

# **PILLARS OF THE PAST**

**Volume II**  
**By Charles Ginenthal**

## **MESOPOTAMIAN, ANATOLIAN, MYCENAEAN, MINOAN, AND HARAPPAN CHRONOLOGY**

**With an Appendix**  
**By Lynn E. Rose**



# THE VELIKOVSKIAN

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

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## PREFACE: FORENSIC HISTORY

“And when he reaches early adolescence he [the historian] must become possessed with an ardent love for the truth, like one inspired, neither day nor night may he cease to urge and strain himself in order to learn thoroughly all that has been said by the most illustrious of the Ancients. And when he has learnt this, then for a prolonged period he must test and prove it, observe what part is in agreement, and what in disagreement with obvious facts; thus he will choose this and turn away from that. To such a person my hope has been that my treatise would prove of the very greatest assistance. Still, such people may be expected to be quite few in number, while, as for the others, this book will be as superfluous to them as a tale told to an ass.”

Galen, quoted in Otto Neugebauer,  
*The Exact Sciences in Antiquity*  
(Providence RI 1957), p. XV

In volume I of this series the scientific and technological evidence related to the broad chronological revisions of Gunnar Heinsohn, Lynn E. Rose, and Emmet J. Sweeney was presented in 2003. Why then the necessity for further volumes on this subject? That volume was quite broad but lacked specificity with respect to the finer points of the revisionists. While I did connect certain civilizations with one another to be placed in the first millennium B.C., I did not connect the forensic history to certain of these empires as I believe is required.

Forensic historical analysis unravels the chronology from the scientific and technological evidence and is the basis for any determination of what transpired in the ancient world, and when. The aim of this second volume is to trace the forensic evidence regarding the chronology of the Persians and their *alter egos* (in Heinsohn’s thesis) as these pertain to the Old Babylonian Amorites in Babylonia and the Neo-Assyrians and Neo-Babylonians in Assyria and Babylonia, etc. Forensic history is very much like the forensic evidence uncovered at the scene of a crime.

That evidence overrides the eye witness accounts and other less stringent forms of evidence recovered at a crime scene.

For the purposes of history the most important evidence for recovering the chronology and history is scientific evidence, such as astronomy, geology, etc., followed by technological developments of the ancients, followed by archaeology and linguistics, followed by historical documents. This in no way denigrates these less stringent forms of evidence. Since these forms cannot be tested or falsified as can the evidence of science, they cannot be on a par with science. Rather, these other forms follow and must be in agreement with science. An accepted historical fact or truth is falsified if tested by science and found false in its light. With respect to eye-witness or documentary evidence, Herbert Butterfield remarked:

“We all know the limitations of those who speak as eye-witnesses or give evidence in court, or write personal memoirs. Men cannot see properly—they catch a glimpse of a half thing and piece out the rest in their imagination. They cannot even remember properly—they reconstruct when they think they are remembering—their later reminiscences of the past will be distorted by things that have happened in the meantime. And all this can happen unconsciously before any question arises of a deliberate desire to prevaricate or a determination to mislead the world.”<sup>1</sup>

On the other hand, Jean Bottéro argues for the precision of the ancient written records as described by Roger Matthews:

“Jean Bottéro ... likes to contrast the ‘hazy and uncertain outline of the past’ visible to archaeologists and prehistorians with the ‘precise, detailed, and analytical’ knowledge of the past attainable through written documents ... the ... ‘speechless and intellectually vague’ world of archaeology with the ‘irreplaceable eloquence’ of philology..., and the ‘often ambiguous and uncertain’ answers of archaeology, ‘absolutely unsuitable for ever responding to the great essential questions...’, with the pellucid reality of written documents,

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<sup>1</sup> Herbert Butterfield, *The Origin of History* (NY 1981), p. 186



[which are] the surest, most complete, the most indispensable sources for our rediscovery of the past'.<sup>2</sup>

The great problem is the viewpoint or filter that a historian brings to any interpretation of historical evidence. He sees the world from an educated but nonetheless particular angle. That viewpoint, in large measure, determines what the history must be. Thus, regarding a particular era in history, there will be several historical theories to explain it, representing various schools or historians who write of it. In this respect there is a psychological and sociological interaction between the evidence and the way the historian sees it. In his great novel, *Lord Jim*, Joseph Conrad attempts to understand the actions and character of his protagonist through the eyes of several witnesses. In spite of each witness's interaction with Lord Jim, we do not receive a single interpretation. Each had a somewhat different story to tell which contradicted aspects of those of the others.

To further complicate the problem of the reliability of the ancient accounts of the past, the recollections suffer other major problems of reliability. Roger Matthews succinctly captures the essence of these as late as 2003:

“It is one of the mild yet frustrating ironies of Mesopotamian archaeology that so much of the known textual material comes from unprovenanced or inadequately provenanced sources, and is therefore of restricted value, while from highly controlled excavations in modern times the finds of textual materials have been sparse or from less informative secondary contexts.”<sup>3</sup>

What Matthews has shown is that the overwhelming number of documents, upon which the historians have erected their chronological edifice of these ancient civilizations, comes not directly from these as primary sources, but rather from other cultures and regions writing about them, being secondary sources. Hence *these* written materials were created by foreign scribes who did not experience that culture at first hand, but indirectly, and therefore presented only what they thought they understood. Much of this may very well have been erroneous, or biased, or fraudulent. Furthermore, many of these documents were not actually

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<sup>2</sup> Roger Matthews, *The Archaeology of Mesopotamia, Themes and Approaches* (London/NY 2003), p. 59

<sup>3</sup> *Ibid.*, p. 61

written at the time these civilizations and their various kings flourished but in later, distant times.

In these cases we have memories subject to all the problems discussed by Butterfield. With a vast number of documents coming from secondary historical sources there should be great reluctance to rely almost entirely on them. In courts of law such secondary sources are called “hearsay” and are not even permitted to be used in evidence. Yet historians to an exceptional degree rely upon these. On this problem, Herbert J. Muller writes:

“The most objective history conceivable is still a selection and an interpretation [of these written materials] necessarily governed by some special interests and based on some particular beliefs ...”  
[because]

“... the great laws of history ... are always arbitrary, man-made laws that man can break as he cannot break the laws of gravity.”<sup>4</sup>

Responding to this viewpoint that documents are the *sine qua non* for delineating the past, Matthews answers:

“I contend that every ‘frank and distinctive’ statement made by [Jean] Bottéro about the Mesopotamian past based on his reading of written documents is itself open to reinterpretation, expansion, revision, and even refutation by any other epigraphist who might approach those same documents and/or by the recovery of new evidence in the years ahead. The suggestion is not that there is no solid ground of reality contained in the message of the texts, only that every so-called ‘fact’ of the past whether drawn from an object or a document is only a contextualized meaning read into it by the scholar who has chosen, usually implicitly, to focus on a limited number of aspects of the total available pool of evidence.”<sup>5</sup>

And that is precisely the point; while historians can interpret documents to say what they want them to say, even going to the extreme of claiming that what

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<sup>4</sup> Herbert J. Muller, *The Uses of the Past* (NY 1952), p. 444 and p. 46

<sup>5</sup> Matthews, *op.cit.*, p. 60

was written is scribal error, the forensic historian may go only where the science leads. The forensic historian cannot arbitrarily claim that the evidence of science may be dispensed with when evaluating historical chronology.

What, then, of the primary documentary sources on monuments or cuneiform texts? How much trust can be placed in them, that is, from *in situ* eyewitnesses to history? Much of this material is the record of various kings, governors, and their underlings whose entire aim was to appear important. They wanted the record they left to the future and to their contemporaries to contain no words hinting that they were weak, dishonest, murderous, unjust, or even that they lost a battle. Such evidence they excluded from their records and monuments. As Joyce Tyldesley states of Egyptian kings, which also applies to the monarchs of Mesopotamia:

“Such was the power of the written word that by excluding all mention of a specific deed from a text the deed itself could be understood not to have occurred.”<sup>6</sup>

Thus we can be quite certain that many actual events were never left to posterity. To this we must add that various kings who hated former monarchs often destroyed their records or even attributed to themselves the heroic deeds of their forebears. Many of the records are duplicitous and include outright lies that cannot ever be detected. As Tyldesley adds:

“We should never lose sight of the fact that the written record is incomplete, randomly selected, and carries its own biases. The monumental inscriptions, for example, are a mixture of religious and propaganda texts which tell the story that the king him- or herself wished to convey, and which cannot be taken as literal truth. The translators of these inscriptions are faced with problems not just of accuracy but of interpretation; even the most scrupulous of scholars is aware that he or she is likely to read a text through the lens of personal feelings.”<sup>7</sup>

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<sup>6</sup> Joyce Tyldesley, *Hatshepsut the Female Pharaoh* (London 1996), p. 9

<sup>7</sup> *Ibid.*, p. 12

Here, also, we sometimes discover that these primary sources are contradictory. H. Gasche, *et al.*, readily admit that “in rare cases where multiple written exemplars of a native chronological tradition actually exist these exhibit discrepancies [among one another].”<sup>8</sup> What is required on the part of the historian or chronographer is objectivity; that is the vital question: How can one be objective with the contents of an ancient written document or monument? Without some unbiased, neutral testing or falsifying device there can be no objectivity. M. van de Mieroop offers the following about historical objectivity:

“... the scholar’s own historical condition determines the account that is being written, ... objectivity is an elusive ideal, and ... the questions asked and models and interpretative frameworks employed [by the historians] are determined by the scholar’s contemporary concerns rather than by the sources investigated.”<sup>9</sup>

I. Morris adds with respect to archaeological as well as historical contexts that “both categories of evidence are generated by actors manipulating shared but contested cultural expectations in the process of their lives [and work].”<sup>10</sup>

There is no scientific or rigorous way by which to test these documents or falsify them. The historical judgments—really interpretations—of them must be based on the belief that the documents are true or that when they are false, this can be discovered. This reliance on them hardly merits the depth and weight of consideration that historians place on these records. As W.H. Auden expressed it in his poem “Archaeology”, archaeology which characterizes such historical interpretations will often bring out “the criminal in us.”<sup>11</sup>

This is particularly true with regard to the dig carried out at Tell Munbaqa in Syria, discussed in volume I of this series. Historians made the claim that there was a 700- to 800-year settlement gap between the Old Akkadians, *ca.* 2300 B.C., and the Mitanni, *ca.* 1500 B.C. At digs carried out in 1988 and 1989 they discovered to their dismay that 700 to 800 years of history had been invented because

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<sup>8</sup> H. Gasche, *et al.*, *Dating the Fall of Babylonia* (Chicago 1998), pp. 5-6

<sup>9</sup> Marc van de Mieroop, *Cuneiform Texts and the Writing of History* (London 1999), pp. 8-9

<sup>10</sup> I. Morris, “Archaeology as Cultural History”, *Archaeological Review from Cambridge*, vol. 14, no. 1 (1997), pp. 8-9

<sup>11</sup> W.H. Auden, *Collected Poems*, E. Mendelson ed. (London 1976), p. 663

there was no settlement gap between these civilizations. This was scientifically proven by geological as well as by archaeological evidence.<sup>12</sup> Rather than admit this colossal contradiction to the established chronology, the historians are engaged in a scholarly-criminal-historical conspiracy to suppress and thereby deny the evidence they themselves have uncovered.

With respect to the monumental sources the very same considerations as with the excavations carried out at Tell Munbaqa pertain. Manfred Bietak demands in terms of these monumental inscriptions that the

“... ideal case [for dating] is an architectural object *in situ* as a doorway or a stele, mentioning the name of a king or well-known person whose place in history has already been established.”<sup>13</sup>

While historians demand this “ideal case”, when they find such a stele “*in situ*” with “a king or a well-known person whose place in history has already been established” but with a statement on it which completely contradicts their chronological edifice, they simply reject this “ideal” evidence. As reported by Colin Wilson:

“One of the major discoveries of Auguste Mariette—the first great ‘conservationist’ among nineteenth-century archaeologists—was a limestone stela he uncovered ... in the mid-1850’s. The inscription declares that it was erected by the Pharaoh Cheops, to commemorate his repairs to the Temple of Isis. It became known as the Inventory Stela, and would certainly be regarded as one of the most important of all Egyptian records ... if it were not for one drawback: its hieroglyphics clearly dated it from around 1000 B.C., about 1500 years after Cheops.

“Now scholars would not normally question the authenticity of a record merely because of its late date, for, after all, the stela was obviously *copied* from something dating much earlier. Another valuable record of early kings is contained on a block of basalt known

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<sup>12</sup> Charles Ginenthal, *Pillars of the Past*, vol. I (Forest Hills, NY 2003) pp. 272-289

<sup>13</sup> Manfred Bietak, “Problems of Middle Bronze Age Chronology: New Evidence from Egypt,” *American Journal of Archaeology*, vol. 80, no. 4 (1984), p. 474

as the Palermo Stone (because it has been kept in Palermo [Italy] since 1877). This contains a list of kings from the 1st to the 5th Dynasties..., and is known to date from about 700 B.C., when it was copied from some original list. But the fact that this is 1500 years later than the last king it mentions causes Egyptologists no embarrassment, for they take it for granted that it is an *accurate* copy of the original. Indeed, why should it not be accurate? Scribes copying in stone are more likely to be accurate than scribes writing with a pen.

