In this case, Heinsohn, who calls the
astronomical approach “pseudo-astronomical retrocalculation,” accepted Rose’s
analysis. Why should he have done so if it is “pseudo”? Rose has also moved the
chronology of the Neo-Assyrians into Persian time, in fact into the very period
Heinsohn claims does not exist. Yet here too he has accepted this correction, based
on “pseudo-astronomical retrocalculation.” That is, Rose has repeatedly used
retrocalculations for astronomical data that moved the chronology of Neo-Assyrian
kings into Persian times where Heinsohn has placed them. These kings followed
one another in the ancient documents and when Rose retrocalculated their
astronomical placement, they also followed one another in roughly the same order,
but, most significantly, certain of them could only be placed somewhere in the
400–200 B.C. period that Heinsohn claims does not exist. To place them where
they will not contradict those two phantom centuries, Heinsohn must find a fit that
keeps them in the same order about 500 years closer to the present. That, we
suggest, cannot be done.

Heinsohn has also argued, somewhat like Laurence Dixon with many in the
SIS, that retrocalculation beyond a certain time in the past will necessarily be
“seriously flawed” since there are two non-existent intervening periods. His
challenge is that those of us who accept or employ astronomical retrocalculation
“must be able to precisely translate coordinates and data found in ancient texts into
coordinates used today”. We will do just that. At present we know the precise
coordinates of the Moon and the five naked-eye visible planets—Mercury, Venus,
Mars, Jupiter, and Saturn. For this example, we will examine the precise
coordinates first of Jupiter and Saturn, and then of Mercury, Venus, Mars, and the
Moon to show that there have been no 500 years missing from the calendar. We
will show that when these bodies are retrocalculated to the time before Heinsohn’s
phantom centuries, they were then in precisely the same orbits they follow today.

Jupiter and Saturn are two extremely precise celestial chronological clocks
running in tandem with each other and the other planets and the Moon through
time such that if we were to retrocalculate their positions from, say, June 28, 2005,
back beyond 400–200 B.C. (Heinsohn’s phantom centuries), knowing the precise coordinates of Jupiter and Saturn today they must also be in coordinates in the pre-Heinsohn period that correlate with their coordinates today. Since, according to Heinsohn, there have been no great catastrophes that have befallen the Earth to change its orbital period or axial tilt after the 8th century B.C., we can nevertheless retrocalculate their precise orbits to see if they were still in their proper orbits earlier than 400 B.C. If we can find a period prior to 400 B.C. where Jupiter and Saturn (in this case) are still in their correct retrocalculated orbits, it will prove that the phantom centuries of Heinsohn and Illig are not phantoms. If, however, Heinsohn and Illig’s phantom centuries thesis is correct, it will be impossible to have these planets following the same retrocalculated orbits prior to 400 B.C. Furthermore, if we remove about 500 years or 497 years from the calendar and then find by retrocalculation that there are “large” discrepancies for the positions/coordinates of the planets, then it becomes rather obvious that Illig and Heinsohn’s non-existing centuries do in fact exist. The simplicity of this test is self-evident.

In order to determine whether Heinsohn and Illig’s chronology is correct or if our chronology is, we must return to examine the cuneiform document VAT 4956 in which the approximate coordinates not only of Jupiter and Saturn are given but of Mercury, Venus, Mars, and the Moon prior to 400 B.C.

This again is the same situation we found for the Annals of Ulster where 23 eclipses are accurately described and/or dated to a precise hour of the day before, during, and after Illig’s phantom centuries. This is also the same situation we found for Lynn Rose’s analysis of the El-Lahun papyri which correlated the heliacal rising of the star Sirius with 34, now 37 lunar festival dates. The totality of these many precise astronomical agreements make any movement of the celestial bodies out of their time frame, as found in the short chronology, impossible. Then we have the orbits of Jupiter and Saturn before Heinsohn’s phantom centuries in their correct positions correlating with their correct coordinates after Illig’s phantom centuries. The test Heinsohn calls for shows that these five non-existent centuries exist. The same condition applies to Mercury, Venus, Mars, and the Moon. They, too, are in their proper positions/coordinates before Heinsohn and Illig’s phantom centuries and after them as they are today.

In terms of the orbits of Jupiter and Saturn, if we remove Heinsohn and Illig’s 497 or 500 years, they should still be in their proper orbits today with these missing centuries. We will use the more rounded numbers from the orbital periods
to examine this question. Jupiter orbits the Sun in almost 11.86 years, Saturn in 29.64 years. When we divide these numbers into 497 years, we find

<table>
<thead>
<tr>
<th>Planet</th>
<th>Phantom Centuries</th>
<th>Periods</th>
<th>Discrepancy</th>
</tr>
</thead>
<tbody>
<tr>
<td>Jupiter</td>
<td>497 ÷</td>
<td>11.86</td>
<td>44.9055</td>
</tr>
<tr>
<td>Saturn</td>
<td>497 ÷</td>
<td>29.46</td>
<td>16.8703</td>
</tr>
</tbody>
</table>

The discrepancy shows that Jupiter has gone around the Sun 41.9055 times instead of a full 42 times. The 0.9055 period left over means that Jupiter is out of its proper coordinate by 1.121, that is behind its proper coordinate by 1.121 years. As Saturn has gone around the Sun 16.8703 times instead of a full 17 times during the 497 years, Saturn is out of its proper coordinate by 4.241 years. If we lower the length of the phantom centuries by 1.121 year so that Jupiter will complete 42 full orbits and be in its proper position, Saturn will still be behind its proper position by 3.120 years. On the other hand, if we reduce the length of the phantom centuries by 4.241 years so that Saturn will complete 17 full orbits, and be in its proper position, Jupiter will be ahead of its proper position by 2.999 years. It may be possible to find a time frame such that both planets can complete full revolutions by adding years or subtracting years from the 497 years. But that, of course, will be culling the data to make these non-existent two periods fit Illig’s and Heinsohn’s theory. And this may be close enough to the 497 time frame to be justified. However, at that point we then have to make this new number fit Mercury, Venus, and Mars going round the Sun in complete orbits. This, we maintain, cannot be done in around a 450 or 550 year time frame. Interestingly, Illig’s 297 non-existent years can be accommodated to fit the orbits of Jupiter and Saturn such that both complete full orbits, Jupiter completing 25.042 full orbits and Saturn 10.081 full orbits. But then when we try to fit Mercury, Venus, Mars, and the Moon into this period, the numbers do not show that these bodies complete full orbits. The astronomy cannot be made to agree with these missing centuries.

As Heinsohn has asked for precise coordinates that fit the orbits of celestial bodies today and into the past, prior to 400 B.C., that fit our chronology, we have done just that and more. We have shown that they cannot be made to fit Illig and Heinsohn’s non-existent centuries. To keep all these bodies in their proper orbits and periods, we cannot remove 500 years or anything fairly close to 500 years. If Heinsohn or any of the proponents of his hypothesis can prove otherwise, by all means let them present that evidence.
On top of all this, there is another total eclipse during Heinsohn’s phantom centuries, this one in China. It is considered one of the best records for such eclipses. Peter J. Metevelis shows:

“Chinese accounts of total solar eclipses are fairly rare: the most detailed examples are preserved for 181 BC [etc.]. A complete translation of two records of [181 B.C.] is as follows:

“Empress of Gaozi, 7th year, 1st month, day jichou, the last day of the month [March 4th]. The Sun was eclipsed; it was total and it became dark in the daytime; it was 9 degrees in Yinghsi [lunar lodge] …”

This 181 B.C. eclipse is also shown to be extremely accurate in Jean-Paul Zahn and Magda Stavinschi’s *Advances in Solar Research at Eclipses from Ground and from Space* (Dordrecht, the Netherlands 2000), page 8, and Kelley, Milone, Aveni, *op. cit.* p. 123. The accuracy of the description of this total solar eclipse is not to be denied. All these precise eclipses, positions of the planets, etc., cannot be fortuitous and agree precisely with retrocalculations and with one another just by chance. Yet this is what Heinsohn is suggesting. The probability that they so well mesh with one another and with retrocalculations cannot be doubted. They all fit together so well because they reflect astronomical reality.

Heinsohn also faces the same problem we raised for Fomenko, or Laurence Dixon, and many at the SIS. As Rose has repeatedly said, there are simply too many precise eclipses in the ancient past that correlate with the positions of the planets. The fact that they fit the Delta T curve and with one another from medieval times as given in the *Annals of Ulster* to ancient Babylonia as shown in VAT 4956 and with the El-Lahun papyri precludes any significant deviation of the calendar from ancient times to the present. To call, as do Heinsohn and some of his supporters, astronomical retrocalculation a “pseudo-evidence” proves nothing. It would be equivalent to calling the stratigraphical evidence of Tell Munbaqa and elsewhere that supports Heinsohn’s thesis “pseudo-stratigraphy.” Such a response is in fact an evasion.

Another problem Heinsohn faces once he rejects astronomical dating is, how does he anchor his chronology to any time with an astronomical date? We, on the other hand, have multiple astronomical dates, especially the El-Lahun papyri data, the VAT 4956 data and the data in the *Annals of Ulster*, plus the 136 B.C. eclipse

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data that are all precise, correlate with one another on the Delta T curve and run from before Heinsohn’s B.C. date through Illig’s A.D. 911 date, right up to the present time. Heinsohn has based his chronology, which includes Illig’s phantom centuries, expressly on stratigraphy, but this only gives him an ordered sequence of civilization, it cannot and does not with any degree of exactitude tell how long they existed nor when they began or ended. Lynn Rose’s astronomical evidence correlates with the chronology of the 12th Dynasty as found in the El-Lahun papyri and gives far more exact dates for its chronology and its beginning and ending. Stratigraphy is not a precise clock; astronomy is. Without astronomy, which is accurate to the day and often to the hour, there is no way to anchor and organize time in the ancient world. Our methodology has been to employ stratigraphy (especially geological stratigraphy) and astronomy as well as other scientific, technological and other evidence to organize that chronology. Heinsohn has, in our opinion, failed to incorporate this most accurate, powerful chronological tool into the corpus of his work. Stratigraphy by itself, we maintain, is not enough. It should be corroborated by astronomy and as many other forms of scientific, technological and other kinds of evidence as available, such that they correlate and are congruent with one another. The forensic historical method we have employed incorporates all these methodologies and accepts the totality of what they say.

Heinsohn and his advocates may now claim, as he does in his Chronology & Catastrophism Workshop paper, that there are absolutely no stratigraphical materials lying in the ground from between around 400 to 200 B.C. In his discussion of this missing stratigraphy, however, he has failed to deal with Egypt. Here we will explicitly follow Heinsohn’s method to show there are undoubtedly stratigraphical materials in the earth running from 400-200 B.C. This we covered in Pillars of the Past vol. III, pages 82-84 where we analyzed Egyptian stratigraphy for this period, which we must repeat here for those unfamiliar with this material or who have forgotten it.

“As readers of volume I should recall, the excavations carried out at Tall Munbaqa by geologist Ulrike Rösner proved that the 700-800 year settlement gap placed there by archaeologists and historians did not exist. What the archaeologists did was simply to write the word ‘hiatus’ or other such word or words to claim the settlement gap existed without determining whether there were aeolic/wind-blown materials separating the cultures above and below the gap. Ramses III reigned over a city built in the Nile Delta, today’s Tell el-Dab’a, also known by its Greek name of Avaris or that of its close neighbor, Piramesse, built by Ramses II. Here, too, we have a comparable situation to Tall Munbaqa because, as we will see, the
archaeologist who excavated there put a ‘hiatus’ in the strata; in doing so, he correlated this settlement gap with the established chronology without showing intervening wind-blown materials between the two cultures.

“This stratigraphic evidence was found by Manfred Bietak and his team who have been excavating this site for decades. Although other regions of Egypt rarely if ever show Egyptian dynastic chronology such that many dynasties’ relics lie in strata above or below the other in such a manner that they give a picture of which ones come first, second, third and so on for centuries, Avaris in Egypt is therefore unique in this respect. It is located in the eastern Nile Delta, which is not desert, but was a somewhat swampy delta region. Buildings, walls, temples in such a setting will in time be covered by soil or wind-blown sand from the nearby deserts. Grasses, bushes, trees, reeds will grow over any abandoned city and leave a layer of material in the streets and abandoned houses that will become deeper over time. Bietak explains that ‘Tell el-Dab’a is the largest and most impressive of all sites, and by its fine stratigraphic series and abundant excavated materials the most representative [of the chronology of Egypt].’

“Therefore, if Ramses III of the 20th Dynasty lived in the 12th century B.C., the stratigraphy there should exhibit this by showing Ramses III’s relics and stratum separated from the Ptolemaic strata by either intervening dynasties or, if abandoned, by about 700-800 years of materials laid down between the two cultures. In this instance the situation is quite similar to that at Tall Munbaqa with its 700-800 year hiatus. However, if Ramses III’s stratum is found beneath Ptolemaic strata with neither several dynasties’ relics above it nor a deep layer of material separating the two, this will be clear evidence that Ramses III lived less than a century before the Ptolemites, around 390 B.C., where Velikovsky placed him. One cannot presume that if Ramses III’s 20th Dynasty stratum was abandoned for 700-800 years there would not exist a deep layer of materials to indicate this fact, especially as this city was on the flood plain. That Nile floods for 700-800 years would have failed to deposit silt over the site, that vegetation also failed to grow and decay there, leaving dead grasses, bushes, leaves, trees, and even here and there the remains of wild animals, is clearly impossible.

“The fact of the matter is that Ramses III’s 20th Dynasty materials are located directly beneath Ptolemaic strata with no evidence reported by anyone of such a deep layer of material existing to separate them. As with Tall Munbaqa, the word ‘hiatus’ is simply inserted between the Ptolemites and the 20th Dynasty. Bietak states of the

topmost two strata at Piramesse that the topmost is Ptolemaic, labeled stratum ‘A’, and directly beneath it stratum ‘B’ is that of Ramses III’s 20th Dynasty:

“‘The stratigraphy of Tell el-Dab’a can be presented for the time being as follows:


“‘hiatus

“‘Str[atum] B: Settlement, large temple, stores, fortification (?) from the end of the 18th dynasty to the 20th [Ramses III] dynasty (± 1310-1080 BC) and scanty evidence of 21st dynasty [materials].’

That is, the 20th Dynasty came less than a century before the Ptolemaic one and therefore in that very short time the 21st Dynasty and 22nd would have left some relics here. This of course had to be explained away and here is that explanation presented by John Van Seters:

“‘The city of Piramesse was largely abandoned at the end of the 20th Dynasty, probably because of the silting-up of the waterway on which it was located [for trade] and the shift of the marine traffic to a new watercourse through Tanis. It was at this time that Tanis became the new capital of the 21st Dynasty. Piramesse became a quarry for valuable stone blocks and monuments to be used at Tanis and other sites, especially Bubastis. Yet the name and remembrance of Piramesse did not entirely disappear. It appears in a list of place-names of the 21st Dynasty date, along with Tanis. Under Sheshonq I (Shishak) of the 22nd the city of Piramesse seems to have had a brief revival …’

That is, for 600 years after Sheshonq I, Piramesse was abandoned and even over this lengthy period an appreciable layer of debris should have accumulated between the Ptolemies and these dynasties. No such report, as far as I have been able to research, has been put forward to contradict Velikovsky’s placement of Ramses III in 390 B.C.”

The 20th Dynasty stratum lies directly below that of the Ptolemaic Dynasty, which fully covers the entire 400-200 B.C. period that Heinsohn maintains does not exist. We have employed the very same form of stratigraphical evidence that Heinsohn employed, but in Egypt this evidence clearly contradicts the

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108 ibid.
stratigraphical evidence he has presented for other regions. How can these two centuries exist in Egypt, based on stratigraphical evidence, and somehow not exist elsewhere? This needs to be addressed. More than that, the Egyptian stratigraphy agrees with and correlates with the astronomical evidence that shows these two centuries do exist. Heinsohn further argues:

“Numismatic evidence provides the strongest hints that the suspicious 200 years are indeed not real, but a phantom period which has to be deleted from history all the way from Portugal to Japan. Athens, of all places, is without any change in coinage from 404 BC to a maximum of 180 BC. Coin experts were always surprised by the unchanged appearance and the hardly-changing value of Athenian coins from the 6th to the early 2nd century BC. In my view, coins covering a maximum of 200 years were stretched [to cover] 500 years to furnish two to three blank centuries before the 2nd century BC brings the first variations in coinage and value.”

But we do have the coins of the Seleucid/Greek kings that reigned in Babylon from 321 B.C. through the rest of Heinsohn’s phantom centuries. Edward Theodore Newell points out:

“A first sight the vast material remains [of the Seleucid/Greek Dynasty in Babylon] seem to present almost insurmountable difficulties, but the inducements offered to students and historians to solve the problem are correspondingly many. The Seleucid coinage in particular is closely associated with, and therefore partakes of, the vicissitudes of the many [Seleucid] rulers who issued it; its long and splendid line of living portraits not only of legitimate scions of the House of Seleucus but also [coins of] usurping regents and rebellious satraps such as Achaeus, Timarchus, Typhon, and others—give it a vital and human interest that is not surprising in Greek Numismatics. Its many mints of long existence give it a variety of types that is most attractive.”

Newell goes on to describe certain of these Seleucid coins on pages 4 and 7, dating coins to Seleucus II and Seleucus III who lived in the 3rd century B.C., alongside the Ptolemaic Egyptian Dynasty, the century Heinsohn claims does not exist. What is most important in terms of stratigraphy: Newell claims that the Seleucid Dynasty has left “vast remains.” Heinsohn has claimed that the Old Babylonians and New-Assyrians lived in Greek times, i.e. the time of the Seleucids, because their strata lie directly beneath those of the Greeks. This,

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110 Heinsohn, op.cit., p. 8
therefore, presents stratigraphy for the 5th century B.C. as well, the century Heinsohn claims does not exist. In both Babylonia, Neo-Assyria, and Tell Ed-Daba, there is stratigraphical evidence contrary to Heinsohn’s assertion that these centuries do not exist. In terms of numismatics, there is in Egypt a series of Ptolemaic coins running from the founder of that dynasty, Ptolemy I Soter, who reigned 306-283/282 B.C., down to and beyond Ptolemy V Epiphanes who reigned 204-180 B.C. and beyond. Like the coins of the Seleucids, these prove the 3rd century, 300-200 B.C., exists contrary to Heinsohn. To find out what these coins are, go to Ptolemy I down to Ptolemy XII on Wikipedia on the Internet which shows the gold coins of these various kings. In both the case of the Seleucids and the Ptolemies, these coins are found in hoards.

While Illig has shown that many documents of his phantom centuries are forgeries, we question the concept that counterfeiters would have produced gold coins with the faces of kings that in terms of Heinsohn’s chronology should not have existed. Counterfeiters never produce gold coins in place of other gold coins. There is no profit to be gained by such a measure. That would be similar to minting gold coins during the 1800’s A.D. that are the same size and weight in gold as others.

In addition the Ptolemaic kings had statues carved of themselves and inscribed their names in hieroglyphics on temple walls. For example, Alan K. Bowman writes:

“...Ptolemy I Soter...was nominally the satrap, first of Alexander’s half-brother Philip Arrhidaeus and then of his son, Alexander IV. But the great hieroglyphic ‘Satrap Stele’ which he had inscribed in 311 BC indicates a degree of self-confidence transcending his viceregal role: ‘I, Ptolemy, the satrap, I restore to Horus...the territory of Patanut, from this day forth for ever, with all its villages, all its towns, all its inhabitants, all its fields.’ The [hieroglyphic] inscription emphasizes Ptolemy’s own role in wresting the [Egyptian] land from the hated Persians...”

Why would the first of the Ptolemy pharaohs issue gold coins and erect a stele on which his name appears in hieroglyphics and say he rid Egypt of the hated Persians? With regard to Ptolemy II Philadelphus and his wife Arsinoe II, Bowman

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shows a bas-relief on page 23 which depicts both with their names inscribed in hieroglyphics denoting their rulership.

More than that, the Ptolemies erected temples, as discussed by John Anthony West: “For the visitor [to Egypt], the Ptolemaic temples are particularly interesting because several of them are so very well preserved…though far smaller and less significant than earlier works…”\textsuperscript{113} Gyözö Vörös describes the earliest of these Ptolemaic temples, speaking of the “stones of the Sharuna temple of Ptolemy I [which] belong to the earliest surviving pieces from the last great age of Egyptian temple architecture, the era of the Ptolemaic Empire.”\textsuperscript{114}

What we have is precise astronomical (VAT 4956) data from Babylon before Heinsohn’s phantom centuries, with an accurate, well-described and dated eclipse at Babylon during Heinsohn’s phantom centuries in 322 B.C., and in 136 B.C., the most precisely delineated eclipse known after Heinsohn’s phantom centuries, all showing these centuries existed. We also have stratigraphy at Babylon showing that the Old Babylonians/Persians lay directly beneath the Seleucid Greeks. In addition, we have coinage used at Babylon during the 4th century B.C. showing the period 300-200 B.C. existed. As with what we have shown in Ireland to dispute Illig, we have the people, the strata, and the precise astronomical data all at one place that contradict Heinsohn’s hypothesis, along with 4th century Seleucid coinage, that prove these centuries exist. The totality of these different forms of evidence clearly disproves Heinsohn’s thesis. We have met him on his own grounds—stratigraphy—and answered his challenge by presenting precise astronomical evidence of the celestial coordinates of Mercury, Venus, Mars, Jupiter, Saturn, and the Moon in ancient times that precisely correlate with their coordinates today. Moreover, we have presented Egyptian stratigraphy at Tell Ed-Daba, the type of evidence Heinsohn holds above all others, that also support our chronology and contradict his, with a series of Ptolemaic coins that do the same and, to paraphrase Heinsohn, “Numismatic evidence provides the strongest hints that the suspicious 200 years are indeed…real.” Are we to conclude that all this evidence for the reality of this 200-year period at Babylon can be swept aside? As we stated in our discussion of Illig, above, we need only present stratigraphical and precise astronomical evidence for one place on Earth that proves these supposed non-existent centuries do exist, and we have supplied that evidence for Ireland and

\textsuperscript{113} John Anthony West, \textit{The Traveler’s Key to Ancient Egypt} (Wheaton IL 1985), p. 26
\textsuperscript{114} Gyözö Vörös, \textit{Egyptian Temple Architecture: 100 years of Hungarian Excavations in Egypt} (Budapest, Hungary 2007), p. IV
Babylon. To paraphrase Heinsohn once more: “by presenting hard evidence for [the existence of] this combined time-span of 500 years, any chronology which tries to reinstate these phantom centuries, and simultaneously ignores the evidence we have presented in answer to Heinsohn and Illig will be seriously flawed.”
CONCLUSION

“History without the support of science sees nothing real”
Charles Ginenthal

Sir Fred Hoyle has written two seemingly contradictory statements regarding research. “To achieve anything really worthwhile in research, it is necessary to go against the opinion of one’s fellows.”\(^{115}\) He further points out: “It is also easy, when doing research only, to become fixated by something and then go steadily more and more wrong…”\(^{116}\) The questions are: When does one go against the opinion of one’s fellows? And how does one know the fixated theory is going steadily more and more wrong? The answer to both is to use science to see if one’s fellows, when doing research, have gone steadily more and more wrong. This is no guarantee that one’s own theory does not suffer from these same problems, but when more and more scientific evidence goes against one’s fellows, while more and more science supports one’s own research, one is justified in going against fellow researchers and sticking to one’s thesis, in this case our chronology.

In these four volumes of Pillars of the Past we have certainly done just that, employing scientific evidence we have undoubtedly shown it contradicts the established chronology more and more, but corroborates the short chronology more and more also. Furthermore, we have scientifically demonstrated the other historical/chronological revisionists have theories that suffer with these same scientific problems/contradictions. While we have learned a great deal from both the proponents of the established chronology and the revisionists, we have opted to follow them only when the science clearly supported any aspect of their hypotheses, and rejected them when it clearly contradicted them more and more and more.

To date neither group has evidenced any indication of answering this massive compilation of scientific evidence except to raise imaginary concepts without scientific proofs that we are wrong. We, on the other hand, have taken their criticisms seriously and have shown these to be false, based on science, along with technology and other forms of evidence. We strongly doubt that our critics can

\(^{115}\) Hoyle, op.cit., p. 235
\(^{116}\) Ibid.
answer what we have presented and that they therefore will particularly ignore the strongest scientific elements that uphold our chronological revision and contradict theirs. Until they do so in the arena of full scholarly/scientific debate, our chronological revision stands. We have thrown down the gauntlet and challenge them to pick it up. Failure to do so is a tacit admission that they haven’t any science as support. We have produced in these volumes a great mountain of such support and therefore claim that this mountain is far too daunting to attempt to scale. From where we now stand, at the pinnacle of all this scientific evidence, we see our critics in the valley below neither able to scale the summit, nor willing to do so.

We hope that those researchers who understand the depth and breadth of this work, and who agree with it, will take into their hands our work honestly and expand it, and, if the science leads elsewhere, correct it. At least, that is our hope. We have laid ancient history, prehistory, and chronology on new, solid foundations and pillars of science. These, we submit, cannot be toppled. We have taken history and chronology out of the hands of historians and archaeologists and placed them firmly in the hands of scientists, making the entire field, as far as possible, a branch of science. The historians and archaeologists have said that this is what they wanted their discipline to become, and it has become just that in these volumes of *Pillars of the Past*. Their opposition to this new methodological approach was well summarized by Mark A. Runco:

“Dietrich concluded that ‘It seems that as we age, a certain vision of reality becomes so “Hardwired” through decades of reinforcement that the continuous diminishing ability for cognitive flexibility is overpowered,’ or, in Nietzsche’s words, ‘convictions are greater enemies of truth than lies.’”

It is these convictions that our work has undermined and destroyed. Against this wall of chronological conviction stand a host of proponents for the established chronology ready and willing to defend it to the last. They have built imaginary Maginot Lines to keep outsiders from taking their fortress. Tragically, they cannot see that their defense system, that denies our evidence, will leave them to muddle about historical/chronological questions and problems that permeate their field. They will undoubtedly continue, in vain, to pour forth millions of pages, to create one thesis on top of another as they wrestle in an endless struggle to make history scientific without recourse to science. In that regard, their efforts are doomed to

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117 Mark A. Runco, *Creativity: Theories and Themes* (Amsterdam, the Netherlands/Boston MA 2007), p. 245
appear to them to succeed only for the moment and to fail because it is not, nor ever was, science. Rather than building a history/chronology based on science, they will continue to “create” historical chronology on foundations that exist nowhere in reality.
SUPPLEMENT

THE “LAND OF PUNT” REDUX

Lewis M. Greenberg

INTRODUCTION

It has been sixty years since the publication of *Ages in Chaos* wherein Velikovsky equated the female Pharaoh Hatshepsut with the Queen of Sheba, who purportedly visited the court of King Solomon, and the land of Punt with Phoenicia/Palestine. (1) Many scholarly discussions of the subject have taken place during the nearly past forty years with pro and con positions forcefully put forward by both sides. (2)

Most recently, Emmet Sweeney has written a brief two-page article in *C&C WORKSHOP* reiterating his earlier support for Velikovsky’s identification. (3) Ironically, in the very same issue of *C&CW*, Sweeney wrote a letter castigating his fellow SIS “contributors [for] completely ignoring the work of SIS members and associates”. (4) Yet, as is his wont, Sweeney himself continues to neglect and overlook the scholarly material on the Punt/Sheba subject proffered by those who put pen to paper some thirty years before he began to publish his own ideas on the matter.

Worse still, he and many of his British colleagues of both a heterodox and orthodox persuasion consistently fail to cite – in a number of instances – the scholarly publications of their Western hemispheric counterparts even when they are well aware of that scholarship. Arguably, one of the worst – if not the worst – of the offenders when it comes to citing revisionist literature is David Rohl for whom *Pensee, KRONOS, AEON*, and even many of the published articles in the *SISR* might just as well have never existed. (5)

A select example from conventional scholarship should also suffice to make the point: The youthful Pharaoh Smenkhkare, generally accepted as the brother of Tutankhamen, has been so marginalized of late by the likes of Rohl, Reeves, and Wilkinson that he has become a non-person in favor of and replaced by a resurrected Nefertiti with a supposed masculine nomenclature. (6) Yet, the Canadian Egyptologist Donald Redford, for one, closed the door on this far-fetched hypothesis years ago. (7) But, by ignoring Redford and eliminating Smenkhkare from the picture, the aforementioned individuals with the aid of some forensic juggling can now
claim that the mummy found in KV55 is none other than the heretic Pharaoh Akhenaten which is who they want it to be.(8)

With that said, the information presented below is intended to bring new material support and a new perspective to the carefully considered thesis proposed so long ago by Immanuel Velikovsky – that the female Pharaoh Hatshepsut was indeed one and the same as the Queen of Sheba, and the land of Punt was one and the same as Phoenicia/Palestine. And to all those in the past who have refused to address or even acknowledge the published work of their international colleagues, you are free to ignore the essay presented here as well. But you do so at your own peril. The days of “intellectual usurpation” are over.

A MEMORABLE JOURNEY

Whether Hatshepsut physically participated in her expedition to Punt has been a subject of debate for decades. Yet clearly, the visual prominence of this journey – as depicted on the walls of Hatshepsut’s own funerary temple at Deir el-Bahari – combined with her own exuberant pronouncements regarding the beauty, wonder, and glory of “God’s Land” (Punt) makes it a highly reasonable conjecture that she did in fact actually travel to Punt. Without doubt, it was one if not the highlight of her reign.

Velikovsky’s seminal evidence for identifying Hatshepsut with Sheba, Punt with Phoenicia/Palestine, followed by the supportive scholarship of Danelius and Sweeney may be consulted in the appropriate references cited below. For now, what follows is both a new and expanded interpretation of the meaning and significance of “God’s Land” which should further emphasize the geographical reality and importance of Hatshepsut’s voyage to Punt.

As we shall soon see, Hatshepsut’s visit to the land of Punt was not merely for the purpose of courtly politics or mercantile and sight seeing activity. On the contrary, her sojourn to Punt may have had relevance of a far more personal import. Velikovsky was the first to offer a clue when he noted that “Queen Hatshepsut undertook the journey like a devout pilgrim”.(1) Sweeney recently echoed his words: “Hatshepsut was a great devotee of the goddess Hathor and her journey to Punt was in large part a pilgrimage to the land of her tutelary deity”, Hathor.(2) Sweeney then proceeded to emphasize Hatshepsut’s devotion to the goddess Hathor which explains the queen’s “almost mystical love for [Punt]”.(3) In time, Hatshepsut actually identified herself with the goddess and eventually codified her divine status on the hypostyle walls at Deir el-Bahari.(4)

However true all that may be, there may have been a more profound reason for the Punt expedition – one that involved a religio-primordial scenario of even greater significance for Hatshepsut; and it appears that she intended to emulate and relive it.
HATSHEPSUT, HATHOR, AND ISIS

One of the oldest and greatest gods in the Egyptian pantheon was Osiris who was slain through the jealous machinations of his brother Seth. Tricked into allowing himself to be sealed alive in a beautiful form fitting chest, what then became his coffin was cast upon the waters and ultimately washed ashore in the land of Byblos (Phoenicia) where it came to rest in a heath/tamarisk tree. Osiris’s sister-wife Isis relentlessly searched for her lost husband and eventually learned of the chest’s whereabouts. She went to Byblos, retrieved the chest containing the body of Osiris, placed it in a boat, and set sail presumably via the Mediterranean Sea back to Egypt. (5) The balance of Osiris’s story need not concern us here. It is noteworthy that by the time of Tuthmosis III, Hatshepsut’s sometime co-regent and successor, the myth of Osiris and Isis had taken on the form which it retained for the rest of Egyptian history.