“Then why are they suspicious about Cheops’ Inventory Stela—to the extent of denouncing it as an invention, a piece of fiction? Because its ‘facts’ sound too preposterous to be true. Referring to Cheops, it says ‘he found the house (temple) of Isis, mistress of the pyramid, beside the house of the Sphinx, north-west of the house of Osiris.’

“The [chronological] implications are staggering. Cheops found the Temple of Isis, ‘mistress of the pyramid,’ beside the Temple of the Sphinx. In other words, both the Sphinx and *a* pyramid were already there on the Giza plateau at least a century or so before Cheops.

“This is all very puzzling. If Isis is the ‘mistress of the pyramid,’ then presumably one of the Giza group must be her pyramid. Which? Cheops also mentions that he built *his* pyramid beside the Temple of Isis and that he also built a pyramid for the Princess Henutsen. Now we know that Henutsen’s pyramid is one of the three small pyramids that stand close to the Great Pyramid. It is therefore just conceivable that one of its sister pyramids is the pyramid of Cheops.

“In any case, what it amounts to is that we do not know for certain that the Great Pyramid was built by Cheops. It may have been, but on the other hand it may not have been...

“Meanwhile, one thing seems clear: that according to the Inventory Stela, the Sphinx was already there in the time of Cheops and

so was a 'Pyramid of Isis.' It is hardly surprising that Egyptologists are anxious to consider the stela an 'invention.'<sup>14</sup>

Thus it is clear that when written documents, such as the Inventory Stela, contradict the established chronology, historians label it a fraud, and by being so labeled, in their eyes it becomes a fraud.

How reliable is the testimony of archaeological evidence? Leonard R. Palmer, citing F.T. Wainwright's *Archaeology, Place Names and History*, p. 97, states:

"After reminding archaeologists that their work inevitably involves the destruction of evidence he goes on to consider the nature of excavation reports. He insists that these are not archaeological evidence at all. They have to be treated as historical documents. 'If they seem to have the character of historical evidence, that is not surprising, for they are [still] historical evidence though of a rather specialized kind. They are not archaeological evidence at all. They pose the same questions of trustworthiness as other historical sources.... An author's state of mind is involved and therefore the reliability of all these written [documentary] accounts is open to debate and suspicion in a way that archaeological evidence proper can never be. Questions of honesty and competence arise.... One archaeologist has said in print that he keeps a mental list of excavations on whose reports he can rely, and everyone who tries to use excavation reports must do the same. The same questions of reliability must be raised against all excavation reports whether they come from contemporaries or from an earlier generation. One must apply to them the same tests as a historian applies to his sources, using whatever internal or external checks present themselves, taking into account other work by the same author, and even bearing in mind that an obsession or blind spot may or may not colour the whole report?'"<sup>15</sup>

The most basic neutral test of an archaeological report is: WHAT DOES THE SCIENCE AND TECHNOLOGY SAY? In this volume this will be the

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<sup>14</sup> Colin Wilson, *From Atlantis to the Sphinx* (NY 1996), pp. 53-54

<sup>15</sup> Leonard R. Palmer, *Mycenaeans and Minoans* (NY 1965), pp. 288-289

final arbiter of archaeological reports. If science and technology contradict an archaeological interpretation, that interpretation is invalid and must be made to conform to the science and technology, not the other way round. *Scientia vincit omnia!* To suggest otherwise is to create “historical fiction” and “archaeological unreality”.

Like any other form of organized knowledge, the established chronology has become so entrenched that it is inconceivable to historians that what they have so carefully and meticulously put together as the history and chronology of the ancient world is simply wrong. Their incapacity to deal with evidence that contradicts that paradigm is a symptom of dogmatic thought. In this regard the popular historian Will Durant explained:

“Ultimately our troubles [regarding historical truth] are due to dogma and deduction; we find no new truth because we take some venerable but questionable proposition as the indubitable starting point, and never think of putting this assumption itself to a test of observation or experiment.”<sup>16</sup>

R.G. Bednarik’s well-reasoned remarks on chronology encapsulate the concept of forensic history to be presented in this volume. In altered and paraphrased form,

“Direct dating of the chronology of the ancient world is contingent on two prerequisites; [first] the physical scientific relationship of the history and the dating criterion for it must be direct and indisputable, and second, the propositions concerning the chronological relationship of the history and the dating criterion should be falsifiable and testable. Historians experience difficulties with the first of these requirements, apply scissors and paste interpretations and analyses to paper over their inability to do this. Archaeologists experience difficulties with the second of these requirements, by applying traditional modeling dynamics of archaeological

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<sup>16</sup> Will Durant, in *The New Dictionary of Thought* (1961), p. 690



interpretation to direct dating information. In both instances we have interpretation rather than testing and falsification of the data.”<sup>17</sup>

In *Pillars of the Past*, vol. I, analyses of several forms of testable and falsifiable dating evidence, such as astronomy, geology, agronomy, metallurgy, etc., were presented, which indicated rather directly that the established chronology was thoroughly and repeatedly contradicted.

“False or non-existent data had been used by the historians to validate their arrangement of the history. By ignoring or misrepresenting well established facts they pieced together a chronology that seemed to fit their expectations, and these were published in mainstream historical journals. The contradictions were suppressed because these did not fit.... This illustrates how data is filtered by historical and archaeological interpretation and how inappropriate it is to accept and repeat them uncritically.”<sup>18</sup>

“One of the basic requirements of science is the replicability of experiments. A dating analysis based on historical documentation or archaeological analysis is not an experiment since it cannot be effectively repeated, tested and falsified; thus the requirement of replicability is not satisfactorily met.”<sup>19</sup>

“When we compromise refutability by the use of scissors and paste historical interpretation and archaeological traditional modeling dynamics, the true relationship between a ‘date’ for a king, nation, city, battle, work of art, etc., cannot be established. In preferring the use of these methodologies to science we sacrifice precision to obtain what appears to be reliable.”<sup>20</sup>

Leonard W. King long ago reported what the most important forms of historical evidence are:

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<sup>17</sup> R.G. Bednarik, Review article “Only Time Will Tell...”, *Archaeometry*, vol. 38, no. 1 (1996), p. 1

<sup>18</sup> *Ibid.*, p. 9

<sup>19</sup> *Ibid.*, p. 10

<sup>20</sup> *Ibid.*

“The most important of our sources consists of the contemporary inscriptions of the early kings themselves, which have been recovered upon the sites of the ancient cities... [or elsewhere].”<sup>21</sup>

“The second class of material, which is of even greater importance for settling the chronology ... comprises the chronological documents drawn up by scribes, who incorporated ... the history of their own time and that of their predecessors.”<sup>22</sup>

“The third class of material for settling the chronology has been found in the external evidence afforded by the ... historical and votive inscriptions..., and by tablets of accounts, deeds of sale, and numerous documents of a commercial and agricultural character.”<sup>23</sup>

In the final analysis what has been passed down to us as a true chronology employing interpretation analyses is neither precise nor reliable. Ambrose Bierce in his *Devil's Dictionary* coined a word that describes historical chronology that is both “incompatible with scientific and technological evidence, and impossible” in the light of these: “Impossible: two things are impossible when the world of being has scope enough for one of them, but not for both.”

In the following chapters of this volume it will be shown that the established chronology for the first and second millennia of the ancient Near East is “impossible” because the scientific and technological as well as other related evidence indicates that the world only has scope for the short chronology that is based on these methodologies but not for the long chronology, which is not. I recall that Lee Benson once said, in short: “that generations of historians have resorted to what might be called ‘proof by haphazard quotation’ does not make the procedure valid or reliable, it only makes it traditional.”

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<sup>21</sup> Leonard W. King, *A History of Sumer and Akkad* (NY 1910), reprint, p. 56

<sup>22</sup> Ibid.

<sup>23</sup> Ibid., p. 59

## CHAPTER 1: PERSIANS AND OLD BABYLONIANS

“The study of ancient Mesopotamia can move forward rapidly when archaeologists, Assyriologists and natural scientists cooperate. Lateral thinking and a desire to solve [chronological] problems and explore domains for which no ready synthesis exists can lead to an entirely new brand of study in which archaeology, philology and the natural sciences combine forces to elucidate problems which no one of these disciplines can, in isolation, ever resolve.”

D.T. Potts,  
*Mesopotamian Civilization*  
*The Material Foundations*  
(Ithaca NY 1977), p. 307

“Those who advocate any other system of chronology must examine multiple [forms of] evidence.”

Sidney Smith,  
*Alalakh and Chronology*  
(London 1940), p. 47

## FIRST THINGS FIRST!

Before one can claim to have an accurate chronology of historical events there must be a scientifically rigorous anchor that generates an absolute date to which the chronology prior to it and following it can be arranged. As Carl Olof Jonsson explains, documents by themselves do:

“... not show how... chronological datings may be connected with our own era. A chronology that is in this sense ‘hanging in the air’ is simply the type of chronology called a *relative chronology*. Only if the [documentary] information supplied us with the exact distance from the time of [the event] up to our own era—either by the aid of a complete and coherent line of lengths of reign, or by detailed and dated astronomical observations—we would have had an *absolute chronology*, that is, a chronology that gives us the exact distance from the [event’s time] to our own time.”<sup>1</sup>

This absolute or scientific date permits the historian to arrange the historical data so that the sequence of dates—dynasties, kings, battles, etc.—tells a valid history of events and not a fictional one. As Mark Twain stated, “Fiction is obliged to stick to possibilities. Truth isn’t.” Scientific chronological truth must stick to facts.

It is thus essential in beginning the work before us that a scientific anchor must come first, and from that absolute anchor the chronology and history of the ancient Near East can begin to be assembled. Egyptian chronology was the anchor for the established chronology, based on astronomical, Sothic dating, and, as we will see, it is the only anchor that exists for the short revisionist chronology as well, which was delineated by Lynn E. Rose.<sup>2</sup>

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<sup>1</sup> Carl Olof Jonsson, *The Gentile Times Reconsidered*, 4th ed., revised and expanded (Atlanta GA 2004), p. 74

<sup>2</sup> Lynn E. Rose, “A Lowered Chronology for the Twelfth Dynasty,” *The Velikovskian*, vol. II, no. 4 (1994), pp. 46-53; and Lynn E. Rose, “From Calendars to Chronology,” in *Stephen J. Gould and Immanuel Velikovsky. Essays in the Continuing Velikovsky Affair*, Dale Ann Pearlman, ed., (Forest Hills NY 1996), pp. 663-726; see also Charles Ginenthal, *Pillars of the Past*, vol. I (Forest Hills NY 2003), pp. 81-117.

In a recent communication Rose further informed me that in addition to having correctly correlated 34 out of 36 lunar festival dates with the heliacal rising of Sirius, that place the 12th Egyptian Dynasty in the mid- to late first millennium B.C., there were found an additional five new lunar dates, bringing the total to 41. Of these, 37 lunar festival dates fit with Rose's chronology. This, as we will learn, is the only absolute scientific anchor that exists and it is from this we will begin the reconstruction of the chronology of the ancient world.

However, the 18th Dynasty is also anchored by Sothic astronomical analysis to the 15th century B.C. Unfortunately for the established chronology, it is misdated because the Ebers Papyrus from which this astronomical data is derived, according to Sir Leonard Woolley, is a calendrical impossibility since it lacks intercalary months. In a lunar calendar, a year of twelve lunar months equals 354 days, eleven and a quarter days short of 365.25. A lunar month must be added about every 2.7 years to keep the seasons in line with the year. But, Woolley tells us:

“it must be admitted that in the Ebers Calendar the months are not of [30 and 29 days'] length as the months should be and as they are in the Illahun papyrus, and there is nowhere any suggestion of the intercalary month which a lunar calendar should contain.”<sup>3</sup>

Sidney Smith shows that even within it the dates are only conjectural:

“The dating of the events [of the 18th Dynasty] ... is dependent upon the exact dates assigned to the reign of Thutmose III. These dates ... depend ... on the record of a rising of Sothis on the 28th day, 3rd month of summer, and later [in the month of] Epiphi, in an unknown year, which must be one of the years 1474/3–1471/0. A new moon fell on the 21st day, 1st month of summer, the later [month of] Pakhon, in the 23rd year, and another on the 30th day, second month of winter, the later [month of] Mekhir, in the 24th year. These new moons, which must necessarily be within an estimated range of years 1474–1470, have been variously identified (1) as those of May 18th –1492 and February 26th –1490 by Lehmann and Ginzel, (2) as those of May 15 – 1478 and February 23 –1476 by Eduard Meyer, (3) as those of May 12

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<sup>3</sup> Jacquetta Hawkes and Sir Leonard Woolley, *Prehistory and the Beginning of Civilization*, vol. I (NY 1963), p. 681

–1467 and February 20th –1465 by L. Borchardt. Borchardt accordingly dated Thutmose III 1490–1436 and Thutmose IV 1423–1415 or 1420–1412. These dates can be dismissed; they are not justified by the Egyptian evidence and are quite impossible when events in Syria are considered.”<sup>4</sup>

Smith goes on to show that the various periods assigned to the 18th Dynasty kings cannot work for various reasons.<sup>5</sup> The astronomical evidence for anchoring the 18th Dynasty is not exact enough to do so. And different archaeologists have come up with series of dates that range over many Ebers years. The chronology derived from the Ebers papyrus leads to dates which are conjectural, not absolute. Thus we have the 12th Egyptian Dynasty solidly, scientifically anchored to the first millennium B.C., but the 18th Dynasty not accurately anchored to the second millennium at all. For now, all that need be said is that the 12th Dynasty is the only rigorous scientific anchor of ancient Egypt to which the chronology of the surrounding civilizations has been connected.

Here are a few comments by authorities regarding astronomical dating:

“Scholars consider this [organizing Egyptian chronology] a difficult task. As Dr. Robert A. Hatch of the University of Florida puts it:

“ ‘The problem is two-fold: 1) there are internal problems of assigning beginnings and endings to various Dynasties, and 2) externally, the problem is reconciling dates in the Egyptian calendar with attested dates in other calendaric systems, for example Greek, Jewish, Assyrian, Persian and Julian/Gregorian.’

“Egyptian chronology is in a constant state of transition, with much of the terminology and dating in dispute. Professor E.J. Bickerman, *Chronology of the Ancient World*, 1980: [pp.] 83-84 and 106, has properly called it ‘the rather fluid chronology of the pharaohs. ...’ Professor Heinrich Otten has noted it is a ‘rubber chronology’ that

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<sup>4</sup> Sidney Smith, *op.cit.*, pp. 41-42

<sup>5</sup> *ibid.*, p. 43 ff

you can stretch or shrink anywhere, by arbitrarily established lengths of co-regencies between rulers and even overlapping dynasties ...”  
 “firm” dates cannot be supported astronomically. ...”

“According to John Brug, *the astronomical dating of ancient history before 700 BCE* ... rests on a host of ... assumptions. ‘There is a surprising amount of uncertainty and conjecture in the data ... which form the basis for the ... chronology of the Ancient Near East. We run a very real danger of debating about millimeters and centimeters when we should rather [be using] measurements of ... meters, ... and perhaps even the centuries are in doubt,’ he adds.”<sup>6</sup>

The chronology of the Persian Empire is also, seemingly, well established and no one except perhaps Fomenko and (recently) Heinsohn questions its place in the history of the Middle East. Heinsohn maintains that the Old Babylonians, also known as the First Babylonians, as well as the Amorites (Amurru) and also the Martu were not separate nations that ruled over Babylonia from around 2000 to 1600 B.C., but are, in reality, the Persian rulers of Babylonia from around 600 to 331 B.C. when they dominated this region. The space between their chronological placements is profound.

The reason why this author has begun with this equation of Heinsohn’s is that we have a solid anchor—Rose’s 12th Dynasty date—from which to reconstruct this chronology; first things first! In addition, we also have a second anchor, namely Alexander the Great who conquered the Persian Empire and, in terms of Rose’s Sothic dating, also conquered the 12th Dynasty prior to his Persian conquest. This directly connects the Persians to the 12th Dynasty, but more significantly for our purposes, the 12th Dynasty has always been attached in time by historians to the Old Babylonians. In so doing, I am merely following Rose’s suggestion:

“The Velikovsky movement contains many who will be able to *build* upon this redating of the Middle Kingdom. They will construct edifices that I cannot even imagine. They will prove theorems tomorrow that I would never have thought of ... let alone accepted. They will sift through the masses of historical data that I have always

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<sup>6</sup> *Egyptian Chronology BCE*, Art History Search, Internet, p. 1

found unsiftable, and [in time] they will put everything in its proper place.

“They will destroy fundamentals and axioms that I still see as indubitable. This redating of the Middle Kingdom is a powerful weapon, with which they will be able to demolish the conventional chronology—and more. But that demolition will seem almost incidental, in comparison to what they will then be able to *build*.”<sup>7</sup>

In analyzing this material some of Ev Cochrane’s published criticisms of Heinsohn’s thesis will be evaluated. He did write a somewhat lengthy critique of Heinsohn’s equation that the Old Babylonians were really the Persian rulers of Babylonia. I shall do so not mainly on Heinsohn’s and Cochrane’s historical evidence, but rather from scientific, technological, linguistic, and other grounds. Since Cochrane has presented what I conceive to be several forms of scissors-and-paste evidence, it is only appropriate that these be evaluated in the light of forensic historical evidence. Cochrane’s material will therefore be dealt with *in extenso*.

## DATING THE OLD BABYLONIAN AMORITES

“Historians must ... develop critical tests not merely for their [historical and chronological] interpretations but also for their methods of arriving at them.”

David Hackett Fischer  
*Historians’ Fallacies*  
(NY 1970), p. XIX

The scientific method by which the Old Babylonians were dated to the early part of the second millennium B.C., and not to Persian times, was based on astronomy and in particular on the Venus tablets of an Old Babylonian king named Ammisaduqa. This was taken to be the absolute anchor of Mesopotamia in the second millennium B.C. to which it was fastened. Since this placement aligned itself

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<sup>7</sup> Lynn E. Rose, in *Stephen J. Gould and Immanuel Velikovsky, op.cit.*, pp. 718-719



with that of the 12th Egyptian Dynasty, also in the early part of the second millennium B.C., it was seen as a double anchor point. However, since the 12th Dynasty is scientifically now anchored in the first millennium B.C., this connection is broken. The question is: Does the Mesopotamian Old Babylonian anchor hold? As Velikovsky explains:

“In the library of Assurbanipal in Nineveh were stored astronomical books of his and of previous ages; in the ruins of the library Sir Henry Layard found the Venus tablets.

“There arose the question: From what period do the observations of these tablets date? Schiaparelli investigated this problem and ‘as an example of method his work is excellent.’ He decided that ‘the inquiry could be limited to the seventh and eighth centuries [B.C.]’.

“The year-formula of an early king, Ammizaduga, was discovered on one of the tablets, and since then the tablets are usually ascribed to the first Babylonian dynasty ... in the beginning of the second millennium [B.C.]”.<sup>8</sup>

This was greatly elucidated by Lynn E. Rose and Raymond C. Vaughan:

“[The] use of the Venus tablets as a means of dating the reign of Ammisaduqa [and the Old Babylonian Empire] is generally seen as the only exact basis for the second millennium [B.C.] chronology of the entire Middle East [especially Mesopotamia]. For once Ammisaduqa is dated, and the First [or Old] Babylonian Dynasty with him, the chronology of that entire region of the world is supposedly placed on a firm [scientific] footing.”<sup>9</sup>

But are these astronomical tablets credible evidence for placing the Old Babylonians in the second, not the first millennium B.C.? Rose and Vaughan go on

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<sup>8</sup> Immanuel Velikovsky, *Worlds in Collision* (NY 1950), pp. 195-196

<sup>9</sup> Lynn E. Rose, Raymond C. Vaughan, “The Venus Tablets: A Fresh Approach?”, *Kronos*, vol. X, no. 2 (Winter 1985), p. 1

to explain what is wrong with the methodology the historians have employed to fit these observations into that early period:

“For more than a century now it has been the almost-universal practice [of archaeo-astronomers] to work only with censored and astronomically edited ‘data’ [for these tablets]. Which observations are deleted or edited will vary slightly from author to author.”<sup>10</sup>

They then describe several forms of editing or, more accurately, culling the data in the tablets to fit the chronological presuppositions:

“When the observations are in one way or another purged of whatever will conflict with retrocalculation [so the data are forced to fit the present state of the Solar System and the dating of the Old Babylonian Amorites to the early second millennium by this procedure], what can be learned from the relationship between retrocalculation and the observations that remain? We suggest that the only rational answer is, Nothing[, since the] actual *data* [in the tablets] play[s] too small a role for any meaningful results to emerge.

“The case for attributing the Venus tablets to the [second millennium] reign of Ammisaduqa has been severely eroded over the years, though this erosion is usually not appreciated or mentioned.”<sup>11</sup>

The late astronomer G.O. Abell is one who argued that Peter Huber, a statistician who changed at least 30 percent of the data in the tablets, making them congruent with the present configuration of the Solar System in order to disprove any possibility that the chronology of the ancient Near East could be greatly shortened, claimed that “Huber’s arguments alone are sufficient to completely rule out the Velikovsky view” that the history of the ancient world could be shortened by even 500 to 800 years.<sup>12</sup> Huber and Abell are not alone in making the claim that the Venus tablets prove the Old Babylonians are solidly anchored in the second, not the first millennium B.C.:

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<sup>10</sup> *ibid.*, p. 3

<sup>11</sup> *ibid.*, p. 5

<sup>12</sup> *Kronos*, vol. V, no. 4 (Summer 1980), p. 54

“A long list of people have so reported [this] ... Payne-Gaposchkin, Kaempffert, Edmondson, de Camp, Stephens, van der Waerden, and now Huber. ...”<sup>13</sup>

Based solely on consensus, such a long list of scientists, mathematicians, and science writers would seem to indicate that the Venus tablets indeed properly place the Old Babylonians in the second millennium and that, therefore, Heinsohn, Sweeney, and Rose are greatly mistaken to suggest otherwise.

Nevertheless, Otto Neugebauer originally maintained that because the Venus tablets “are given in the contemporary lunar calendar, these documents have become an important element for the determination of the chronology of the Hammurapi [Old Babylonian] period. ...”<sup>14</sup> This was in 1957. Then in 1983 he claimed:

“From the Old Babylonian period only one isolated text is preserved which contains omina ... from the later astrology. Predictions derived from observations of Venus made during the reign of Ammisaduqa (*ca.* 1600 B.C.) are preserved only in copies written almost a thousand years later and clearly [were] subjected to several changes during this long time. *We are thus again left in the dark as to the actual date of the composition of these documents.*”<sup>15</sup> [emphasis added]

This statement by Neugebauer appears to render the view that Huber and all the others who date these tablets to the second millennium B.C. are also in the dark regarding their value for chronology. Rose has clearly shown that this consensus is false. Without rehashing all of Rose’s evidence, I will merely cite what the chronographers now state specifically regarding the Venus tablets for dating the Old Babylonians. Fredrick H. Cryer in discussing “Chronology Issues and Problems” as late as 1995 admits in agreement with Rose and Vaughan:

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<sup>13</sup> Lynn E. Rose, “Just Plainly Wrong: A Critique of Peter Huber,” *Scientists Confront Scientists Who Confront Velikovsky*, *Kronos*, vol. IV, no. 2 (Winter 1978), p. 41

<sup>14</sup> Otto Neugebauer, *The Exact Sciences in Antiquity* (Providence RI 1957), p. 100

<sup>15</sup> Otto Neugebauer, *Astronomy and History Selected Essays* (NY/Berlin/Heidelberg/Tokyo 1983), pp. 56-57

“The entire discussion of this issue is based on a number of undemonstrated assumptions—namely, that it is possible [as Peter Huber and several others have done] by text-critical means to remove false and misleading data that have crept into the text [of the Venus Tablets] during the history of its transmission and that the original text was based on celestial observations made at a single location. ... [Because of these problems] there is no reason to place any great faith in the Venus Tablets for the purpose of reconstructing the chronology of the second millennium [B.C.], as a number of scholars have remarked.”<sup>16</sup>

The repudiation of the statistical work of Huber and those who supported his work such as Owen Gingerich, as well as those who also culled the data in these tablets with the same flawed techniques, could not be more explicit. As Andrew Lang claimed, “He [Huber and the others] uses statistics the way a drunkard uses a lamppost for support, not illumination”<sup>17</sup> or, as Anatole France once said: “If 50 million Frenchmen say a foolish thing, it is still a foolish thing.”<sup>18</sup> So, too, if generations of historians, archaeo-astronomers, and mathematicians maintain a false and, therefore, foolish chronology for the Old Babylonians, based on culled data, it is still false and foolish.