What is most relevant here is the fact that Hatshepsut, through a complex iconography, became merged with Isis. “By the early 18th Dynasty, queens were...portrayed wearing a range of distinctive crowns”, and the goddesses Isis and Hathor were depicted wearing similar crowns comparable to one worn by Hatshepsut. Thus, “the distinction between the mortal queen [Hatshepsut] and the immortal goddesses becomes deliberately blurred.” (6) In fact, “Hathor, in her greatness absorbed other goddesses throughout her history but in the end assimilated to Isis.” (7) Furthermore, “the identification of the goddess Hathor with Isis enhanced the latter’s reputation at sea, since Hathor was herself known as ‘Mistress of the Barque’, as well as ‘Lady of Byblos’.” (8) “The cult of Hathor at Byblos apparently arose out of trading contacts [with] the Levant.” (9)

Hathor was referred to at times as “Lady of Byblos” due to the way she was depicted and her similarity to Baalat-Gebal, a local goddess who was worshipped there. In addition, there are “numerous monuments to Hathor in Egypt which cite her as Lady of Byblos and also Lady of Punt,” though the author of this statement made no connection and separates the two geographically. (10) Yet, is this not similar to the statement of an Old Kingdom official who claimed to have visited Byblos and Punt eleven times? (11)

Tyldesley also cites Hathor “in her role as ‘Mistress of Punt’” (12) and emphasizes the fact that Hatshepsut’s “famous expedition to Punt, clearly one of the highlights of her reign, should not be seen as an isolated event but as the climax of a series of trading missions which included visits to Phoenicia to collect the wood which Egypt so badly needed to build her ships...” (13) Tyldesley, too, made no connection between the two regions. Yet, even on the walls of the Speos Artemidos temple we can read: “Punt overflows for me on the fields, its trees bearing fresh myrrh.” (14) Does this describe southeastern Africa or southwestern Arabia?

Isis and Hathor were also interchangeable as the mother of Horus or the Horus king of Egypt; (15) and occasionally Hathor was considered to be “Mistress of All the Gods” as was Isis. (16) As such, “Hathor would thus appear to be an alter ego of Isis”. (17) The linkage between the two goddesses continued throughout Egyptian history. Some queens of Egypt, particularly
the principal queens and mother to the heir of the throne, were also considered divine and were either equated with Hathor or Isis.(18)

The power, adoration, and longevity of Isis endured even through Ptolemaic and Roman times and rested on ancient precedents. Cleopatra, last of the pharaohs, actually presented herself as the living or “New Isis”, Isis incarnate.(19)

She was also depicted as Hathor, with disk and horns, on the rear wall of the temple at Dendera.(20) If it were not for the ruined state of Hatshepsut’s mortuary complex at Deir el-Bahari, we could almost imagine it as the inspirational source of Cleopatra’s divine status and iconography. Unfortunately, the would-be model was long gone, defaced by time and vindictive successors.(21)

The conflation of Hathor, Isis, and Hatshepsut is now fully established. We must now turn our attention to Osiris and Punt in order to identify the latter as the land of Osiris and disclose the deeper meaning that it holds.

**THE LAND OF OSIRIS**

Punt was also known as “God’s Land” or the “Divine Land”. The question inevitably arises as to which god, if any in particular, does this epithet apply. Fortunately, the answer has already been provided by Sweeney following Rohl:(1)

“…the Egyptians themselves specifically connected Osiris with Phoenicia. We know also that it was in honour of Osiris (Netger) that the Divine Land (Ta Netger) received its name [and] that this was the appellation applied by the Egyptians also to Punt…”(2)

More importantly, Sweeney has also noted that “the word netjer or ‘god’ in Egyptian is said to be related to the word natron (Greek natrin), a substance used in the embalming process….Now according to Egyptian tradition the first being to be mummified was the god Osiris…”(3) What is especially interesting is a statement in the Pyramid texts translated by Budge: “The Natron standeth up at the head of the Great Company of the Gods.”(4) As king of the gods this phrase can only be associated with Osiris or one of his incarnations. Thus, we may conjecture that Osiris was conceptually merged with the very substance that preserved his mortal form; and, since some of that material specifically came from the cedars of Lebanon, this is yet another reason why Byblos and Phoenicia were sacred to Osiris and his cult.(5)

Morenz, however, throws in a word of caution.

“It is impossible to substantiate the theory that ntr [neter] is derived from the word ntr , ‘natron’,… Nor can it be ruled out that ntr may not originally have been a generic name (appellative) at all, but rather a proper name. In any case it is striking that the most human of gods, Osiris, is called ntr in a particular context: in quite a number of puns this word is used almost as though it were his name.”(6)
A bit later on, however, Morenz has this to say: “all over the world it is normal for persons to be given names which denote their nature, characteristics or function.”(7) Neter-Natron would therefore be a most appropriate name for Osiris.

From all of the above, Sweeney rightly concluded that the answer to the question – “Which god is Ta Netjer, the Land of? The answer is Osiris: And his land is Byblos and Phoenicia….Punt is the Land of Netjer (Osiris), and the Land of Netjer is Palestine/Lebanon.”(8)

The probability aside that it was mainly the tale involving the casket containing the mortal remains of Osiris landing in Byblos, among other things, that gave rise to the concept of Phoenicia/Palestine being the Divine Land (Ta Netjer) or the Land of Osiris, there is other possible evidence associating Osiris with Phoenicia/Palestine.

One of the most ancient names associated with Jerusalem is the term Zion. First applied to a fortress in that city, it was expanded to designate the Temple Mount, further expanded to refer to Jerusalem itself, and finally to the people of Israel as a whole. However, “the larger significance of the biblical term ‘Zion’ lies not in the domain of topography but in that of theology…The term evokes a whole range of concepts having to do with the kingship, might, justice, and faithfulness of YHWH…”(9)

The name Zion first appears in the narrative of David’s conquest of Jerusalem.

Following his victory, he then substituted the expression “City of David” for that of Zion, though the latter survived in various incarnations as elucidated above. The most important aspect of the term Zion is the fact that it ultimately came to represent the very land of Israel and its civilization.(10) Yet, for all that, the etymology of the name is uncertain. Both Hebrew and Arabic roots have been explored but remain hypothetical.(11) An alternative approach to the meaning of Zion was taken by Ahmed Osman.(12) According to Osman, the name “Zion is not an original Hebrew word but consists of two elements, one Hebrew, the other Egyptian. The Hebrew first element, ‘Zi’ means ‘a land of drought, a barren place’. ‘On’ is the biblical name of the ancient holy city known from Greek as Heliopolis…” The joining together of “Zi” and “On”, the result of a minor linguistic adjustment produced “Zion” whose original meaning – “the On (holy place) of the desert” – was obscured.

A key point which Osman overlooked, however, is that On or An is an alternate name for the great god Osiris.(13) Therefore, it is possible to translate Zion as “the Desert Place of Osiris”. Of even greater significance is the fact that Osiris was – among other attributes – a Saturnian deity, identified with the planet Saturn.(14) Additionally, Jerusalem – which means “the foundation of Shalem” – was also “the city of Saturn” based upon the planetary identification of the god Shalem.(15) Jerusalem’s other names, “City of David”, “Kadesh (Qadesh)” – which means Holy – and “el-Kuds or Al-quds” in Arabic likewise all point in the same direction.(16) Furthermore,

“It is likely that in the cult of Jerusalem, eyon (‘the most high’) was another name for Shalem. Gen.14:18-20 indicates that the god of the city was invoked as el eyon (‘God Most High’)…Saturn was…called ‘the most high’.”(17)
The city of Byblos (Gebal) was also regarded as a holy city whose primary deity was the god ‘El (Saturn). (18) Indeed, Saturn was the chief deity of the Phoenicians as he was for so many peoples. (19) Up until the Middle Ages, the Jews were still remembered as the people or children of Saturn. (20) Human sacrifice was brought to him in the guise of Moloch who consumed numerous children in the “divine” fire of his dreaded persona. (21)

From all of the above, we may conclude that Phoenicia/Palestine was emphatically the “Land of Saturn” or the “Domain of Saturn”.

Queen Hatshepsut’s voyage/pilgrimage to Punt now comes into a possibly much clearer and more meaningful focus. As the “living Hathor/Isis”, Hatshepsut was “rehearsing” or “reenacting” the journey of her divine namesake and predecessor – the goddess Isis – who came to Byblos in search of her lost husband Osiris. Not only did Hatshepsut “return” to the “Land of Punt”, her meeting with Solomon as the Queen (of) Sheba brought her into contact with Osiran/Saturnian symbolism and geography. As stated above, Jerusalem was symbolically, theologically, and interpretatively the “City of Osiris/Saturn” and Phoenicia/Palestine was the “Land of Osiris/Saturn”. And even the name of Hatshepsut/Sheba’s host – Solomon – had relevant theophoric implications.

While Solomon was the throne name of Israel’s third king, called so by his father David, he was earlier given the name Jedidiah by the prophet Nathan. (22)

Interestingly, the name Jedidiah (“beloved of the lord”) contains the divine component Dod or Dah, a Saturnian deity (see below) while the name Solomon contains the divine component Shalem who was also a Saturnian deity. (23) Thus, the very meeting of Solomon and Sheba may have had archetypal religious import. (23a) This would lend additional support for the theory that the expedition to Punt had wide ranging and far greater significance than has been realized to date.

A final note has to do with Solomon’s wisdom and the Queen of Sheba’s testing of it.

“When she had concluded her series of tests, she not only affirmed the accuracy of the reports which had come to her ‘of your affairs and of your wisdom’…, but said…blessed the Lord who ‘made you king, that you may execute justice and righteousness’… Here we have a clear turn from riddles to the ability to govern judiciously.” (24)

Coincidentally, it was Saturnian deities who formulated the laws of justice and were its dispensers. (25)

For now, we may comfortably posit that Phoenicia/Palestine – during the reign of Queen Hatshepsut – was indeed the “land of Punt” and fulfills the most germane and sensible parameters for that identification. Yet, while this might appear to be enough – and enough it is – there is still much more supportive material to be presented below which should make this supposition even more decisive.
MAKERA AND MAKE-DA

The throne name of Queen Hatshepsut as it is inscribed on the Punt reliefs has been read as Maatkare by Tyldesley. Velikovsky wrote it as Make-ra (2) which is just as permissible though Tyldesley’s spelling may now be more conventional. Wells presented it as Makere (Kamere)(3) and Gardiner noted that “two God’s Wives” of later times were named Makare.(4) As Tyldesley has pointedly noted:

“…Naville, Buttes and other turn-of-the-[20th] century egyptologists reverse Hatchepsut’s throne-name Maatkre to read as Kamara. Unfortunately for modern readers, the ancient Egyptians wrote their hieroglyphic texts with no weak vowels and with an assortment of consonants not found in our modern alphabet, so the correct pronunciation of any Egyptian name must be a matter of educated guesswork.”(5)

In any event, the rationale for bringing up the throne name – Makera – of Hatshepsut has to do with the interesting fact, as mentioned by Velikovsky, that in Ethiopian legend the Queen of the South (Sheba) is called Makeda.(6)

“The main part of the name of the Egyptian queen is identical with the first two syllables in the name of the Queen of the South. It was preserved in the Ethiopian tradition; it did not come from the Scriptures.”(7)

It is now appropriate to bring up two thought provoking items regarding an Hatshepsut-Sheba equation:

I) As Velikovsky observed, the last part of Make-ra – “Ra” – is the divine name of a god, and the last part of Makeda – “Da” – “could be the divine name Adad or Ada [in shortened form], which is part of several scriptural names.”(8) Actually, it would appear that the name Makeda is Hebrew and, most recently, is spelled Makkedah in that language.(9) Therefore, the shortened ending “Da” or “Dah” might refer to the name Dod, Dodo, or Daud (all alternates for David) which is but another name for Adonis or Tammuz (10) both of whom were divine Saturnian counterparts of Osiris (see above).(11) Since the great god Ra has also been identified as a Saturnian deity (12) with linkage to Osiris (13), the names Maatkare (Make-ra) and Maakedah (Make-da) could be theophorically interchangeable which further enhances their identification as one and the same person. En passant, calling Jerusalem “the City of David” further reinforces the idea of that city being “the City of Saturn”.

II) One of the titles of the Queen (of) Sheba was “Queen of the South”.(14) Much later, because of her relationship to the city and island of Philae, Isis of the myriad names bore the special titles “Queen of the South” and “Queen of the Southern Peoples”.(15) The melding of Hatshepsut, Hathor, and Isis previously elucidated would also make Hatshepsut a “Queen of the South” – a title that squares with Josephus’ statement pertaining to the story of the Queen of Sheba:

“Now the woman who at that time ruled as queen of Egypt and Ethiopia…”(16)
Sweeney has also proffered additional evidence that Hatshepsut “was very definitely a queen of the South…whether or not Hatshepsut was the Queen of the South.”(17)

OF ANIMALS, AQUATICS, AND PHARAOHS

In a trenchant exposition, John Bimson debunked the Hatshepsut-Sheba equation in a lengthy 1986 article published by the Society for Interdisciplinary Studies (England).(1) Bimson raised many provocative points, the majority of which have already been rebutted by both Sweeney and Ginenthal:(1a)

I) Bimson dismissed Danelius’ proposal that Sheba of the Bible, transcribed as “Saba” in Greek, was the name of the capital of Ethiopia at that time and rejects the further proposal “that Sheba was once a homonym for Ethiopia”(2). Bimson believes that the fact that the visiting queen was “not described in the Bible as the ruler of Egypt” is telling; and then compares this to the US President merely being referred to as “(e.g.) President of Alaska”. However, the terms White House, Washington, or Potus have been used as substitutes for the US President on many an occasion (sometimes, even just “President XXXXX” suffices).

Furthermore, Sweeney, following Danelius’ lead, moved the Hatshepsut-Sheba debate in the direction of Thebes, Egypt’s capital city. With the assistance of Lisa Liel, Sweeney argued that linguistically Thebes’ Egyptian name was actually Shewa (Sheba). Thus, calling Hatshepsut the “Queen of Sheba” was quite sensible. He also stressed the fact that in Josephus’ time “‘Ethiopia’ was the name given to Nubia”, the northern part of which was firmly under Egyptian rule.(3)

Bimson also failed to consider the fact that Hiram – Solomon’s friend and ally – was known as Hiram of Tyre, not Hiram of Phoenicia.

II) Camels and gifts: Bimson contends that “the camels with which the Queen of Sheba and her retinue made their journey to Jerusalem are also anomalous in the context of Velikovsky’s identification.” He also focused on the fact that various items given and received by that Queen are not “mentioned in [Hatshepsut’s] record of the expedition” which her scribes would have been expected to list. Yet, as Bimson himself admits, “the gifts and the camels mentioned in the biblical account are perhaps not insurmountable obstacles to Velikovsky’s identification…”(4) Indeed. Both Sweeney’s full rebuttal to Bimson in an online article (5) and Meeks’ discussion of animal types in the Deir el-Bahari reliefs (6) easily nullifies Bimson’s objection. First of all, the Bible states that the Queen of Sheba came “with camels bearing spices, and very much gold, and precious stones.” It does not say that the Queen rode or sat on or was carried by any camels. As for the various animals that Hatshepsut’s artists displayed on her temple walls, since Solomon is known to have received apes and other exotic items from afar, and had an arboretum, he no doubt had a zoological garden as well which would explain the variety of animal types depicted on the temple walls of Deir el-Bahari and their possible provenance.(7)

As for the mutual exchange of gifts: after the Queen of Sheba “expressed to Solomon her amazement at his wisdom, achievements, and wealth (1Kgs 10:6-8; 2 Chr 9:57)” “thereupon she gave the king an enormous amount of gold, spices, and precious stones (1Kgs 10:10; 2 Chr 9:9).
King Solomon [then] gave her an even greater amount in return, whereupon she left with her retinue for her own country (1Kgs 10:13; 2Chr 9:12)” [emphasis added].

An interesting and relevant variant to the paragraph above was offered by Philby (p. 30):

“And King Solomon gave to the queen of Sheba all that she desired, whatever she asked besides what was given her by the bounty of King Solomon (she had brought to the king). So she turned and went back to her own land, with her servants.”

“Thus runs the simple story as related in the Old Testament in its two recensions. In the text as this is set out above, the Kings passage has been used as a basis, since it was the first to be written, but the variants are indicated thus: words or phrases not found in Chronicles have been given in italics, and words or phrases found only in Chronicles have been placed in brackets. The text used here is that of the Revised Standard Version. It will be obvious to every reader that the variations range between the minor and the insignificant. Even the gifts emanating from Solomon’s bounty may indicate no more than ‘a remission of gifts brought by the queen herself’” [emphasis added].

When it comes to gift giving, why would one enumerate what was given by one’s host if it overshadowed that of the visitor?(8) The gifts Hatshepsut received that might have exceeded the lavishness of what she gave could have influenced their omission from her own presentment “bill of lading” to Solomon. Those gifts that Hatshepsut brought and were listed by the scribes highlighted the unique products of Egypt. It would be pointless, even unseemly, to list identical gifts of exchange when those of Solomon outdid her own.(9) As Velikovsky vividly put it: “Solomon and Hatshepsut vied with each other not only in gifts but also in appreciation of the generosity of the other.”(10) As for spices, the ones given to Solomon were not necessarily the same as those received. Bimson turned a pepper corn into a cornfield.

An important overlooked clue identifying Punt with Syria-Palestine (which would put Phoenicia in its orbit) is a reference in the Punt reliefs to the “green gold of the land of Amu” which Hatshepsut also received among the many gifts given to her.(11) What is noteworthy here, as Velikovsky pointed out, is that the Amu were identifiable with the Hyksos.(12) Further, Velikovsky also equated the Amu with the Amelekites who originated in Arabia and came to occupy and dominate Syria and Egypt, thereby establishing a pharaonic dynasty in the latter.(13) The Amu were known to the Egyptians as late as the early Eighteenth Dynasty and are mentioned in an inscription of Queen Hatshepsut at Speos Artemidos.(14) According to Van Seters, Amu “designates the Amurrite population of Syria-Palestine in the Middle Bronze Age” and the usage of the term Amu continues well into the New Kingdom and was considered “the most important ethnic component of Syria-Palestine at the time”.(15) Strong Amurrite-Canaanite cultural influences, a result of the Hyksos legacy, are evident in the number and importance of Semitic deities and place-names found in the Nile Delta down to the reign of Tuthmosis III.(16)

In a discussion of his own chronological revision for the time of Solomon and Hiram, Rohl mentioned contemporary Egyptian texts which “simply refer to the people of coastal Lebanon/Syria as ‘Asiatics’ (Egy. Amu).”(17)
The reference to “the green gold of the Amu” should certainly help to solidify the identification of Punt with Phoenicia-Palestine. But, there is more.

Bimson (18) disputes Velikovsky’s suggestion that the name Sheba (Shwa in Hebrew) “might be derived from the last part of the name Hatshepsut” which could be spelled Hatshepsowet with the final t in her name being silent.(19) However, earlier in Ages in Chaos, Velikovsky stated that “the opinion is expressed in the Talmud that ‘Sheba’ in the name Queen of Sheba is not a geographical designation but a personal name.”(20) Bimson, citing Day, disputes this as well by claiming that “the title ‘queen’ is in the construct state in the Hebrew (Queen of Sheba), which would not be the case with a personal”. (21) In an interpretive contest between Velikovsky and the Talmud on the one hand and Bimson and Day on the other, regarding the Hebrew language, I would accede to the former. Besides, Solomon’s mother was named Bathsheba and the second part speaks for itself.

III) Punt as a southern country: Here Bimson admits that “it is possible that the title ‘Punt’ was used differently in different periods”.(22) True enough as we shall see below. What is of immediate interest here is the fact that the distance from the Theban area to a point in Nubia is almost exactly the same as the distance to a key point in Palestine.(23) Hatshepsut’s statement that “my southern boundary is as far as the lands of Punt…” could have been a mere figure of speech.(23a) More importantly, Meeks has amassed considerable data that reveals:

> “Texts locating Punt beyond doubt to the south are in the minority, but they are the only ones cited in the current consensus on the location of the ‘country’. All the other texts, despite their large number, have been ignored. Punt, we are told by the Egyptians, is situated – in relation to the Nile Valley – both to the north, in contact with the countries of the Near East of the Mediterranean area and also to the east or southeast, while its furthest borders are far away to the south.”(24)

In the end, Meeks is forced to conclude that “only the Arabian Peninsula satisfies all these indications”. Yet, his conclusion does not preclude the possibility that Hatshepsut traveled to the region of Phoenicia-Palestine.

IV) Lack of reference to Egypt: This objection has already been addressed in part by Sweeney (see supra). Now we must deal with Bimson’s specific question as to why “a visit from the Queen of Egypt should not have been described as such if it really took place” and why Biblical references to Egyptian pharaohs at this time do refer explicitly to “the King of Egypt”.(25) Simply put, the problem here may be one of gender.

Hatshepsut wasn’t acknowledged to be a king of Egypt until the seventh year of her regency;(26) and there was “no formal Egyptian word for ‘queen’” since female royalty were known as “either a ‘King’s Wife’ or a ‘King’s Great Wife’ [or] ‘King’s Mother’ [or] ‘King’s Daughter’”.(27)
“An Egyptian queen regnant simply had to be known as ‘king’; she had no other title.” (28)

Faced with this restrictive problem and still wishing to be duly respectful, Hebrew scribes may have concluded that the epithet “Queen (of ) Sheba” was a satisfactory solution. Hatshepsut’s femininity, political status, and primary geographical power base (Thebes/Sheba) would have all been conjoined without violating the fundamental regnal traditions of Ancient Egypt. Additionally, the absence of any female figure as sole ruler of any ancient Hebrew kingdom may also have posed some awkwardness to the scribal position of the host, thereby requiring an unorthodox compromise.

A final point of speculation has to do with the fact that Hatshepsut’s Punt expedition took place in her ninth regnal year, only two years after being acknowledged “King” of Egypt. (29) It may have been too confusing, undesirable, or premature for her to present herself in that guise to a foreign ruler. Gardiner summed up Hatshepsut’s ambivalent would-be appearance quite nicely:

“Twice before in Egypt’s earlier history a queen had usurped the kingship, but it was a wholly new departure for a female to pose and dress as a man. The change did not come about without some hesitation, because there is at least one relief where she appears as King of Upper and Lower Egypt, and yet is clad in woman’s attire. But there are various places, particularly at Karnak, where Hashpsowe is depicted in masculine guise and taking precedence of Tuthmosis III, himself indeed shown as king, but only as a co-regent.” (30)

V) The temples of Jerusalem and Deir el-Bahri: We must now deal with Hatshepsut’s magnificent mortuary complex at Deir el-Bahari, most likely the product of her great architect Senenmut.

Velikovsky opined that “the design of the Most Splendid of Splendors in Deir el Bahari did not follow the contemporary Egyptian style” and cited the much earlier reaction of Mariette to this architectural wonder. (31)

“Mariette, struck by the strange appearance of the edifice, thought that it betrayed a foreign influence, and supposed that Queen Hatshopsitu [Hatshepsut] had constructed it in the model of some buildings seen by her officers in the land of Puanit [Punt]…It is an exception and an accident in the architectural life of Egypt.” (32)

This last comment was also noted by Tyldesley who, however, did nothing with it. (33) Velikovsky concluded that Hatshepsut’s temple in Deir el-Bahari “must have had features in common with the Temple of King Solomon.” (34)

He then proceeded to elaborate on this theory by comparing both the temple architecture and temple service comprising the two structures. (35)
Bimson dismissed this proposal completely and preferred a different explanation for the architectural appearance of Hatshepsut’s temple, one that depended upon its architectural neighbor – the mortuary temple of Mentuhotep II (I) of the eleventh Dynasty.(36)

First, it must be stated that the line drawing illustration of the Deir el-Bahari complex used by Bimson is outdated. Mentuhotep’s temple has now been reconstructed in a different way(37); and even though Wildung proffered the thought that “the terrace temple…of Queen Hatshepsut…, with [its] two terrace levels mimics that of the Temple of Mentuhotep II…built more than 500 years earlier”, (38) he fails to elaborate on the matter and doesn’t even consider the possibility that Mentuhotep’s temple might have already been in a ruined state by the time of Hatshepsut. And neither does Bimson.

According to Velikovsky’s revised chronology, more than 1000 years might have elapsed between the two temples – something Bimson overlooked since he does accept the new later date proposed for Hatshepsut (10th century BC). The destruction wrought by the Hyksos and commented on by Hatshepsut should not be discounted either.

Furthermore, Lynn E. Rose has downdated the time of the Middle Kingdom and that of Mentuhotep to the mid-first millennium BC which would put his temple at Deir el-Bahari at least a century after that of Hatshepsut.(39)

Finally, we must consider the architectural analysis of Hatshepsut’s temple by one of the most respected Egyptologist’s of any generation – W. Stevenson Smith. Contrary to the opinion of Bimson that the Hatshepsut “temple at Deir el-Bahri was clearly patterned [after and] inspired by Mentuhotep II’s construction” (40), Stevenson Smith viewed Hatshepsut’s completed structure as being much more:

“…there is evidence that she at first only started on a project which adhered more closely to the plan of the Mentuhotep temple. This soon was replaced by the grander scheme we now know….It is the replacing of a squared plan by elongated terraces that makes one suspect some connexion between the [more recent] building of Ahmose and Hatshepsut’s completed temple.

“Evidently no search for origins can explain the satisfying results which have been obtained in Hatshepsut’s temple. There is a rare understanding of the possibilities of the site which is lacking in the earlier buildings [of Ahmose and Mentuhotep]….The building remains a unique conception, in a way that recalls the Step Pyramid group of Zoser at Saqqara.”(41)

Locked into the conventional chronology, Stevenson Smith had no other alternative but to look at architectural examples within the confines of Egypt.

Bimson also argues that “the terraces of almug trees [are] a fiction”.(42)
Yet, Baldwin Smith, when praising Hatshepsut and her architect Senenmut, observed that when the queen “conceived the idea of building an earthly place for [her god], like the ‘myrrh terraces’ of Punt”, Senenmut probably played a key role in obtaining “the myrrh trees [from Punt] for the terraced gardens of the ‘Paradise of Amon’”. (43) This implies the possibility that Hatshepsut may have already “gotten wind” of the newly built Great Temple in Jerusalem prior to her Punt visit – yet another incentive for her expedition there – and she and her prime architect were ultimately inspired by it architecturally.

Whether or not Solomon’s temple employed almug or algum trees, the main point here would be the terraces. And if the voyage to Punt went via the Mediterranean Sea (as this author believes) and disembarked in Phoenicia, near Byblos (some 200 miles north of Jerusalem) or Tyre (some 90 miles north of Jerusalem) or even Jaffa in Israel (some 40 miles) northwest of Jerusalem, this would hardly entail an overly long journey inland. Certainly, this would be a far shorter, less dangerous and tedious trip than the 1400 mile putative one that the “Queen of Sheba” purportedly took from Yemen to Jerusalem.

Velikovsky’s efforts, rightly or wrongly, to bring a specific historical solution and synthesis to the “Solomon & Sheba” and “Punt” questions should not be dismissed lightly. As the “Epilogue” to this discourse will reveal, the story of Solomon and Sheba and the locale of “the land of Punt” remain to this day in a kind of fairy tale/twilight zone world. Equal arguments for and against their actuality and location have been forthcoming with no clear absolute proof or acceptance thereof prevailing.

Even with more and more recent archaeological discoveries since 1986 (44), there has been no finalized satisfactory resolution of the manifold problems inherent in any consideration of the subjects at hand. One is still left to ponder the words of Pritchard written back in 1974:

“Modern students of the history of the bible and the ancient Near East are sharply divided on the question of the historicity of the story of Solomon and the Queen of Sheba. One opinion is that the story is largely the product of the imagination of an oriental story-teller, a tribute to his hero Solomon. In support of this view is the prevalence of superlatives characteristic of the folk tales, as a ‘very great retinue’, ‘very much gold’, a ‘very great quantity of spices’, and ‘such an abundance of spices’. The vagueness of the title ‘Queen of Sheba’, without specific reference to her name or any mention of time or circumstance, it is argued, makes appropriate the designation of this story as a fanciful oriental legend. The vast distance that separates the capital of Sheba (if Sheba is identified as the ancient Saba in South Arabia, as it usually is) from Jerusalem, 1,400 miles of rugged desert, lends an air of improbability to the story.” (45)

So long as the conventional chronology stands, perhaps there may never be an acceptable answer to the “riddle” of the meeting of Solomon and Sheba or the whereabouts of the land of Punt. Only time will tell.
EPILOGUE

The search for Punt and the true identity of the Queen of Sheba may not be over but, regardless of the outcome, at the least Velikovsky deserves plaudits for his detailed attempt to locate this “mysterious land” and flesh out the wondrous story of King Solomon “in all his glory” and the Queen of Sheba.

Despite the scholarly articles of support starting in 1975 by Juergens and the present author as well as those by Danelius in 1975 and 1976, the negative critiques of Lorton and Bimson in 1984 and 1986 respectively, the ongoing debates pro and con in the pages of C&CW and C&CR from 1997-2012, Sweeney’s positive evidential stance in his Empire of Thebes (2006), Ginenthal in his Pillars of the Past, Vol. III (2010), and Meeks’ independent scholarly chapter on Punt in Mysterious Lands (2003), no final consensus to the Solomon, Sheba, Punt conundrum has been established.

It is hoped that the weight of the present author’s contribution may tip the scales in favor of one of Velikovsky’s most erudite proposals and help to bring about the recognition and acceptance that it deserves. Meanwhile, the reader will find reproduced below (with its title slightly altered) a reprint from KRONOS 1:2 of the article by the late Ralph Juergens and myself which supported, from a linguistic standpoint, Velikovsky’s identification of Punt with Phoenicia-Palestine. Its omission and failure to be cited in the scholarly literature of the past 36 years is but one example of the attitude of recent writers on this subject and others who act as if they were the “first” and sole proponent of a particular idea, though they have come decades late to the party.
A LINGUISTIC NOTE ON THE “LAND OF PUNT”

Ralph E. Juergens and Lewis M. Greenberg

In his “Theses for the Reconstruction of Ancient History” (Scripta Academica Hierosolymitana, 1945) and in Ages in Chaos (1952), Velikovsky identifies the “land of Punt”, mentioned in numerous Egyptian texts, (1) as Phoenicia-Palestine. (2) This, like so many other ideas he has put forward, has been essentially ignored by professional Egyptologists, who continue to disseminate the long-held view that Punt must have been located on the African or Arabian littorals (e.g., Somaliland) of the Red Sea or the Gulf of Aden. Yet, since the publication of Ages in Chaos, advances in the phonological reconstruction of the Egyptian language itself appear to have added weight to Velikovsky’s suggestion concerning the location of Punt.(2a)

Among the changes to be noted in the works of modern Egyptologists, as compared with those of their predecessors, is an abandonment of the transliteration “Punt” in favor of such versions as “Pwenet”(3) and “Pwene”.(4) In the case of the latter, however, Gardiner still indicated linguistic caution. “In most Egyptological books the name is given as Punt, with a pronunciation that is certainly wrong; but that adopted here [Pwene] is also conjectural.”(5) Gardiner was also well aware of a potential negative reaction on the part of his colleagues to this phonological change. “One innovation which I have allowed myself will probably not find general favour: it being certain that the feminine ending –et, though shown in the writing, had disappeared from pronunciation as early as the Old Kingdom, Hebrew and Arabic presenting a like phenomenon, I have replaced the usual ‘Punt’, ‘Wawat’, and ‘Hatshepsut’ by ‘Pwene’, ‘Wawae’, and ‘Hashepsowe’.”(6)

Be that as it may, the phonetic values conveyed by these new renderings of Punt seem almost identical with those of a likely ancient Greek antecedent of “Phoenícia”, a name that comes down to us by way of Latin in its spelling.

The Greek word for Phoenicia was Phoinike, while “the adjective ‘Phoenician’ [Phoinix] for certain imported goods appears already in the Linear B texts as ponika (=phoinika), which also meant, along with the form ponikija, ‘painted crimson, dyed crimson’. Ventris and Chadwick, therefore, correctly stated that ponika was ‘probably a loanword’…”(7)

If, then, the term phoinix (“Phoenician”) “can no longer be considered a Greek word, its source must be sought, most probably, among the very people who were famous as crimson and purple dyers and whom the Greeks called Phoinikes. Now Hebrew puwwa, Arabic fuwwa, is the name of Rubia tinctorum L. or dyer’s madder, a herbaceous plant at home in Syria, Palestine, and Egypt, one of the most common sources of red dye and imitation purple in antiquity. Pwt appears as early as Ugarit in a context that firmly establishes its meaning as ‘madder-dyed textile’. A
Hebrew clan of Galilee (which was contiguous to Phoenicia) bore the name of Puwwa, or Pu’a, and is quoted next to Tola, ‘crimson’ (Gen. 46:13; Num. 26:23; I Chron. 7:1f). The gentilic of Puwwa is Puni (Num. 26:23), and there is no need to suspect an error. This form Puni, adjective from puwwa, ‘red dye’, provides us with the prototype of the Greek phoin-ix and of the Latin Poen-us, pun-icus. It corresponds to the Greek word both phonetically and semantically. As a Semitic loanword, it is by no means exceptional in Mycenaean Greek, which contains several other words of Semitic origin.”