In spite of these rather direct scientific negations of the chronology for the second millennium B.C. in Mesopotamia and Egypt, the general literature gives no indication that the chronology of this period is unanchored, that it is not an absolute chronology. Yet the established chronology is still the accepted paradigm. As Rose explains:

“A historian may consult an astronomer regarding an eclipse or other astronomical event [to pin down the chronology]. The astronomer will calculate possible dates for the event [or series of events]. The historian will then arrange his chronology so as to fit the astronomer’s retrocalculations. Then some time passes, and the chronology becomes orthodox. The [original] grounds for the chronology are forgotten, and

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<sup>16</sup> Frederick H. Cryer, “Chronology Issues and Problems,” in *Civilizations of the Ancient Near East*, vol. I, J.M. Sassoon *et al.*, eds. (NY 1995) p. 658

<sup>17</sup> Andrew Lang in Leo Rosten, *Leo Rosten’s Carnival of Wit* (NY 1996) p. 8

<sup>18</sup> Anatole France in Wm.R. Fix, *The Bone Peddlers* (NY 1948), p. 151

it is *assumed* to rest on solid historical evidence. No one remembers or can find out any more that it rests [as does the Old Babylonian chronology] on astronomical retrocalculations. Then a new generation of astronomers and historians play the game again, this time in reverse direction. The chronology is taken as independently fixed, and the eclipse or other event [or events] is [are] taken as datable on purely historical grounds. Then someone [like Peter Huber] retrocalculates in the same manner as before, but not in order to set up a chronology this time—just to check it. And of course all the pieces fit. Different generations have made the same numerical computations and obtained the same results. ... Nothing about what really happened in antiquity can be shown by such procedures. What is perceived as an independent check is not a check at all, let alone independent.”<sup>19</sup>

Rose adds:

“Each new generation of scholars tends to flatter itself regarding its supposed breakthroughs. But the fact is that very little has fundamentally changed [regarding Old Babylonian chronology] during the past one hundred years in the way scholars treat antiquity: the conventional chronology is still adhered to by the vast majority of today’s authors; and the archaeological, stratigraphical, monumental, and literary evidence against that conventional chronology is swept under the rug today even more carefully than it was two or three generations ago.”<sup>20</sup>

“Whatever the ancients said that conflicts with those [assumed] ‘facts’ must be ‘scribal error’ or otherwise unreliable [as the Inventory Stela of Egypt, discussed above]. Such texts have routinely been ‘corrected’, that is, rewritten, to make them fit modern assumptions. Or else they are ignored.”<sup>21</sup>

Now that the 12th Egyptian Dynasty has been placed well into the first millennium B.C., that placement fully correlates with the chronology of the Persians.

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<sup>19</sup> Rose, “Just Plainly Wrong...”, *op.cit.* pp. 39-40

<sup>20</sup> *ibid.*, p. 34

<sup>21</sup> *ibid.*, p. 42

The heliacal rising of Sirius with 37 lunar dates for this chronology has so many points of astronomical corroboration and validation that it would take many millennia for this astronomical situation to reoccur before it and after it. The uniqueness of this data to that time is absolute. On the other hand: How do we tie the Old Babylonians to that time as well?

Historians have repeatedly claimed that one cannot ignore synchronisms of Egyptian history dovetailing with that of neighboring countries. Thus, if Heinsohn, Rose, and Sweeney are correct in placing the Old Babylonians in the time of the 12th Dynasty, the evidence should also dovetail with it as confirmation and corroboration. And, indeed, such has always been the case according to the established chronology. That is, historians have long held that the 12th Dynasty of Egypt was contemporary with the Old Babylonian Dynasty of Mesopotamia, based on a number of connections both archaeological and historical. This requires that the Old Babylonians were living in Persian times and confirms Heinsohn, Rose, and Sweeney's theses that the Old Babylonians were the Persian rulers over Babylonia.

One connection linking the Old Babylonians directly to the 12th Dynasty was uncovered at Platonas on Crete. There a Hammurabi [Old Babylonian]-type seal was found in a tomb along with Middle Minoan pottery of a type that was discovered at other sites which is definitely from the 12th Dynasty.<sup>22</sup> These relics on Crete were dated by Sidney Smith to the earlier age of Minoan Crete.<sup>23</sup> This evidence is taken as proof that the 12th Dynasty and the Old Babylonian Dynasty were contemporaneous. In *Peoples of the Sea*, Velikovsky commented on this:

“The lawmaker king Hammurabi of the First [Old] Babylonian Dynasty ... has been transferred to about 1700 [B.C.] in order to synchronize the Egyptian Middle Kingdom [of the 12th Dynasty] with the First Dynasty of Babylon, on the basis of material from both places found in a common deposit on Crete.”<sup>24</sup>

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<sup>22</sup> *CAH*, vol. II, pt. 1 (Cambridge, UK 1973), p. 144

<sup>23</sup> Smith, *op.cit.*, p. 58

<sup>24</sup> Immanuel Velikovsky, *Peoples of the Sea* (NY 1977), p. 205

Smith also tells us “The relation of the XIIth [Egyptian] Dynasty in [the] time of the First [or Old] Dynasty of Babylon is particularly clear at Ras Shamra.”<sup>25</sup> On pages 13-16 of *Alalakh and Chronology*, he presents much additional archaeological evidence that links the Old Babylonians directly to the 12th Dynasty.

This of course is the type of evidence that historians have always used (such as pottery, relics, etc.) to link the various civilizations of the ancient world to one another. This form of material generally creates a relative chronology which is not precise in time compared to an astronomical, absolute chronology that *is* precise in time. The absolute chronology that links the Old Babylonians with the 12th Dynasty of Egypt will be presented in the next unit titled “Calendars and Chronology.”

Before getting to that unit, a brief discussion of other forms of scientific and technological evidence to further confirm, corroborate and correlate the dating of the Old Babylonians to the first millennium is in order. If the Old Babylonians are truly the Persian rulers of Babylonia, then their relics, etc., in the strata should be found directly beneath those of the Greeks. This will be discussed in the unit on stratigraphy. If the Old Babylonians are the Persian rulers of Babylonia, they would possess metals and other materials unknown to the early second millennium Old Babylonians but known and available in Persian times. These other forms of forensic historical evidence would then confirm the astronomical data and make the evidence for the equation of Old Babylonians and Persians in Babylonia overwhelming. That is the goal of the rest of Chapter 1. As Fekri Hassan suggests:

“With clearly stated goals and ... accord, a critical attitude towards ... validity of evidence, and the coherence of ... theories to a broad range of fields of archaeological and historical knowledge, we are likely to converge on a plausible reading of the past.”<sup>26</sup>

Along with these elements, scientific and technological evidence is of paramount importance. In fact, without these various elements as evidence we cannot have a plausible, nor a chronologically coherent reading of the past.

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<sup>25</sup> Smith, *op.cit.*, p. 15

<sup>26</sup> Fekri Hassan, “Environmental Perception and Human Response in History and Prehistory,” *The Way the Wind Blows*, R.J. McIntosh *et al.*, eds. (NY 2000), p. 137

## CALENDARS AND CHRONOLOGY

“An excellent proof of the correctness of a chronology is when it is in agreement with the chronologies of *other* contemporary nations, provided that these other chronologies are independently established and there are *synchronisms*, that is, dated connecting links that serve to join the two or more chronologies together at one or more points.

“The reason why it is important that they be independently established is to rule out any attempt to discredit their worth by claiming that the chronology of a certain period in one nation has been established simply by the aid of the chronology of the contemporary period in another nation.”

Carl Olof Jonsson,  
*The Gentile Times Reconsidered*,  
4th ed., revised and expanded  
(Atlanta GA 2004), p. 139

While historical synchronisms between, say, the Old Babylonians and the 12th Dynasty exist as discussed above, these synchronisms are not enough. What is needed, as Jonsson indicated, is that these empires must be linked by an absolute form of evidence to give an absolute chronology with precise links with one another: “an absolute chronology is usually best established by the aid of ancient astronomical observations.”<sup>27</sup>

This brings us to the finer points of Heinsohn’s thesis. He maintains that King Darius I, the Great, of Persia is the same as King Hammurabi of the Old Babylonian Dynasty; and that King Artaxerxes III Ochus of Persia is the same as King Ammisaduqa of the Old Babylonian Dynasty.

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<sup>27</sup> Jonsson, *op.cit.*, p. 153



It should be emphasized that more recently Heinsohn has been inclined to treat these relationships in terms of contemporaneity rather than strict identity. This opens up the possibility that a king of Babylon, like Hammurabi, might have been a vassal king under the king of Persia, in this case Darius. We should note that some of Heinsohn's readers may *not* have abandoned the strict identity view. For purposes of our discussion here, I too shall be speaking of it as an identity.<sup>28</sup>

With regard to calendars and the chronology of the ancient world, it is well known that prior to around 500 B.C. the various rulers of Mesopotamia used a lunar calendar. Because twelve lunar months equal 354 days, an additional or intercalary month had to be added to the year about every 2.7 years. All ancient lunar calendars prior to 500 B.C. are taken to have these intercalary months added to them. Further, because the lunar month is 29.5 days long, the ancients had both 29- and 30-day months.

The question is: Is it probable or even plausible to expect that two sets of Persian/Old Babylonian kings, following each other in the same order of their dynasty but living over one thousand years apart in time and several hundreds of miles apart from each other, should over their reigns just happen to have identical 30-day months in identical years and months of those years? The probability of finding such an exact correlation by chance is quite remote.

It is important to note the significance of the absolute dating of Hammurabi to Mesopotamian chronology. G. Roux states that "the date of Hammurabi is the keystone of the chronology of the second and third millennium B.C."<sup>29</sup> In other words, since many historians now reject the Venus tablets as the keystone for dating the Old Babylonians, another set of astronomical dates must be employed to establish the absolute chronology of that king and those who preceded and followed him.

Before beginning this analysis a digression is in order. This analysis will be presented in the light of Ev Cochrane's critique of Heinsohn's evidence related to the Persian/Old Babylonian and Darius/Hammurabi equations. I wish to inform the reader of the manner by which Cochrane has chosen to deal with me and

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<sup>28</sup> This development has not yet been published; I learned about it in 2007 from email exchanges between Gunnar Heinsohn and Birgit Liesching.

<sup>29</sup> Georges Roux, *Ancient Iraq* (NY 1980), p. 43

my defense of Heinsohn's chronological revision. (Cochrane is the former editor-in-chief and now publisher of the journal *Aeon*.) Upon learning of vol. I of *Pillars of the Past*, he wrote about me on the Kroniatalk group on December 15, 2003, prior to having read my book. Clark Whelton considerately sent me this material wherein Cochrane wrote:

“Am I the only guy who finds it embarrassing that folks are touting Ginenthal's latest book on chronology? I mean looking to Ginenthal for insights on chronology is like looking to Leroy Ellenberger for information on the Saturn theory. The presentation is so biased and so wrong-headed as to be virtually worthless.”

On January 4, 2004, Cochrane further wrote:

“I now plan to offer a detailed article on Ginenthal's foray into chronology, this one every bit as wrongheaded [*sic*] as his *Velikovskian* article arguing that Hammurabi is to be identified with Darius. Rather than make the folks on Kronia wait until the next issue of *Aeon*, however, I'll begin offering brief posts outlining the evidence early next week.”

On December 20, 2003, Cochrane had added:

“However, I am interested to learn what Charles [Ginenthal] will have to say on astronomical dating. Isn't this the same guy who chided me for raising the issue of astronomical retrocalculations in my detailed analysis of the fantastic reconstruction of Heinsohn?”

Notice how Cochrane has employed the same miserable and dishonorable devices that were used against Velikovsky. He attempts to tarnish me through guilt by association; he claims I am no different than Leroy Ellenberger, a wildly enraged critic who stoops to any crude level to discredit those of us in the Velikovsky movement [see *The Velikovskian* vol. IV, no. 4 (1999) pp. 102-114]. My own interaction with Ellenberger, as many others have unfortunately experienced, is perhaps best summed up by Oscar Wilde with this remark attributed to him: “He [Ellenberger] leads his readers to the latrine and [then] locks them in [it].” As for Cochrane's use of Ellenberger as a link with me, this too can be summed up by Wilde

who also said “While with his right hand [Cochrane] dealt with grandiose ideas [of ancient chronology], his left hand let the rat out of the sewer.”

What is also extremely inappropriate and unscholarly on Cochrane’s part is not only the rude *ad hominem* he employs on me, but his going to the length of attacking a book he had at that time not read. As a scholar in the field of Velikovskian studies, he is, presumably, well aware that Velikovsky’s many critics had also not read Velikovsky’s books before raising that same form of hateful comment. Not having read my book, Cochrane says it is “wrongheaded”, “biased,” and “virtually worthless.” This sounds much like Cecilia Payne-Gaposchkin who used quite similar language and the same shrill tone to denounce Velikovsky and his book *Worlds in Collision*, also, like Cochrane, not having read it.

Interestingly, his rude tone and words about me will, I am sure, be kept from the readers of his critique of my book, if it is ever presented. I believe this unseemly side of Cochrane will not see the light of day in *Aeon*. However, more than four years after his promise to write a daily or weekly point by point rebuttal of *Pillars of the Past* I have found nothing from Cochrane or from his associates in touch with the Kronia talk group of his promised critique.

With respect to Cochrane accusing me of “chiding” him “for raising the issue of astronomical retrocalculation,” let him produce such evidence. I am rather a great supporter of astronomical dating of the ancient world via the attested documents supported by careful retrocalculations. I do not recall ever having claimed that astronomical evidence is of little or no value. If Cochrane has such statements by me, let him publish them. In his critique he has presented no astronomical retrocalculated evidence whatsoever that places the Old Babylonians in the early second millennium B.C. If he has such evidence that gives an absolute dating, let him produce it. There simply is none, and that is the problem.

The question at hand is: Does Cochrane’s critique of Heinsohn vis-à-vis the Persian/Old Babylonian equation, and in particular Heinsohn’s Darius/Hammurabi equation stand up to scrutiny? As will be shown, the attested documents that produce astronomical data for the Old Babylonians and retrocalculations of these give absolute dates that connect the Persians to the Old Babylonians and in particular correlate the reigns of Darius and Hammurabi. Cochrane writes:

“Hammurabi was the most prominent ruler of the so-called first [or Old] dynasty of Babylon which saw Mesopotamia dominated by the Amorites. ...

“The royal inscriptions from this period (the so-called date formulae) provide valuable clues for reconstructing Hammurabi’s various activities year by year, inasmuch as they typically record the king’s major deeds and accomplishments.”<sup>30</sup>

Notice Cochrane talks of documents that provide “clues” from “royal inscriptions” that delineate deeds and accomplishments of Hammurabi’s reign. But none of this data deals with absolute astronomical dating. However, the *Cambridge Ancient History* reports with respect to the documented material for Hammurabi:

“The materials left by the king himself or derived from any source connected with him which can be of use in writing the history of his reign are scanty in the extreme. His official inscriptions are few ..., almost wholly devoted to ... building. ...”<sup>31</sup>

This is not to suggest in any way that these materials are all in error, but how do we test and/or falsify them? They must be taken at face value. It is well known that ancient Near Eastern monarchs were given to only reporting history to aggrandize themselves. Let us compare battles fought by Darius and Hammurabi as presented by Cochrane to learn if such documents are to be trusted to determine the histories of these supposedly two separate kings.

“According to Roux, Hammurabi ‘patiently waited for five years before making the first move’ to enlarge his kingdom. Between the 6th and 11th years of his reign, Hammurabi defeated Isin, Uruk, Malgium, and invaded Emutbal. For the next twenty years ... Hammurabi devoted himself ‘solely to the embellishment of temples and the fortification of towns.’ After nearly three decades on the throne, Hammurabi set about expanding the boundaries of his kingdom. In his 30th year he conquered Elam. Hammurabi defeated ... Larsa, in year

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<sup>30</sup> Ev Cochrane, “Heinsohn’s Ancient ‘History’,” *Aeon*, vol. V, no. 4 (July 1999), unpaginated but on the basis of the index, p. 58

<sup>31</sup> *CAH*, vol. II pt. 1 (1973), p. 177

31. The same year saw him defeating Eshnunna, Subartu, and Gutium. In his 32nd year, he overthrew Zimri-Lim and Mari. Two years later, Hammurabi returned and sacked Mari. ... In his 36th and 38th year, Hammurabi ‘overthrew the army of the country Subartu (Assyria) and defeated all his enemies as far as the country of Subartu.’ ... All told, Hammurabi reigned a period of 43 years.

“Darius in contrast ... succeeded in ... claiming the throne for himself. The apparent chaos ... inspired most of the satrapies to revolt and thus Darius was forced to put down one rebellion after another, first in Elam, then in Babylon, as well as in Armenia, Persia, Media, Assyria, Parthia, and Scythia. It took over two years of heavy fighting for Darius to establish himself. ... Thereupon Darius set about the task of restructuring and expanding the empire. ... This was followed by campaigns in India and along the Mediterranean where he gained control of the Ionian islands. In 513 BCE, finally, Darius campaigned against the Scythians around the Black Sea, conquering European Thrace and most of the northern Aegean. ... All told, Darius reigned for a period of 36 years. ...”<sup>32</sup>

Here we have a resume that is valid only in terms of documents. There is not one scintilla of scientific or technical evidence offered to test or falsify Heinsohn’s Darius/Hammurabi equation.

According to Cochrane, Darius “took over two years of heavy fighting ... to establish himself.” But the document upon which this is based does not say this. According to Jim Hicks *et al.*, Darius tells us “of the rebellions and how his army fought 19 battles and defeated nine would-be kings, all within ‘one and the same year’.”<sup>33</sup> Cochrane claims the rebellion was put down over two years, whereas Darius claims he did it in one year. Of Darius’ boastful claim, Heleen Sancisi-Weerdenburg writes: “As Darius says repeatedly: in one year he reestablished order throughout his empire. ... This time limit has caused scholars considerable headaches: in no way can the dates given in the inscription be fitted into a one-year

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<sup>32</sup> Cochrane, *op.cit.*, pp. 57-58

<sup>33</sup> Jim Hicks *et al.*, *The Persians* (NY 1975), p. 28

schedule. It seems probable that Darius sacrifices chronological precision and historical truth ... for the immediate cause of legitimizing his rule.”<sup>34</sup>

Cochrane’s own source also points out “It seems ... simplest to assume that Darius nevertheless falsified the record.”<sup>35</sup> Another of Cochrane’s sources, Olmstead, shows “It is significant that in Herod iii 72 Darius is made to give an elaborate defense of lying.”<sup>36</sup>

Heleen Sancisi-Weerdenburg tells us: “Darius’s one-year autobiography [on the Behistun Rock] was full of lies.”<sup>37</sup> She adds: “Darius tells of a victorious campaign against the Scythians in his third year [519 BCE]” [yet] “Darius was anything but victorious, he barely saved his army from destruction.”<sup>38</sup>

The question is not of one or two years over which the rebellion was put down, since one year makes very little difference in the chronologies of the two kings. The fact is that the historians interpreted the concept of 19 battles fought in a single year and found it preposterous, and so the length of the rebellion was changed, contrary to the document, to two years. We, in fact, don’t know if it was three, four, five or more years for this to have been the time span, nor just when the rebellions began and ended. How can this be put to the test and falsified? Nevertheless, Cochrane, while presenting the accepted rendition of these rebellions, carefully failed to report any of these problems to his readers. After raising this part of his critique, he then attacked Heinsohn for citing a source that has been called quite unreliable, namely Ctesias:

“Is there any reason, then, to take Ctesias as a reliable witness for the specifics of Persian history? On the contrary, he is known to be a most unreliable source. Amelie Kuhrt, writing for the *Cambridge Ancient History*, had this to say about the Greek doctor:

“ ‘Unfortunately there is little evidence to indicate that Ctesias had access to any particularly reliable source about

<sup>34</sup> Heleen Sancisi-Weerdenburg, “Darius I and the Persian Empire,” *Civilizations of the Ancient Near East*, vol. II, J.M. Sassoon *et al.*, eds. (NY 1995), pp. 1036-1037

<sup>35</sup> *The Cambridge History of Iran*, I. Gershevitch ed., vol. 2 (London 1985), p. 210

<sup>36</sup> A.T. Olmstead, *History of the Persian Empire* (Chicago, London 1948), p. 109 fn

<sup>37</sup> Sancisi-Weerdenburg, *op.cit.*, p. 1038

<sup>38</sup> *ibid.*, p. 1038 and 1043

earlier Persian history, so that his use for the purposes of this chapter is negligible.’

“Fol and Hammand, writing for the same publication, describe Ctesias as ‘far from dependable.’ J.M. Cook cautioned that modern historians should disregard Ctesias as a historical source altogether:

“ ‘When we discover that ... Ctesias’ familiarity with the Persian records did not prevent him from interpreting Darius’ Behistun text as a description of Semiramis’ ascent of the cliff on a mountain of pack-saddles, we have no choice left but to reject his entire claim ... we have little means of checking—the specific information that he gives is usually quite false. ... On the balance, it seems most prudent to disregard him as a serious historical source.’

“Yet this is the ‘authority’ Heinsohn would have us follow. ...”<sup>39</sup>

When Cochrane cites a source for Darius’s fighting 19 battles in one year, which historians claim is an out and out lie, we do not have “a reliable witness for the specifics of Persian history.” That source exhibits “little evidence that [Cochrane] had access to any particularly reliable source ... so that his use for the purposes [he presents] ... is negligible.” His source is “far from reliable,” and “on ... balance it seems most prudent to disregard him as a serious historical source. ... Yet this is the ‘authority’ [Cochrane] would have us follow [in establishing Persian history].” When we turn to the documents provided by kings about themselves we are treading on dangerous grounds. As Herbert Butterfield explains:

“For the most part it [ancient Near Eastern history] is official history produced on behalf of the monarch and serving his interests or that of [his] state. ... The annals were produced by the king himself, or were written for him—presenting him as speaking in the first person; and their purpose was to celebrate his building achievements ... or his prowess in the hunt ... or his bravery in battle and his success in war [as Darius boasted of his one-year conquests] ... they were just about the most boastful pieces of writing ever produced by anybody. ... One

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<sup>39</sup> Cochrane, *op.cit.*, p. 64

thing is clear: they are not to be taken as evidence of the interest [ancient] man showed in ... the recovery of the past.”<sup>40</sup>

So, too, with Darius’s blatant propaganda to which Cochrane gives his approval. Literature produced by these ancient kings about themselves is generally pure propaganda just as it is today in various states. How much of the atrocities committed by modern nations is presented by these states? For example, General Charles Cornwallis, whose coffin lies in Westminster Abbey, London and who was defeated at Yorktown by George Washington with the French fleet and a French expeditionary force, has this epitaph written on his tomb: “He defeated Americans with great slaughter.” Not a breath of his great defeat is presented. If this is the case in the near past, it is only reasonable that the battles fought by ancient kings be taken for what they are—propaganda, not to be relied on as solid historical fact.

This is the problem inherent in assuming, as Cochrane does, along with nearly all historians of these periods, that Darius’s record carved on the Behistun Rock or elsewhere is proof that what is contained therein is true. As I believe Samuel Johnson aptly put it: “In lapidary inscriptions a man is not upon oath.”

Cochrane’s more important criticism, however, is that “Hammurabi reigned a period of 43 years,” while “Darius reigned for a period of 36 years,” that is, “the length of their reigns is different. ...”<sup>41</sup> This, too, is based on the documentary evidence. But what does the calendrical, astronomical evidence, upon which absolute dating depends, say with regard to Cochrane’s assertion? In this respect, we come back to our 30-day months attested for Hammurabi’s reign and those of Darius. Lynn E. Rose as long ago as 1995 presented the data that shows Heinsohn’s Darius /Hammurabi equation is valid:

“In a series of publications ... Heinsohn has suggested that the First [or Old] Babylonian Dynasty and the Persian Empire are identical. As part of this thesis he maintains that Hammurabi of the [Old] Babylonian Dynasty is identical with Darius the Great of the Persian Empire and that Ammisaduqa of the [Old] Babylonian Dynasty is identical with Artaxerxes III Ochus of the Persian Empire.

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<sup>40</sup> Butterfield, *op.cit.*, p. 45; see also p. 46

<sup>41</sup> Cochrane, *op.cit.*, p. 58



“... there are two ... lines of argument that did eventually convince me of Heinsohn’s thesis. Both involve retrocalculation, and thus both would be condemned by Heinsohn himself. ...

“One line of argument proceeds from the rather extensive archaeological evidence [presented by Sidney Smith and others] that the [Old] Babylonian Dynasty would have been nearly or at least roughly contemporary with the Twelfth [Egyptian] Dynasty. BUT THERE IS OVERWHELMING ASTRONOMICAL EVIDENCE that the Twelfth Dynasty ended with the coming of Alexander the Great. Thus I *must* put the [Old] Babylonian Dynasty at about the time of the Persian Empire anyway.” [Capitalization added]<sup>42</sup>

This, of course, was discussed earlier. As Cryer clearly understands and as do nearly all serious historians: “Establishing the nature of the calendar or calendars used in a given ancient society is of primary historiographical importance.”<sup>43</sup>

Let us now examine the calendrical, astronomical evidence by which Rose was able to anchor the Old Babylonians alongside the Persians and specifically to do this for Darius/Hammurabi and Artaxerxes III Ochos/ Ammisaduqa.

“The other line of argument directly addresses the identification of Hammurabi ... with Darius the Great ..., as well as the identification of Ammisaduqa ... with Artaxerxes III Ochos of the Persian Empire.

“I *love* to be able to prove my case by citing my opponents. Thus it is with great pleasure that I call your attention to an esoteric work by Peter J. Huber ... *Astronomical Dating of Babylon I and Ur II* [which] contains the attested intercalations under Ammisaduqa (page 60) *as well as the attested 30-day months under Ammisaduqa* [emphasis added] (page 65). We can also make use of Langdon-Fotheringham-Schoch here (pages 61 and 77). Further ... the little book by Parker and Dubberstein entitled *Babylonian Chronology 626 B.C.-A.D. 75* ...

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<sup>42</sup> Rose, “From Calendars to Chronology”, *op.cit.*, p. 722

<sup>43</sup> Cryer, *op.cit.*, p. 656

contains retrocalculations of all the lunar months under Artaxerxes III Ochos (page 35). ...