The author of the above commentary, M.C. Astour, goes on to conclude that “the establishment of the West Semitic origin of phoinix and, probably of porphyra is interesting not only from the etymological point of view. It also serves as another confirmation of steady Greco-Semitic contacts during the Mycenaean Age. It now becomes more difficult to consider the toponyms and personal names Phoinix, Phoinike, Phoinikus as purely Greek, without any relation to the Phoenicians, and to claim that the Greek traditions on Phoenician penetration of the Aegean were based on a misinterpretation of these names. At the very least, the Greeks must have met the Phoenicians and borrowed from them the word for ‘red dye’ before they could use it in their onomastica.”

In addition to our discussion so far, consider the fact that the consonantal digraph PH, with a phonetic value of F, was the Latin attempt to render the Greek letter Phi, which was in original Greek a real aspirated p. “The letters Φ, θ, Χ, seem to have had at first the sounds of ph, th, ch, in Eng. uphill, hothouse, and blockhead. But afterwards they came to sound as in Eng. graphic, pathos, and German machen (the last being a rough palatal sound no longer heard in English).” Further, as W.B. Stanford points out, “the Romans, so endearingly modest about the limitations of Latin, frequently expressed their admiration for the euphony of Greek. They contrasted the euphony of phi (pronounced p-h as in ‘shepherd’) a littera iucundissima, with the cacophony of their own f (a harsh labio-dental fricative like ours), which they considered a littera insuavissima. Unhappily we now pronounce it as a rough f.”

Even the classical author Quintillian “states that the Greek phi was not pronounced like the ugly Latin f. Yet in spite of this definite statement – supported by other evidence – most of us willfully mispronounce phi as f, instead of approximately as in ‘shepherd’.” As for the Greeks, they only recognized the Latin f was the nearest sound to the phi of their language, “a similarity which became an identity later.”

In the case of the letter grouping OI (Latin oe), “a diphthongal pronunciation is clearly indicated at least until Roman times (e.g. Phoebus, poena...). The most obvious interpretation would be as [oi] in e.g. English toy, coin; but in some Greek dialects there is evidence which seems to suggest that, by a process of assimilation, the first element of the diphthong had been fronted, giving something of the type [oi], approximately as in French feuille.” The feu of the latter would have a vowel sound made with the lips rounded in position for o as in over, while trying to say a as in able. (Pronunciation key from The American College Dictionary, 1964.)
By putting the digraph and the diphthong together – PHOI – we thus obtain, phonetically, something like p(h)u, or simply pu, which, via still another route, seems to hark back to a West Semitic or Hamitic name pronounced P-u-ne-[t?] or even P-we-ne-[t?] (15).

Before this note on the “land of Punt” is concluded, some additional information shedding more light on the linguistic problem needs to be brought forward.

In the section of Ages in Chaos, “The Origin of the Words ‘Pontifex’ and ‘Punt’”, Velikovsky stated the following:

“Even before the conquest of Joshua the land of Jerusalem was called in Egyptian inscriptions Divine Land, God’s Land (Toneter). Was Jerusalem a holy place before David conquered it, and even before the arrival of the Israelites under Joshua?

“In the Bible there is an allusion to the holiness of Jerusalem in early times and to a sanctuary in that place. When the patriarch Abraham returned from pursuing the kings of the north, who had captured his kinsman Lot, ‘Melchizedek king of Salem [Jerusalem] brought forth bread and wine: and he was the priest of the Most High God’ (Gen. 14.18).

“The name Divine (or Holy) Land, given to the region of Jerusalem in Egyptian inscriptions of the Old and Middle Kingdoms, casts light upon the religious significance of Jerusalem and Palestine generally in the days before David, even as early as the days when the Israelites were still nomads. Since then and up to the present day they have been called ‘the Holy City’ and ‘the Holy Land’.” (16)

Since the concept of the “Divine Presence” was so strongly linked to the region of Palestine and Phoenicia, it is of especial importance to note an observation made by W.F. Albright on the religion of Carthage, a Phoenician colony. “The term ‘Presence’ reminds one strongly of the late Canaanite (Carthaginian) idea that [the goddess] Tanit was the ‘presence (power) of Baal’ (Tanit pene Ba’al).” (17) The very word Pene (Pwene?) could therefore have signified not only the “Divine Presence” but “God’s Land”, “the Holy Land” – Palestine-Phoenicia – identical terms applied to the “land of Punt”. (18)

With regard to this last point, there is a most relevant Egyptian text from around the time of Queen Hatshepsut. In a pre-Amarna Eighteenth Dynasty hymn to Amen, passages of which go back to the Second Intermediate Period, we read: “Lord of the Medjai and ruler of Punt…the beautiful of face, who comes (from) God’s Land (to the east)….” (19)

ADDENDUM (2012)

In an attempt to find the origin of the word “Punt”, Velikovsky first considered the origin and meaning of the word “pontifex” and the related word “pontiff” which he said “is not of Latin
origin. It is not derived from *pons*, but probably from Punt. When it is said that Queen Hatshepsut, after visiting Punt, built a ‘punt’ in Egypt, Queen Hatshepsut also introduced the institution of the high priest, copying the service of the Temple in Jerusalem, built on a Phoenician model.”(20) After concluding that the name Punt was the origin of the word “pontiff”, Velikovsky then asked the crucial question: “What is the origin of the name Punt?” Before we proceed to his answer, a slight detour must be taken. Interestingly, *Webster’s New World Dictionary of the American Language* has proposed that the origin of the word “pontiff” probably derives from the “Osco-Umbrian *puntis*, a sacrificial offering”.(21) Compare this with the word *shelamim* that referred to the sacrifices performed in Solomon’s temple which, in R. de Vaux’s opinion, implied “the idea of a tribute offered to God”.(22) In this instance, the god in question was most likely the great *Saturnian* god of Jerusalem – Shalem.(23) If the words *puntis* and *shelamim* can relate to the same religious act, then it is highly plausible that their relationship can be expanded to include the greater region of Phoenicia-Palestine – the “Land of Punt”.

In light of the above, it is instructive to repeat Velikovsky’s ultimate theory regarding the origin of the name Punt.

“If Punt was originally the word for Phoenician temples, then it could have been derived from the Hebrew *panot* [which] means to face, incline, address, turn to. It is applied innumerable times in connection with worship…in this case the Phoenicians received their name from the houses of worship they built….Of the same root is ‘Presence’ (of the Lord) – an idea found with the Phoenicians of Carthage.”(24)

Could it be that the word *puntis*, along with the word *panot*, both contributed to the name Punt?
REFERENCES

THE “LAND OF PUNT” REDUX

Introduction:

1. AinC, pp, 103-141.
2. See the Editorial Note in C&C Workshop (2011:2), p. 5; E.
3. Sweeney, Empire of Thebes, p. 27, footnotes 54, 55.
4. E. Sweeney, “Punt: Homeland of the Egyptians”, C&CW, pp. 11-12. Also see Sweeney, Empire, Chapter 2, pp. 25-59; Sweeney, Genesis... pp. 70-76.
6. For a fairly complete history of the current situation and the abject failure to recognize the previous writings of others, see L.M. Greenberg, “Citing the Work of Others: A Critique”, AEON VI:6 (Oct., 2005), pp. 35-39 and footnotes 15, 17-27. Also see, e.g., the published work of David Rohl (Pharaohs...). Despite the fact that Velikovsky’s chronological revisionism served as a template for Rohl’s “New Chronology”, whether he cares to admit it or not, he prefers to worship at the altar of Kenneth Kitchen (a fellow Brit) to whom Rohl acknowledges “the greatest debt”. When Velikovsky is finally acknowledged, he is disparaged as an outsider (“this non-Egyptologist”), his professional status (“psychoanalyst”) is erroneously given, a negative adjective (“infamous”) is pinned to his name, the “Velikovsky Affair” is overly highlighted, the identifications in his later books in the Ages in Chaos series are referred to as a “game”, and his supporters are called “non-academics” [sic]. Yet, Rohl has no qualms about utilizing Velikovsky’s erudition when it suits him; see pp. 283, 288 in Ibid. and “The Ipuwer Papyrus” in The Lords of Avaris, pp. 52-56, the latter without credit or citation. Instead, Van Seters’ identical conclusion – 14 years after Velikovsky - is cited and my own “The Papyrus Ipuwer”, Pensee III (1973), pp. 36-37, is ignored. Rohl is being totally disingenuous here.
7. Wilkinson, pp. 275-276; Reeves, pp. 80-84; Rohl, Pharaohs...p. 397.
8. Redford, p. 192. “Even were we to deny an identification [of the mummy in KV55] with Smenkhkare, we should still have to find a place in our historical reconstruction for a male, clearly royal, who undoubtedly reigned, was a close relative of Tutankhamen, and who bore an epithet otherwise known only from the titular of Smenkhkare! Finally, although the matter is incapable of proof one way or the other, the appearance of a queen in art as the husband of her daughter, the latter entitled in consequence ‘great king’s wife,’ seems a preposterous perversion even for the Amarna Age.
9. Wilkinson; Reeves; Rohl, loc.cit.

A Memorable Journey:

1. AinC, p. 117 and p. 118, n.6 (emphasis added).
3. Ibid., p. 55 (quoted sentence is inverted).
4. Ibid., and notes 112, 113.
Hatshepsut, Hathor, and Isis:
1. For a detailed overview of the “history” of Osiris, see Otto, pp. 61-64.
2. Tyllesley, p. 46.
3. Lesko, p. 67; Herm, pp. 28, 35.
4. Donalson, p. 68; Shafer, p. 58.
5. Quirke, p. 173.
6. Lesko, p. 97; Sweeney, Empire, p. 38.
8. Tyllesley, p. 173.
9. Ibid., p. 144.
10. Ibid., p. 145 and n.27.
11. Lesko, pp. 76, 82, 85; Witt, pp. 15, 123.
12. Lesko, p. 85; Witt, pp. 14, 121.
15. Donalson, pp. 122, 133; Grant, p. 118; Tyllesley, p. 139 (and p. 138 for Cleopatra III); Roller, p.75 for the nautical Isis (so, too, Hatshepsut); Fletcher, p. 86 and 449 in the Index; Ludwig, pp. 166-167; Chauveau, pp. 77, 80.
17. Tyllesley, Cleopatra, p. 45; Tyllesley, Hatchepsut, pp. 77-78, 114-115.

The Land of Osiris:
3. Sweeney, Empire, p. 36.
5. Sweeney, loc. cit.
6. Morenz, p. 19; Sweeney, Ibid., pp. 36-37.
10. Erlich, inter alia.
11. Mare, p. 1096; Neil, p. 959.
12. Osman (1992), pp. 129-130; Osman (2004), pp. 142-143. Because Osman follows the conventional chronology, he believes that Jerusalem acquired the name Zion as a result of Tuthmosis III’s stay there.
15. Rosenberg, pp. 820-821; Greenberg, KRONOS X:2, pp. 94-98; Cardona, KRONOS X:2, pp. 86-93.
18. Herm, pp. 108-110; Roth, p. 922; Moscati, p. 126 – where it is stated that El was considered identical to Ra; Byblos claimed to be founded by the god El – Strong, p. 68; For El as Saturn, see Cardona, *KRONOS* III:3, pp. 34-35, 41; also see Holst, pp. 31, 48-49.
22. Myers, p. 399.
23a. For one example of the archetypal role of Cleopatra VII, see Ashton, p. 119; for the Queen of Sheba as a Christian archetype, see Pritchard, p. 15.
25. E.g., Osiris, Shamash, El, Kronos, Ahuramazda; also see Talbott, *inter alia*.

**Make-ra and Make-da:**
3. Wells, p. 113.
10. Greenberg & Sizemore, p. 64 and n.51; Rohl, pp. 228-229. For the name Dido, a possible Phoenician feminine equivalent, see Moscati, p. 655.
15. Witt, p. 61.

**Of Animals, Aquatics, and Pharaohs:**
5. Sweeney, *Animals and People...*
15. Van Seters, *Ibid.* Also see Roaf, pp. 134-135; Meeks, pp. 65-67 where three New Kingdom sources (Amenhotep II, Seti I, and Ramses III) “indicate that Amu and Punt could have been involved...in Egyptian campaigns in the Levant.”
18. Bimson, p. 12 and n.5.
19. Velikovsky, p. 141, n.2; Gardiner, p. ix.
21. Bimson, p. 12 and n.6. Bimson and Day are strongly contradicted by Silberman in Pritchard, et al.: “The targum says that this land [from which the queen came] was ruled by a woman whose name was Malkath Sheba. These words may be translated ‘Queen of Sheba’, but the grammatical construction suggests this was not understood by the targum to be her title but her proper name. It is interesting to note here that in medieval Jewish texts the reading is often ‘Malka’ Sheva’, to be translated *Queen Sheba, the second word thus being understood as her proper name, the first as her title’*[Emphasis added].
23a. The use of the plural word “lands” should also give one pause since this seems to be a comparative use of the term. The “Holy Land” to the east and the sacred precinct of the Siwah Oracle in the Libyan desert to the northwest would have made excellent pendants to any border region in the south.
29. Tyldesley, *Chronicles*..., p. 99; Gardiner, p. 184; *The Cambridge Ancient History* II:1, p. 329, places the expedition to Punt six or seven years after the assumption of the pharaonic titles (but doesn’t say which ones).
31. Velikovsky, p. 129.
33. Tyldesley, *Hatchepsut*, p. 165 and n.3.
A LINGUISTIC NOTE ON THE “LAND OF PUNT”

2. Thesis #42; AinC, pp. 116ff.

2a. Back in 1949, in referring to “the mysterious Land of Punt”, M. Murray stated that “the root of the word [Punt] is Pwn, the T being the usual feminine ending for a foreign country. Is this a word of some primitive language meaning ‘sea-shore, littoral’ and is it the origin of ‘Phoenician’, the coast people of Palestine, and ‘Punic’ the littoral of North Africa?” – The Splendour that Was Egypt, p. xxi.

5. Ibid., p. 37 (emphasis added).
6. Ibid., p. ix. “The problems how best to transcribe Proper Names is one that has often vexed even classical scholars; with Orientalists it is much more acute, and among the latter the Egyptologist is worse off than any. The hieroglyphs write no vowels and the correct supplying of these from Coptic or elsewhere is seldom possible: guesswork is therefore inevitable, but it is necessary because vowellless transcriptions would be an austerity which no ordinary reader could stomach. Furthermore, Egyptian consonants by no means all correspond to our own; the ancient writing shows two kinds of $h$, two of $k$, two of $kh$, two of $s$, and no less than four of $t$ and $d$, besides possessing among other peculiarities an important guttural in common with Hebrew and Arabic, there called the ‘ayin’.” – p. viii, Ibid.

10. J. Hadley, *A Greek Grammar* (NY, 1869), p. 7; W.B. Stanford, *The Sound of Greek* ((LA, 1967), p. 140 – “The Greek comic writers give us examples of how foreigners distorted Greek sounds. (But we must remember that these are caricatures and no doubt exaggerated.) Their commonest fault, apparently, was to mispronounce certain types of letters. Thus in *Thesmophoriazousai* Aristophanes represents a Scythian policeman as being unable to pronounce the aspiration in the Letters *theta, phi, and chi*, for which he substitutes *t, p, and k*... (Here we have further evidence that these Greek aspirates were not pronounced like our *th, f, and Palatal ch*...); W.S. Allen, *Vox Graeca* (NY, 1968), p. 16 – “…in classical Attic the sounds written Φ, θ, Χ were aspirated plosives, like the *ph, th, kh* of Sanskrit and the modern Indian languages (and similar to the initial p, t, k of English or German) and not fricatives as in modern Greek...”; Stanford, *Ibid.*, p. 125 – “On the other hand, the fact that Sanskrit had aspirated *kh, ph*, and *th* strengthens the evidence for pronouncing *chi, phi, and theta* as aspirates. Some of the phonetic developments in modern Greek also help to determine pronunciations in ancient Greek.”


15. In the word phoin-ix, “the suffix *ix (= ıks)* is Greek. The diphthong –oi- is due to the normal transformation ‘phon-ıo-s (no relation to *phonos* ‘murder’!) >phonos. The vowel *o* for Semitic *u* is quite common in Greek transliterations (especially in the Septuagint). Latin *punicus* may have retained the original vocalization (the Romans could have borrowed the word directly from the Carthaginians); the diphthongization in *Poenus* is an inner-Latin development.” – Astour, *op.cit.* p. 349, n. 32. In discussing “The Origin of the Words ‘Pontifex’ and ‘Punt’” (*Ages in Chaos*, pp. 132-134), Velikovsky recalls that “Rome waged so-called ‘Punic Wars’ against Carthage, which was built by immigrants from Tyre.”; R.D. Barnett, “Phoenician-Punic Art”, *Encyclopedia of World Art*, XI (NY, 1966), p. 294 notes that Western Phoenician culture (Carthaginian) was “usually designated by the term ‘Punic’, derived from a Latin name for the Phoenicians (Poeni, Puni).”

16. Velikovsky, *op.cit.*, p. 134; Velikovsky also suggests that “if Punt was originally the word for Phoenician temples, then it could have been derived from the Hebrew word *panot*, and in this case the Phoenicians received their name from the houses of worship they built.” – *Ibid.* In the case of the latter suggestion, it is especially interesting to read the following comments by Gardiner, *op.cit.*, pp. 1-2: “…the great city of Memphis, an alternative name of which – Hikuptah, ‘Mansion of the Soul of (the god) Ptah’ – may have furnished Homer with the word *Aigyptos* (Egypt), used by him to designate both the river Nile and the country which it watered.”

text No. 347 of the Proto-Canaanite inscriptions of Sinai, we find the name TNT (tinnit) used as an epithet to qualify Asherat, ‘the Lady of the Sea Monster or Dragon’. Thus TNT would be the feminine of TN, a name which is found in its plural form TNM in Ugaritic texts. The Lady of Carthage would indeed be Asherat, as Dussaud had suggested. In support of this theory we quote the union of Tanit with Ba‘al Hammon [Saturn, according to most authorities] which is reflected in her appearance as Pene Ba‘al [“the ‘Face of Ba‘al’”], and which corresponds to that of Asherat with El [Saturn] in Phoenicia…Tanit is represented by the crescent moon [sic?], and Astarte by the planet Venus.” Tanit “is the dispenser to mankind of the vital energy which is Ba‘al Hammon’s.” Tanit resembled Isis and Cybele closely and “the Romans adopted her cult and renamed her Coelestis” (p. 154, emphasis added); also see Ex. 33:11, Ex. 33:14. The Hebrew word Panim also means “presence” or “face”.

19. J.A. Wilson, The Culture of Ancient Egypt (Chicago, 1959), p. 211 and n. 7 (emphasis added); also see AinC, p. 109 and n. 7. “To the east” can hardly refer to Somalia. Even today there is the following blessing intoned in the Jewish Synagogue and Christian Church: “May the Lord bless you and keep you. May He make His countenance (face) to shine upon you and grant you and all this world Peace – Amen.”
24. AinC, p. 134 and n.6 (text and reference material is combined).
BIBLIOGRAPHY

Fletcher, J., *Cleopatra the Great* (NY, 2008).
Ginentalheil, C., *Pillars of the Past III* (Forest Hills, 2010).
Lassner, J., *Demonizing the Queen of Sheba* (Chicago, IL, 1993).
Lajtar, A., *Deir El-Bahari in the Hellenistic and Roman Periods* (Warsaw, Poland, 2006).
Strong, A., *The Phoenicians in History and Legend* (Bloomington, IN, 2002).


Tyldesley, J., *Cleopatra Last Queen of Egypt* (NY, 2008).


Wildung, D., *Egypt from Prehistory to the Romans* (NY, 1997).


APPENDIX

ASTRONOMY AND THE SHORT CHRONOLOGY

LYNN E. ROSE

The Short Chronology that Charles Ginenthal and I have been advocating has attracted a number of negative evaluations, usually rather frivolous. The most recent of these were the ones by Carl Olof Jonsson and by Barry Curnock that were published by the SIS. Jonsson dealt at some length with my Appendix to Ginenthal’s *Pillars of the Past*, Volume II, and Curnock dealt at some length with Chapter Nineteen of my *Sun, Moon, and Sothis*. I prepared separate answers to Jonsson and to Curnock. Unfortunately, the editors at SIS were willing to print multiple and wide-ranging charges by Jonsson and by Curnock against me, but were not willing to let me reply in what I took to be an appropriate manner. I have therefore incorporated my answers to Jonsson and to Curnock at later places in this Appendix.

Other criticisms of the Short Chronology have been even more frivolous. One of them, which is now flitting about the Internet, is that the various written forms of Egyptian — notably hieratic and demotic — were never used at the same time, and that any chronology that suggests that this might have been done is therefore false. As it happens, Ginenthal has already dealt with this very point in *Pillars of the Past*, Volume II, pages 106-112, where he shows that various written forms of Egyptian were indeed used at the same time, sometimes in the very same document. Thus on page 111 he quotes Eugene Cruz-Uribe regarding a 26th Dynasty document that “contains ‘witness copies’ of a business document written variously in Late New Kingdom style Hieratic, Abnormal Hieratic and early Demotic”. That same Internet personage also took me to task for not knowing that the š in šmw should be written with a wedge above it, to indicate that it was pronounced sh rather than s. I did know that, and I also knew that the season of šmw is sometimes written out as Shomu, but for many years I never had a typewriter or a computer that was up to that particular task; furthermore, I thought the matter too trivial to bother with, quite aside from the costs of printing. Sometimes, however, the difference is very much worth stressing: for example, Shoshenk and Shishak feature the sh sound, while So and Sesostris and Osorkon feature the simple s sound. (I should point out, however, that even such familiar names as these may frequently be rather different from the original forms.)
Our Short Chronology is not the only radically shortened chronology in the field. The Heraklean labors of Emmet Sweeney and Gunnar Heinsohn, and especially of Immanuel Velikovsky, have taught us much about the radical shortening of ancient chronologies, even though we have come to disagree with all three of them on various details. We also disagree in toto with the very modestly shortened chronologies of David Rohl and Peter James, as well as with the similarly shortened “anno domini” chronology of Heribert Illig and his supporters. Perhaps the very shortest fully-developed chronology that we shall ever see, outside the mind of a solipsist, is that of Anatoly Fomenko. All that I want to do regarding Fomenko is one illustrative test.

ANATOLY FOMENKO

Fomenko puts the birth of Jesus Christ in +1152, which would presumably have been several centuries after the death of Alexander. In between would be the conventional placement of the Seleukids, with the well-known eclipse report from -135 apparently now falling somewhere roughly around +1020. Note that -135 + 1152 – (-3) = +1020. The -3 is the most popular date for the birth of Jesus Christ. To be generous, let us look at the entire stretch from +800 to +1200.

I picked this eclipse because it is widely regarded as one of the better attested and better timed total solar eclipses from antiquity. It was reported as total in Babylon during the early morning of day 29, month Adar the second, year 175 of the Seleukid Era, with mid-totality at about 42 $\text{uš} = 2$ hours and 48 minutes after Sunrise. An $\text{uš}$ was equal to four minutes.

The Julian date of that total solar eclipse seems to have been 15 April -135. Both Lange and Swerdlow 3.1 and Starry Night — and even Huber and De Meis — indicate that there was a total solar eclipse in Babylon on that Julian date, with mid-totality about 2 hours and 50-some minutes after Sunrise. (The timing is about the best that can be expected; remember that the ancients had no mechanical or pendulum clocks.)

Even though, astronomically, the eclipse seems to have been on day 28 of the lunar month, this report is very precise both about the totality and even about the time of day. It does sometimes happen that bad seeing or other problems will throw off the lunar count by a day or so. This is still a very valuable report, however, despite the questionable day-number. Strangely enough, some of the leading specialists in this area — notably F. Richard Stephenson — have completely failed
to notice the 28 versus 29 problem! See his Historical Eclipses and Earth’s Rotation, pages 129-131 and 136-137.

Actually, the 29 just might be correct. Schoch puts the New Crescent on March 18, but with barely an hour to spare. What we learn from Doggett and Schaefer’s studies of amateur observers is that while most such observers do spot the New Crescent on the predicted day, some may not see it until the following day, and a few sharp-eyed ones may even spot it the previous day. If the New Crescent was indeed spotted on March 17, then the eclipse actually would occur on day 29. Thus the 28 versus 29 problem need be of no particular concern to us. We should also remember that the ancient stargazers — whether astronomers or astrologer-priests — were usually experienced professionals rather than novices.

Anyway, using both Lange and Swerdlow 3.1 and Starry Night, I have determined that there were no total solar eclipses in the morning in Babylon in either February, March, April, or May between +800 and +1200. That is enough for me. I shall spend no further time on Fomenko until such astronomical difficulties have been resolved to my satisfaction.

Notice that the š in uš likewise needs a wedge. To those who obsess about such matters I say, Ou phrontis Hippokleide. That traditional quotation itself needs some accents and breathings, as well as a subscript. And the final e is really an eta. So what? I cannot be bothered about whether even my newest computer can handle such things.

HERIBERT ILLIG

I have long claimed that the many ancient astronomical records that we possess preclude Illig’s attempt to remove 297 years from the late first millennium of this era. The -135 eclipse that undermined Fomenko is just one of numerous such astronomical records. That same eclipse gives Illig fits as well, since there is no such eclipse in the years between +140 and +180, either! (Note that -135 + 297 = +162.)

Even the obvious need to rearrange various ancient peoples strengthens rather than weakens my claim. When the Twelfth Dynasty with its Sothic date and its El-Lahun papyri is moved by 1477 years, it fits better. Similarly, when some of the Assyro-Babylonians are moved by 274 years, they fit better. (Actually, these various societies did not fit originally, though they do fit beautifully now that I have moved them.) Those astronomical fits, together with the astronomical fits of the Persians, Seleukids, Greeks, Romans, and others who do not need to be moved
at all, constitute insurmountable evidence against Illig. Nonetheless, there are a number of additional matters relevant to Illig that might well be looked into, such as: the Julian Reform, the Augustan Repair, the birthday of Augustus, and the Gregorian Reform. We already know that Illig is wrong, but he has attracted so much attention, and his muddying of the waters has caused so much confusion, that a lot of clarification is called for. This may take a while.

As best I can make out, Illig seems to believe that the calendar had slipped more than ten days in what he takes to be the $1582 + 44 - 297 = 1329$ years that he thinks separated the Julian Reform from the Gregorian Reform. Illig also seems to think that Pope Gregory XIII was correct in saying that the vernal equinox fell on March 11 in his own day; furthermore, Illig seems to think that it follows from that that at the start of the Julian Reform the vernal equinox must have been on March 21. We shall see.

In the following list (based upon the astronomical tables of Bryant Tuckerman as well as upon *Starry Night*), it is the dates that are important. Unless there are very close calls, the approximate times of day can be neglected. As it happens, there are no truly close calls in this list, anyway. All of these dates are in the standardized Julian calendar that came into being after the Augustan Repair of the original Julian Reform (which had itself been misapplied by intercalating every three years instead of the intended every four years). In other words, I am using the corrected, retrojected Julian calendar, not the quasi-Julian mess that persisted from -41 to +8 (or to +5, if you prefer).

<table>
<thead>
<tr>
<th>Vernal Equinox</th>
<th>Autumnal Equinox</th>
</tr>
</thead>
<tbody>
<tr>
<td>-62</td>
<td>March 23, mid-afternoon</td>
</tr>
<tr>
<td>-44</td>
<td>September 26, just after midnight</td>
</tr>
<tr>
<td>-11</td>
<td>March 23, after midnight</td>
</tr>
<tr>
<td>-10</td>
<td>September 25, morning</td>
</tr>
<tr>
<td>-9</td>
<td>March 23, just after Sunrise</td>
</tr>
<tr>
<td></td>
<td>September 25, late afternoon</td>
</tr>
<tr>
<td>+325</td>
<td>March 20, mid-morning</td>
</tr>
<tr>
<td>+1582</td>
<td>September 22, evening</td>
</tr>
<tr>
<td></td>
<td>March 11, just after midnight</td>
</tr>
<tr>
<td></td>
<td>September 13, mid-afternoon</td>
</tr>
</tbody>
</table>
If we are going to talk about equinoxes, this is the way to start.

There has been debate about when the first intercalation of the Julian Calendar occurred. In *Sun, Moon, and Sothis* (page 82), I argued, in opposition to Samuel, that it was in -44, while Julius Caesar was still alive. (He was assassinated in -43.) I have also argued (pages 308-312), not only that the Julian Reform was a Sothic reform, but also that the Julian Reform was intended to include a reform of the Egyptian calendar, giving it as well a length of 365¼ days.

In addition to the arguments in my book, I would add two more points:

(1) While we tend to think of leap year as the last of a group of four years, that does not mean that Caesar would have had to start his calendar off with three years of 365 days and then have a leap year of 366 days; rather, I would argue that he chose to have a leap year right away in -44, and planned to have others in -40, in -36, and so on. His additional motives are elaborated on pages 310-311.

(2) An interesting point, which I noticed several years ago but have never had occasion to mention in print, is that in -44 the vernal equinox occurred only a fraction of a degree from the beginning of the constellation of Aries, at least as that boundary is reasonably drawn in *Starry Night*. That might well have had considerable appeal to the ancients; that is, they might well have thought that this was a highly opportune time to inaugurate a calendar reform. As I write, in +2011, it is 2055 years since -44. According to Illig, it would now be only 2055 – 297 = 1758 years after -44. Another way of expressing this is that the Julian Reform was really in the year that we have been thinking of as +253, because -44 + 297 = +253. But by +253 the vernal equinox had precessed nearly four and one-half degrees into Pisces, and was not really at the familiar “first point of Aries”. Indeed, the vernal equinox was by then already passing most of the stars constituting the eastern, upright fish of the Pisces pair. This presents a further embarrassment for Illig. (It might be noted that Pisces is a very wide constellation, with no wiggle-room at either end.)

The first of the mistakenly three-year intercalations was in -41. That would put the twelfth and last of the mistakenly three-year intercalations in -8. Notice that -44 would be the starting-point for both series. The confusion set in later, after Caesar's death. But -44 need never have been mentioned, and was not mentioned, by those
who were later contrasting what was done after Caesar’s death with what should have been done after Caesar’s death.

As I noted on page 81 of Sun, Moon, and Sothis, “Both Macrobius and Solinus tell us that there were twelve intercalations over the course of thirty-six years, and that there should have been only nine.” Just how those sources counted the thirty-six years is left unclear (see pages 81-82). If they counted inclusively down to -8 from -43, the year of Caesar’s death, that would indeed be thirty-six years, but there are other possibilities as well, which I enumerated. In any case, there should indeed have been only nine of the four-year intercalations in that stretch, if the intended four-year rule had been correctly applied. In Chapter 5 of Sun, Moon, and Sothis, I pointed out that those intended intercalations would have been in -40, -36, -32, -28, -24, -20, -16, -12, and -8, but that the actual intercalations in that stretch were in -41, -38, -35, -32, -29, -26, -23, -20, -17, -14, -11, and -8.

On the assumption that the dates changed at midnight, the vernal and autumnal equinoxes of, say, -11 or -10 or -9 would have occurred on the dates that were then being called March 20 and September 22, respectively. But when we are retrojecting the later version of the Julian calendar, we call them March 23 and September 25, as in the above table.

By, say, -11 or -10 or -9, there would have been eleven intercalations instead of eight. There you have it: Illig’s three days are not missing at all. Illig is obsessed with those three days. He has no idea where they came from. As we shall see, he has failed to take into account the erroneous intercalation practices that obtained from -41 to -8.

In -17, -14, -13, -11, -10, -9, -8, -7, -6, and -5, there were three-day discrepancies. (See Alan Eduard Samuel’s chart of such discrepancies on page 157 of his book. That chart is almost unintelligible but is nonetheless correct. I have incorporated Samuel’s figures in my own list below.) It was precisely in order to remove that three-day discrepancy that Augustus allowed no further intercalations until +8. That is, he had no intercalations in -4, in 0, or in +4, but he resumed intercalating on a proper four-year basis in +8. (Perhaps it should go without saying that he also had no three-year intercalations in -5, -2, +1, +4, or +7.) The first discrepancy of -4 days would have been in the year -5, if the Augustan Repair had not intervened.
THE BIRTHDAY OF AUGUSTUS

The birthday of Augustus is a can of worms in its own right. If he was known to have been born on Calends 9 October, at a time when September was still a 29-day month, then he was born on September 22 of that old Republican calendar. (The year of his birth was -62.)