“If we have Year 1 of Ammisaduqa start on February 12 Julian, -357, then 24 of the 28 attested 30-day months seem to check out. All four of the misses are one day late and are at the end of 29-day months; thus bad seeing at those four points is very strongly indicated.”<sup>44</sup>

This means that all the attested 30-day months of Ammisaduqa are identical to the particular years and months of 30 days of Artaxerxes III Ochos. Why is this so unusual? When the Old Babylonians supposedly lived in the early part of the second millennium B.C., intercalary months were added to the year by decree. But during Persian times these extra intercalary months were added to the calendar by formula. What then is the probability that 24 of 28 attested 30-day months from the reign of Ammisaduqa should just by chance happen to be identical to the Persian calendar of Artaxerxes III Ochos? Both kings reigned for the same number of years, but if, say, Ammisaduqa in his 13th year, 8th month had a 30-day month, then Artaxerxes III Ochos in his 13th year, 8th month also had a 30-day month. Rose found 24 such correlations out of 28 30-day months spread across the various but widely different years of their reigns. And significantly, the four misses all occur at the very end of Ammisaduqa’s 29-day months which in this small number of cases points strongly to the fact that the sky was overcast and the astrologer viewing the sky could not see the Moon that day and therefore claimed it was a 30-day month rather than one of 29 days. The probability that in so many cases both Ammisaduqa and Artaxerxes III Ochos should have in particular years of their reigns, and particular months of these years, identical 30-day months is inordinately low. Thus Ammisaduqa of the Old Babylonian Dynasty is dated precisely and *absolutely* to the time of the Persian king Artaxerxes III Ochos. This can only be so if they are one and the same person or if their reigns are co-extensive. What, then, of Hammurabi and Darius the Great? Here Rose shows:

“If we have Year 1 of Hammurabi start on April 1, -520, then 25 of 27 attested 30-day months seem to check out. ... There is one complication in the Hammurabi-Darius case, in that Hammurabi reigned for at least 43 years and Darius for only 36, but I would argue that there was a revised year-count ... which was for the purpose of

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<sup>44</sup> Rose, “From Calendars to Chronology,” *op.cit.*, p. 723

back-dating the reign by nine years, and thus [have Darius deprive] ... the hated Kambyses [who ruled before him for around 8-9 years] of his very existence! This [taking away years from an earlier king and adding them to one's reign] was a common practice in antiquity. It seems that the [Old] Babylonian-‘Hammurabi’ went along with this move but the Persian-‘Darius’ tradition did not. ...”

“I conclude [based on these 30-day months for Ammisaduqa/ Artaxerxes III Ochus and Hammurabi/Darius] that Heinsohn’s thesis is correct.”<sup>45</sup>

Huber, who had raised the importance of using months in the calendar for explicating chronology for the Venus tablets, had to change 30 percent of the astronomical data to place the Old Babylonians back in the early part of the second millennium B.C. Rose, on the other hand, except for bad seeing, changed none of the astronomical data to have these 30-day months in separate years at definitive numbers of months into those years for both sets of kings to fit the astronomy and the retrocalculations. Huber nevertheless described his findings thus: “Apart from scribal errors [my findings] are compatible with modern calculations. Unfortunately, we do not know precisely when Ammizaduqa reigned.”<sup>46</sup>

Rose’s evidence shows precisely when Ammisaduqa as well as Hammurabi reigned; his list of hits does not require a single scribal error, and his analysis is strikingly compatible with modern retrocalculations.

One would expect kings living over a thousand years apart in time and hundreds of miles apart geographically not to have so many 30-day months situated in exactly the same years and the same months of those years. One would expect that perhaps as many as half of the 30-day months attested for Ammisaduqa and Hammurabi would fall on 29-day months of Artaxerxes III Ochus and Darius, and vice versa. But this is not the case.

Egyptologist William H. Stiebing Jr. has also raised the issue of probability as a method of testing evidence:

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<sup>45</sup> *ibid.*

<sup>46</sup> Peter Huber, “Cuneiform Evidence for Venus”, *Scientists Confront Velikovsky*, (Cornell NY 1979), pp. 126-127

“Scientists [and, I add, historians] 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.”<sup>47</sup>

In this respect Cryer states:

“There is no law requiring that king[s] A [Hammurabi and Ammisaduqa] ... employ the same calendrical reckoning as king[s] B [Darius and Artaxerxes II Ochus] even within the same culture or society; nor is there any that requires one and the same society to employ the same calendar. ... Nor need we assume that calendars were identical throughout even a great empire. ... Hence, there is no assurance that the length[s] of the reign[s] of king[s] A [Hammurabi and Ammisaduqa] as recorded by scribe X [were] measured using the same calendar as that employed by scribe Y to define the reign[s] of king[s] B [Darius and Artaxerxes III Ochus]. Nor is it likely that the scribes in question will have known of such problems; if they did, it is uncertain whether they regarded calendrical problems as significant.”<sup>48</sup>

Not only do we have the same Old Babylonian and Persian kings adding the same 30-day months to particular years at particular months of these years, but the various scribes who prepared these texts were either indifferent or unaware that they were preparing identical lists of the 30-day months and years. Whether Cochrane, Stiebing, or any other historian critical of Heinsohn’s thesis realizes it or not, his equation of the Old Babylonians with the Persians is now dated absolutely. It is extraordinarily improbable that the placement of the Old Babylonians back in the early second millennium can stand.

Cochrane has written on his website, [www.maverickscience.com](http://www.maverickscience.com), an attack on my work wherein he writes: “... Ginenthal fails to inform his readers exactly which ‘scientific and technological established facts’ ... show that Hammurabi lived *after* Cyrus and the downfall of the Medes/Mitanni. The truth is, of course, that no such facts exist.” Cochrane’s piece is titled “The Wildgoose Chase”.

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<sup>47</sup> William H. Stiebing Jr. “Cosmic Catastrophism”, *Aeon* vol. II, no. 6 (May 1992), p. 60

<sup>48</sup> Cryer, *op.cit.*, p. 656

The question I now raise for Cochrane is: Why did he completely ignore this astronomical-calendrical evidence? All of Rose's evidence regarding the lengths of the reigns of Darius and Hammurabi as well as Artaxerxes III Ochos and Ammisaduqa appeared a good four years before Cochrane raised his Darius-Hammurabi critique in *Aeon* in 1999. In *Stephen J. Gould and Immanuel Velikovsky* (1995) it was all spelled out for him. In fact, Cochrane contributed a significant chapter to this tome, and I thanked him for doing so by giving him a copy. Why then did he fail to report, or deal with, this Darius-Hammurabi material when addressing his readers? The astronomical-calendrical evidence clearly and directly contradicted his argument. His obligation as a critic was, at the very least, to deal with Rose's material and let his readers know that contradictory evidence to his criticism exists. But that scholarly obligation was evaded.

While Cochrane raised documentary evidence to assail Heinsohn's thesis, the forensic astronomical-calendrical documents and retrocalculations solidly support Heinsohn.

There is one further unique aspect of having these attested and retrocalculated 30-day months in particular years at specific months of these years agreeing with each other in the reigns of Darius/Hammurabi and Artaxerxes III Ochos/Ammisaduqa which has to do with intercalary months. Huber argues:

“... Rose chooses to ignore some of the really important points, in particular the matter of intercalary months. ... Intercalary months were a reality throughout Babylonian history from before Hammurabi to the latest times, and you simply cannot afford to ignore them when discussing calendaric [and chronological] matters.”<sup>49</sup>

Huber notes in particular:

“The [Old] Babylonian calendar is based on the true lunar month: the month begins on the evening when the new moon first becomes visible. Thus the length of the month varies irregularly between 29 and 30 days; twelve lunar months correspond to about 354 days. In roughly every third year the Babylonians inserted an additional (thirteenth) month in order to keep the month in step with the seasons.

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<sup>49</sup> quoted in Rose, “Just Plainly Wrong: A Critique of Peter Huber,” *op.cit.*, p. 38

“Before about -500, these intercalations were handled in a rather haphazard fashion by royal decree.”<sup>50</sup>

Thus under Hammurabi and Ammisaduqa, who supposedly lived prior to about 1600 B.C., there was no formula for adding intercalary months to the year. They did so in a “haphazard fashion.”

Robert R. Newton tells us in this respect:

“Since the need for the extra month was determined by observation made by officials who had political interests and political power, we may be sure that the assignment of the extra [intercalary] month was somewhat erratic. It has been estimated that [because of extra intercalary months] the first day of the year might have come any time from the middle of March to the middle of June in middle Babylonian times.”<sup>51</sup>

30-day months did not necessarily follow 29-day ones. There were short strings of 30- and 29-day months occurring without apparent rhyme or reason in so-called Old Babylonian times.

Clearly, one would not expect the Old Babylonian and Persian kings to add just the correct intercalary months in the correct spaces (not necessarily the identical months in the identical places) to permit all the 30-day months to be identical for both sets of kings. The calendars, under the established chronology, would only allow for this condition to occur if the Persian/Old Babylonian kings were one and the same person and if the Persians were bringing the subject peoples' calendar in line with their own. If they were different kings living apart in time and place they would not have added intercalary months in just such a way as to keep their calendars in time with one another so closely.

This, in fact, is how the Romans forced the Egyptians to change their calendar to be in alignment with the Roman calendar. The Egyptians had a fixed 365-day calendar, without leap years. The Romans required the Egyptians to add a

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<sup>50</sup> Huber in *Scientists Confront Velikovsky, loc.cit.*

<sup>51</sup> R.R. Newton, *The Crime of Claudius Ptolemy* (Baltimore, London 1977), p. 396; see also p. 395

sixth day every four years; this was so that, at least eventually, both calendars would average  $365\frac{1}{4}$  years. In a similar (not identical) sense, the Persians forced their Old Babylonian subjects to align their calendar with their own.

This astronomical-calendrical evidence indicates rather conclusively the correctness of Heinsohn, Rose, and Sweeney's thesis that the Old Babylonians were the Persian rulers over Babylonia, that Darius and Hammurabi were one and the same king, and that Artaxerxes III Ochus and Ammisaduqa were also one and the same king. The possibility or probability of finding so many exact calendrical correlations between these two sets of kings defies possibility and probability. These identical correspondences in their calendars do not happen by chance, indicating that the Persians and Old Babylonians were living at the same time and that Hammurabi was the same king as Darius under a different name, while Ammisaduqa was the same as Artaxerxes III Ochus under a different name. Kings often had many names and the name they were known by in their homeland was not necessarily the one they used for their subject peoples. (We should remember, of course, that Heinsohn has recently expressed reservations about his own equations, and that he may prefer to view them in terms of contemporaneity rather than in terms of strict identity. We shall see.)

In terms of forensic historical evidence we now have a solid astronomical-calendrical foundation showing the Old Babylonians were actually the Persian rulers over Babylonia. Nevertheless, there is a great deal more forensic historical evidence that supports the same conclusion that will be outlined with the following materials. *In toto*, we will discover that there are several levels of scientific and technological evidence that correlate, corroborate and converge, along with the astronomical-calendrical evidence, to show that the Old Babylonians were the Persians.

## GEOLOGICAL STRATIGRAPHY VS. ARCHAEOLOGICAL STRATIGRAPHY

“Perhaps the strongest [chronological] link ... is the continuous stratification, over a period of years.”

Sidney Smith  
*op.cit.*, p. 47

There are two forms of stratigraphical research that must first be distinguished from one another before one can properly determine the chronology of the ancient world. There is a fundamental difference between geological stratigraphy and historical-archaeological stratigraphy. Geological stratigraphy is based on a well-understood scientific process. When an ancient city, town, or village was abandoned in Mesopotamia and never resettled or, after being abandoned for a long period the same site was resettled, a geological process occurs that leaves clear-cut evidence to show what happened. Once a place has been abandoned, the wind blows sand, soil, etc., into the streets and the ruins of the buildings of the first settlers that were left standing. These materials, called Aeolian or wind-blown layers, are a rather clear geological marker that tells a geologist that the site was abandoned. When it rains—as it does in Mesopotamia during the winter season—the mud brick walls still standing above these Aeolian deposits slump and flow down over these wind-blown materials, sealing them in place. Erosion does play a part in this process but it is negligible because once the mud-brick walls flow as mud over the Aeolian deposits, desert weeds grow on the mounds of these settlements and protect the mound, or “tell”, from further erosion. Even after thousands of years of abandonment, the ancient city, town, or village mounds of Mesopotamia have not been weathered by sandstorms or eroded away. Aeolian layers are fundamental scientific proof that a site has been abandoned.

Historical, archaeological stratigraphy is not based on this geological marker but rather on markers of a very different nature. These markers are the various artifacts: pottery, tools, metals, architectural forms, etc., that are left at a site



by different cultures one above the other. By interpreting the level or stratum in an ancient mound where these ancient relics are found, archaeologists determine the sequence of the cultures, which is the stratum at the bottom of the mound. Above that in different strata are the relics of the cultures that came afterwards. The archaeological stratigraphical interpretation of the chronology of these ancient sites is not as clear-cut as stratigraphical geology but an interpretation of what the forms of these ancient relics are and at what level or stratum in the mound they are found would be instructive.