If Augustus was indeed born on September 23 of the old Republican calendar, that would have been Calends 8 October. Or perhaps he really was born on Calends 9 October of the Republican calendar, which would have been September 22, and then this was simply converted to Calends 9 October of the Julian calendar, which would have been the September 23 that was eventually recognized as his official birthday. (Even George Washington had his birthday changed, from February 11 to February 22.)

Calends 9 October was indeed September 23 of the Julian calendar and September 22 of the old Republican calendar. But where the autumnal equinox fell in terms of the old Republican calendar in the year of Augustus’s birth is, so far as I can tell, both unknown and unknowable from the presently-available evidence. Even the reports concerning the time of his birth are themselves less than convincing.

The individual years of the Republican calendar varied so much in length, with its weird intercalary “months” of 22 or 23 days supposedly occurring every other year (added to the base year of 355 days, but with the last five days of February apparently just skipped), that it is difficult for me to see how the vernal equinox could have been regarded as remaining fixed on any single calendar date.

If the Julian intercalation rule had been applied properly, Augustus’s birthday would have preceded the autumnal equinox by a couple of days or so, year after year, at least throughout the remainder of his own lifetime. It was only because of the misapplication of the Julian intercalation rule that the autumnal equinox ever fell on Augustus’s merely quasi-Julian birthday during his lifetime.

In order to see the overall context and pattern, I list the Julian dates of the autumnal equinox from -43 to +14 — that is, from the year in which Julius Caesar was assassinated to the year in which Augustus died (in his own namesake month of August); then the discrepancies, which are always either 0 or negative; and then the recognized or observed dates that result from adding the previous two columns.
<table>
<thead>
<tr>
<th>Year</th>
<th>Julian Date Of The Autumnal Equinox, From Tuckerman And <em>Starry Night</em></th>
<th>“Triennial” Discrepancy, From Samuel <em>et al.</em></th>
<th>Officially Observed Date</th>
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Note that, with the ongoing discrepancy, September 23, as that date was understood at the time (because of the mistakenly three-year intercalations), would have fallen on the autumnal equinox in -26, -23, -22, -21, -20, -19, -18, -16, -15, -12, -4, -3, -2, and -1. (Actually, once September had thirty days, the Romans more often referred to September 23 Julian as Calends 9 October.) Those first ten instances might have been enough to get the sycophants going, and to favor the conceit that Augustus’s birthday was on the autumnal equinox. As noted earlier, perhaps his birth in -62 really was on the day of the autumnal equinox. But if the Romans of the last quarter or so of the first century ever claimed that the birthday of Augustus was always on the day of the autumnal equinox, they were being extremely casual and indeed sloppy about the actual timing of the autumnal equinox.
THE GREGORIAN REFORM

The Gregorian Reform was announced on February 24 of 1582 in the Papal Bull, *Inter Gravissimas*, in the tenth year of the pontificate of Gregory XIII. That Bull says nothing about Julius Caesar, nothing about the Julian Reform, nothing about Augustus Caesar or his silly birthday, nothing about the autumnal equinox, nothing about the misapplication of the Julian calendar over several decades, and nothing about the Augustan Repair of that calendar. The Bull says nothing about the astronomical situations in the lifetimes of Julius Caesar, or of Augustus Caesar, or even of Jesus Christ. Clearly, there was no intention whatsoever that the Gregorian Reform would look that far back in time. The only equinox dates that Pope Gregory XIII was interested in were: the date of the vernal equinox at the time of the Nicaean Council, which according to him was March 21; the date of the vernal equinox in Gregory’s own time, which according to him was March 11; and the date of the vernal equinox that his Reform would ensure for ever after, which according to him was that same date of March 21 that had been sanctified 1257 years earlier by the Nicaean Council. (The Nicaean Council was held in the year 325 of this era.)

Throughout Gregory’s Bull, it is clear that the orientation is primarily theological. The aim is simply to keep the vernal equinox on March 21 for ever after, and thereby to provide an appropriate and perpetual basis for determining the date of Easter, which would always be on the first Sunday following the first day fourteen (of the lunar month) that fell on or after the date of the vernal equinox.

The calendrical provisions of the Gregorian Reform are by now quite familiar: that ten days will be omitted between October 4 and October 15 of 1582; that the year 1600 will be a leap year; that the years 1700, 1800, and 1900 will not be leap years; that the year 2000 will be a leap year; and so on, for ever more. (More generally put, the intention was that a century year that was evenly divisible by 400 would be a leap year, and that a century year that was not evenly divisible by 400 would not be a leap year.)

Gregory’s simplistic views about March 11 and March 21 are a rather striking example of papal fallibility. In order to see this, we have only to turn to any of the retrocalculation programs or tables that give us the actual dates of the vernal equinoxes. For this purpose, I have again used the published tables of Tuckerman as well as the *Starry Night* program. Those two sources are in complete agreement with each other regarding all of the following facts.
For Jerusalem, the dates of the vernal equinoxes in the ten-year period putatively preceding the crucifixion of Jesus Christ were 22 March of the year 21 of this era, 23 March 22, 23 March 23, 22 March 24, 22 March 25, 23 March 26, 23 March 27, 22 March 28, 22 March 29, and 23 March of the year 30 of this era. Notice the jumping around. There is no one date on which the vernal equinox was occurring. But Gregory is not interested in any of that. His focus is on the traditions of the Roman Church itself, not on Jesus Christ or his early followers — and certainly not on those unrepentant pagans of ancient Rome.

For Nicaea, the dates of the vernal equinoxes in the ten-year period preceding the Nicaean Council were 20 March 316, 20 March 317, 20 March 318, 21 March 319, 20 March 320, 20 March 321, 20 March 322, 21 March 323, 20 March 324, and 20 March 325. Notice that eight of these vernal equinoxes were on 20 March rather than 21 March. If, as Gregory claims, the Nicaean fathers did in fact opt for 21 March, they were quite mistaken. In following their lead, Gregory himself would also have been mistaken.

In Rome, the dates of the vernal equinoxes in the ten-year period preceding Inter Gravissimas were 10 March 1572, 10 March 1573, 11 March 1574, 11 March 1575, 10 March 1576, 10 March 1577, 11 March 1578, 11 March 1579, 10 March 1580, and 10 March 1581. Notice that even in Gregory’s own time the vernal equinox was on 10 March more often than on 11 March!

Also in Rome, the dates of the vernal equinoxes in the ten-year period following the implementation of the Gregorian Reform were 21 March 1583, 20 March 1584, 20 March 1585, 21 March 1586, 21 March 1587, 20 March 1588, 20 March 1589, 20 March 1590, 21 March 1591, and 20 March 1592. Notice that even in that first decade of the Gregorian Reform, the vernal equinox was on 20 March more often than on 21 March!

The exactitude that Gregory brags about is not there. The Gregorian Reform quickly failed to achieve its own stated goals.

Quite aside from Gregory’s own failures in the field of calendars, Illig has completely distorted the Gregorian Reform by trying to construe it as an attempt to restore a situation that allegedly obtained during the second half of the first century before this era. As we have seen, the Gregorian Reformers never had any such intention. They did not care what was going on at the time of Julius Caesar, or at the time of Augustus Caesar — or even at the time of Jesus Christ! The fact is that
they simply wanted to restore the state of affairs that they perceived as having obtained at the time of the Nicaean Council in 325. They sought to “maintain intact the ancient rites of the Church, and that was our first concern in this business” (¶4).

Pope Gregory’s Bull does not help Illig at all; in fact, it refutes him. Despite his own inaccuracies and other infelicities, Gregory requires ten days’ worth of slippage between 325 and 1582, which is just about right for a period of 1257 years, especially since he is looking at whole days, with no fractions. He thus leaves no room whatsoever for Illig’s removal of “the 297 years between August 614 and September 911” (C&CR 2011, page 3). \(365.25 - 365.2422 = \text{about} \ 0.0078, \) and \(1257 \times 0.0078 = \text{about} \ 9.8 \text{ days}.\) Illig’s theory would require only about \(960 \times 0.0078 = 7.488 \text{ days}.\) Since everything is in whole days, it is a matter of ten days versus only seven or eight days. Illig has Pope Gregory XIII testifying against him. We do indeed now have it on papal authority that Illig is wrong. At least in this one instance, it was Gregory who had the correct number of days.

STONEHENGE

I do not like referring to the first and last positions of the rising Sun as higher limb on the horizon and lower limb on the horizon, respectively. The problem is that the higher limb is involved when the Sun is lower and the lower limb is involved when the Sun is higher. This is potentially rather confusing. I shall instead refer to first contact of the Sun with the horizon and last contact of the Sun with the horizon. In between, roughly, would have been the central or straddling contact, meaning that the center of the Sun was on the horizon.

On page 275 of this book, Ginenthal cites Aubrey Burl’s Heelstone azimuth of \(51° \ 3′\) and his Partner Stone azimuth of \(48° \ 21′\). That Partner Stone is now missing, but its one-time position has been identified by the archaeologists. Burl’s figures would imply a midpoint of exactly \(49° \ 42′\), which would be \(49.7°\). We need to investigate what the Sun was doing in the general vicinity of that midpoint at various dates.

I have obtained the following figures from Starry Night. Since we are more interested in the first contacts, I have checked them every 25 years; the last contacts are given only every 100 years. The instant of contact is difficult to identify from the graphics. The minutes of arc are not accurate to three decimal places, maybe not even to one. Indeed, if I determine the azimuth and then determine it again later, I always get something at least a little different. This is
because of the guessing-game about just when the screen shows contact. We should look only to the *overall* pattern and to the *approximate* place between the two stones where the first contact would have occurred.

The date of the Summer Solstice should not be confused with the date of the Sunrise nearest the Summer Solstice. Roughly one-third of the time, these are *not* the same thing, which causes even *further* jumping around.
<table>
<thead>
<tr>
<th>Date Of The Sunrise Nearest The Summer Solstice</th>
<th>Azimuth Of First Contact With The Horizon On That Date</th>
<th>Azimuth Of Last Contact With The Horizon On That Date</th>
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</thead>
<tbody>
<tr>
<td>June 22 Gregorian +1975</td>
<td>49° 16.625′</td>
<td>50° 7.501′</td>
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<tr>
<td>June 16 Julian +1000</td>
<td>49° 1.655′</td>
<td>49° 54.875′</td>
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<tr>
<td>June 17 Julian +975</td>
<td>49° 1.763′</td>
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<td>June 17 Julian +950</td>
<td>49° 1.146′</td>
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<td>June 17 Julian +925</td>
<td>49° 0.509′</td>
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<td>June 17 Julian +900</td>
<td>49° 0.570′</td>
<td>49° 52.609′</td>
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<tr>
<td>June 18 Julian +875</td>
<td>48° 59.927′</td>
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<tr>
<td>June 18 Julian +850</td>
<td>48° 59.705′</td>
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<tr>
<td>June 18 Julian +825</td>
<td>48° 59.887′</td>
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<td>June 18 Julian +800</td>
<td>48° 59.185′</td>
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<td>June 19 Julian +775</td>
<td>48° 58.725′</td>
<td>49° 51.438′</td>
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<td>June 19 Julian +750</td>
<td>48° 58.831′</td>
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<td>June 19 Julian +700</td>
<td>48° 57.625′</td>
<td>49° 50.069′</td>
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<td>June 20 Julian +675</td>
<td>48° 56.763′</td>
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<td>June 20 Julian +650</td>
<td>48° 55.970′</td>
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<td>June 20 Julian +625</td>
<td>48° 55.701′</td>
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<td>June 20 Julian +600</td>
<td>48° 55.783′</td>
<td>49° 47.306′</td>
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<td>June 20 Julian +575</td>
<td>48° 54.929′</td>
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<td>June 20 Julian +550</td>
<td>48° 54.703′</td>
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<td>June 20 Julian +525</td>
<td>48° 54.525′</td>
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<tr>
<td>June 20 Julian +500</td>
<td>48° 53.685′</td>
<td>49° 46.675′</td>
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<tr>
<td>July 11 Julian -2025</td>
<td>48° 20.781′</td>
<td>49° 14.465′</td>
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The “window” or the “gunsight”, whichever metaphor we prefer, is well over two degrees. The sequence of three key events is first contact, center on horizon, and last contact. That full sequence fills only about one-third of the space between the Heelstone and its one-time Partner Stone. If they were interested in the entire sequence, where do we put it? Right in the middle? Or perhaps somewhat over to the left? That way, the southward movement of the solar azimuths would keep the solar events between the sighting stones for some time, which might well have
appealed to them, especially if they realized that, over time, everything was going to shift slowly to the right, anyway.

We also have to bear in mind that they might have been interested in just one of those phenomena. Thus many people in the establishment seem to believe that they focused on the last contact. I believe that we have to leave all of these possibilities open, and see what we can find from retrocalculation. (By the way, the center of the Sun on the horizon occurs about halfway between the tangencies. It is not exactly halfway between, because the transitions are more like gentle curves than like straight lines.)

In terms of Ginenthal’s own time frame, the +875 has the first contact some distance to the left of center (49° 42′ − 48° 59.927′ = 0° 42.073′), and the +675 has it a little farther to the left (49° 42′ − 48° 56.763′ = 0° 45.237′). As noted, the left-of-center starting-point might have been deliberate: this would compensate for the slow shift to the right, and would allow the monument to work for a longer period of time.

These results are strongly supportive of the Short Chronology.

THE ALIGNMENT OF STONEHENGE

Earlier in this Volume IV of Pillars, on pages 268 ff., Ginenthal noted that there is really only one clear-cut astronomical alignment at Stonehenge: the alignment from the center of the monument out the open end of the trilithon horseshoe and along the so-called Avenue toward the Heelstone and/or its one-time Partner Stone. As we have seen in the previous section, Burl has determined the Heelstone’s azimuth as 51° 3′ and the Partner’s azimuth as 48° 21′. Even if we take the midpoint of 49° 42′, such values are at best rather approximate.

I agree with Ginenthal that that alignment, approximate or not, is the one and only clear-cut astronomical alignment at Stonehenge. But that does still leave open the question of whether the perpendicular to that line is of any significance.

In an article in C&CR 2011, pages 7-18, F. Slade Barker has claimed that that perpendicular (page 9) is directed toward the region of Baffin Bay, which he suggests would have been the north pole of Earth’s rotation at some point in the past. He calls this point $P_a$ meaning “archaeo-pole” (page 8). That archaeo-pole of rotation was also recognized by Velikovsky, who mentions Baffinland, the Davis
Strait, and more precisely the Boothia Felix Peninsula (*Worlds in Collision*, pages 325-326). Velikovsky sees the ice of the Ice Age as a polar cap. In “Variations on a Theme of Philolaos”, KRONOS V:1 (1979), pages 12-46, I elaborated at great length on Velikovsky’s ideas, particularly his idea that Earth had once been a satellite of Saturn. Velikovsky knew and approved of my elaborations, but did not explicitly endorse them. Among other things, I used Velikovsky’s idea that Aden was Eden, and I suggested that a line from Aden or Eden to the former pole of rotation would have passed through or near the sites of Mecca, Gizeh, Alexandria, Knossos, Delphi, Dodona, and Stonehenge (page 39). I even noted that that line passed through the southeast and northwest corners of the Great Pyramid (page 39).

(Later, in “The Afar Triangle As the Nether Reaches of Eden and Babel”, *Aeon* II:4 (1991), pages 5-28, I suggested that when Aden or Eden overlay the Afar Triangle, it was atop a bulge that constituted the so-called World Mountain. That sub-Saturnine point was always turned toward Saturn, and was for that reason the most important place on Earth.)

Thus Barker’s $P_a$ may indeed have pertained to the time when Earth was in orbit around Saturn. Much geophysical change in the topography of Earth would have occurred in the meantime (shift happens, as C. J. Ransom has observed), quite aside from any lesser changes due to continental drift, and it follows that any such alignments will be at best approximate.

I have never thought for a moment that either the Great Pyramid or Stonehenge would date from the time of the former pole of rotation. I merely suggested that past cardinal points might still have been venerated by the architects of the Great Pyramid; indeed, the Great Pyramid seems to have incorporated *both* the present cardinal points (in its sides) *and* the former cardinal points (in its diagonals).

As I see it, the line that ran from Aden atop Afar through those various sacred sites to the vicinity of Boothia Felix was only very roughly perpendicular to the main axis of Stonehenge; I would say that it was closer to $95^\circ$ than to $90^\circ$. On that basis, I am suspicious of Barker’s somewhat lubricious conclusions regarding Stonehenge.

It is regrettable, though not surprising, that neither Barker nor his SIS editors saw fit to cite either Velikovsky or me.
HALLEY’S COMET

The earliest recorded observation of what we now know as Halley’s Comet is thought to have been from China, in 240 BCE. With the Short Chronology, we can now do much better than that.

Hunger has two documents — Report 339 and Report 456 — each of which refers to a comet. The reverse of 456 mentions the month of Marchesvan, but neither Report gives any other date; furthermore, there is no king named, and there is very little detail about the comet itself . . . just that it was in the path of Anu. Nonetheless, there is some other detail, about Jupiter, that might help us to some degree in dating these Reports.

The key passages in Report 339 are lines 1-2 of the obverse and lines 5-6 and 8 of the reverse:

1 [If] a comet [becomes visible in the path of the stars of] Anu:

r 5 [Ju]piter remaine[d] ste[ady in] the sky for a month of da[ys].

8 [From Ašaredu.]

The key passages in Report 456 are line 1, lines 5-6, and line 8 of the obverse, and lines 1-2, 4, and 8 of the reverse:

1 If a comet becomes visible in the path of the stars of Anu:

5 — Jupiter stood there one month over its period.

6 (If) Jupiter passes to the west:

8 — Jupiter stood there one month over its period.

r 1 Marchesvan (VIII) is the month of the king my lord.

2 If the moon becomes visible on the 1st day:

4 — On the 1st day the moon became visible.

8 From Bel-le’e, descendant of Egibi, exorcist.
Hunger dates both of these Reports to “-674 Oct 30”. As he sees it (and as I see it), Ašaredu and Bel-le’e are referring to one and only one comet here. According to Hunger’s Subject Index, those are the only two of his hundreds of Reports that mention any comets. (According to Parpola’s own Subject Index, there are no comets at all in any of his hundreds of Letters.) And if all of those Assyrian and Babylonian Reports (and Letters) mention only this one comet, why should it not be the one that is the most conspicuous of all of the repeating, shorter-period comets?

Lange and Swerdlow, Version 3.1, have Jupiter’s Evening Last on -674 October 30 at Nineveh, on October 31 at Ashur, and on November 1 at Babylon. Schoch has Evening Last on -674 October 29 at Babylon, with New Crescents on September 30 and October 30. Version 3.1 has New Crescents on September 30 and October 30 at all three cities, as does Starry Night. Thus it is indeed the case that “the moon becomes visible on the 1st day” (as opposed to the 30th). There is also complete agreement about November 29, if that is the “1st” that they had in mind. In -674, however, Halley’s Comet was nowhere to be seen: it had passed perihelion some fifteen years earlier and was by then more than half-way to the orbit of Neptune!

240 BCE is -239. If we go back from there, in two steps of 75 or 76 years each, we arrive at -390 or thereabouts. Version 3.1 puts Jupiter’s Evening Last on 11 October -390 at Nineveh and Ashur and on 12 October at Babylon; Schoch puts it on October 10 at Babylon. Parker and Dubberstein have New Crescents on September 10 and October 10 at Babylon. Schoch (as would be expected) and Version 3.1 and Starry Night agree with Parker and Dubberstein for Babylon. But Version 3.1 has the New Crescent at Nineveh and at Ashur on September 11, even though Starry Night has September 10 for both places. Thus it is not entirely clear whether “the moon becomes visible on the 1st day” (as opposed to the 30th), unless they meant November 9, about which all three cities and all of these programs agree.


Even though it would be only a rough approximation, mid-September of -390 fits the expected time of year beautifully. Note that the comet is the first item both in Report 339 and in Report 456. It is therefore reasonable that the arrival of the comet should precede the Evening Last of Jupiter, which was in mid-October. (Marchesvan usually fell in October or November.)
I am much encouraged by all this, and I think that we now should, with due caution and with appropriate caveats, continue to wave Halley’s Comet under entrenchment noses. Hunger will have a cow.

The perihelion passage of Halley’s Comet that Ginenthal proposes to use for the pillar of cloud by day and the pillar of fire by night at the time of the Exodus is stated by Yeomans and Kiang as “-762 Aug. 5.53008”. See Pillars, Volume II, page 528. Thus we now have two visits of Halley’s Comet that support the Short Chronology! (The -762 remains valid as a retrocalculation of Halley’s Comet; from a Velikovskian perspective, however, Earth’s subsequent encounters with Mars would make the year-count problematic.)

By the way, Yeomans and Kiang are quite concerned about planetary perturbations, as well as about out-gassing and “non-gravitational forces” generally. They do take such factors into account, rather convincingly. For example, they argue that out-gassing is a more-or-less steady process, and does not preclude careful retrocalculation. What is precluded, according to them, is any retrocalculation beyond -1403. This is both because they retrocalculate that Halley was very near Earth in -1403 and because there are no historically-dated visits from prior to that that they can use as benchmarks; consequently, Yeomans and Kiang cannot tell what happened earlier than -1403. Any known earlier visits would have allowed them to retrocalculate past -1403, as they had already done in the case of several other such close passes; in the absence of any known earlier visits, -1403 is where they have to stop retrocalculating. Yeomans and Kiang do not deal with anything like the Velikovsky Divide, but we would not have expected them to do that, anyway. From a Velikovskian perspective, however, there is no way to tell precisely where Earth was located in -1403, and Velikovskians cannot say that Halley came close to Earth at all in that year. Earth might have been in some other sector of its orbit, whatever its orbit at that time was.
PREAMBLE TO JONSSON

My own work on the Neo-Assyrian and Neo-Babylonian issues got into high gear when Charles Ginenthal (after numerous requests, pleas, and reminders) finally succeeded in getting me to study the Saturn Tablet of Kandalanu. The results of that long study appeared at the beginning of my Appendix to Volume II of Pillars of the Past. That Appendix provoked a strongly-worded reply from Carl Olof Jonsson, which appeared in C&CR 2011, and which provoked a strongly-worded further reply from me. The SIS editors, who had already printed Jonsson’s piece, chose not to publish mine, because it was too long and too nasty, so I include it here. With regard to nastiness, what could be nastier, or sillier, than having the Chair of the SIS compare my decades of work on the El-Lahun papyri to playing Sudoku? In my opinion, however, escalation is always the best policy, except in nuclear war. As Sean Connery’s character said in The Untouchables: “If they put one of your men in the hospital, put one of theirs in the morgue.” Such policies make for more effective communication. There is even a deterrent value.

REPLY TO CARL OLOF JONSSON

In Carl Olof Jonsson’s critique (C&CR 2011, pages 33-37) of my Appendix to Charles Ginenthal’s Pillars of the Past, Volume II, pages 597-644, he introduces the Saturn Tablet of Kandalanu as something that he “briefly described” (page 34) in his book, The Gentile Times Reconsidered. What Jonsson actually quoted there from the Saturn Tablet was indeed only part of the surviving text, but his overall discussion of the Saturn Tablet does run to more than two pages (pages 169-171). The treatment is fairly detailed, as well as emphatic, and the Saturn Tablet gets just about as much space as many of the other important sources that Jonsson discusses. Why say “briefly”? To distance himself from what has turned out to be a mess?

Jonsson and others were eager enough to embrace the Saturn Tablet when they thought that it supported the entrenched chronology. They should be equally willing to embrace it now that it supports a different chronology.

Jonsson also says (page 34) that in his book it was C. B. F. Walker who “was quoted as stating” that the Saturn phenomena would not fit again for another seventeen centuries plus. Why say only that Walker “was quoted as stating” something, when the fact is that Jonsson himself stated the same thing? In this situation, giving only a carefully selected part of the story amounts to spreading a
false impression among the readers that Jonsson himself bears no responsibility for the subsequent fiasco.

Jonsson (page 34) seizes upon Teije de Jong’s reference to a possible “uncertainty of up to five days” (page 177) and applies it in an invalid manner. de Jong mentions those “five days” in only one sentence and then never mentions them again. We are not even told how he arrived at such a figure. The “five days” remark is just put there in passing, for the unwary to be taken in by. I do note that one of Walker’s retrocalculated dates differs from the reported date that de Jong uses by five days. But that may be only a coincidence. In any case, de Jong’s “five days” is not an across-the-board figure; it would be an extreme or worst-case situation, referring to a rare combination of circumstances that might happen once in a while within the course of a substantial stretch of time. de Jong’s five days should not be elevated into an indulgence or license to wreak havoc with history. A retrocalculated date that was wrong by five days would surely not happen in the case of every single event within the 14-year stretch covered by the Saturn Tablet of Kandalanu. Thus Jonsson’s belief that Walker’s fit is good because he never misses by more than five days is ridiculous. We have to look at the details of an alleged fit before we can say anything about its quality.

Jonsson seems not to understand how retrocalculation in fact works. Retrocalculation gives us the most likely date for an event. Adjacent dates are sometimes possible, but are less likely. In the case of a retrocalculated solar eclipse occurring at mid-day, say, twenty-three centuries ago, the chance of the retrocalculated Julian date being wrong is virtually nil; the adjacent dates can be ruled out. A retrocalculated New Crescent of the Moon might be off by one day in a small fraction of cases; we might then have to say that that date is too close to call. Relative to the Sun, the Moon moves across the sky quite rapidly. Saturn’s own separation from the Sun changes rather slowly. Thus a retrocalculated appearance or disappearance of Saturn would be less certain than a lunar appearance or disappearance; one or more of the near-by dates might also come into play. Even so, retrocalculation would give us the most likely date. The near-by dates would be possible, but less and less likely as we go farther into de Jong territory.

We should bear in mind here that most modern retrocalculations appear to be fairly accurate, and that the seeing in Mesopotamia seems for the most part to have been fairly good. In no way are things quite so bleak as Jonsson would have it.
It is entirely appropriate that we compare and test two rival chronologies by seeing how well they match up against the most likely retrocalculated dates. That is exactly what I have been doing.

Jonsson seizes upon reported last visibilities that are later than retrocalculation would suggest. He says that I have “four of the eight [recorded] days of last visibility” too late by “a day or two” (page 34). That is false. The eight values are +1, -2, +1, 0, 0, +1, +1, and 0. The four +l’s are the ones that Jonsson is referring to. Notice that, despite Jonsson’s “or two”, there is no +2 here! Some or even all of those +l’s might be explainable in terms of Schoch’s own pronouncement that his retrocalculations for Saturn involve “an uncertainty of one day only” (page 96).

Jonsson proceeds similarly with the reported first visibilities that are earlier than retrocalculation would suggest. He says that I have “3 out of the 9 days of first visibility” too early by “a day or more” (page 35). This time he is correct. The nine values are +2, 0, -1, -3, 0, +1, 0, -1, and +2. The -3 and the two -l’s are the ones that Jonsson is referring to. But both of the -l’s might be explainable with Schoch’s ±1 day. They might also be explainable if bad seeing delayed the start of the lunar month for one day, in which case the lunar day numbers would be reduced by one.

The only real oddity here is the -3, which leaves me with only one miss out of seventeen, or less than 6%, as opposed to the “41%” that Jonsson tries to saddle me with.

What about Walker? Jonsson claims that for Walker “only 2 out of the 17 observations — 11.7% — do not fit”. But if we count Walker’s results exactly the way that Jonsson counted mine, Walker misses 8 of 17, or 47%, as compared to the 41% that Jonsson reports for me. How does 47% become only 11.7%?

Whether we count Jonsson’s way or my way, my fit comes out better than the Walker-Jonsson “fit”. But both of us should be treated the same way. Thus if we invoke Schoch’s ±1 day, Walker would miss 5 of 17, or 29%, as opposed to my less than 6%.

Are any of Walker’s five misses helped by a delayed beginning of the month? Or by inaccurate retrocalculation? If each of Walker’s three first visibility scores of -2 involved a previous month of 29 days that because of bad seeing was counted as a 30-day month, thus delaying the day-count within the current month by one day, or if there was some similar circumstance during the previous several months, involving multiple cases of bad seeing at just the right points, once again delaying
the day-count within the current month by one day in each case, and if each of
those three first visibilities of Saturn was actually one day earlier than modern
retrocalculations would suggest, then each of those three first visibility scores of -2
would become acceptable. For purposes of discussion, let us momentarily allow
each of those if’s. That leaves Walker with two misses, the -5 and the -4, or
11.76%. Apparently Jonsson was indeed granting himself and Walker each of
those if’s. That is the only way that I can make any sense of what Jonsson says
about “only 2”. But I still win, with only one miss and with 5.88%. I am neither
surprised nor impressed by this close, initial “victory” of mine; there is far too
much else that has not yet even been considered.

Jonsson claims that he tried Lange and Swerdlow’s *Planetary, Lunar, and
Stellar Visibility*, and that “most of the problems disappeared”. I too have checked
Lange and Swerdlow — several years ago and again just now — and the problems
with the Walker-Jonsson dating are still there. Some of the numbers from Schoch
do change, as would be expected, but the Walker-Jonsson dating actually fares
even worse in the hands of Lange and Swerdlow. The fact is, however, that I do not
fully trust Lange and Swerdlow on horizon phenomena, especially planetary and
stellar. For one thing, they use a simplistic formula for the visibility conditions that
has not yet been shown to be reliable. For another, their heliacal risings of Sirius in
the fourth century are consistently early by a full day. They often recognize too
much visibility; furthermore, they ignore important benchmarks that are available
and that would have kept them from messing up so. Schoch’s procedures are both
subtler and more sophisticated. Lange and Swerdlow do seem to do better in the case
of horizon events involving the Moon than they do with the planets and the stars, but
they still do not do nearly as well with the Moon as Schoch does.

(Just what are those “problems” that arise with retrocalculations for the seventh
century? If anything within five days is OK, there should presumably not be any
problems at all! By the way, how can “most” of two items have “disappeared”? What is “most” of two?)

In the Appendix I showed that the Walker-Jonsson “fit” is skewed sharply to
the negative. That is, the algebraic sum of their scores, including both last
visibilities and first visibilities, is -28, which reflects the fact that the Saturn Tablet
is actually being placed too early with respect to the retrocalculations for that
epoch. Walker and Jonsson’s mode score is -2. Their median score is -2. The
average of the absolute magnitudes of their scores is 1.7647. The algebraic sum of
my scores, including both last visibilities and first visibilities, is +2, which is
negligible. My mode score is 0, which is ideal. My median score is 0, which is ideal. The average of the absolute magnitudes of my scores is 0.941. A little lower average would have been nice; nonetheless, I did beat Walker and Jonsson soundly: 0.941 is barely 53 percent of 1.7647. For the record, it should be noted that Jonsson does not mention any of the results now repeated in this paragraph. Why not? Such results are my case here.

(Another way to approach this — which I did not even mention in the Appendix — is to retain the signs while averaging the scores. Walker and Jonsson would then have a mean of \(-28 \div 17 = -1.647\), while I have a mean of \(+2 \div 17 = only +0.1176\), which is very close to the ideal of zero.)

What do all of these results mean? They mean that my fit is centered and balanced and relatively tight, as it should be. They mean that the Walker-Jonsson “fit” is skewed and unbalanced and relatively loose, as would be expected if somebody decided to force the Saturn phenomena into a century where they do not belong. That is precisely what Walker and Jonsson have done. It is no wonder that they are so far off the mark. They are merely pretending to have a good fit when they do not have one. They do not seem to know what a good fit looks like, even when I wave one under their noses.

Both Walker and Jonsson said that there could not be a fit for the Saturn Tablet any closer than seventeen centuries plus from the seventh century before this era. Then I did it. They had both said that that could not be done. Why did Jonsson not bother to tell his C&CR readers that part of the story?

If I had declared that something could not be done, because it was impossible, and then Jonsson had come along and done it, I would have been devastated, and I would have been forced to reexamine my entire world-view. Why doesn’t Jonsson react that way? Is it because his life’s work would collapse?

It is ironic that the Walker-Jonsson product of the mean synodic month (about 29.53 days) and the double sidereal period of Saturn (about 58.9 years) is sound, at least to the extent that good fits apparently are indeed separated by between 17 and 18 centuries. But things can work out well for Walker and Jonsson only if their seventh-century “fit” is good. Since their “fit” is bad, and since my fit is good, the rule of seventeen centuries plus should be invoked for my fourth-century placement of the Saturn Tablet, and we could then say that there should not be another good fit for seventeen centuries plus in either direction from there. That would put us
either in the Renaissance or else just over four millennia ago. Neither of those seems plausible, which leaves the Saturn Tablet in the fourth century. The seventh century is out of the picture.