If the chronology of ancient Mesopotamia is 3000 to 4500 years long, as the historians claim, then the settlement gaps at certain sites will be hundreds or even a thousand years or more in length and must be reflected by Aeolian layers as evidence of any assumed settlement gap. If, on the other hand, the chronology of the ancient Near East is only around 850 or so years in length, as Heinsohn claims, then none of these civilizations could have been separated from another by 700, 800, to over 1000 years. These civilizations would largely have overlapped one another almost continuously and thus their stratigraphy would rarely show long-time settlement gaps. Under the chronology Heinsohn posits, these cultures would tend to be found one directly above the other without Aeolian layers separating them.

Usually, the bottom strata are older than those above them. But where there is immediate overlap of civilizations we encounter a further problem. The conquering society may employ the artisans of the subjugated one to produce pottery, jewelry, temples, etc., for them as well as using their own artisans to do the same. This creates a somewhat subjective interpretation to the archaeological stratigraphy which cannot be tested and falsified by scientific tests. Therefore, the evidence of geological stratigraphy, because it is based on science and can be tested and falsified, must override historical-archaeological stratigraphy because it is based on interpretations that may be false. Forensic historical evidence comes before interpretive historical-archaeological evidence! Only after the geological stratigraphy has been presented can the archaeological stratigraphy come into play in order to interpret the chronology, and not the other way round. An archaeological analysis only becomes acceptable after it is validated and underpinned by geological stratigraphy.

With respect to the Persians, it is also important to note a further distinction. The Persians ruled over a vast empire and therefore they could not and did not occupy every city, town, or village. Rather, they generally, at first, permitted

the original rulers, who had surrendered to them, to act on their behalf as their agents. Only here and there did they station military and political overseers of their own people who were entrusted with carrying out the duties of running their empire. As Heleen Sancisi-Weerdenburg explains:

“Although military settlements of various types are detectable ... there simply could not have been so many Iranians [that is, Persians] that effective control of the whole territory of the empire by coherent groups was possible. To give another modern example, German occupation of Holland in the Second World War cannot be described in any other way than as rather effective. There were, however, entire villages where no German was ever seen during the entire period. Needless to say, these were the villages where the hiding of [Jewish] refugees was most successful. If the territorial coverage of a small country like Holland was apparently impossible for a modern occupation-force such as that of National-Socialist Germany, it really should make us wonder if we are not assuming too much when we take it for granted that the whole of the Persian Empire was indeed under effective control. If parts of it were effectively controlled, this must have been due more to the merits of the pre-existing socio-economic structure than to the efforts of the ‘ethno-classe dominante’.”<sup>52</sup>

Under such conditions the Persians would have left very little stratigraphical evidence of their presence in Babylonia except at a few sites, the vast number of sites being run by the indigenous people who would of course leave behind only those relics that reflected their own culture. Furthermore, the Babylonians were more advanced in many respects than their Persian conquerors who would have used local artisans to produce whatever they required. These relics would therefore reflect the Babylonian culture rather than that of Persia.

Heinsohn and Sweeney as well as Rose maintain that the Old Babylonians were the Persian rulers and many of the officials were Old Babylonians; one would, therefore, hardly expect to find a plethora of Persian materials in the strata there. Since the Persians only stayed here and there, the strata of this period would overwhelmingly contain Old Babylonian materials. But most important for

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<sup>52</sup> Heleen Sancisi-Weerdenburg, “The Quest for an Elusive Empire,” *Achaemenid History IV Centre and Periphery*, H. Sancisi-Weerdenburg, A. Kuhrt, eds. (Leiden, Holland 1990), p. 268

this thesis: because the Persian Empire was conquered by Alexander the Great, followed immediately by the occupation of the Greeks, this Persian stratum would have to lie directly beneath Greek-Hellenistic strata, and settlement gaps between the Old Babylonians and the Greeks should not exist.

Nevertheless, because the historians believe that the Persians must have left ample evidence of their stay in Babylonia, the skimpy finds of their materials have bewildered them. About this question of Persian strata missing in Babylonia, as well as elsewhere throughout their empire, A. Leo Oppenheim opines:

“The encounter between the Achaemenid [Persian] empire [and the people of] Babylonia seems to have left surprisingly insignificant impact on the latter. Much of this impression is patently caused by the inherent sterility of the extant writings and the scarcity of archaeological evidence. Still, one can hardly use this state of affairs with good conscience as an excuse for shelving the problem here involved until the happy day when more and better evidence will turn up. There is one reason that should prompt us to try, at least, to answer the question posed by the nature and extent of the encounter . . . . [For] two no less fateful confrontations occurred in Babylonia subsequent to the Persian conquest, twice again the slowly disintegrating Mesopotamian civilization was to suffer foreign domination. . . . First came the traumatic impact of the invasion by Alexander the Great, which was followed by the overwhelming surge of the Hellenistic civilization engulfing not only Mesopotamia but all of western Asia. Then followed a second tide from Iran which produced the Parthian Arsacid rule enduring for at least as long as the Achaemenian [Persian], to be followed by the Persian Susanean Dynasty.

“Though evidence is still far from adequate, the flowering created by the contacts of Babylonia with Hellenism and the Parthian civilization, respectively, stands in unmistakable contrast to the sterility and lack of interaction which seemed to characterize the Achaemenid [Persian] presence in Babylonia.”<sup>53</sup>

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<sup>53</sup> A. Leo Oppenheim, “The Babylonian Evidence of Achaemenian Rule in Mesopotamia,” *Cambridge History of Iran*, *op.cit.*, pp. 585-586

The difference is accounted for by the fact that the Greeks and Parthians were highly advanced people who not only conquered Babylonia but transported thousands of administrators and artisans from their homelands to create the various comforts, utensils, buildings, etc., to make their life in this foreign land seem as much like Greece and Parthia as possible. The Persians, as we will see below, were not as advanced. Rather, they found their Babylonian subjects superior in most respects except military prowess. Instead of changing the cultures over which they ruled, they employed the skilled Babylonians and their materials, which were superior to their own. They had little in the way of material development to contribute to their subjects' way of life. All this will be gone into in much greater detail below, but for the present, Sancisi-Weerdenburg explains the situation:

“... the Persians did not have at their disposal an administrative tradition with which to replace the institutions of conquered nations. The best option after a conquest was to keep the local structure in place, more often than not including the local magnates. ... There were advantages to such a policy: presumably the local population could bear the burden of taxation more easily. ...”<sup>54</sup>

With regard to Cyrus the Great who created the Persian Empire, Geo Widengren reports:

“Cyrus took over the administrative organization ... and evidently left the governors in their office. His policy was to act in everything as a real Babylonian.”<sup>55</sup>

Thus we know nothing of Persian rule in Babylonia. This is because the Persians were not an intrusive element there. A. Kuhrt states “It should be clear from the foregoing that the evidence for Persian rule of Babylonia from 539 to 465 [B.C.] presents major problems and that a reconstruction of the political history of the area is an almost impossible task.”<sup>56</sup>

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<sup>54</sup> Heleen Sancisi-Weerdenburg, “Darius I and the Persian Empire”, *op.cit.*, p. 1042

<sup>55</sup> Geo Widengren, “The Persians,” *People of Old Testament Times*, O.J. Weissman, ed. (Oxford, UK, 1973), p. 318

<sup>56</sup> A. Kuhrt, “Babylonia from Cyrus to Xerxes,” *CAH*, 2nd ed., vol. IV (1988), p. 135f

This is entirely in agreement with Heinsohn's, Sweeney's, and Rose's theses. With respect to the Persian presence in Egypt, the Levant and elsewhere, Kuhrt writes:

“... our main, often sole, guide to [Persian history] is, therefore, Greek historiography and the Old Testament [which are secondary sources]. But these sources give us a very partial insight, because of their generally circumscribed perspective: the history of Greek-Persian relations in the Aegean and western Asia Minor predominates; we can piece together a little on Persian policy in the Levant and Egypt, the rest [namely Persian Babylonia and Assyria] is virtually a blank.”<sup>57</sup>

Historians have been unwilling to accept that because the Persians, while ruling these lands, were barely intrusive, they could not have left a great deal of evidence of their sojourn in Babylonia and elsewhere, so that these “blanks” and “gaps” in their history naturally exist. As Kuhrt and Sancisi-Weerdenburg admit: “The Persian empire is so frequently almost invisible in the archeological [*sic*] record, that at the end of one long session, the chairman exclaimed in exasperation: ‘Was there ever a Persian empire?’”<sup>58</sup>

The same exists for the Amorites who took over Babylonia, as outlined by Alan Millard:

“The texts attest the advent of the Amorites to dominate Babylonia. However, nothing among the material remains from the first half of the 2nd millennium in Babylonia discloses any evidence of cultural change that might point to the presence of such newcomers in society. In architecture the monumental buildings usually adhere to old plans, and kings proudly proclaim their restoration of old temples. Their work shows little difference from their predecessors of the Third Dynasty of Ur. Where they built new structures, there is no distinctive non-Babylonian element in them. Houses of the wealthier classes followed the pattern current in the previous centuries, and that pattern

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<sup>57</sup> A. Kuhrt, *The Ancient Near East c. 3000-330 BC*, vol. 2 (London, NY 1995), p. 667

<sup>58</sup> A. Kuhrt, H. Sancisi-Weerdenburg, “Introduction,” *Achaemenid History IV*, *op.cit.*, p. XIII

continued into the subsequent Kassite period. Pottery fashions, too, display development, but no major changes.”<sup>59</sup>

As Kurinsky points out:

“... archaeologists rarely consider that the installation of a new population at a particular site does not necessarily mean the destruction of the old site and the obliteration of its population, nor the introduction of a new type of products, even if the occupation was brought about by conquest. A conquering group [such as the Persians] might well, and probably often did, take over a city without damaging a single element of its physical plan and proceeded to dwell in it without making changes or additions that deviate from the existing norms. The artisans among the previous inhabitants may well have melded into the new community to produce ware in the style to which they were accustomed, even to apprentice the newcomers into that style. The presence of such artifacts, the lack of a layer of ashes or lesser evidence of destruction, the dearth of distinctively new architecture, and the continuation of old cultural modalities is inevitably interpreted as stratigraphic proof that no occupation or conquest had taken place [when in fact it had]. Scholarly literature is saturated with such negative [untestable, unfalsifiable] ‘proofs’.”<sup>60</sup>

Based on Heinsohn’s and Rose’s theses, these “blank” periods disappear once we give the Persian stratum in Babylonia to the indigenous Old Babylonians, and the stratigraphical evidence that proves this, according to Heinsohn, is that the Old Babylonian stratum lies directly below the Hellenistic Greek stratum.

The historians who understood this then had to explain why the Persian stratum was missing just below that of the Greeks. In this regard Cochrane has challenged Heinsohn’s stratigraphic conclusions:

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<sup>59</sup> Alan Millard, “Amorites and Israelites: Invisible Invaders—Modern Expectation and Ancient Reality,” *The Future of Biblical Archaeology: Reassessing Methodologies and Assumptions*, James Karl Hoffmeier, ed. (Grand Rapids MI/Cambridge UK 2004), p. 151

<sup>60</sup> Samuel Kurinsky, *The Eighth Day* (Northvale NJ, London 1994) p. 158

“Heinsohn has become famous for arguing that the key to understanding ancient chronology is a properly ordered stratigraphy. ... Heinsohn claims that, at [the city of] Mari and a handful of other sites, the strata associated with the ‘Old Babylonian’ period are found directly beneath the Greek levels, thereby supporting his identification [because] the archaeological remains of the [Persians] would naturally be sought for immediately [beneath those] of Alexander the Great [and the Hellenistic Greeks].”<sup>61</sup>

Cochrane, however, claims that Heinsohn’s thesis is invalidated by stratigraphy, particularly at Tell Hariri, beneath which the city of Mari is located. Since the Assyrian empire apparently came well before that of the Persians, it would seem an impossibility to find an Assyrian palace above the Old Babylonian/Persian strata there. Citing J.C. Margueron, Cochrane states:

“The destruction of 1760 BCE [by Hammurabi] put an end to Mari as the capital of a realm ... However, the traces of later structures [above the destroyed city] attest that the city did not disappear overnight. People continued to live in the ruins of the city Hammurabi devastated. The remains of that epoch, ... (the seventeenth-sixteenth centuries BCE) are generally rather poor; ... The Middle Assyrian period (thirteenth-twelfth centuries BCE) is represented by a modest structure located on the tell’s northwest promontory and chiefly by a cemetery installed in the ruins of the Royal Palace. ...”<sup>62</sup>

One will first notice that Cochrane is arguing from historical-archaeological stratigraphy and not at all from geological-scientific stratigraphy. Before one can turn to archaeological evidence, at the very least some *kind* of scientific or technical evidence is required on which to base it. Cochrane has, of course, provided none; only interpretive archaeology is offered. Furthermore, as was noted above, the Persians did not intrude themselves on the peoples they subjugated. As a matter of fact, they employed Assyrian architects among others all through their reign.