Jonsson has two principal objections to my placement of Nabopolassar. One is that I have the reign of Nabopolassar overlapping that of Kandalanu. That is indeed what the astronomy indicates, and those who, like Jonsson, do not believe it are in conflict with the astronomy here. Any other evidence that they present does not alter the situation: they and their evidence are still in conflict with the astronomy.

Jonsson’s second objection is that I dare to associate the beginning of the year with the vernal equinox. Judgments about when the Assyro-Babylonian year began are abundant and varied in the literature. Many who opine about this matter have started with the conventional, entrenched chronology and then looked to see where various years seem to have begun, on that chronology. If we change the chronology, the picture can be quite different. For whatever it is worth, my overall impression is that the Neo-Assyrian and Neo-Babylonian year often began a little earlier than most scholars have recognized. In particular, I suspect that Parker and Dubberstein often have the beginnings of the Babylonian years somewhat late.

There seems to have been considerable jumping around. To intercalate or not to intercalate; that was the question. Such decisions could have been guided by the position of the vernal equinox, by stellar observations, by the desire of tenants to have 13 months for the price of 12, by the desire of landlords not to allow 13 months for the price of 12, by the desire of borrowers to delay repayment by a month, by the desire of lenders not to delay repayment by a month…. The list of motivations goes on and on. Even when there were supposed to be intercalation rules, those rules might have been set aside on occasion. Autocrats could do as they pleased. Julius Caesar even appears to have decided to have four intercalary months (averaging 22½ days) in one and the same year. For over three decades, his immediate successors decided to have an extra day every three years rather than every four years. Then Augustus decided to have no extra days between -8 and +8, and thereafter to have one every four years.

We can retrocalculate the beginnings of lunar months, but we frequently have no information at all about the names or identities of those lunar months. If we do not know which one was Nisan, we do not know when that year began. Whatever we do, we should not allow the entrenched chronology to influence our judgments regarding
these questions. (By the way, my opinions here are derived not just from the *Mul Apin*, but from working on ancient astronomical records for over forty years.)

I find Jonsson’s entire discussion here worthless; everything that he says or cites (Hunger, for example) is based on the entrenched chronology, the very point at issue. Logicians call this *petitio principii*. I cannot accept something just because Hunger or somebody says it. Neither should Jonsson. Hunger is just another cheater. I caught Hunger with his proverbial pants down. See my discussions of his Reports 300 and 487 on pages 622-623 of my Appendix, as well as my letter on his Report 300 (that letter is quoted in Chapter Two of *Pillars*, Volume II, on pages 239-242).

I do commend Jonsson for having the good sense not to defend Walker’s use of the seriously flawed planetary tables of van der Waerden. *Nobody* should ever trust van der Waerden — or his loyal amanuensis, Huber. Unfortunately, some of Walker’s own tables are also flawed, as I have pointed out (Appendix, pages 609-610).

The one and only thing that I learned from Jonsson’s critique is that B.M. 41222 shows that, on the conventional chronology, Year 12 must have begun *after* the vernal equinox.

My long paper on Nabopolassar has never been published, but it was abridged and summarized as part of my Appendix. The project involved making use of the attested intercalary months and then seeing whether the attested 29-day and 30-day months would actually occur under Nabopolassar. With the conventional placement of Nabopolassar, there were only 13 of the 27 such attested month-lengths that were actual hits. *Any* chronology might be expected to hit about half of the month-lengths; 13 hits is just below average, that is, just below random. The true chronology should do considerably *better* than fifty percent.

Since a score as low as 13 out of 27 was and is a matter of major concern, and since I had long been of the opinion that scholars may have sometimes started the month Nisan a little late, anyway, I thought that, in all fairness to the conventional placement of Nabopolassar, I should investigate what might happen if the entire sequence were started one month earlier. Much to my surprise, and even to my chagrin, I found that the score then became 19 out of 27, which is just above *seventy* percent. My own score in the fourth century was 20 out of 27, and I did not like having to admit the comparison.
What Jonsson has done is to point out that B.M. 41222 has Mars passing just above Aldebaran on the fifteenth day of month V in Year 12. In -613, that month V must have been the one that started on July 19, so that the year would have started on March 23, just as I had had it in the original sequence. Thus anyone who accepts the Mars material from B.M. 41222, as Jonsson does, would have to conclude, as Jonsson does, that the sequence that started one month earlier and that scored 19 of 27 is invalid and must be discarded. It also means that the conventional chronology falls back to a mediocre score of 13 of 27. I am deeply grateful to Jonsson for having provided me with such a strong piece of evidence against the conventional chronology, and for sparing me the continuing embarrassment of having to grant my opponents a score of 19 out of 27.

But does the Mars material from B.M. 41222 apply to -329? Not necessarily. Astronomical Diaries as we have them are not contemporary accounts of the events that they describe. They are neat, polished accounts that were put together at least some months later than and possibly even centuries later than those events. Many of the Diaries contain internal evidence or even statements that they have been copied from earlier sources.

In the section “The Easy And The Impossible” on pages 636-638 of my Appendix to Pillars II, I was quite mistaken in grouping Diaries with Reports and Letters as contemporary sources, the sorts of documents that describe events that took place only shortly before the documents were written. (See especially page 637.) I now realize that I should group many of the Diaries with other historiographical materials, such as many of the LBAT materials, that were part of ancient efforts to rewrite history.

The way I described this in 2008 was as follows (page 637):

“Suppose that the Original Chronology, which was essentially true, and which was at least roughly consistent with the Short Chronology, did record eclipses and other such items, either by reign or by some other scheme. Then suppose that someone fabricated what is now the Fake, Conventional, or Establishment Chronology, and transferred the appropriate historical eclipses to it. Voilà! Ages in Chaos! Something along those lines must have happened; otherwise, there would have been no Ages in Chaos in the first place.”

This applies not just to eclipses, but to any astronomical material, including planetary and lunar positions. I summed it up this way (page 638):
“What I end up with, then, is that Kandalanu did live in the fourth century, that someone active in the third century did move Kandalanu, the other Neo-Assyrians, and the Neo-Babylonians back to the eighth, seventh, and sixth centuries, and that a number of generally accurate astronomical records from the eight and seventh and perhaps the sixth centuries were duly plugged into the appropriate reigns, with whatever adjustment of the reigns was necessary.”

(If I were writing that today, I would say “third century (or perhaps later)”, and I would definitely delete the “perhaps the”.)

The overall situation is easily described. Ancient history has many of its Ages in Chaos. This came about because of radical distortions, some ingenuous and some disingenuous. It is now up to us to correct those distortions. This will require radical rearrangements of many ancient peoples and events.

The radical distortions that were part and parcel of the Fake Chronology were perhaps difficult to sell, but the Fake Chronology did eventually become not only accepted but firmly entrenched. The Short Chronology that aims to replace the Fake Chronology will also be a hard sell. Radical ideas are seldom welcomed.

For example, the way that I handle the two Nebuchadnezzars will displease nearly everyone. But with Nabopolassar locked in down to -320, and with Year 2 of Nabonidus now locked in at -293, I am required to depart radically even from the entrenched relative chronology of Mesopotamia. I am quite prepared to do just that.

Jonsson has for years been writing letters to entrenchment scholars from the fields that interest him. These letters have often yielded valuable comments and explanations; they have also yielded copies of unpublished source materials and copies of unpublished manuscripts. No doubt the generosity and openness of some of those entrenchment scholars has resulted in feelings of gratitude and respect on Jonsson’s part. The gratitude is understandable, but the respect is frequently misplaced, and constitutes a barrier to clear insight. Thus Jonsson tends to ignore the shortcomings of Sachs, Hunger, Walker, and Stephenson. Some, perhaps most, of their work may be useful, but we need to take all of it with a barrel of salt.

With regard to Sachs, see pages 700-701 of “From Calendars to Chronology”, where I discussed his hateful diatribe against Velikovsky. Even his scholarly competence is questionable. On pages 698-699, I summarized Sachs’s mishandling of the four Ninsianna fragments in Late Babylonian Astronomical and Related Texts [13], namely Numbers 1560, 1561, 1562, and 1563 (all on page 249).
Sachs’s efforts on page xxxvi to describe the content of these four fragments are wrong, wrong, wrong, and wrong, respectively.

Calling Stephenson “reliable” (page 36) is quite a stretch. Apparently Jonsson is unaware of Stephenson’s faulty research and outright deviousness. Read Stephenson about Ugarit. Read Muller and Stephenson about Shang (pages 489 and 523). Read Keightley about Muller and Stephenson. Read R. R. Newton about Muller and Stephenson. Then read Stephenson’s whitewashed Bibliography in his magnum opus, and you will find that it suppresses all of the things that you have just read. (Stevenson does list the Muller and Stephenson paper, but he does not reveal what their paper had said about their alleged Shang “eclipse”, or what Keightley had to say about them.) For further discussion of all these matters, see eventually my “Schoch, Tuckerman, and Goldstine: A Defense of Their Lunar Retrocalculations for Antiquity”, in progress. In the meantime, I have listed all of these items in the References at the end.

Jonsson makes much of the lunar eclipse on 26 September -553. Stephenson’s efforts in behalf of Jonsson and in behalf of the conventional or entrenched chronology do nothing to change a rather close call into a clear-cut result. The sixth century is rather early, and ΔT does involve some margin of uncertainty. Did the Moon set still partially eclipsed on 26 September -553? Or was it not still eclipsed? Frankly, my dears, I don’t give a damn. If it did set eclipsed, it was indeed a fairly close call, and my total eclipse on 2 October -293 is more clear-cut. If it did not set eclipsed, then the entrenched chronology misses, and I still have a clear-cut hit. Whether narrow or one-sided, the victory is mine. (By the way, I see 26 September -553 as a rather piddling little incident, not necessarily a worthy or compelling omen for the Moon-God to use in communicating in effect that a daughter of the king should be installed as High-Priestess of the Moon-God. 2 October -293 is a clear-cut, dramatic, and unquestionable event: the Moon was totally eclipsed for some time before setting and for some time after setting. Even ΔT cannot change that.)

What I do care about here is the sad spectacle of Jonsson bowing low before the great idols Stephenson, Hunger, and Sachs. From that position, he ought to be able to see the feet of clay, but he remains stoutly and devoutly oblivious.

Some of the fits that I produce for the fifth, fourth, and third centuries result in a tie with the conventional fits that have been produced for the eighth, seventh, and sixth centuries. I do not offer those of my fits as evidence of anything, except that whatever my opponents can do in the earlier period I can do in the later period. But
in a number of other cases I do better than they have been able to do. Such was the case with Kandanalu; with Nabopolassar; with Reports 42, 64, 102, 207, 300, 384, 396, 487, and 506; with Letter 371; and with Nabonidus. In several of those cases, the orthodox scholars have been able to do nothing at all, while I have been able to find an acceptable fit. The overall astronomical situation, clearly, is advantage Rose.

Along the way, Jonsson mentions this or that bit of historical or historiographical lore that is alleged to support the entrenched chronology. None of that changes the overall situation: Jonsson and his confrères are in conflict with the astronomy. That is a most uncomfortable position in which to find oneself. No wonder Jonsson is squirming so. His life’s work is in jeopardy.

When Jonsson does have some historical materials that seem to him to point toward the entrenched chronology, he is quick to claim that my chronology is incorrect. My answer is that the astronomy proves that his chronology is incorrect. I see Jonsson as trying to believe six (or six thousand) impossible things before breakfast. He obviously sees me the same way. At least I am trying to work my way through the many problems that we face. Jonsson does not even allow himself to acknowledge those problems.

Jonsson makes much of the bed of Prokrustes. But that bed could lead to death in two different ways, either by truncation or by stretching. Jonsson thinks that the short chronology is doomed because Prokrustes, representing the historical facts, will stretch it back to its proper length. I counter that the entrenched chronology is doomed because Prokrustes, representing the astronomical facts, will lop off its excess centuries to make it finally fit as it should.

Jonsson and others need to scrutinize those “tens of thousands” (page 37) of proofs that I am wrong. And they need to do much better than Walker and Jonsson did with the Saturn Tablet. Every time that Jonsson alludes to something that he thinks refutes me, he should be reminded that he thought much the same thing about the Saturn Tablet and that he was dead wrong. How does he know that the outcome will be any different this time? His record fails to inspire much confidence in his judgment.

Ginenthal and I take the evidence from hard sciences like astronomy much more seriously than the pedantic propaganda and poetic puffery that we sometimes get from soft sciences like history. While I contemn both Henry Ford and his namesake grandson, each of those jerks did manage, during his lifetime, to say one thing worth
pondering here: Henry Ford said “History is more or less bunk”, and Henry Ford II (apparently getting it unawares from a remark by Benjamin Jowett?) said “Never apologize, never explain”. I do think that much of history and historiography is bunk, and I do not apologize to anyone for putting the hard evidence from such fields as astronomy much higher than the soft evidence that is bandied about.

(I have made a number of efforts, not so much to apologize, but merely to explain my position. Yet people like Jonsson remain oblivious to such explanations. On the other hand, I do sometimes regret that I have wasted so much of my time explaining things to people who are not even listening. Perhaps I myself should have listened a bit more to Benjamin Jowett and/or Henry Ford II.)

Jonsson, like many others, is mired in a slough of contradiction. He and the others have assembled a considerable amount of material that seems to them to point toward the entrenched chronology, or at least toward the entrenched relative chronology (that is, the relative sequence as well as even the overall duration of the relevant historical events). But no matter how much such material they assemble, their problem is that the astronomy still points toward a different chronology.

We — and they themselves — need to go over their soft evidence, item by item. Ginenthal and I have for years been doing as much of that as we can. Quota pars operis tanti nobis committitur? A very small part. It is a task for many hands, and will require many years, perhaps many generations. We need to see if each item does or does not do what it has been said to do. All of those items have been viewed through entrenchment lenses for days, decades, centuries, or millennia, as the case may be. After they have been sifted, purged, interpreted, construed, edited, and “restored”, they are blandly and blindly cited by people who are completely confident that they already know the correct chronology, anyway. That is why so many of them believe, often sincerely and sometimes even devoutly, that such items must fit the entrenched chronology.

I have already been down this same path too many times to count. I find a reference to something that is said to support some aspect of the entrenched chronology. I try to look it up. Then I usually find that the item does not say what the scholars claimed that it said. For example, the Turin King-List is on the back of a Tax-List sometimes assigned to Ramses II (and therefore to the second millennium). But it could be any Ramses. Velikovsky puts the later Ramesides in the fourth century. The King-List on the back of this Rameside Tax-List could be from even later than that. For all we know, it could have been written by Manethon!
The idea of ancient Ages that are in Chaos may not appeal to Jonsson, who has devoted so much of his life to trying to validate the entrenched Neo-Babylonian chronology. But it does appeal to me, and I think that it still appeals even to many readers of *Chronology & Catastrophism Review*, most of whom will have wondered how those Ages got to be in Chaos, and most of whom, even if they are not Velikovskians at all, do still have some degree of interest in revised (non-entrenched) chronologies of one sort or another.

I do not claim that the ancient Ages always got into their present condition of Chaos by fakery. In many cases they may have gotten into that condition by error or accident. But I do think that fakery was a major factor, especially during Hellenistic times, and that Manethon and Berossos were major players in such fakery, though there may have been many others involved. Hellenistic kings set their pedants and savants to proving that their countries were older than the countries of the other kings. *My history is longer than your history*, as it were. Such veneration of the hoary can be seen even today, except that today it is the entrenchment scholars themselves, not their kings, who set themselves to defending false chronologies that are much too long. Where is Procrustes when we need him?

After the Appendix had gone to press, I found that the eclipse that marked the death of Psammetichus I could be identified with the total solar eclipse that crossed northern Egypt on 4 July -335. (*Everybody* now computes *that* one as total.) This permits some fine-tuning of the dates of the Neo-Assyrian kings. The astronomical material in my long letter (see pages 242-254) concerning Report 102 remains unchanged, but that Report, which both Hunger and I had taken to be from late in the reign of Esarhaddon, would now be from early in the reign of Ashurbanipal. Year 1 of Sargon in Babylon, which is mentioned in Report 501, would be -434/3, with Year 1 of Sennacherib being -430/29, with Year 1 of Esarhaddon being -406/5, and with Year 1 of Ashurbanipal being -394/3. (In each case here, I do indeed mean Year 1, *not* the Accession Year.) The total lunar eclipse of 8 July -391 would have been in Year 4 of Ashurbanipal, and thus Letter 371 can be dated to that same regnal year. Report 102 would have been from March of -391, which can thus be dated to late in Year 3 of Ashurbanipal. I had previously assumed that both of those documents were from Esarhaddon, but that assumption was a mistake.

With some caveats, I accept virtually all of Edwin Thiele’s work, especially the *durations* that he recognizes. As will be explained later, however, I accept Thiele as a relative chronology only. In order to make it absolute, I have to move his
whole chronology forward in time by 274 years. The results appear quite satisfactory (pace Damien Mackey, who speaks of “Thiele’s chronological slaughterhouse”).

Those who say that historical records contradict my chronology should not be saying that unless they also admit that astronomical records contradict their chronology. If they are unable to see that, they must have their heads in the sand.

A few closing points:

“8 years (Rose says 7)” (page 35). I have examined this from every perspective that I can think of, but I still do not know what Johnson is talking about here.

I maintain that “the fact that the 294 BC eclipse was total” is “a valid criterion for giving it preference to the 554 BC eclipse” (page 36).

I deny that “the Venus Tablet [sic] is useless for chronological purposes” (page 37).

I have not even moved Cyrus (see page 37), let alone put him after Alexander the Great!

With regard to the opening words of footnote 24 (page 37), have you ever noticed that when someone introduces a claim with the words “of course”, that claim is often not a matter of course at all? In much the same way, the “you know” and the “I mean” of current speech are almost always false. And as for “like” . . . spare me.

THE TWELFTH DYNASTY

The El-Lahun papyri in particular and the Twelfth Dynasty chronology in general have been featured in several of my previous publications that are listed in the References. These include the lengthy JNES article (1994); the article in The Velikovskian (1994), which was the first time that my placement of most of the El-Lahun papyri in the first half of the fourth century appeared in print; the long article “From Calendars to Chronology” (1996); the book Sun, Moon, and Sothis (1999), especially Part III; and the C&CR article (2005) entitled “The Feasts and the Crescents”.

That “Feasts” paper also corrected several errors from the book. The most serious of these, from my point of view, were that I had failed to distinguish clearly enough between psdntjw-based dates and Crescent-based dates, and that I had
failed to stress that the Crescent-based dates are much more informative and much more important than the psdntjw-based dates.

Another regret that I have is that there was only the one book. Perhaps the most notable feature of reactions to Sun, Moon, and Sothis is that nearly everyone focuses on Chapter Nineteen (in Part Three) and ignores the rest of the book. Almost no one has paid any attention to Part One or Part Two. For many years the book-in-progress consisted of and was a long Preface, Part One, Part Two, and an Appendix or two. Part Three was something that I stumbled upon later. I now wish that I had published the book as originally written, without Part Three, and then published Part Three as a separate book some years later. That way, perhaps Parts One and Two would have been given their due.

I would like to be able to say that readers will find in “The Feasts and the Crescents” an accurate statement of my present views on the El-Lahun papyri. Before I can do that, however, I must enumerate those few aspects of “Feasts” that are incorrect or that are otherwise objectionable. Some of these aspects are trivial; others are far from trivial. (One of the latter concerns Berlin Museum papyri 10007 and 10090 (or “A”), which are the principal subjects of this present section of the Appendix. Another is Daphne Chappell’s work and its impact on document D; that work will be discussed in a later section of the Appendix.)

I have a vague memory that there was an “of” missing somewhere in “Feasts”, but, upon rereading the paper, I cannot find it. The eye tends to see what is supposed to be there. Or perhaps I was thinking of some other paper. In any case, if there is an “of” missing, it should be reinserted.

In the chronological chart on page 17, the expression “-One” should be a little to the left, so that Dynasty Twelve and Dynasty ‘Thirty-One’ end simultaneously, with the coming of Alexander the Great. A correct version of that chart is given on page 293 of my book.

The phrase “a bad seeing” appears on page 18. That was not my doing. I have been totally unsuccessful in my efforts to explain to SIS editors that “bad seeing” is a legitimate English expression, one that is widely used by astronomers, both professional and amateur, and one that not only does not need, but also should definitely not have, an article in front of it. It is like “bad weather”. Only a person with a limited knowledge of English would say, “We are having a bad weather today.” I do not defer to the British on matters of English usage.
On page 20, for “).”, read “.)”. On page 21, for “king-Lists”, read “King-Lists”. Notes 19 and 26 need commas after the authors’ names; note 21 needs two commas. In note 34, for “Sesostris III”, read “Amenemhet III”; I cannot forgive myself for that one.

Aside from all that — and aside from the views of Chappell about document D, and aside from the status of 10007 and of 10090 (or “A”), all three of which will be discussed in due course — there is nothing in the “Feasts” paper that needs to be changed. I might add that no one has laid a glove on any of the major theses of the “Feasts” paper. That is not for lack of trying. The References also include several of my replies to critics.

Let us turn first to my new findings regarding 10007. This may not seem like very much of a breakthrough, but to me it is a major breakthrough, because it gives us the absolute regnal years of Sesostris II and thereby the absolute regnal years of all three of his Twelfth-Dynasty predecessors. It also gives us an El-Lahun document attributable to the reign of Sesostris II (something rather rare), and at the same time a document that constitutes additional support for my overall placement of the Twelfth-Dynasty.

The super document that does all of this is indeed Papyrus Berlin 10007, which I had looked at many times before, but without ever being able to see what was there. Eventually, I was provoked into looking at it again by the outrageous things that Rolf Krauss, in SCIEM II, pages 186-187, says about the moveable wag-feasts on 10419 and 10165. That led me to review all of the surviving moveable wag-feasts from the El-Lahun papyri, whether they have fully-stated dates or not. One of those partially-dated moveable wag-feasts is on 10007; it is in II šmwy, but with no day-number.

10007 mentions various feasts that are spread out over about ten months, from III šprt of Year 1 to III 3ht of Year 2 of an unnamed ruler. The only surviving day-numbers in the entire text are a III 3mwy “last day”, that is, 30, and the I 3ht 1 that marks the beginning of Year 2. In all other cases, we have only the month-number and the season (or sometimes nothing at all). Later in Year 1, there was a feast of the heliacal rising of Sirius in IIII šprt and there was that moveable wag-feast in II šmwy, but we do not have either of the day-numbers. Those two day-numbers would have been of considerable interest to me.
Under III \(prt\) of Year 1, there are two virtually obliterated lines, then a line with a New Crescent feast followed by the numeral 7, and then a line with a Full Moon followed by a sign that often serves as the numeral 9. Luft (pages 46-47) interprets the 7 as the feast of the seventh lunar day and the “9” as \(psdntjw\) or lunar day 1. (There is ample precedent and justification for these readings, and I do not object at all to what Luft does here.) Thus we would have a New Crescent feast, a feast of the seventh lunar day, a Full Moon, and then a \(psdntjw\) or lunar day 1, all occurring within III \(prt\). That is a rather restrictive situation. The interval from New Crescent day to the following \(psdntjw\), inclusive, is sometimes 28 days, sometimes 29 days, and sometimes 30 days. Having all four of those feasts squeezed within III \(prt\) is not something that happens even every decade. The New Crescent must be at or near the beginning of III \(prt\), and the \(psdntjw\) must be at or near the end of III \(prt\). I tried Year 1 of Sesostris III in -400; then Year 1 of Amenemhet III in -381; then Year 1 of Sebeknefru in -334; then Year 1 of “Amenemhet IV” as co-regent in -343; and then my tentative Year 1 of Sesostris II in -406. Not one of these even came close.

At this point, the reader may wish to review my chronological table for the Twelfth Dynasty from page 269 of *Sun, Moon, and Sothis*:

<table>
<thead>
<tr>
<th>Year</th>
<th>Event</th>
</tr>
</thead>
<tbody>
<tr>
<td>-500  (x + y)</td>
<td>Amenemhet I, Year 1 (ascension on II (\tilde{smw}) 9 = September 30 Julian?)</td>
</tr>
<tr>
<td>-480  (x + y)</td>
<td>Amenemhet I, Year 21 = Sesostris I, Year 1</td>
</tr>
<tr>
<td>-472  (x + y)</td>
<td>Amenemhet I, Year 29 = Sesostris I, Year 9</td>
</tr>
<tr>
<td>-471  (x + y)</td>
<td>Sesostris I, Year 10</td>
</tr>
<tr>
<td>-438  (x + y)</td>
<td>Sesostris I, Year 43 = Amenemhet II, Year 1</td>
</tr>
<tr>
<td>-436  (x + y)</td>
<td>Sesostris I, Year 45 = Amenemhet II, Year 3</td>
</tr>
<tr>
<td>-435  (x + y)</td>
<td>Amenemhet II, Year 4</td>
</tr>
<tr>
<td>-406  (x + y)</td>
<td>Amenemhet II, Year 33 = Sesostris II, Year 1</td>
</tr>
<tr>
<td>-404  (x + y + z)</td>
<td>Amenemhet II, Year 35 (+ z = Sesostris II, Year 3 + z)</td>
</tr>
<tr>
<td>-403  (x + y + z)</td>
<td>Sesostris II, Year 4 (+ z)</td>
</tr>
<tr>
<td>-401</td>
<td>Sesostris II, Year 6 (+ x - y)</td>
</tr>
<tr>
<td>-400</td>
<td>Sesostris II, Year 6 (+ x - y + 1 = Sesostris III, Year 1)</td>
</tr>
<tr>
<td>-399 (+ y - 2)</td>
<td>Sesostris II, Year 6 (+ x = Sesostris III, Year y)</td>
</tr>
<tr>
<td>-394</td>
<td>Sesostris III, Year 7, III (prt) 16 = July 13 Julian</td>
</tr>
<tr>
<td>-382</td>
<td>Sesostris III, Year 19</td>
</tr>
<tr>
<td>-381</td>
<td>Sesostris III, Year 20 = Amenemhet III, Year 1</td>
</tr>
<tr>
<td>-362  (- x + y - z)</td>
<td>Sesostris III, Year 39 (- x + y - z = Amenemhet III, Year 20 - x + y - z)</td>
</tr>
<tr>
<td>-361  (- x + y - z)</td>
<td>Amenemhet III, Year 21 (- x + y - z)</td>
</tr>
<tr>
<td>-344</td>
<td>Amenemhet III, Year 38 = Sebeknefru’s last complete year as co-regent?</td>
</tr>
<tr>
<td>-343</td>
<td>Amenemhet III, Year 39 = Amenemhet IV, Year 1 (replacing Sebeknefru?)</td>
</tr>
<tr>
<td>-335</td>
<td>Amenemhet III, Year 47 = Amenemhet IV, Year 9</td>
</tr>
</tbody>
</table>
Notice that the table is cluttered with five sets of either brackets or braces, as well as with numerous occurrences of x, y, and z, which are unknowns pertaining to Sesostris II. The x is the number of years completed by Sesostris II beyond Year 6. The y is the number of complete years that Sesostris III served as co-regent with Sesostris II. The z can be thought of in either of two ways, as I explained on page 270: “The z represents the number of years that Amenemhet II completed beyond Year 35; it can also be thought of as the number of years that Sesostris II completed as co-regent beyond Year 3.” This table from page 269 was based on the assumptions that x = 0, that y = 0, and that z = 0. But the table was also designed in such a way that it could easily be revised if x, y, or z should ever turn out to have some definite numerical value.

The bracketed and braced material was to be deleted or retained in the various circumstances that I enumerated on pages 270-271.

Since my placement of Year I of Sesostris II in -406 was admittedly tentative and even “shaky” (page 270), we can consider -407, -408, and so on. -407 does not work, but -408 works beautifully! III prt began on 1 June -408, and that very day was psdntjw. The New Crescent was on 2 June, and the subsequent psdntjw was on 30 June, which was the very last day of III prt. This was a 29-day lunar month. It is a tight squeeze, as expected, but it does work.

-409, -410, -411, and so on miss, and we have to go all the way back to -419 before we find another fit. We can dismiss the -419; there is no basis whatsoever for giving Sesostris II a reign of 19 years. It is just a coincidence that scholars for many years did exactly that (Parker, pages 68-69, is one example). They were mistakenly assigning the basic 19 years of Sesostris III to Sesostris II. Over the past several decades, Egyptologists have now straightened out their own mess, and the reign of Sesostris II has been greatly shortened, to about 6 or 8 years. None of this is my doing, despite some of the falsehoods that have been printed regarding my work.

To correct those sorts of falsehoods, I must stress that the idea that Sesostris III had a meaningful reign of only 19 years, and that for the next 20 years or so Amenemhet III served not only as co-regent but as de facto regent for the presumably-incapacitated Sesostris III, was published by others, notably by
Wegener, before I published my book. Even though I had been independently working on that very idea for several years, I claim no priority in this matter.

Along those same lines, I must stress that, except for A, where I was wrong, and for 10007, where I hope that I am right, I have never even been tempted to assign an El-Lahun papyrus to a regnal year of a particular ruler unless that assignment had already been sanctioned by Ulrich Luft. That is a deliberate strategy on my part: I want to be able to say — as I have said before, at every opportunity — that I have beaten them at their own game, by their own rules, and with their own data.

A seeming exception is 10166, which contains two dates, a Draping or Investiture on II 3ht 19 and a Line of the Nile Mile on III 3ht 25. Luft assigned the first date to Year 9 of Amenemhet III and ignored the second date, presumably because it did not make sense in Year 9. But it fits perfectly in Year 10, and I placed it there. The photograph in Luft’s Tafel 22a shows a rather pronounced space between the two dates, suggesting that they are separate items. Other El-Lahun papyri likewise mention dates from more than one year. Besides, that very same date of III 3ht 25 for the Line of the Nile Mile also occurs on 10079, which Luft does put in Year 10. With the fourth-century chronology, every recorded Line of the Nile Mile occurs 19 days after the New Crescent.

(At this point, many readers may be getting lost. What I would stress here is that every one of my 24 New Crescents has been sanctioned by Luft: not one of them is my doing. I would also stress that those who simply want to play at chronology should leave the El-Lahun papyri alone. Pace Daphne Chappell, this is not Sudoku.)

In order to articulate the table on page 269, while still allowing for contingencies and future discoveries, I very much needed x, y, and z. Ever since then, I have taken it for granted that any further progress would come in the form of a discovery of the exact value of x, or of y, or of z. To my surprise and amusement, the progress, when it came, was in the form of 10007. I still do not know the value of x or the value of y or the value of z, and I must continue to regard all three of them as unknowns. We can now say that x ≥ 2, and also that x - y = 2. Since x ≥ 2, we can also say that my original guess that x = 0 was wrong. But we still cannot determine the exact value of x, or of y, or of z.

What 10007 gives us is something else, namely, the information that Year 1 of Sesostris II was -408. Since we already know that Year 1 of Sesostris II was Year 33 of Amenemhet II, and since the first four kings of the Twelfth Dynasty,
including their various co-regencies, are already locked in sequence, we can shift the beginning of the Twelfth Dynasty from my speculative 30 September -500 to a considerably less speculative 1 October -502. It is that simple. We have by-passed x, y, and z, without having to determine any of their exact values.

We might now guess that x = 2, y = 0, and z = 0, but we do not know.

There would be a number of additional repercussions to all this, but they are not necessarily bad ones. I have reviewed pages 270-271, and I believe that I have there anticipated and provided for all of these repercussions. The most important of them is that A can no longer be assigned to Year 3 of Sesostris II. It did fit in -404, but it will definitely not fit in -406. It has to be left in Year 3 of Amenemhet III = -379, where it comes one day late, which could be attributed to bad seeing. That is a price that I am quite willing to pay. I can afford to be magnanimous. I am already on record that the psdntjw-based dates are of far less value than the Crescent-based dates. Furthermore, I still have those 23 of 24 hits on the New Crescents, and I still have the only extant explanation of how the Crescent-based Feasts were positioned. When it comes to the NC + n formula that governs those positions, I am the only game in town; nobody else has squat. (See “Feasts”, especially pages 16-18. There I even made a claim that sounds outrageous but is actually quite reasonable: that 23 of 24 is better than 24 of 24, because we should be expecting and finding at least some bad seeing on occasion. In that sense, my 23 of 24 is a perfect score.)

By the way, Year 3 of Amenemhet III is exactly where Luft had placed document A, anyway!

Since x - y = 2, the two occurrences of x - y in the table can be replaced by 2, and then simplified. And since it follows from x - y = 2 that -x + y = -2, the fifteen occurrences of -x + y can be replaced by -2, and then simplified.

There is an attested but possibly-problematic Year 8 for Sesostris II. See Wegener, page 250, and Simpson, pages 899-903. When I tentatively took Year 6 to be the highest completed regnal year of Sesostris II (that was the decision that I called “shaky” on page 270), I was partly relying on some of the scholarly reservations about the Year 8, but I was mainly swayed by the fact that A hit in -404. That is not to reject Year 8. I simply had Year 6 of Sesostris II as the last completed year of sole rule. He could still have lived into Year 8, which would have been Year 2 of his co-regent, Sesostris III, namely, -399. Thus I did recognize
the possibility of a longer reign for Sesostris II; this took the form of my allowing for the possibility that \(x\) and \(y\) might have values greater than 0. Using \(x\) and \(y\) kept the entire matter open. In any case, even if that attested but problematic Year 8 remained as a minor puzzle for me when I held Sesostris II to 6 completed years, the entire problem is now resolved: assigning the III \(prt\) of 10007 to -408 lets me neatly accommodate the Year 8, which is now no longer quite so problematic as it may once have seemed. We can even infer, thanks to 10007, that Sesostris II completed that Year 8, and therefore that that Year 8 report is very probably correct. In any case, Year 8 of Sesostris II would have immediately preceded Year 1 of Sesostris III, whether or not they had a co-regency. It is even reasonable to assume that Sesostris II at least began his Year 9 = Sesostris III, Year 1. But there is no evidence about this, and I have left the matter unresolved. Besides, we are listing completed regnal years.

Now that I have found that the Year 1 in 10007 is -408, I can suggest day-numbers for the heliacal rising of Sirius in III \(prt\) and for the wag-feast in II \(šmw\).

My previous work suggests that a heliacal rising of Sirius was recognized as having occurred at Elephantine on III \(prt\) 16 = 13 July -394, which was the third year of a 12-12-13-13 tetrad. Thus there would have been a heliacal rising of Sirius on III \(prt\) 19 = 12 July in -408, which was the first year of a 12-12-13-13 tetrad. The day-number omitted from III \(prt\) was 19. (At El-Lahun, they would not have been up-to-date about what was actually going on at Elephantine. El-Lahun may have determined the heliacal risings of Sirius at far-off Elephantine with the aid of an idealized 4-year schema of some sort. Any such schema might have been spoiled by the occurrence of a triennium, but there is no evidence whatsoever that the ancients ever even noticed the occurrence of triennia, anyway; see \textit{Sun, Moon, and Sothis}, pages 188-191.)

The moveable wag-feast was always 16 days after the New Crescent day. The relevant New Crescent day was 30 August, and 30 August + 16 = 15 September -408 = II \(šmw\) 24. With the first invisibility or \(psdntjw\) on 28 August, that wag-feast would have to have been on lunar day 18. In any case, the day-number omitted from II \(šmw\) was 24 — unless there was bad seeing involved.

Here, without the three variables, and without all the brackets and braces, is the revised chart for the Twelfth Dynasty:
-482  Amenemhet I, Year 21 = Sesostris I, Year 1
-474  Amenemhet I, Year 29 = Sesostris I, Year 9
-473  Sesostris I, Year 10
-440  Sesostris I, Year 43 = Amenemhet II, Year 1
-438  Sesostris I, Year 45 = Amenemhet II, Year 3
-437  Amenemhet II, Year 4
-408  Amenemhet II, Year 33 = Sesostris II, Year 1
-406  Amenemhet II, Year 35 = Sesostris II, Year 3
-405  Sesostris II, Year 4
-401  Sesostris II, Year 8
-400  Sesostris III, Year 1
-394  Sesostris III, Year 7, IIII prt 16 = July 13 Julian
-382  Sesostris III, Year 19
-381  Sesostris III, Year 20 = Amenemhet III, Year 1
-364  Sesostris III, Year 39 = Amenemhet III, Year 20
-344  Amenemhet III, Year 38 = Sebeknefru’s last complete year as co-regent?
-343  Amenemhet III, Year 39 = Amenemhet IV, Year 1 (replacing Sebeknefru?)
-335  Amenemhet III, Year 47 = Amenemhet IV, Year 9
-334  Sebeknefru, Year 1
-332  Sebeknefru, Year 3
-331  Sebeknefru, Year 4 (until III šmw 25 = 4 October Julian)

In the interest of simplifying the chart even further, I considered abandoning any attempt to accommodate an earlier co-regency for Sebeknefru, that is, a co-regency prior to the co-regency of Amenemhet IV. We do not have any solid proof of such a co-regency for Sebeknefru, anyway. On the other hand, the idea (not invented by me) that there was a co-regency between Amenemhet III and Sebeknefru has never been refuted, so I have left it in, if only as a very weak conjecture.

213

The Turin King-List puts the length of the Twelfth Dynasty at 213 years, 1 month, and 16 days.

It should be explained that, when ancient chronographers were counting Egyptian regnal years, and when they had to deal with a year in which the reign of a previous king ended and the reign of a new king began, it was the custom to assign that entire year to the new king and to assign nothing to the old king. Thus the number of years in a king’s reign was taken to be the number of Egyptian years that ended with that king on the throne. That was workable for kings who were not
the first or last of that dynasty, but circumstances were somewhat different for kings who were the first or the last of the dynasty, especially when it came to calculating the overall length of the dynasty itself.

We should also remember that the 213 is a bizarre ancient mistake, a piece of fallacious and flawed historiography, since all of the reigns, including the co-regencies, are simply added together, as if they were end-to-end. The 213 is not the truth, but we may be able to use it in order to get a better handle on the truth, by asking whether our (frequently overlapping) reign-lengths add up to 213.

Was the 213 abysmally stupid or fiendishly clever? Might it have been a deliberate deception rather than an error? Actually, I cannot distinguish between idiocy and mendacity in this case. But I can make use of this person as an informed witness. After all, the reign-lengths that that person saw might well have been correct.

What that person did not have was modern retrocalculations that provide absolute Julian dates for many of the regnal years of Sesostris III and Amenemhet III. In that respect, we have better information than the 213 person had.

Actually, the person who came up with 213 may not have been an idiot; we may be dealing with a clever sycophant, who found a way to make this segment of Egyptian history appear longer. (Hellenistic kings seem to have competed in making their kingdoms older than the kingdoms of their rivals.) Just as Manethonian types seem to have stretched Egyptian history by placing contemporaneous dynasties end-to-end, so this person may have wanted to stretch the Twelfth Dynasty by placing partially or even wholly contemporaneous reigns end-to-end.

In any case, the 213 could be salvaged, but only at some cost. The completed regnal years of the eight recognized monarchs are $29 + 45 + 35 + 8 + 39 + 47 + 9 + 3 = 215$. Since the highest attested regnal year of Amenemhet III is 45, there is more than enough wiggle-room so that we could easily reduce his total of completed years to 45 and hit the 213 exactly. That would mean allowing Amenemhet IV to reign on his own for two years. Another way would be to suppose that the originator of the 213 did not count Sebeknefru at all, and either that Sesostris II completed his Year 9 or that Sesostris III completed his nominal Year 40. It must be admitted, however, that no one knows either what numbers were available to the originator of the 213 to work with or what the originator of the 213 did with them.
In *Sun, Moon and Sothis*, I had been able to retain the 213 by taking the completed years of the eight recognized monarchs as $29 + 45 + 35 + 6 + 39 + 47 + 9 + 3 = 213$, but the 6 for Sesostris II is now an 8 (or more).

Perhaps the main value of the 213 report is that it causes everyone to look for 20 more years or so. There is an at least incipient consensus that this might be the special co-regency between Sesostris III and Amenemhet III — which was actually more of a *regency* by Amenemhet III.

If $z$ were greater than 0, we would run further afoul of the 213. On that basis, if nothing else, we should perhaps assume for the moment that $z = 0$.

I am still attracted to the idea that “Amenemhet IV” may have reigned for no more than minutes or hours after the death of Amenemhet III before Sebeknefru seized the throne. “Amenemhet IV” might have become co-regent as a child. Sebeknefru could have been his older sister, his cousin, his aunt, or even his mother. We simply do not know.

If Amenemhet I died early enough in Year 30, and if his Year 1 is skipped (or even not), his actual time on the throne becomes 28 years and change. If we count Amenemhet I as only 28, and if we count Sesostris III as only 38, the 215 mentioned above becomes the 213 of the Turin King-List. Perhaps that is the way to go. Year 39 of Sesostris III would then be attested but uncompleted.

It should be emphasized that none of these puzzles or uncertainties has any affect whatsoever on our new (if still tentative) conclusion that the Twelfth Dynasty lasted from 1 October Julian -502 to 4 October Julian -331.
DAPHNE CHAPPELL AND DOCUMENT D

The following materials are taken directly from my unpublished “Reply to Barry Curnock”. It seemed better to present these materials about Chappell in a separate section at this point.

In “Fitting Lunar Dates”, Daphne Chappell was for the most part attacking me; actually, however, she provided two further reasons for downplaying document D:

(1) “The El-Lahun temple account is just a record of the month-long shifts that a particular phyle of priests worked during a year, not a record of new moons. We do not know whether it was even their responsibility to note or proclaim the new moon. That they started work on the first of the month is simply an assumption of Parker, Read and others. They could have started their shift on day 29 or any day of the month and, perhaps, one on which they had no feasts to prepare would have been more practical.” (page 3)

(2) “If the second phyle was unable to work for some reason and the first phyle had to work an extra day, then it is legitimate to amend the predicted first day of the month.” (page 4)

Taken together, these two remarks relieve us of any need to take the individual dates of document D seriously at all. The fact that I miss two dates in document D is now of no concern whatsoever to me. There is no longer even any need to rid the text of the 31-day month. I am most grateful to Chappell for thus strengthening the case for my Twelfth-Dynasty chronology.

Chappell worries about which emendation of document D should be made. But according to her own ideas, quoted above, there is no need for any emendment. The 31-day period of priestly service is perfectly acceptable, as are all of the other dates of document D.

Since there is no need for emendment, I would not say, “it is legitimate to amend the predicted first day of the month”; instead, I would say, “there is no need to question any of the dates as given”. The periods of priestly service need not always have followed the Moon exactly, anyway. That, I take it, is Chappell’s point here.
With further regard to practicality, if they had tried to change phyles on psdntjw, the new phyle would have to have been standing by at the dawn following day 29, just in case there was a psdntjw at that point. If there was not, they would have had to wait another full day. This would have been most impractical indeed. But if they changed phyles on day 3, the new phyle would usually know, two days before, precisely when their shift would begin.

Thanks in no little part to Chappell, the Edgerton Challenge continues to change form. In the old days, the challenge was to base Twelfth Dynasty chronology on the Sothic date, on Document D, and on a few outliers. Eventually, there were so many “outliers”, mostly featuring Crescent-based feasts, that they began to overshadow document D. Then my NC + n formula put still more emphasis on the Crescent-based feasts. Now, in the light of Chappell’s insights, the rôle of document D has been even further reduced and in fact eliminated.

REPLY TO BARRY CURNOCK

Barry Curnock’s “Assessment” (Workshop 2011:2, pages 13-15) of Sun, Moon, and Sothis focuses on Chapter Eighteen and especially on Chapter Nineteen, both of which deal with the El-Lahun papyri. Among other things, I intend to show that most of Curnock’s remarks about those papyri are cursory, ill-informed, outdated, irrelevant, and fallacious. (I am unable to make any comment about several of his other remarks, simply because I find them unintelligible.)

Aside from various typos, there are several errors of substance in my book. But Curnock seems not to be aware of them, even though I pointed them out myself in “The Feasts and the Crescents”, which he appears not to have seen.

Curnock begins by announcing that he recently “was asked to look at the mathematics of” my book. It would be interesting to know who asked him to do that.

He briefly describes Part One of the book with words like “accurate”. He also says that he read the book in full. That is difficult to believe. Chapters Two, Three, and Four of Part One establish the unreliability of the 25-year cycle table. Yet Curnock continues to speak favorably of that and other such cycles while trying to build arguments against my Twelfth-Dynasty chronology!

It is false that lunar events will recur on the same dates “after a cycle of nineteen years”. It is also false that “in the Egyptian calendar” there is “a cycle of twenty-five
years”, after which “the lunar dates are repeated”. If either of these claims were true, you could correctly predict the exact dates of lunar phenomena many years in advance, simply by consulting a one-page table that repeats itself. It cannot be done.

“Once the first date of document D was determined, the rest of the dates followed. Knowing that the lunar month is 29.5 days long, it is simple arithmetic to add twenty-nine and a half days to find the next date.” That is not only not “simple arithmetic”, it is simply impossible. The next date is not determinable in this manner. (More of this anon.)

It is false that Parker or anyone else ever “matched [modern] retrocalculated lunar dates” against the date on document B. The day-number of that alleged date would have been well off the surviving edge of the papyrus. See the photograph of document B in Luft’s Tafel 18a. People just guess what that day-number might have been.

It is also false that “Rose looks in the only other possible time, i.e. the 4th century”. Actually, as my book makes clear, I first looked in the late fifth century.

It is Curnock, not I, who misunderstands “the nature of the process carried out by Parker and others” and who misunderstands Neugebauer’s description of document D. (See Sun, Moon, and Sothis, page 212.) The “dates” in document D are not, as Neugebauer would have it, “trivial consequences of the first one”. The first date in document D is II šmw 26. There is simply no way to deduce any of the other dates from that one. Is the next one III šmw 26? Or is it III šmw 25? We cannot tell. Both Neugebauer and Curnock are guilty of elementary illogic here. Curnock goes on to say that “once the first date is defined the rest must follow in strict sequence, obeying the lunar monthly cycle of 29.5 days”. Tell me, please, just how can one date be separated from another date by 29.5 days? It never happens.

Perhaps for that very reason, Curnock takes it as “expected” that “every other new moon” — that is, every second one — will occur after “59 or 60 day intervals”. He seems not to know that there are 58-day intervals (two 29-day months in a row). Why are they not to be “expected”? His elementary astronomy is as bad as his elementary logic. Besides, how do we know that II šmw 26 is psdnitjw (that is, first invisibility)? Might not II šmw 26 be some other date of the lunar month? By the way, there are also 87-day intervals (three 29-day months in a row), but Curnock seems to know nothing of them, either.
“The accuracy of the Egyptian record can be demonstrated by the first and last dates of September 10th and July 3rd Julian, showing 296 days to span ten lunar months. The average lunar month is 29.5 days.” Actually, the last date is I šmw 16 = August 1 Julian. Curnock simply discarded that one in order to have ten lunar months. But all that we can infer from this exercise is that the first date (II šmw 26) and the next-to-last date (III prt 17) might be astronomically compatible with each other. Absolutely nothing about the “accuracy of the Egyptian record can be demonstrated by” this procedure; it does not tell us whether II šmw 26 and III prt 17 are accurate, and it does not tell us whether any of the nine dates in between are accurate.

“If Parker’s first date for Document D was right, all the others also had to be right within the accuracy of the process. They are either all right or all wrong.” That is ridiculous. They can all be wrong, but there can also be various mixtures of right ones and wrong ones. More to the point, however, I have never seen a fit for all twelve of the dates of document D (even allowing the standard emendation of II 3ht 20 to II 3ht 19) that handled the Sothic date and the Crescent-based Feasts in a credible manner. I myself hit only ten of the dates in document D. (In my book, I thought that I had hit eleven, but I was wrong about one of them. See “Feasts”, page 16.)

By the way, I did say on page 223 that Parker “has ten acceptable results out of twelve” and that he “has two misses that remain unacceptable”. I was speaking of the twelve results individually, but for some reason Curnock takes me to mean that I consider “ten out of twelve with two misses . . . as ‘unacceptable’”. Actually, I think that ten out of twelve is acceptable. Since no one respectful of the Sothic date and of the 24 New Crescent dates has ever scored twelve out of twelve on document D, I am hardly in a position to call ten of twelve an “unacceptable” score. If I ever did say anything like that, it was wrong.

In any case, those psdntjw-based dates are not nearly so important as the Crescent-based Feasts. The reasons are elaborated in “Feasts”, especially in note 34. See also Daphne Chappell’s “Fitting Lunar Dates” (Chronology & Catastrophism Review 2007, pages 2-6), discussed in the preceding section.

I do so wish that Curnock had troubled to read “Feasts”. Then he might have realized that the NC + n formula for the Crescent-based Feasts works only for my chronology, and for no other. To the best of my recollection, neither Curnock nor any other critic of mine has ever even mentioned the NC + n formula. On pages 16-18 of “Feasts”, there is a full-scale elaboration and updating of my rather brief remarks on
page 239 of *Sun, Moon, and Sothis* about the placement of the Crescent-based Feasts, remarks whose significance Curnock and others seem to have overlooked.

My defense of the Elephantine alternative is not “untenable”; it is obligatory. From the reign of Sesostris III, there are 22 Crescent–based Feasts. With my chronology, every single one of those 22 is a hit. (If you prefer, you can say that there are 15 New Crescents upon which those 22 Feasts are based, and that, with my chronology, every single one of those 15 is a hit.)

That puts Year 1 of Sesostris III in -400, with his Year 19 in -382. The Year 7 Sothic date would be in -394. And that requires the Elephantine alternative rather than the Memphis alternative. Elephantine fits beautifully; Memphis misses by 5¾ days.

Curnock gets no mileage at all from his remark about El-Lahun being “over 500 miles from Elephantine”. Some of us are a lot farther than that from Greenwich. Having a far-off Delphi or Mecca or Greenwich is nothing new under the Sun. He also gets no mileage at all from his suggestion that we should count the errors. Good luck with that! There are no errors with Sesostris III. Just what is it that he is going to count?

The Elephantine alternative had already been taken quite seriously in the Egyptological literature — and not just by Krauss, as Curnock would have us believe. Ulrich Luft, the world’s leading authority on the El-Lahun papyri, also took the Elephantine alternative quite seriously. The mediocre results that Luft obtained for Memphis in the early second millennium were indeed better than the mediocre results that he obtained for Elephantine, and on that basis he did, in the end, opt for the Memphis alternative. Even his fit with Memphis, however, was random and paltry in comparison to my fourth-century fit with Elephantine. What is of more importance in terms of Curnock’s charges, however, is that I did not invent, choose, or select the Elephantine alternative. What I did do was prove it correct. (By the way, it’s Krauss, not Kraus.)

If, for the moment, we overlook the Velikovsky Divide, we can say that once the Egyptian calendar is carefully aligned with the mean Moon, about half of the New Crescent dates should hit, anyway, on any chronology. Egyptologists who try to force-fit the El-Lahun papyri into the wrong century and even into the wrong millennium get results that are, as we would expect, random and paltry. They may hit the New Crescents somewhat less than half of the time, or about half of the time, or somewhat more than half of the time. But their results are still random and paltry, especially if they are respectful of the Sothic date.
It continually amazes me that Curnock and others are unable to see that there is a profound difference between what the Egyptologists are satisfied with and what I have attained. What the Egyptologists have for, say, Sesostris III is random and paltry, and what I have for Sesostris III is a 100% fit. Doesn’t that bother anyone? I believe that it does, but that they are constitutionally unable to say so.

Curnock cites Chappell’s 2007 paper to the effect that there seems to have been a Year 39 of Sesostris III. This is supposed to be news to me. If Curnock looks more closely at page 264 of the book that he claims to have read, he will find that in 1999 I had already mentioned the fact that there seems to have been a Year 39 of Sesostris III. I even included a “Year 39” of Sesostris III in my chart on page 269. Both the idea of a basic reign of nineteen years and the idea of a regency of about twenty years (possibly because of some incapacity of Sesostris III) are ideas that had been advanced in the literature before anyone ever heard them from me. Curnock now seems to favor counting how many Egyptologists would vote for or against these ideas. I prefer to be guided by the clear weight of the evidence rather than by the opinions of Egyptologists.

From the reign of Amenemhet III, there are ten Crescent-based Feasts, or, if you prefer, there are nine New Crescents upon which those ten Feasts are based. Either way, all but one are hits. The miss is one day late, which could easily happen with bad seeing. The regnal years of Amenemhet III follow immediately upon Year 19 of Sesostris III. For the two reigns, we have a total of 32 Crescent-based Feasts, or, if you prefer, we have a total of 24 New Crescents upon which those 32 Crescent-based Feasts are based. All but one of the 32 Crescent-based Feasts hit, and all but one of the 24 New Crescents hit. Sesostris III and Amenemhet III are locked in place.

Document D is from Year 30 and Year 31 of Amenemhet III, that is, from -352 and -351. From that placement, it follows that the dates at the beginnings of the lines of document D are not to be taken as psdntfjw (or first invisibility), despite what Curnock or anybody else tries to tell you. (It is better to say “at the beginnings of the lines” here, because the dates being referred to are to the left in English but to the right in hieratic.)

Document D specifies alternate months of priestly service, usually starting on lunar day 3 and running down to (but not including) the next lunar day 3. There is no choice or selection in this. The regnal years of Sesostris III and Amenemhet III make such a reading of D obligatory. And there is nothing wrong with such a
reading. Indeed, the correctness of such a reading and of such a dating is neatly confirmed by document C, which also has — on anybody’s chronology — a period of monthly priestly service starting on lunar day 3. The only difference is that in document C the month is construed as running from lunar day 3 down to (and including) the next lunar day 2, which would be the last full day of service. In both documents, the customary starting-point of the service is lunar day 3. Curnock is oblivious to all that. I have not chosen how I would like to read document D; I have proved how it must be read. (In the light of Chappell’s work, however, we need not even be concerned about a few misses in document D. The work schedule may simply have deviated occasionally from the overall lunar orientation. With the -352/1 placement, and with no emendation, the phyle seems to have left work a day early twice, and to have worked one day overtime once. The other phyle would have started a day early twice, and would have started a day late once. As Chappell seems to indicate, these are administrative and personnel matters, nothing of any astronomical significance.)

My handling of the data in terms of hits and misses is entirely appropriate. The hits and the misses are there. They are real. If the recorded date and the retrocalculated date are the same, we have a hit. If they are not the same, we have a miss. I, too, think that a miss by two days is worse than a miss by one day. I, too, think that, when there is a multitude of errors of various magnitudes, some mathematical or statistical technique of evaluating and measuring the errors and their magnitudes might be appropriate. But none of that applies to my chronology. With the Sothic date, with the Crescent-based Feasts, and with the NC + n formula, I have a grand total of one miss by one day. How do you do a statistical analysis on one item, the one and only such item that you have?

Nonetheless, I grant that I do have three misses among the psdntjw-based dates, one on document A and two on document D. If Curnock wants to play around statistically with those three misses, good luck to him. I do not care what he finds. There will still be three misses on the psdntjw-based dates, and there will still be only one miss on the Crescent-based dates. And I shall still be quite content to rest my case on the Sothic date, the Crescent-based Feasts, and the NC + n formula.

Remember, however, that if Chappell is right (and I think that she is), the two misses on D can simply be ignored. It is enough then to say that document D is a record of priestly service that is generally geared to lunar day 3, but that document D is not an exact astronomical record. That leaves me with a grand total of two misses (the one in -379 reported on 10090 or A and the one in -358 reported both
on 10104 and on 71583); both of these misses are easily explained on the basis of 
bad seeing. I am greatly indebted to Chappell for making all of this possible.

I am not sure that I would even want any fewer misses than that. After all, we 
should expect to encounter at least some bad seeing on occasion!

As I stressed in “Feasts” and have been stressing ever since, the Crescent-based 
Feasts are much more important than the psdntjw-based dates. Not one of those 24 New 
Crescents is a close call. The one New Crescent that comes closest to being a close call 
is, according to Schoch’s tables, over an hour and one-half away from missing.

Chappell’s retrocalculated New Crescents for Cairo during +2006 are a quite 
different matter and are entirely irrelevant. Curnock is mistaken in calling them “an 
important [sic] ‘control experiment’”. They indeed do feature some very close calls. 
So what? It is still the case that the 24 El-Lahun New Crescents do not have any 
close calls, and it is still the case that Schoch hits 23 of those 24 New Crescents, 
for 95.833%. Even that one miss is not due to any error on Schoch’s part. Starry 
Night and Lange and Swerdlow confirm Schoch’s date of May 21, -358. It is not 
that the retrocalculators have missed anything; what seems not only likely but also 
rather obvious is that the observers simply started their lunar month one day late, 
presumably due to bad seeing on April 21 and again on May 21, with a 29-30-30 
sequence being counted as a 30-30-29 sequence.

Chappell’s -1426 is not a “perfect fit” for document D. Curnock might better 
have left both Cairo and -1426 unmentioned. The -1426 is based only on the totally 
bogus “completed” version of the 25-year cycle table. Both the original version and 
the “completed” version of that table are unreliable. In my opinion, retrocalculations 
that far back in time are at best rather shaky, anyway — mainly because we have no 
idea what the value of ΔT was in the second millennium. Nonetheless, both Starry 
Night and Lange and Swerdlow show misses that Chappell has overlooked. Her 
actual result is very easy to accomplish — especially if you allow yourself over a 
millennium of unrestricted wiggle-room, if you ignore the Crescent-based Feasts, if 
you ignore the NC + n formula, and if you ignore the Sothic date! I do not have any 
wiggle-room, I do not ignore any of those things, and I still manage ten hits on 
document D. Thanks to Chappell, of course, we no longer have to worry about 
document D, anyway. She has enabled me to cut my misses by half!

To suggest that Schoch’s tables are accurate only 80% of the time is itself 
grotesquely inaccurate. Schoch speaks of his collection of 30-day months “from
Rim-Sin to Ammizaduga and from Nebuchadnezzar to Xerxes”, and of his other
collection of crescents “[f]rom the reign of Xerxes to the year -7”. We do not have
those collections and cannot directly test Schoch’s figure of 80%. Even if that
figure is correct, it does not mean that Schoch’s tables are wrong 20% of the time
despite Schoch’s own self-depreciating and entirely unnecessary remarks on page
98). The reports themselves might be wrong 20% of the time (or even more than
that), especially in the case of those very early 30-day months. How likely is it that
two different data sets, from different epochs, featuring different (but not
independent) lunar phenomena, would both score 80%? In my opinion, much of
the underlying chronology is wrong, anyway. After all, the chronology that Schoch
was using has been extensively revised, particularly in the last six decades. For
example, if the “Rim-Sin to Ammizaduga” were in the first millennium, as some of
us have suggested, maybe Schoch’s score would be higher. All of that remains to
be seen. There are vast opportunities for serious, constructive work here. It is too
bad that no one in places like England is interested in doing any of it.

My own experience in working with Schoch’s tables as well as with other
retrocalculation resources is that Schoch does as well as anyone at retrocalculation,
and far better than most. The more recent computer programs are spectacularly
faster and much more convenient, but many of them incorporate shortcuts that
Schoch himself eschewed. Garbage in, garbage out.

I have completed, but never mentioned in print, a test based on 209 well-attested
Babylonian sightings of the New Crescent compiled by Fatoohi, Stephenson, and
Al-Dargazelli (“The Babylonian First Visibility of the Lunar Crescent: Data and
196 of them, for 93.78%. The misses are all fairly close calls. I do not see how
anyone could do much better than that, especially since there usually are a number
of close calls involved when we deal with these larger data sets.

By the way, Fatoohi, Stephenson, and Al-Dargazelli count hits and misses, just
as I do. Perhaps Curnock and others should be attacking them, not me.

Curnock seems to want “historical evidence”. I have chosen to concentrate on
the astronomical evidence, for two reasons: (1) the astronomical evidence not only
is critically important but also is in many cases decisive; (2) most others, including
Curnock, have not given the astronomical evidence its due. As long as (2)
continues to be true, I shall continue to emphasize the astronomical evidence. But
that does not mean that I disrespect history. After all, I did have a joint major in
ancient history and Greek, and I have from time to time written about more strictly historical matters, as I did in Chapters Twenty and Twenty-One of my book. I respectfully suggest, to Curnock and others, that when the astronomy contradicts the history, we should not stick our heads in the sand and ignore the astronomy; rather, we should put at least a little effort into checking the history. After all, it may be the conventional history and especially the conventional chronology that is at fault here. But do not expect me to do it all for you. If you are serious about any of this, make (2) false. As long as (2) is true, I have little choice but to work on astronomy. For further kinds of evidence, however, including historical evidence, Curnock and others should in the meantime read Charles Ginethal’s *Pillars of the Past*, Volume III, Chapter Five, especially the last section, “Agronomy and the Chronology of the 12th Dynasty”, pages 225-233.

The remaining topics revisited by Curnock have been discussed *ad nauseam* by myself and many others. He and anyone else concerned about the uncertainties and vicissitudes of retrocalculation, or about observation locations and viewing conditions, or about error-bars and the like, should read “A Review of Wells’s Review of *Sun, Moon, and Sothis*”, *Aeon* VI:5, pages 8-14. There I dealt not only with Ronald A. Wells, but also with Bradley E. Schaefer and, although I did not know it at the time, with Teije de Jong and with Barry Curnock. Repeating all of that earlier work here would be “déjà vu all over again”. More recently, I again dealt unknowingly with Curnock when I discussed the uncertainties of retrocalculation in my “Reply to Carl Olof Jonsson”, which has been incorporated earlier in this Appendix.

Perhaps all that remains to be said is that a hit is still a hit, and that I have a uniquely high number of them, while the results reached by my predecessors and/or my opponents are at best random and paltry. I have beaten them at their own game, by their own rules, and with their own data.

**EDWIN THIELE**

Before we get into Assyria and Babylonia, I should say what I think of the work of Edwin R. Thiele. In the recent past, I have alluded very favorably to Thiele, without being explicit enough about where I agree with him and where I disagree with him.

I agree very strongly with Thiele’s overall analysis and reconstruction of Biblical chronology. He deals rather briefly with the United Kingdom of Saul, David, and Solomon, and then in great detail with the Divided Monarchy of Israel and Judah. His
chronology seems to me to be brilliant. So far, I have little reason to doubt that it is correct both as a relative or sequential chronology, and also in terms of durations.

Where I disagree with Thiele is in two respects, one minor and one major.

The minor one is that I wish that he had stuck to scholarly investigation, and had skipped the frequent professions of his own religious faith. Perhaps he had to do that for the sake of his job: he had already rocked a number of theological boats as it was.

The major respect in which I disagree with Thiele is that his entire chronology fails as an absolute chronology. He tries to make it absolute by linking Biblical events to Assyro-Babylonian events. He takes it as established that Assyro-Babylonian chronology is both absolute and correct. (This includes the Eponym Canon, the Eponym Eclipse, Ptolemy’s Royal Canon, the Era of Nabonassar, the conventional placements of the Neo-Assyrians and the Neo-Babylonians, and all of the usual establishment buttressing.)

As I see it at the moment, Thiele’s chronology not only of the United Kingdom of Saul, David, and Solomon but also of the Divided Monarchy of Israel and Judah is not absolute, but can be rendered absolute, simply by lowering everything by 274 years. Much of Assyro-Babylonian chronology can also be rendered absolute by lowering it by 274 years or thereabouts. There are some places where the required adjustment will vary. This is because the conventional Assyro-Babylonian chronology sometimes does not even have the durations right.

Thus I do not accept all of the putative synchronisms that Thiele finds, even if they are biblical.

An important consideration here is that the books of the Bible, like many other ancient texts, were “edited” and “corrected” and “fleshed out” by later editors, especially in the Hellenistic period. Many details were added as part of this rewriting of history. Many of those details conflict with my chronology. I know that. There is no need to inform me about what I already know. Just because various things are said in our rather late recensions of, say, the book of Jeremiah or the book of Ezekiel, that does not mean that Jeremiah himself or Ezekiel himself actually said such things. We need to work through all of this on a case-by-case basis.
I do reserve the right to demur on some of the details, should that turn out to be warranted. For example, if we lower Thiele’s placement of Year 5 of Rehoboam by 274 years, we get -651/650. But that may not work. . . .

THE LUNAR DATES OF THUTMOSE III AND RAMSES II

In an article that is too long to include here, I have reexamined Richard A. Parker’s “The Lunar Dates of Thutmose III and Ramses II”, *Journal of Near Eastern Studies* XVI:1 (1957), pages 39-43. The first of those lunar dates is a psdntjw that occurred in Year 23 of Thutmose III on the day of or the day after the battle of Megiddo. Ginenthal places the reign of Thutmose III in the mid-seventh century, and he places most of the independent reign of Ramses II in the early- to mid-sixth century. (See Pillars III, pages 220 and 385-388.) What is noteworthy at the moment is that none of the eight possible placements of Year 23 in the seventh century fits Year 5 of Rehoboam in -651/650. Either that lunar date of Thutmose III is wrong, or Velikovsky is wrong in identifying Shishak as Thutmose III, or Heinsohm, Sweeney, Ginenthal, and I are wrong in placing Thutmose III in the seventh century, or I am wrong in lowering Thiele’s dates for Rehoboam by 274 years. Take your pick.

The psdntjw on II prt 27 in Year 52 of Ramses II works out much better. I would tentatively put that one on 10 July -577. Astronomically, a 29-day month had ended on the morning of 9 July, but bad seeing that morning could easily have caused them not to start psdntjw until the morning of 10 July.

REPORT 102 AND REPORT 501

Hunger’s Reports rarely mention any kings by name. Important exceptions are Report 102 and Report 501.

Report 102 was discussed in my letter of April 19, 2007, which was quoted in its entirety in *Pillars*, Volume II, pages 242-254. I remain satisfied with the astronomical conclusion of that letter, that the Report pertains to 14 March -391. Indeed, I remain satisfied with the way I summed up the astronomical situation in the “Appendix” to Volume II, page 621: “Suffice it to say here that Hunger’s date of 15 March -668 is inadequate and that my date of 14 March -391 is successful in the extreme”. But I am no longer satisfied with my assignment of that Report — and its -391 date — to the reign of Esarhaddon. Hunger’s date does fall within the conventional dates for Esarhaddon, and Hunger had interpreted the references to
the king, the crown prince, and “Šamaš-[šumu-ukin]” as designating Esarhaddon, Ashurbanipal, and Shamash-Shum-Ukin, respectively. That seemed reasonable enough at the time, and I did not question Hunger’s interpretation. I should have.

Report 501 refers to a seen together of the Moon and the Sun on “Kislev (IX), 14th day, year 1, Sargon king of Babylon”. Hunger puts the month in italics to indicate that it is his own reconstruction. On the assumption that -391 was during the reign of Esarhaddon, I then suggested several places where the seen together might have occurred: -429, -428, and -426. Fortunately, I summarized the situation this way (page 624): “Pending further developments, however, this is another tie.”

“PENDING FURTHER DEVELOPMENTS. . . .”

As soon as the Appendix had gone to press, there were indeed “further developments”. I became aware of an eclipse that came soon after the death of Psammetichus I. This turns out to be the total solar eclipse of 4 July -335. If that was Year 55 of Psammetichus I, then his Year 1 would have been -389. Psammetichus I came to the throne in Year 5 of Ashurbanipal, which means that Ashurbanipal came to the throne in -394/3, which would have been counted as his accession year. That requires another look at those three people mentioned in -391 in Report 102. Instead of being Esarhaddon, Ashurbanipal, and Shamash-Shum-Ukin, it now appears that they were Ashurbanipal (who was by then King of Assyria), Ashurbanipal’s own crown prince at that point (so far as I know, his name is not known), and Shamash-Shum-Ukin (who was by then King of Babylon).

Thus we now have Sargonid chronology tied down to the year. From the reign-lengths, Year 1 of Sargon in Babylon would have been -434/3, and the seen together mentioned in Report 501 would have been on 27 March -433 = Adar 14 (or Adar II 14) of Year 1 of Sargon in Babylon.

The total solar eclipse of 4 July -335 is the key to all of this. That eclipse and its import need to be examined in greater detail.

THE ECLIPSE OF PSAMMETICHUS I

On page 267 of Ancient Egyptian Chronology (2006), Leo Depuydt (whose work I usually do not like) says that:
“A passage in Demotic papyrus Berlin 13588 that has been interpreted variously as a solar eclipse (E. Hornung, “Die Sonnenfinsternis nach dem Tode Psammetichs I”, ZAS 92 (1966), 38-9) and as a lunar eclipse (M. Smith, “Did Psammetichus I Die Abroad?”, OLP 22 (1991), 101-9) has possible chronological relevance (see also L. Depuydt, “On the Consistency of the Wandering Year as Backbone of Egyptian Chronology”, JARCE 32 (1995), 43-58, at 53, note 50); for a different view, see Chapter III. 4.”

Note that there are three papers cited here, by Hornung (1966), by Smith (1991), and by Depuydt himself (1995), as well as the cross-reference to Krauss (2006) in Chapter III of that same book, Ancient Egyptian Chronology.

Depuydt (1995), in the first paragraph of footnote 50 on page 53, writes as follows:

“Since the Egyptian month and day date of the astronomical date recently proposed by Smith (1991) for the year 610 B.C.E. is unknown, a minor calendar adjustment after 610 would not significantly affect Smith’s proposal, and the date is therefore not absolute evidence for the consistency of the wandering year back to that time.”

I find this statement by Depuydt very confusing. Is he saying that the ancient report of the eclipse does not give an Egyptian day or month, or is he saying that we do not know for certain the Egyptian day and month that would correspond to the partial lunar eclipse of 22 March -609 that Smith is advocating? My best guess is that Depuydt means to be saying both of these things. But I still think that his manner of speaking is dreadfully unclear.

If we assume what Depuydt calls “the consistency of the wandering year”, that is, if we assume that the Egyptian 365-day calendar was not altered between, say, the seventh century and the Alexandrian Reform in the first century, then we could indeed equate 22 March -609 with II 3ht 29 of the Egyptian calendar. Depuydt is correct in saying that we cannot arrive at such an equation without assuming that there was no adjustment. Nonetheless, most people would make that assumption without a moment’s hesitation.

Depuydt’s comments in his subsequent paragraph, together with the much earlier comments of Hornung (1966), make it clear that the reported eclipse occurred a short time after the death of Psammetichus, indeed, during the time of his embalming, and thus prior to his burial.
Hornung’s account (pages 38-39) of the most important part of the demotic Papyrus Berlin 13588 will be quoted in German and then translated into English. I cannot tell where Psammetichus died, where he was embalmed, or where he was buried, nor can I tell where the priest Amasis did what.

“Die wichtigste Angabe des Papyrus findet sich zu Beginn der 3. Kolumne: ein Priester namens Amasis berichtet dem leider anonymen König, er habe in Daphne davon gehört, dass “der Himmel die Sonnenscheibe verschlungen hat” (‘m t3 pt p3 jtn), worauf er sich (nach Sais?) zur Balsamierungsstätte (w’bt) begeben habe, in welcher der Leichnam des König Psammetich zum Begräbnis vorbereitet wurde. ‘m pt mit Objekt eines Gestirnes ist neben knhw die gebräuchliche ägyptische Bezeichnung einer Sonnen- oder Mondfinsternis. Nach den Angaben des Papyrus hat also kurz nach dem Tod Psammetichs I. oder Psammetichs II. eine Sonnenfinsternis stattgefunden; wenn wir zunächst die Möglichkeiten 664 und 663 v. Chr. für den Dynastiebeginn offen lassen, kommen für diese Finsternis die Jahre 610, 609, 589, und 588 v. Chr. in Frage.”

In translation:

“The most important assertion of the Papyrus is found at the beginning of the third column: a priest named Amasis reports to the regrettably anonymous king that he has heard it in Daphnae that “the sky has swallowed the solar disk” (‘m t3 pt p3 jtn), whereupon he has betaken himself (to Sais?) to the embalming place (w’bt), in which the remains of King Psammetichus were being prepared for burial. ‘m pt with a star as object is after knhw the customary Egyptian representation of a solar or lunar eclipse. According to the assertion of the Papyrus, therefore, a solar eclipse had taken place shortly after the death of Psammetichus I or Psammetichus II; if we initially leave open the possibilities 664 and 663 B.C. for the beginning of the dynasty, the years 610, 609, 589, and 588 B.C. come into question for this eclipse.”

The twenty-one years between 610 and 589 or between 609 and 588 would span the reigns of Necho II and Psammetichus II. In fact, however, Hornung opts for Psammetichus I and for the solar eclipse of 30 September 610 B.C. (= -609).

As Depuydt (1995) notes, on page 53, footnote 50:

“Because the text is fragmentary, relations between persons, places, and objects are obscure. . . .”
That is probably why no one seems to know exactly where the priest Amasis was at various times, and why we do not even know which city Psammetichus died in, or which city he was being embalmed in.

On pages 377-378 of *Ancient Egyptian Chronology* (2006), Rolf Krauss (with whom I have many differences) writes:

“A solar or lunar eclipse supposedly occurred when Psammetichus I died. The source is the Demotic papyrus Berlin 13588, written in late Ptolemaic or early Roman times. The papyrus relates how a priest heard that the sky swallowed the disk (*jtn*) when Psammetichus I died; later the priest copied the “Book of Breathing” onto the mummy wrappings of Psammetichus I. The reported time of the supposed eclipse does not suit the solar eclipse of September 30, 610 BC; instead, it would fit the lunar eclipse of March 22, 610 BC. The context of the eclipse is fictitious, insofar as it would date the “Book of Breathing” — a creation of the Ptolemaic period — to the time immediately after the death of Psammetichus I. An eclipse that is reported in a fictitious tale cannot be deemed historical.”

If “the sky swallowed the disk (*jtn*) when Psammetichus I died”, that sounds to me like a solar eclipse, indeed a total one, rather than a lunar eclipse. If the disk was swallowed, it was out of sight, whereas the disk of the Moon usually remains visible throughout a lunar eclipse. The disk of the totally eclipsed Moon is severely shaded, and may even be slightly reddened, but it is *not* “swallowed” by the sky. So why does anyone take this as a lunar eclipse?

We are quite in the dark about the exact date of the death of Psammetichus I. So how can Krauss say that: “The reported time of the supposed eclipse does not suit the solar eclipse of September 30, 610 BC; instead, it would fit the lunar eclipse of March 22, 610 BC”? I do not understand that remark at all! And why does Krauss believe that: “An eclipse that is reported in a fictitious tale cannot be deemed historical”? I cannot agree with that. For one thing, the word “fictitious” is much too strong. For another thing, even if the writer of the Demotic papyrus was mistaken about the age of the “Book of Breathing” and/or about just what it was that was written on the mummy wrappings, it still remains possible that the tradition about an eclipse shortly after the death of Psammetichus was entirely accurate.

For whatever it is worth, the solar eclipse on 30 September 610 BC was total in southern Russia, but it was visible only as a smallish partial in Egypt. But the partial lunar eclipse on 22 March 610 BC was not visible at all in Egypt. (That partial lunar eclipse *was* visible farther to the east, at Jerusalem, Damascus,
Babylon, Ashur, and Nineveh, for example. That may be why Smith asks whether Psammetichus I died abroad.)

There is Apis evidence that Year 26 of Taharqa immediately preceded Year 1 of Psammetichus I. Gardiner (Egypt of the Pharaohs, page 353) does seem somewhat skeptical about this evidence: “On an Apis stela he [Psammetichus I] follows immediately upon Taharka, Tanuatamon not being alluded to.” But see also the “inquest” by Kitchen, TIP2, pages 161-163.

There is also Apis evidence that Psammetichus I reigned for 54 years. (For example, see Gardiner, page 357, and Jonsson, page 142.)

I continue to doubt that this eclipse was an eclipse of the Moon. The metaphor of “swallowing” seems to suggest a complete disappearance from sight. That is not the case with a partial solar eclipse, nor is it the case with a partial lunar eclipse. Even during a total lunar eclipse, the Moon usually remains at least faintly visible; it has not been “swallowed”. Consequently, I am looking for a solar eclipse that is total, since that is the only thing that seems to fill the bill.

Thus I even doubt Hornung’s claim (page 39) that the swallowing of a heavenly body meant either a lunar or a solar eclipse. (Occultations are not an issue here.) My searches through Gardiner and Faulkner have yet to turn up any talk of swallowing with regard to a lunar eclipse. Even Hornung’s own translation from the text does not say that this was a lunar eclipse. Instead, his translation speaks of the “solar disk” being “swallowed”. Nonetheless, it seems quite clear that the original demotic text does not say “solar”. I regret that I have not yet been able to get my hands on that original text (or on the paper of Smith), but in the meantime I cannot believe that Smith, Depuydt, and Krauss would be even slightly open to the possibility of a lunar eclipse if the text had specified “solar”. Further work is needed here.

Saïs, the capital of the Twenty-Sixth Dynasty, is conventionally thought to have been in the western part of the Delta. Velikovsky challenged this, and argued that Saïs was the same city as Tanis, which is in the eastern part of the Delta. See especially Ramses II and His Time (1978), pages 209-212. I am convinced that Velikovsky’s identification of Saïs as Tanis is correct, but in recent years I have come to doubt that the kings of the Nineteenth Dynasty are to be equated with the kings of the Twenty-Sixth Dynasty, and I have come to suspect that they might be completely separate sequences of kings. (Indeed, it appears that the Nineteenth Dynasty, like the Eighteenth Dynasty, was independent of any foreign domination,
and that the Twenty-Sixth Dynasty was for a long time subservient to the Neo-Assyrians, who were themselves subservient to the Persians and then to the Macedonians. One of the more striking aspects of the Short Chronology is that there were more layers of vassals than have conventionally been recognized.)

On 4 July -335, there was a total solar eclipse along the northern Delta and into southern Israel. According to *Starry Night*, as well as according to *Planetary, Lunar and Stellar Visibility*, Version 3.1, this solar eclipse was total at all of the following sites: Alexandria, Damietta, Saïs, Tanis, Daphnae, Port Said, Raphia, Jerusalem, and Tel Aviv. Since both Saïs and Tanis are on this list, it does not even matter whether we accept Velikovsky’s identification of Saïs with Tanis; we are covered either way.

Version 3.1 of Lange and Swerdlow shows no other total solar eclipse in Egypt between -350 and -310. That is good enough for me. -335 would correspond to the uncompleted Year 55 of Psammetichus I. His Year 1 would correspond to -389, and Year 26 of Taharqa would correspond to -390. Thus Taharqa’s uncompleted Year 27 would also correspond to -389.

(Taharqa may already have been dead, but his year-count seems to have lived on, at least on that one Apis stele. It happens. Perhaps the Apis scribes remained loyal to Taharqa, or perhaps they did not wish to mention his successor, Tanuatamon. Perhaps the Ethiopians had decided to keep the Egyptians in the dark about the death of Taharqa. I do not know the answers here, and I have not found anybody else who does know them.)

THE SEEN TOGETHERS

The technique that they were using in Report 501 and elsewhere was to note the lunar day on which the Sun and the Moon could, for the first time that month, be seen together in the morning. If they were seen together on day 14 or earlier, that month would probably have 29 days. If they were seen together on day 15 or later, that month would probably have 30 days. This technique is far from foolproof, but the mere fact that they tried to use it, and that they reported the key observations upon which it depended, can be useful to us, just in terms of what they actually saw.

The situation here is similar to the general situation with Assyrian astronomy, which is heavily infused with astrology. We can make use of the astronomical observations that they report, without accepting any of the astrological infusion. In
the same way, we can make use of the seen together observations that they report, without accepting the prediction device that they seem to have been toying with.

(By the way, I agree with Velikovsky that “astrology is superstition”. But I also agree with him that our past featured Worlds in Collision. The influences of other planets on human life in general would have been profound. But those experiences of our ancestors are not the sort of planetary and stellar influence on individual lives that both ancient and modern astrologers have talked so much about.)

There is an important aspect of the seen-together technique that I did not notice until recently. In order for the technique to be usable, the day count must be astronomically accurate. The start of the lunar month must not have been thrown off by bad seeing. Similarly, the day of the seen together must be astronomically accurate. It must not have been delayed for even a day by bad seeing.

The point is that the technique would have been inapplicable if either the day count or the seen together date was anything other than astronomically accurate. This means that when we are looking for places where a seen together report might fit, it needs to fit astronomically. In other contexts, one can legitimately invoke bad seeing to explain a discrepancy, but not here. A seen together report would have been of no value to them (at least for purposes of this technique) if either the lunar day count or the date of the seen together might have been affected by bad seeing. I think that they would have known that, even though it took me an inordinately long time to realize it.

A seen together is a valid astronomical observation, whether or not the technique that they based upon it happened to work in the month instant.

For me, the most promising seen together was on 27 March -433, which was on lunar day 14. (It does not matter, but the relevant New Crescents were on 13 March and 12 April, so that this was actually a 30-day month, despite what the technique would imply.)

**SARGON**

Report 501 is dated to “Kislev (IX), 14th day, year 1, Sargon king of Babylon.” Since Hunger states (page XXIV) that “Uncertain or conjectural translations are indicated by italics”, we need not follow him in reconstructing “Kislev”. Hunger’s only reason for using “Kislev” here seems to be that Kislev would fit his date of “-708
Nov. 27”. (Saturn would indeed have been in the “halo” of the Moon on that date, which is likely to have been in mid-Kislev, and there would have been a seen together of the Sun and the Moon the next morning, which would have been day 14.)

If we prefer a fifth-century placement, however, some month other than Kislev might be more appropriate. Thus I continue to think that Report 501 pertains to the seen together on the morning of 27 March -433 (which was day 14), with Saturn quite close to the Moon on the previous evening and still close enough to be in the “halo” on the following evening. Saturn would have been about fifteen or so degrees west of the Moon on 27 March, which according to Campbell Thompson (Volume II, page xxiv) was still close enough for them to say that Saturn was in the mysterious “halo” of the Moon. On 26 March, as noted above, it was quite close.

Year 1 of Sargon in Babylon is conventionally taken as his Year 13 as King of Assyria. (Sargon conquered Babylon in his Year 12 as King of Assyria, which would have been his Accession Year in Babylon.) I equate his Year 1 in Babylon with -434/433.
THE OVERALL CHRONOLOGY

Here is the overall chronology, with any lunar dates that are recorded. Most of this is a blending of work by Ginenthal, Thiele, and Rose.

-762    Halley’s Comet visits at the time of the Exodus.
-749/748?  Ascension of Saul.
           (The Eighteenth and Nineteenth Dynasties were in succession, and
           extended from the eighth century to the sixth century.)
-651/650  Possible date for Thutmose III’s attack on Jerusalem, Year 5 of Rehoboam,
           Year 23 of Thutmose III. (This needs further investigation.)
-577    Year 52 of Ramses II, counted from his co-regency.
-575    Year 34 of Ramses II, counted from his ascension.
-528    Year 1 of Cambyses may also be Year 1 of the Ninsianna observations.
-448/447  Fall of Samaria, Year 9 of Hoshea.
-447/446  Military coup against Shalmaneser, Ascension Year of Sargon in Assyria.
-434/433  Year 13 of Sargon in Assyria = Year 1 of Sargon in Babylon.
           Report 501 on 27 March -433 = Adar or Adar II, day 14, of Year 1 in
           Babylon.
[no year]  The Ninsianna document W. 1924. 802 is from the reign of Sargon in
           Babylon.
-431/430  Death of Sargon. Ascension Year of Sennacherib.
-426    Siege of Jerusalem, Year 14 of Hezekiah.
-407/406  Death of Sennacherib on Tebit 20 of Year 24.
-406    Ascension of Esarhaddon on Adar 8 or 18. (Pritchard, pages 290, 302.)
[no year]  A text from the reign of Sesostris III notes a raid into the Rezenu of
           Manasseh.
-395/394  Esarhaddon dies on Arasamnu 10 of Year 12.
-395/394  Ascension of Ashurbanipal in Assyria.
-394/393  Ascension of Shamash-Shum-Ukin in Babylon.
-391    Report 102 from 14 March -391, Year 3 of Ashurbanipal.
           Letter 371, total lunar eclipse in Tammuz on 8 July -391, Year 4 of
           Ashurbanipal.
-390    Halley’s Comet perihelion on September 14. See Reports 339 and 456.
-390/389  Taharqa officially deposed in Year 27. Some say that he was already dead.
           Ascension of Psammetichus I.
-374/373  Death of Shamash-Shum-Ukin in Year 20.
-351    “Year 1” of the Saturn Tablet of Kandalanu.
Ascension Year of Nabopolassar.

Death of Psammetichus I in Year 55. Ascension of Necho II.

Total solar eclipse on 4 July -335.

Josia slain by Necho II.

Alexander the Great comes to Egypt.

Jehoiachin, Ezekiel, and others taken in exile to Babylon.

Death of Necho II. Ascension of Psammetichus II.

Death of Psammetichus II on 1 3lt 23.

No total solar eclipse that year, anywhere. Ascension of Apries.

Fall of Jerusalem, Year 11 of Zedekiah, Babylonian Exile

Ascension Year of Nabonidus. Apries succeeded by Amasis.

The Moon set totally eclipsed on Ululu 13 = 2 October in Year 2 of Nabonidus.

At the moment, that is my best shot. Nonetheless, this chronology has to be considered a work in progress.

We are now getting into the third century. Apries straddles the turn, and Amasis would have reigned entirely in the third century. They must have been vassals, nomarchs, or less. Yet both of them seem to have been already known to Herodotos in the late fifth century! The truth may be that the passages about Apries and Amasis were added long after Herodotos by Alexandrian editors, who edited practically everything, anyway. I once criticized Heinsohn for using such an argument in order to get himself out of difficulty. Having now found myself in a similar difficulty, I retract my criticism of Heinsohn. I hate it when that happens. While I am at it, I must retract my criticism of Rohl for speaking of “The New Chronology”, as if there were only one new chronology. I have in the meantime grown rather fond of speaking of “The Short Chronology”, even though the one that Ginenthal and I favor is but one of many such short chronologies.

SESOSTRIS III AND MANASSEH

Though I am a Velikovskian, I must confess that I have not reread Ages in Chaos for several decades. On November 3, 2011, however, I looked up Velikovsky’s chapter on Thutmose III, and found the following intriguing passage on pages 173-174:

“In the Scriptures Palestine is frequently called “Erez” (country), “Erez Israel” (the land of Israel), and “Arzenu” (possessive case, “our country”). What the Egyptologists read as Retenu or Rezenu is probably the “Arzenu” of the Bible.
“In only one inscription of the Middle Kingdom (Twelfth Dynasty) under Sesostris III is the name Rezenu mentioned — it is a very short account of a raid into that country against M-n-tyw. As we shall find the same name, Mntyw, in Egyptian documents of a much later period, that of King Menashe (Manasseh), the Mntyw of the Middle Kingdom must mean the tribe Menashe. If the inscription is correctly attributed to the time of Sesostris III, the mention of the tribe Menashe would imply that before the Israelites had come to stay in Egypt they had dwelt in Palestine, not as a single patriarchal family, but as tribes strong enough to be regarded as enemies by the pharaoh. This would accord with the tradition of a defeat inflicted by Abraham and the servants of his household on the kings of Shinar and Elam and their allies (Genesis 14), and with the number of Israelites (about two million, including women and children) in the days of the Exodus, after some two hundred years of sojourning in Egypt.”

I wish that Velikovsky had given us a reference here. As I write, in November of 2011, I have just begun to look for this ancient source. In any case, Velikovsky’s puzzlement about how someone living under Sesostris III could refer to a raid on the Rezenu of Manasseh, who on the conventional chronology lived some twelve centuries later, is easily resolved on the Short Chronology: I have long put the basic 19 years of Sesostris III from -400 to -382; I am now putting the independent reign of Manasseh from -411 to -367, which neatly straddles the entire basic reign of Sesostris III.

The salvaging of this synchronism was unintended and unanticipated. I had reason to lower Sesostris III by 1477 years, and now I have reason to lower Manasseh and all of the other kings of Israel and Judah by 274 years. The twelve-century gap that bothered Velikovsky is now closed. Ginenthal calls these things confirmations, correlations, and corroborations. We seem to be running across them almost daily, everywhere we look.

With various wordings, Velikovsky frequently described his principal chronological researches as extending from the end of the Middle Kingdom to the coming of Alexander the Great. For me, there is no such stretch: indeed, it was the coming of Alexander itself that brought the Twelfth Dynasty and the Middle Kingdom to an end. (I regard the Thirteenth Dynasty as mere nomarchs, bureaucrats, and petty vassals, albeit with royal pretensions.) Velikovsky might better have said that he focused on the period from the Exodus to Alexander the Great. For Velikovsky, that would be a period of eleven centuries or more. On the Short Chronology, and with Ginenthal’s placement of the Exodus, that period would now be shortened to 431 years.
If it were not for ten of my articles, a number of reviews, one published book on Aristotle, and one unpublished book on Plato, it would be fair to say that all of my scholarly output has been heavily indebted to Velikovsky. Those of us who are occasionally privileged to see things that Velikovsky did not see should always remember that the reason that we sometimes see farther than he did is that we are like the proverbial dwarfs standing on the shoulders of a giant.

REFERENCES


Daphne Chappell, “Fitting Lunar Dates”, Chronology & Catastrophism Review 2007, pages 2-6, with my “Reply to Chappell” following on pages 6-8 and 68.


Teije de Jong, “Early Babylonian Observations of Saturn: Astronomical Considerations”, in Under One Sky: Astronomy and Mathematics in the Ancient Near East, edited by John M. Steele and Annette Imhausen, Münster, Ugarit-Verlag, 2002, pages 175-192. (Some years ago, I did read de Jong’s paper. It settled nothing about the chronological placement of the Saturn Tablet, and I set it aside. Since Jonsson is favorably disposed toward the de Jong paper, I just now read it again, and reached the same conclusion as before.)


Gregory XIII, *Inter Gravissimas*, 1582.


Heribert Illig, “Calendar Reforms of Caesar and Gregory XIII (Calendar Studies Show that the Thesis of ‘Invented Middle Ages’ is Correct)”, *Chronology & Catastrophism Review* 2011, pages 3-6.


Lynn E. Rose, “A Review of Wells’s Review of Sun, Moon, and Sothis”, *Aeon* VI:5, July, 2004, pages 8-14. Half of the Wells review is highly unfavorable material about me and half is highly favorable material about Bradley E. Schaefer; consequently, half of my review of Wells is highly unfavorable material about Wells and half is highly unfavorable material about Schaefer.


Immanuel Velikovsky, *Theses for the Reconstruction of Ancient History from the End of the Middle Kingdom in Egypt to the Advent of Alexander the Great*, New York and Jerusalem, Scripta Academica Hierosolymitana (Simon Velikovsky Foundation), 1945.

BIBLIOGRAPHY


AA Walks Through Britain’s History (NY 2001)


AEON, vol. II, no. 6 (May, 1992)

AEON, vol. VI, no. 6 (October 2005)

Agius, Dionisius A.: Classic Ships of Islam: From Mesopotamia to the Indian Ocean (Leiden, the Netherlands 2008)

Agrawal, D.P. and Kusumgar, Sheela: Prehistoric Chronology and Radiocarbon Dating in India (New Delhi India 1972)

Akkermans, Peter M.M.G. and Schwartz, Glenn M.: The Archaeology of Syria: From Complex Hunter-Gatherers to Early Urban Societies (ca. 16,000—300 BC)

Alexis, Jonas E.: In the Name of Education (Maitland FL 2007)


Allsopp, D. et al., Introduction to Biodeterioration (Cambridge UK 2004)

Americana Science Annual 1987 (NY 1986)

Anonymous, “On the Learning of the Ancient Irish,” The Dublin University Magazine (Dublin 1834)


Appleton’s Annual Cyclopedia and Register of Important Events, vol. 6 (London 1882)

Armitage, Ella S.: A Key to English Antiquities (Sheffield UK 1897)


Arnold, C.J.: Roman Britain to Saxon England: An Archaeological Study (Beckenham UK 1984)

Arnold, Caroline and Arnold, Arthur: Stone Age Farmers Beside the Sea: Scotland’s Prehistoric Village of Skara Brae (NY 1997)

Aronson, Marc and Parker-Pearson, Mike: If Stones Could Speak: Unlocking the Secrets of Stonehenge (Washington D.C. 2011)

Aronson, Merry; Spetner, Don and Ames, Carol: The Public Relations Writer’s Handbook: The Digital Age (San Francisco CA 2007)

Aston, Michael; Aston, Mick and Taylor, Tim: The Atlas of Archaeology (NY 1998)


Atkinson, Richard J.C.: Stonehenge (NY 1963)
Ausenda, Giorgio, ed.: *After Empire: Towards an Ethnology of Europe’s Barbarians* (Woodbridge UK 2002)


Baillie, Mike: *Exodus to Arthur, Catastrophic Encounters with Comets* (London 1999)

Baity, Elizabeth Chesley: “Archaeoastronomy and Ethnoastronomy,” *Current Anthropology*, vol. 14, no. 4 (October, 1973)

Baity, Elizabeth Chesley: “Reply,” *Current Anthropology*, vol. 14, no. 4 (October, 1973)


Barber, Martyn: *Bronze and the Bronze Age: Metalwork and Society in Britain c. 2500-800 BC* (Stroud UK 2003)


Barker, Graeme: *Prehistoric Farming in Europe* (Cambridge UK 1985)


Barker, Philip: *Technique of Archaeological Excavation* (London 1993)


Barrett, John and Iredale, David: *Discovering Old Handwriting* (Princes Risborough UK 2001)


Bartlett, Robert: *The Natural and the Supernatural in the Middle Ages* (Cambridge UK 2008)


Behringer, Wolfgang: *A Cultural History of Climate* (Cambridge UK 2007)

Benigni, Helen; Carter, Barbara and Ua Cuinn, Eadhmonn: *The Myth of the Year: Returning to the Origin of the Druid Calendar* (Lanham IN 2003),


Berresford Ellis, Peter: *A Brief History of the Druids* (NY 2002)
Birket-Smith, Kaj: *The Paths of Culture* (Madison WI 1965)
Blain, Jenny and Wallis, Robert J.: *Sacred Sites: Contested Rights/Rites* (Brighton UK 2007)
Blair, John and Ramsay, Nigel: *English Medieval Industries: Craftsmen, Techniques, Products* (London 2001)
Borlase, William Copeland: *The Dolmens of Ireland*, vol. III (London 1897)
Boswell, James: *The Life of Samuel Johnson* vol. I (London 1817)
Boutell, Charles: *A Manual of British Archaeology* (London 1858)
Bowman, Sheridan: *Radiocarbon Dating* (Berkeley CA 1990)
Bradley, Richard: *The Prehistory of Britain and Ireland* (Cambridge UK 2007)
Braidwood, Linda S.: *Prehistoric Archaeology along the Zagros Flanks* (Chicago IL 1983)
Bronk Ramsey, Christopher et al., “Radiocarbon-Based Chronology for Dynastic Egypt,” *Science* (June 18, 2010)

Brown, James: *The History of Scotland* (Edinburgh 1909)


Burl, Aubrey: *From Carnac to Callanish: The Prehistoric Stone Rows and Avenues of Britain, Ireland and Brittany* (New Haven CT 1993)


Burl, Aubrey: *Prehistoric Avebury* (New Haven CT 2002)


Burl, Aubrey: *The Stone Circles of Britain, Ireland, and Brittany* (London 1995)


Cabot Hale, Nathan: *Abstraction in Art and Nature* (NY 1993)


Calude, Cristian S. et al., eds.: *Unconventional computation, 7th International conference UC 2008 Vienna, Austria August 2008*,

*Cambridge Alumni Magazine, The*, Issue 61


Carlisle, Rodney P.: *Scientific American Inventions and Discoveries*… (Hoboken NJ 2004)

Carver, M.O.H.: *Sutton Hoo Research Committee Bulletin*, no. 5 (January 1988), (Woodbridge UK 1993)


Castleden, Rodney: *Inventions that changed the World* (Edison NJ 2007)


Caton, J.D.: *Reversion of Domesticated Animals to the Wild State*, (NY 1882)
Childe, Vere Gordon *et al.*, *Skara Brae: A Pictish Village in Orkney* (London 1931)
Childe, Vere Gordon *et al.*: *Ancient Dwellings at Skara Brae* (Edinburgh 1933)
Childe, Vere Gordon: *Scotland Before the Scots* (London 1944)
Childe, Vere Gordon: *The Bronze Age* (NY 1930)
Chippindale, Christopher: *Stonehenge Complete* (Ithaca NY 1983)
*Chronology and Catastrophism Review* (July, 2011)
*Chronology and Catastrophism Workshop* (February, 2011)
*Chronology and Catastrophism Workshop* 2009 no. 3 (November 2009)
*Chronology and Catastrophism Workshop* 2009, no. 2 (May 2009)
Clark, Grahame: *World Prehistory: A New Outline* (Cambridge UK 1979)
Clarke, David and Maguire, Patrick: *Skara Brae: Northern Europe’s Best Preserved Prehistoric Village* (Edinburgh 1989)
Cleal, Rosamund; Walker, K.E.; Montague, R. and Allen, Michael J.: *Stonehenge in its Landscape: Twentieth Century Excavations* (English Heritage 1995)
Clegg, David: *Cornwall and the Isles of Scilly* (Leicester UK 2005)
Clemoes, Peter: *Anglo-Saxon England* (Cambridge UK 2007)
Clutton-Brock, Juliet: *A Natural History of Domesticated Mammals* (Cambridge, UK 1999)
Codington, Thomas: *Roman Roads in Britain* (London 1903)
Coffey, George: *New Grange (Brugh na Boinne) and Other Incised Tumuli in Ireland* (Dublin Ireland 1912)
Coles, John M.: *Field Archaeology in Britain* (London 1972)
Collingwood, Robin George: *Roman Britain*, new revised ed. (Oxford UK 1953)
Colquhoun, Ian and Burgess, Colin, eds.: *The Swords of Britain* (Munich Germany 1988)
Contenau, Georges: *Everyday Life in Babylonia and Assyria* (NY 1966)
Cook, Olive and Smith, Edwin: *The English House Through Seven Centuries* (Woodstock NY 1983)
Coppens, Philip: *Land of the Gods: How a Scottish Landscape was Sanctified to Become Arthur’s Camelot* (Amsterdam, the Netherlands 2007)
Cróinin, Dáibhí Ó., ed.: *A New History of Ireland: Prehistoric and Early Ireland* (Oxford UK 2005)
Crowe, Michael J.: *Theories of the World from Antiquity to the Copernican Revolution*, (Mineola NY 2001),
Cunliffe, Barry; Gosden, Chris and Joyce, Rosemary A., eds.: *The Oxford Handbook of Archaeology* (Oxford UK 2009)
*Current Anthropology*, vol. 24, no. 3 (1983)
*Current Anthropology*, vol. 37, no. 2 (April, 1996)
*Current World Archaeology*, issue 42, Aug/Sept 2010
Curwen, E. Cecil and Hatt, Gudmund: *Plough and Pasture: The Early History of Farming* (NY 1953)
Cutting, Marion Valerie: *The Neolithic and Early Chalcolithic Farmers of Central and Southwest Anatolia* (Oxford UK 2005)
Cymrrodor, Y.: *Embodying the Transactions of the Honourable Society of Cymrrodorion*, vol. IV, pt. 1 (January, 1881)
Darvill, Timothy C.: *Prehistoric Britain* (London 2010)
Darvill, Timothy: *Prehistoric Britain From the Air: A study of space, time and society* (Cambridge UK 1996)
Darwin, Charles: *The Formation of Vegetable Mould, Through the Action of Earthworms, with Observations on their Habitats* (NY 1881)
Davis, Joseph Barnard and Thurnam, John: *Crania Britannica* (London 1865)
De Hamel, Christopher: *Scribes and Illuminators* (Toronto 1992)
Del Mar, Alexander: *The Middle Ages Revisited: or, The Roman Government and Religion and Their Relations to Britain* (NY 1900)
Deutsch, Andreas et al., eds.: *Structure-based drug design: experimental and computational approaches* (Dordrecht, the Netherlands 1998)
Dineley, Merryn: *Barley, Malt and Ale in the Neolithic* (Oxford 2004)
Duan, Wenjie and Tan, Chung: Dunhuang Art: Through the Eyes of Duan Wenjie (New Delhi 1999)
Dublin University Magazine, The (Dublin 1834)
Dubos, René Jules: Man Adapting (New Haven CT 1980)
Duby, Georges: The Early Growth of the European Economy: Warriors and Peasants from the Seventh to the Twelfth Century (Ithaca NY 1974)
Dumont, Henri J., ed., The Nile: Origin, Environments, Limnology and Human Use (Gent, Belgium 2009)
Duncan, Heather: Scottish Pride: 101 Reasons to be Proud of Your Scottish Heritage (NY 2003)
Edmondson, Jonathan: Augustus (Edinburgh 2007)
Edwards, Kevin J. and Ralston, Ian, eds.: Scotland After the Ice Age: Environment, Archaeology and History 8000 BC-AD 1000 (Edinburgh 2005)
Eisler, Riane Tennenhaus: Sacred Pleasure: Sex, Myth and the Politics of the Body (San Francisco CA 1995)
Eisler, Riane Tennenhaus: The Chalice and the Blade: Our History, Our Future (San Francisco CA 1987)
Elkins, James: Our Beautiful, Dry, and Distant Texts: Art History as Writing (University Park PA 1997)
Ellis, Ralph: Thoth: Architect of the Universe (Cheshire UK 2001)
Engels, Donald W.: Classical Cats: The Rise and Fall of the Sacred Cat (London 1999)
Eppler, Mark: The Wright Way (NY 2004)
Eshleman, Clayton: Juniper Fuse: Upper Paleolithic Imagination and Construction of the Underworld (Middletown CT 2003)
Evans, E. Estyn and de Breffny, Brian: The Irish World (NY 1986)
Evans, E. Estyn: “Hearth and Home,” Irish Writing in the Twentieth Century, David Pierce, ed. (Cork Ireland 2000)
Evans, John G.: Land Snails in Archaeology (London 1972)
Exon, Sally et al., Stonehenge Landscapes: Journeys Through Real and Imagined Worlds (Oxford UK 2000)
Fagan, Brian M.: From Black Land to Fifth Sun: The Science of Sacred Sites (Reading MA 1999)
Farris, William Wayne: Japan to 1600: A Social and Economic History (Honolulu 2009)

Feldman, Burton and Richardson, Robert D.: *The Rise of Modern Mythology 1680-1860* (Bloomington IN 1972)

Ferguson, Samuel: “On a Passage in ‘Historia Anglorum’ of Henry of Huntington, Relative to Stonehenge,” *Proceedings of the Royal Irish Academy*, vol. 1 IX (Dublin, Ireland 1867)

Fergusson, James cited in *The Living Age* vol. 66 (Boston MA 1860) (and in Lubbock)


Fergusson, James: *Tree and Serpent Worship: or, Illustrations of Mythology and Art in India in the First and Fourth Centuries after Christ* (London 1868; reprint Whitefish MT 2000),

Feuerstein, Georg; Kak, Subhash and Frawley, David: *In Search of the Cradle of Civilization: New Light on Ancient India* (Delhi 2005)

Field, David: “Great Sites: Silbury Hill,” *British Archaeology*, vol. 70 (May 2003) (no longer available on the net)


Fowke, Gerard: *Archaeological History of Ohio: The Mound Builders and Later Indians* (Columbus OH 1902)


Fowler, P.J.: *The Farming of Prehistoric Britain* (Cambridge UK 1983)

Franklin, Anna: *Midsummer: Magical Celebrations of the Summer Solstice* (St. Paul MN 2002)


Fussell, George Edwin: *The Classical Tradition in Western European Farming* (Cranbury NJ 1972)

Galilei, Galileo: *Dialogue on the Great World Systems*, Thomas Salusbury translation (Chicago, IL 1953)

Gardiner, Alan: *Egypt of the Pharaohs* (London 1961)


Gaynor, Elizabeth et al.: *World History: Connections to Today* (Upper Saddle River NJ 1997)


Ginenthal, Charles: *Pillars of the Past*, vol. I (Forest Hills NY 2003)

Ginenthal, Charles: *Pillars of the Past*, vol. II, Mesopotamian, Anatolian, Mycenaean, Minoan and Harappan Chronology (Forest Hills NY 2008)

Ginenthal, Charles: *Pillars of the Past*, vol. III (Forest Hills NY 2010)

Gingerich, Owen in Ruggles, Clive N.L.: *Astronomy in Prehistoric Britain and Ireland* (New Haven CT 1999)

Goffer, Zvi: *Archaeological Chemistry* (Hoboken NJ 2007)

Goldberg, Paul and Macphail, Richard: *Practical and Theoretical Geoarchaeology* (Malden MA et al. 2006)


Gove, Harry E.: *From Hiroshima to the Iceman* (London 1999)

Gowland, William and Judd, John Wesley: *Recent Excavations at Stonehenge* (1902; reprint Whitefish MT 2009)


Green, Miranda: *Animals in Celtic Life and Myth* (London 2002)

Greene, Kevin and Moore, Tom: *Archaeology: An Introduction*, 5th ed. (NY 2010)

Greenwell, William and Rolleston, George: *British barrows: a record of the examination of sepulchral mounds in various parts of England* (Oxford UK 1877)

Greer, John Michael: *The Ecotechnic Future: Envisioning a Post-Peak World* (Gabriola Island BC, Canada 2009)


Grose, Vernon L.: *Science But Not Scientists* (Bloomington IN 2006)

Gubbins, David and Bervera, Emilio Herrero, eds.: *Encyclopedia of Geomagnetism and Paleomagnetism* (Dordrecht, the Netherlands 2007)

Haarmann, Harald and Marler, Joan: *Introducing the Mythological Crescent: Ancient Beliefs and Imagery connecting Eurasia with Anatolia* (Wiesbaden, Germany 2008)

Habu, Junko: *Ancient Jōmon of Japan* (Cambridge UK 2004)

Hadingham, Evan: *Early Man and the Cosmos* (Norman OK 1984)

Haldar, Alfred: *Who Were the Amorites?* Vol. 35 (Leiden, the Netherlands 1971)


Hanawait, Barbara: *The European World 400-1450* (Oxford 2005)

Harding, A.F.: *European Societies in the Bronze Age* (Cambridge UK 2000)

Hardwick, William A.: *History and Antecedents of Brewing* (NY 1995)

Harris, Marvin and Ross, Eric B.: *Food and Evolution: Toward a Theory of Human Food Habits* (Philadelphia PA 1989)


Harwood, Jeremy, introduction by Bendall, A. Sarah: *To the Ends of the Earth: 100 Maps That Changed the World* (Cincinnati OH 2006)

Haughton, Brian: *Haunted Spaces, Sacred Places* (Franklin Lakes NJ 2008)


Hawkes, Jacquetta, in North, John David: *Stars, Minds, and Fate: Essays in Ancient and Medieval Cosmology* (London 1989)

Hawkes, Jacquetta: *Antiquity*, vol. 41 (1967)


Hawkins, Gerald S.: *Beyond Stonehenge* (NY 2001)

Hawkins, Gerald S.: *Stonehenge Decoded* (NY 1988)

Hawkins, Gerald: *Mindsteps to the Cosmos* (Singapore 2002)

Hayes, Andrew: *Archaeology of the British Isles* (London 2005)

Hayes, Tom: *Jump Point: How Network Culture is Revolutionizing Business* (NY 2008)


Heath, Robin: *Stonehenge* (NY 2001)

Heggie, Douglas C., ed.: *Archaeoastronomy in the Old World* (Cambridge UK 1982)


Heilbron, J.L.: *The Sun in the Church* (Cambridge UK 1999)


Hems, Alison and Blockley, Marion R.: *Heritage Interpretation* (Abingdon UK 2006)

Heng, Chye Kiang: *Cities of Aristocrats and Bureaucrats: The Development of Medieval China* (Singapore 1999)

Henige, David: *Historical Evidence and Argument* (Madison WI 2005)


Herlihy, David: *The Black Death and the Transformation of the West* (Cambridge UK 1985)


Hill, Rosemary: *Stonehenge* (London 2008)

Hooke, Della and Burnell, Simon: *Landscape and Settlement in Britain, AD 400—1066* (Exeter UK 1995)
Horney, Ian Spencer: *A History of Beer and Brewing* (Cambridge UK 2003)
Hume, David: *An Enquiry Concerning the Principles of Morals* (London 1751)
Hunter, John and Ralston, Ian, eds.: *The Archaeology of Britain: An Introduction from the Upper Paleolithic to the Industrial Revolution* (London 1999)
Hunter, John et al.: *Investigations in Sanday Orkney* (Oakville CT 2007)
Hutton, Ronald: *Blood and Mistletoe: The History of the Druids in Britain* (New Haven CT 2009)
Hutton, Ronald: *The Stations of the Sun* (Oxford UK 1996)
Iredale, David and Barrett, John: *Discovering Your Old House* (Princes Risborough UK 2001)
Jackson, Mark and Greene, Kevin: “Ceramic Production,” in *The Oxford Handbook of Engineering and Technology*, John Peter Olesen, ed. (Oxford 2008)
Jacobs, Jane: *The Economy of Cities* (NY 1969)
James, Peter and Thorpe, I.J.: *Ancient Inventions* (NY 1995)
James, Peter et al.: *Centuries of Darkness* (New Brunswick NJ 1993)
Johnston, Wesley: “Climate of Ireland” (Internet)
http://www.wesleyjohnston.com/users/ireland/geography/climate.html
Jones, Andrew: *Archaeological Theory and Scientific Practice* (Cambridge UK 2002)
Jones, Michael E.: *The End of Roman Britain* (Ithaca NY 1998)
*Journal of Derbyshire: Archaeological and Natural History Society*, vol. VI (London January 1884)
*Journal of Iberian Archaeology* vol. I (2006)
Journal of the Anthropological Institute of Great Britain and Ireland, old series vol. XXIX, new series vol. II (London 1899)


Journal of the History of Astronomy, vol. 6 (1975)


Jupp, Peter C. and Gittings, Clare, eds: Death in England: an Illustrated History (Manchester, UK 1996)

Kalof, Linda: Looking at Animals in Human History (London 2007)


Karetsky, Patricia Eichenbaum: Court Art of the Tang (London 1996)


Kent, Susan, ed.: Farmers as Hunters: The Implications of Sedentism (Cambridge UK1998)


Kepler, Johannes, in J.L. Heilbron: The Sun in the Church (Cambridge UK 1999)


Kidger, Mark: Astronomical Enigmas: Life on Mars, the Star of Bethlehem & Other Milky Way Mysteries (Baltimore MD 2005)

Kipfer, Barbara Ann: Encyclopedic Dictionary of Archaeology (NY 2000)

Kleiner, Fred S.: Gardner’s Art Through the Ages: The Western Perspective (Boston MA 2010)


Kline Cohn, Samuel: “Introduction,” in David Herlihy, The Black Death and the Transformation of the West (Cambridge UK 1985)


Knight, Christopher and Lomas, Robert: Proceedings of the Society of Antiquaries of Scotland vol. 108 (Edinburgh UK 1979)

Knight, Christopher and Lomas, Robert: Uriel’s Machine (Beverly MA 2001)

Koch, John T.: Celtic Culture: A Historical Encyclopedia (Santa Barbara CA 2006)

Kohli, Harish: Across the Frozen Himalaya: The Epic Winter Ski Traverse...from Karakoram to Lipu Lekh (New Delhi 2000)

Korn, Frank J.: Hidden Rome (Mahwah NJ 2002),
KRONOS, vol. II, no. 3 (Feb. 1977)
KRONOS, vol. VIII, no. 3 (1983)
Kynes, Sandra: Your Altar: Creating a Sacred Space for Prayer and Meditation (Woodbury MN 2007)
LaFreniere, Gilbert F.: The Decline of Nature: Environmental History and the Western Worldview (Palo Alto CA 2008)
Lancaster Brown, Peter: Archaeoastronomy vol. 3-4 (1980)
Lancaster Brown, Peter: Megaliths and Masterminds (NY 1979)
Lancaster Brown, Peter: Megaliths, Myths and Men: An Introduction to Astro-Archaeology (Mineola NY 2000)
Larned, J.N.: History for Ready Reference (NY 1894)
Leary, Jim; Field, David and Attenborough, David: The Story of Silbury Hill (British Heritage 2010)
Legge, J., translation: The Shu King, The Cannon of Yao (1876)
Leichty, Eric and Ellis, Maria de J. in A Scientific Humanist: Humanist Studies in Memory of R. Abraham Sachs (Philadelphia PA 1981)
Littell, Eleakim and Littell, Robert: Littell’s Living Age, vol. 99 (Boston MA 1860)
Little, Lester K., ed.: Plague and the End of Antiquity: The Pandemic of 541-750 (Cambridge UK 2007)
Littman, Mark; Espenak, Fred and Wilcox, Ken: Totality: Eclipses of the Sun (Oxford UK 2008)
Lockyer, Sir Norman: Stonehenge and Other British Monuments Astronomically Considered (London 1906)
London Quarterly Journal, The: vol. 129 (NY January/April 1870)
London, Jack: The Strength of the Strong (NY 1914)
Long, William: Stonehenge and its Barrows (London 1876)
Lubbock, Sir John: Pre-historic Times: As Illustrated by Ancient Remains and Manners and Customs of Modern Savages (London 1865)
Lucas, Gavin: Critical Approaches to Fieldwork: Contemporary and historical archaeological practice (London 2001)


MacCarthy, B., ed.: *Annala Uladh: Introduction and Index* (Edinburgh 1901)


MacKie, Euan: *An Archaeological view of Neolithic Astronomy, Glasgow Hunterian Museum, A reprint of a talk given on megalithic monuments* (Glasgow UK November 29, 1969)

Magli, Giulio: *Mysteries and Discoveries: Archaeoastronomy from Giza to Easter Island* (NY 2009)


Mango, Cyril A.: *The Art of the Byzantine Empire*, 312-1453 (Toronto 1986)


McIntosh, Jane: *Handbook of Life in Prehistoric Europe* (Oxford UK 2009)

McNeill, William Hardy: *Plagues and People* (NY 1977)

Mellaart, James: *Çatal Hüyük: A Neolithic Town in Anatolia* (London 1967)


Miln, James: *Excavations at Carnac (Brittany): A Record of archaeological researches*… (Edinburgh UK 1877)


Mithen, Steven: *After the Ice: A Global Human History, 20,000–5000 B.C.* (Cambridge UK 2006)

Monaghan, Patricia: *The Encyclopedia of Celtic Mythology and Folklore* (NY 2004)


Moore, Thomas Gale: *Climate of Fear: Why We Shouldn’t Worry About Global Warming* (Washington DC 1998)
Morrisey, Lee: From the Temple to the Castle: An Architectural history of British Literature 1660-1760 (Charlottesville VA 1999)
Motz, Lloyd and Weaver, Jefferson Hane: The Unfolding Universe: A Stellar Journey (Cambridge UK 2002)
Nadaillac, Jean-Francois-Albert du Pouget and Bell, Nancy (N. D’Anvers) translation: Manners and Monuments of Prehistoric Peoples (NY 1892)
Neugebauer, Otto: History of Ancient Mathematical Astronomy (Berlin, 1975)
Neusner, Jacob and Chilton, Bruce, eds.: Religious Tolerance in World Religions (West Conshohocken PA 2008)
New International Encyclopedia vol. VIII (New York, 1915)
New Scientist (February 21, 1963) “Notes and Comments,”
Newell, Edward Theodore: The Seleucid Mint at Antioch (NY 1918)
Newman, Paul B.: Daily Life in the Middle Ages (Jefferson NC 2001)
Newton, Robert R.: Medieval Chronicles and the Rotation of the Earth (Baltimore MD 1972)
Nichols, Thomas Low: Religions of the World: an Impartial History of Religious Creeds… (Cincinnati OH 1855)
NicMhacha, Sharynne MacLeod: Queen of the Night: Rediscovering the Celtic Moon Goddess (Boston MA 2005)
Niel, Fernand: The Mysteries of Stonehenge (NY 1975)
North, John David: Cosmos: An Illustrated History of Astronomy and Cosmology (Chicago IL 2008)
North, John David: Stars, Minds, and Fate: Essays in Ancient and Medieval Cosmology (London 1989)
O’Kelly, Michael and O’Kelly, Claire: Early Ireland: An Introduction to Irish Prehistory (Cambridge UK 2001)

O’Shea, Elizabeth Mary: “Ancient Irish Annals,” *The Smith College Monthly*, vol. XIV, no. 8 (May, 1907)


Okabe, Atsuyuki: *GIS-Based Studies in the Humanities and Social Sciences* (Boca Raton FL 2006)

Olesen, John Peter, ed.: *The Oxford Handbook of Engineering and Technology* (Oxford 2008)

Olmert, Meg Daley: *Made for Each Other: The Biology of the Human-Animal Bond* (Cambridge UK 2009)

Olmsted, Garrett S.: *The Gaulish Calendar: A Reconstruction from the Bronze Fragments from Coligny...,* (Bonn Germany 1992)

Olsen, Brad: *Sacred Places Europe: 108 Destinations* (San Francisco CA 2007)


Pakistan Year Book (Karachi 1981)

Palmer, Allison Lee: *Historical Dictionary of Architecture* (Lanham MD 2008)

Palmer, Douglas; Bahn, Paul G. and Tyldesley, Joyce: *Unearthing the Past: The Great Archaeological Discoveries that have changed History* (Guildford UK 2005)

Palmer, Trevor: “Science, Technology and the Chronology of the Ancient World,” *AEON*, vol. VI, no. 6 (October, 2005)

Palmer, Trevor: *Perilous Planet Earth: Catastrophes and Catastrophism through the Ages* (Cambridge 2003)


Pearce, Susan M.: *Archaeological Curatorship* (Washington D.C. 1990),

Peiser, Benny J.; Palmer, Trevor and Bailey, Mark E., eds.: *Natural Catastrophes During Bronze Age Civilizations: archaeological, geological, astronomical and cultural perspectives* (Oxford UK 1990)

Pepper, Elizabeth and Wilcox, John: *Magical and Mystical Sites: Europe and the British Isles* (Grand Rapids MI 1996)


Pierce, David, ed.: *Irish Writing in the Twentieth Century* (Cork Ireland 2000)

Pinsky, Valerie and Wylie, Alison, eds.: *Critical Traditions in Contemporary Archaeology* (Cambridge UK 1989)


*Popular Science Monthly*, vol. 20 (NY 1881-1882)


Possehl, Gregory L.: *The Indus Civilization: A Contemporary Perspective* (Walnut Creek CA 2002)


*Proceedings of the Cambridge Antiquarian Society* (London 1893)

*Proceedings of the Clifton Antiquarian Club for 1884-88*, vol. I, Alfred E. Hudd, ed. (Stonebridge 1886)


*Quarterly Review, The*, vol. 129 (1870)

*Quaternary Dating Methods: Developments in Paleontology and Stratigraphy* (Amsterdam, the Netherlands 1984)


Raymond, Robert: *Out of the Fiery Furnace: The Impact of Metals on the History of Mankind* (University Park PA 1986)


Reed, Michael: *The Landscape of Britain* (London 1990)


Renfrew, Colin; Chesterman, Judson T., and Aitken, Martin Jim: *Investigations in Orkney* (London, 1979)


Ritchie, Anna: *Neolithic Orkney in its European Context* (Cambridge UK 2000),
Ritchie, Anna: *Prehistoric Orkney* (London 1997)
Romanyshyn, Robert Donald: *Technology as Symptom and Dream* (London 2005)
Rose, Lynn E.: *Sun, Moon, and Sothis* (Deerfield Beach FL 1999)
Rudgley, Richard: *The Lost Civilization of the Stone Age* (NY 2000)
Ruggles, Clive, ed.: *Archaeoastronomy in the 1900s* (1993)
Rumble, Victoria R., foreword by Oliver, Sandra: *Soup through the Ages, A Culinary History With Period Recipes* (Jefferson NC 2009)
Runco, Mark A.: *Creativity: Theories and Themes* (Amsterdam, the Netherlands/Boston MA 2007)
Salzman, L.F.: *English Industries of the Middle Ages, Being an Introduction to the Industrial History of Medieval England* (1923, reprint Charleston SC 2009),
Schiller, Friedrich von: *History of the Thirty Years War* (Charleston SC 2003)
Schlager, Neil: *2000 B.C. to A.D. 699* (Detroit MI 2001)
Schoch, Robert M. with McNally, Robert Aquinas: *Voices of the Rocks: A Scientist Looks at Catastrophes and Ancient Civilizations* (NY 1999)
Schoch, Robert M. with McNally, Robert Aquinas: *Voyages of the Pyramid Builders* (NY 2004)
*Science Reporter, The*, volume 23 (Delhi, India 1986)
Sewall, Robert: *Eclipses of the Moon in India* (London 1896)
Sharples, Niall M.; Sheridan, Alison and Henshall, Audrey S.: *Vessels for the Ancestors: Essays on the Neolithic of Britain and Ireland in Honour of Audrey Henshall* (Edinburgh UK 1992)


Sidharth, Burra Gautam: *The Celestial Key to the Vedas* (Rochester VT 1999)


Singman, Jeffrey L.: *Daily Life in Medieval Europe* (Westport CT 1999)


*Smith College Monthly, The*, vol. XIV, no. 8 (May, 1907)


Smith, Graham T.: *Industrial Metrology: Surfaces and Roundness* (Southampton UK 2001)


Souden, David: *Stonehenge Revealed* (New York, 1997)


Stathakopoulos, Dionysios Ch.: *Famine and Pestilence in the Late Roman and Early Byzantine Empire: A Systematic Survey of Subsistence Crises and Epidemics* (Aldershot UK 2004)

Stearns, Peter N.; Adas, Michael; Schwartz, Stuart B.; Gilbert, Mark J.: *World Civilizations: The Global Experience* (NY 2007)


Steel, Duncan: *Rogue Asteroids and Doomsday Comets: The Search for the Million Megaton Menace that Threatens Life on Earth* (NY 1979)

Steele, John M.: *Observation and Prediction of Eclipse Times by Early Astronomers* (Dordrecht, the Netherlands 2000)


Stephenson, F. Richard: *Historical Eclipses and Earth's Rotation* (Cambridge UK 2008)

Stevens, Frank: *Stonehenge Today and Yesterday* (London 2009)

Stiebing, William E., Jr.: “Cosmic Catastrophism,” *Aeon*, vol. II, no. 6 (May, 1992)


Stokes Brown, Cynthia: *Big History: From the Big Bang to the Present* (London 2007)

Stone, Linda; Lurquin, Paul F. and Cavalli-Sforza, Luigi Luca: *Genes, Culture, and Human Evolution: A Synthesis* (Malden UK 2007)


Stout, Geraldine: *Newgrange and the Bend of the Boyne* (Cork Ireland 2004)


Sweeney, Emmet J.: *The Lost History of Ireland: An enquiry into the pre-Christian History of the Gaels* (Derry Ireland 1992)
Taher, Mohamed: *Encyclopedic Survey of Islamic Culture* (New Delhi 2003)
Taylor, Royal Ervin: *Radiocarbon Dating: An Archaeological Perspective* (Orlando FL 1987)
Thom, Alexander: *Megalithic Sites in Britain* (Oxford UK 1972)
Thomas, Charles: *Christianity in Britain to AD 500* (Berkeley CA 1981)
Thomas, Julian: *Understanding the Neolithic* (London 1991)
Thornton, Russell: *American Indian Holocaust and Survival: A Population History Since 1492* (Norman OK 1987)
Todd, Ian A.: *Çatal Hüyük in Perspective* (Menlo Park CA 1976)
Tomkins, Stephen: *A Short History of Christianity* (Grand Rapids MI 2005)
*Transactions of the American Philosophical Society*, vol. 81, pt 3 (Philadelphia PA 1991)
Trow-Smith, Robert: *A History of British Livestock Husbandry, to 1700* (Abingdon UK 1957)
Trupp, Claudia: *Hard Time & Nursery Rhymes...* (NY 2009)
Turk, Jonathan: *In the Wake of the Jōmon: Stone Age Mariners and A Voyage Across the Pacific* (Camden ME 2005)
Turner, Thomas: *The Metallurgy of Iron* (London 1908)
Van de Mieroop, Marc: *The Ancient Mesopotamian City* (Oxford UK 1999)
Van Dyke, John C., ed: *College Histories of Art* (London 1896)
Vasil’ev, Aleksandr Aleksandrovich: *History of the Byzantine Empire 324-1452* (Madison WI 1952)
Velikovsky, Immanuel: “A rejoinder to Burgstahler and Angino,” *Yale Scientific Magazine*, vol. XLI, no. 7 (April, 1967)
Verhovek, Sam Howe: *Jet Age: The Comet, the 707, and the Race to Shrink the World* (NY 1020)
*Vistas in Astronomy*, vol. 41, no. 1 (1997)
Volk, Katharine: *Manilius and His Intellectual Background* (Oxford 2009)
Vörös, Gyözö: *Egyptian Temple Architecture: 100 years of Hungarian Excavations in Egypt* (Budapest, Hungary 2007)
*Vorzeit-Frühzeit-Gegenwart* (Gräfelfingen Germany, April 1999)
Wacher, J.S., ed.: *The civitas capitals of Roman Britain: papers given at a conference held at the University of Leicester, 13-15 December 1963* (Leicester UK 1966)
Wade, Nicholas, ed.: *The New York Times Book of Archaeology* (Guilford CT 2001)
Wade, Nicholas, ed.: *The New York Times Book of Archaeology* (Guilford CT 2001)
Wailes, Bernard, ed.: *Craft Specialization and Social Evolution: In Memory of V. Gordon Childe* (Philadelphia PA, 1996)
Wallis, Faith: *Bede, The Reckoning of Time* (Liverpool 1999)
Wells, Peter: *Barbarians to Angels: The Dark Ages Reconsidered* (NY 2009)
Wesley Johnston, Climate of Ireland (Internet)
   http://www.wesleyjohnston.com/users/ireland/geography/climate.html
West, John Anthony: *The Traveler’s Key to Ancient Egypt* (Wheaton IL 1985)
Weston Joyce, Patrick: *A Social History of Ancient Ireland*, vol. I (London 1903), p. 515n.a.;
Whittle, A.W.R.: *Problems in Neolithic Archaeology* (Cambridge UK 1988)
Wigelsworth, Jeffrey R.: *Science and Technology in Medieval Europe* (Westport CT 2006)
Williams, John H.: *The Archaeology of Kent to AD 800* (Woodbridge UK 2007)
Willis, Christopher: *Yellow Fever, Black Goddess: The Coevolution of People and Plagues*, (Cambridge UK 1997)
Wills, Josephine and Robinson, Ian: *Bond for Life: Emotions Shared by People and Their Pets* (Minocqua WI 2000)
Wilson, Colin: *From Atlantis to the Sphinx* (Boston MA 2004)
Wiltshire Archaeological and Natural History Magazine, *The* (London 1869)
Yale Scientific Magazine, vol. XLI, no. 7 (April, 1967)
York, Derek: *In Search of Lost Time* (Bristol UK 1997)
Yorke, Barbara: *Wessex in the Early Middle Ages* (Leicester UK 1995)
Young, John K.: *Sacred Sites of the Knights Templar: The Ancient Secrets Hidden in Stonehenge, Rennes-le-Château and Santiago de Compostela*, (Gloucester MA 2003),
Zahn, Jean-Paul and Stavinschi, Magda: *Advances in Solar Research at Eclipses from Ground and from Space* (Dordrecht, the Netherlands 2000)
Zink, David D.: *The Ancient Stones Speak: A Journey to the Worlds Most Mysterious Megalithic Sites* (NY 1979)
Vol. IV of Pillars of the Past: Stonehenge and the Megalithic World examines the chronology of this period. Archaeologists maintain that these monuments, the pottery, graves, metal and stone tools, skeletons, etc., associated with them only make sense if we place these in the Late Neolithic Age, ca. 4000–1500 B.C. However, there is a wealth of evidence that denies this placement and instead points to the Megalithic Age falling in the post-Roman era/the Early Middle Ages ca. A.D. 600–900. All dating methods used to date the megalithic world have failed and this is admitted repeatedly by modern researchers. The astronomical alignments employed to date these monuments that were used to criticize Velikovsky’s chronological thesis have now fallen into ruins under the scrutiny of these authorities. The only non-controversial alignment that exists at Stonehenge apparently fits there between 500–700 A.D. Employing dozens of technological forms of evidence such as burning of peat to obtain red ash, styles and production of pottery, climatology, agronomy, animal husbandry, agricultural methods such as fertilization, plowing heavy earth, etc., architectural forms and artifacts, toilet and sewage drainage, the action of earthworms and much, much more, the evidence that places Stonehenge and the Megalithic Age in post-Roman times is overwhelming. Hard evidence from sites across the Afro-Eurasian landmass dated to the Neolithic Age all show that they are post-Roman entities. The cause of the fall of the great technologically advanced Roman world into the abject poverty of the early Middle Ages/Megalithic Age is fully explained. Those chronographers who have claimed this period does not exist, such as Heribert Illig, Gunnar Heinsohn, Anatoly Fomenko, Emmet Sweeney, are challenged to answer this evidence. Mike Baillie’s and Laurence Dixon’s theses are also examined and they are challenged to show that the forensic historical evidence presented in this volume does not match their chronological concepts. Volume IV completes this examination of chronology and is must reading for those interested in what forensic history tells us about chronology