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<sup>61</sup> Cochrane, *op.cit.*, pp. 66-67

<sup>62</sup> *ibid.*, p. 67

The criticism Cochrane raises is that since Mari was destroyed by Hammurabi, whom Heinsohn and Rose equate with Darius I, then Mari would have been destroyed in Persian times. The Assyrians who had been conquered and annihilated before the Persians could never have built palaces or any other buildings above the ruins of Mari since they no longer existed to do so.

Edward Bell explains:

“... the Chaldeans and Hittites, each [had] distinct and well marked characteristics, but no signs of mutual influence and partial fusion in the later Assyrian [period].”<sup>63</sup>

“[and it] may be inferred that the architectural forms and methods used in those countries which ultimately formed the kingdom of Persia were derived from Babylonia and Assyria, and prevailed until the consolidation of the kingdom, and the adoption of Susa as the winter capital [and] a new style based on influences derived from the more highly cultured western nations.”<sup>64</sup>

“[But most significantly the] Persian king [Darius] had already two principal palaces at Susa ... which was [*sic*] no doubt similar in style to those of Assyria.”<sup>65</sup>

That is, during Persian times the kings of that land were building Assyrian palaces. Why wouldn't Darius/Hammurabi allow an Assyrian palace to be built on the ruins of Mari? After all, as we will see below, Mari was along one of the principal trade routes of Mesopotamia. Assyrian architects were used by the Persians. Cochrane's criticism simply fails to address this fact. Darius/ Hammurabi destroyed Mari in early Assyrian times, thus the people who followed could still build in the Assyrian style.

What is also extremely significant is that the Old Babylonians, just like the Persians, did not influence the architectural styles in the countries of their

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<sup>63</sup> Edward Bell, *Early Architecture in Western Asia – Chaldean, Hittite, Assyrian, Persian: A Historical Outline* (London 1924), p. 2

<sup>64</sup> *ibid.*, p. 201

<sup>65</sup> *ibid.*, p. 212



subjugated peoples. That is, in *Babylonia* the Persians as well as the so-called Old Babylonians had no distinctive architectural style by which the archaeologists would be informed of their presence or intrusive influence. In Persia, this was a totally different story. As Giorgio Buccellati states:

“No distinctive archaeological evidence [such as pottery, architecture, etc.] can be convincingly associated with the [Old Babylonian] Amorites. The material does not exhibit stylistic traits that could be positively identified with them; this holds true even for smaller provincial cities in their [assumed] home ground. ... Nor has any site been identified in the steppe [west of Babylonia where they supposedly originated] that could be dated to the period in question.”<sup>66</sup>

There is no original architectural style in Chaldean Babylonia for the Persians just as there is no original style in Chaldean Babylonia for the Old Babylonians because, based on Heinsohn’s thesis, they are one and the same, and used the indigenous architectural styles of their more highly developed subjects. It was these styles that were kept alive throughout the Persian/Old Babylonian period as stated directly by Margaret Cool Root, “Achaemenid [Persian] achievement revalidated many earlier near eastern manifestations [of art and architecture] and kept them alive.”<sup>67</sup>

The devastating evidence that Cochrane supposedly used to critique Heinsohn’s thesis simply does not stand up to scrutiny. And since he does not produce any forensic historical evidence upon which to base his criticism that Mari was separated by about a thousand years from Darius his argument is a *non sequitur*. Since the Assyrians were building palaces in Assyrian times as well as in Persian times, to suggest, as Cochrane does, that an Assyrian palace on the ruins of Mari must be dated after the Old Babylonian period is meaningless. Before Cochrane can lay claim to the view that the Old Babylonians at Mari (destroyed by Hammurabi) came before the seventeenth-sixteenth, and thirteenth-twelfth centuries and not in the first millennium B.C., he would have to present solid scientific and technological evidence along with an absolute astronomical anchor by which to date them to that

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<sup>66</sup> Giorgio Buccellati, “Amorites,” *The Oxford Encyclopedia of Archaeology in the Near East*, E.M. Meyers ed. (Oxford 1997), p. 107

<sup>67</sup> Margaret Cool Root, “Art and Archaeology of the Achaemenid Empire,” *Civilizations of the Ancient Near East*, *op.cit.*, vol. IV, p. 2615

period. To dismiss this fundamental requirement to build on forensic historical data is essentially building palaces in the air.

What is to be noted is the extraordinary circumstance that both the Persians and Old Babylonians *in Babylonia* have no original architectural style and adopt the styles of their conquered peoples. As we go along, such extraordinary coincidences and correlations between the Persians and the Babylonians will be found again and again.

Cochrane then brings to bear criticism with which he intends to explain why the Persian stratum is missing in Babylonia and elsewhere:

“Heinsohn and his followers are forever pointing to the relative paucity of Persian strata throughout the ancient Near East. In fairness to Heinsohn, this is a valid point and it deserves an answer. Yet a definitive answer to this question will most likely be possible only at some point in the future, once all the relevant sites have been thoroughly excavated. Here’s how one scholar explained the relative scarcity of [Persian] architectural remains from this period in ancient Palestine:

‘Three characteristic features of Persian-period strata have contributed to the archaeological picture and the disappointing results [where Persian strata were expected to be found] from the excavations at the large mounds [under which cities one over the other are buried]: (1) after the Persian period, numerous mounds were abandoned and never resettled ..., and because the stratum from this period was the topmost on the site, it was exposed to the dangers of denudation; (2) at those sites where settlement continued ..., the Persian-period level of occupation was severely damaged by intensive building activities in the Hellenistic-Roman period; and (3) at most of the large sites excavated ..., the mound was largely occupied by a palace-fort or other large building.’<sup>68</sup>“

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<sup>68</sup> E. Stern, “Cities of the Persian Period,” in E. Meyers (ed.), *The Oxford Encyclopedia of Archaeology in the Near East*, *op.cit.*, p. 29, quoted in Cochrane, *op.cit.*, p. 68

Let us examine Cochrane's three explanations for why Persian strata are missing where they should be found.

(1) *After the Persians the numerous mounds were abandoned and never resettled, and the Persian stratum was the topmost and was exposed to the dangers of denudation.*

How do we know that this is a fact? What evidence have Stern or Cochrane presented other than this unsupported *ex cathedra* assertion that what they present is a valid, scientific fact? They have provided no examples for this denudation process nor any scientific evidence as support. What is meant by "denudation"? Did people loot and denude these sites? Were they denuded by erosion? Or did they just become denuded?

If this is the case in Palestine, surely it would occur at other permanently abandoned topmost sites as well. But this is not the case. For example, the end of the so-called Early Bronze Age in Anatolia saw a catastrophic destruction and permanent abandonment of numerous settlements. Yet the fact is, the topmost layers of these were not "denuded" by looting or erosion; they fully survived:

"In the Konya plain [of Anatolia] every town site of the EB2 [Early Bronze 2] period shows signs of conflagration mostly followed by desertion which is neatly dated on each site."<sup>69</sup>

Why weren't all those topmost sites "denuded" of every shard of pottery, or eroded away by wind and rain, or looted away? We are specifically informed that the relics at each site are "neatly dated." How can this be if after being permanently abandoned, denudation processes were at work? This shows that a double standard of inference is being used to explain the evidence. In Palestine and elsewhere denudation (whatever that is) did work on the Persian topmost stratum, but in Anatolia, the topmost layers failed to respond to the denudation processes. Cochrane cannot have it both ways, having denudation work in Palestine but then not working in Anatolia.

In fact, Georges Roux claims that when a site is permanently abandoned no denudation occurs at all. He claims instead that such a site is preserved:

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<sup>69</sup> *CAH*, 3rd ed., vol. I, pt. 2 (1971), p. 407

“Some sites, it is true, were abandoned early and for ever. . . . It is not difficult to imagine what took place then: windborne sand and earth [that form Aeolian layers] piled up against the remaining walls and filled in the streets and every hollow, while rainwater smoothed off the surface of the heaped-up ruins, spreading debris over a large area and planing the flanks [of the mound]. Slowly but inexorably, the town took its present shape: that of a rounded, more or less regular ruin-mound . . . a tell.”<sup>70</sup>

In this respect H.W.F. Saggs informs us:

“When, because of a war, disease or other reasons, the [Mesopotamian] settlements became depopulated, soil deposits laid down by the frequent dust-storms which blast Iraq would gradually cover the ruins of most buildings and in time build up the top of the mound to a more or less level or smoothly curving surface with only the remains of the ziggurat or any other exceptionally tall building projecting above the general level of such a tell [which] are sprinkled all over Mesopotamia.”<sup>71</sup>

Neither Roux nor Saggs suggests that erosion will remove entire layers of such mounds. On the contrary, Roux and Saggs claim in complete contradiction to Cochrane that permanently abandoned sites would keep their top-most layers! First the site would be covered in places by “windborne sand and earth,” filling every hollow. Second, rain would cause the mud brick walls to flow over these to preserve the Aeolian layers and relics beneath. There were in fact several such permanently abandoned mound sites in southern Mesopotamia but their topmost layers are apparently still there and not denuded away. H. Gasche *et al.*, present us with the fact that Ur, Uruk, Lasar, and many other major cities experienced this, yet their topmost layer has defied being denuded.<sup>72</sup> As in Andersen’s tale, even a child can see this emperor has no clothes! The naked truth is that Cochrane has offered nothing of substance.

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<sup>70</sup> Roux, *op.cit.*, p. 37

<sup>71</sup> H.W.F. Saggs, *The Might That Was Assyria* (London 1984), p. 290

<sup>72</sup> H. Gasche *et al.*, *Dating the Fall of Babylon* (Ghent, Belgium & Chicago, US, 1998), pp. 7-8

What is also problematic for Cochrane is that Roux is one of his sources. Thus he read what Roux wrote about sites being permanently abandoned and being preserved. Why didn't Cochrane, at the very least, discuss this? As was pointed out earlier in this volume and in *Pillars of the Past*, vol. I of this series, forensic history demands that if a scientific process such as erosion works in one place (such as at the Sphinx on the Giza Plateau in Egypt), it must do the same elsewhere under similar conditions. A double standard of inference cannot be employed to make the evidence say what Cochrane suggests.

What Cochrane has completely overlooked with regard to the denudation process is the fact that at some sites, pointed out by Heinsohn, the Old Babylonian stratum lies directly beneath that of the Hellenistic Greeks. According to Clark Whelton,

“At a number of archaeological sites in Mesopotamia (Bismaya, Girsu (Telloh), Der, Mari, Mushkan Shapir, al-Ubaid), and a number of others, Hellenistic/Parthian strata dated -300/-200 sit directly on top of “Old” Babylonian strata, dated to the mid-2nd millennium.” (Clark Whelton, personal communication).

According to Emmet Sweeney,

“A typical Lower Mesopotamian stratigraphy (as for example at Bismaya, Der, Shapir, and Al-Ubaid) looks like this: Hellenists (after 300 BC) [directly above] Old Babylonians (2000-1800 BC).”<sup>73</sup>

Gunnar Heinsohn states the same in many of his writings. No one has ever proved that these stratigraphical facts are false although they have been in print over several years. In fact, Ev Cochrane, a severe critic of Heinsohn, even admits this in his criticism to be presented below.

Cochrane suggests that we must wait until all these relevant sites are thoroughly excavated, which may take hundreds of years. In essence, he has no explanation why the Old Babylonians, who supposedly lived over 1000 years prior to the Persians, should have left their relics in a stratum that lies just below that of the Greeks. Not only would his denudation process have to remove the Persian

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<sup>73</sup> Emmet J. Sweeney, *Ramessides, Medes and Persians* (Forest Hills, NY 2000), pp. 6-7

stratum, but also all the intervening strata of the civilizations that had to be sandwiched between the Old Babylonians and the Persians. That is, not only would the topmost Persian stratum have to be denuded but also that of the so-called Kassites who came after the Old Babylonians, and the so-called Mitanni who came after the Kassites, then denudation had to erase the Chaldeans who followed and the Assyrian stratum which followed the Chaldean. After that, denudation had to remove the Medish stratum, then the so-called Neo-Assyrian stratum, as well as the Neo-Babylonian stratum along with that of the Persians.

Cochrane's denudation process not only removed the Persian stratum but also the Neo-Babylonian, Neo-Assyrian, Median, Assyrian, Chaldean, Kassite and Mitanni strata and then, for some unfathomable reason, stopped dead when the Old Babylonian stratum was reached. Why then didn't these denudation processes remove all these strata which are clearly found at other sites in Mesopotamia? This is an amazing feat for denudation processes to accomplish in order for the stratigraphy to support the established chronology. Why these processes selectively worked at the sites where an Old Babylonian stratum lies directly beneath that of the Greeks, but not at the other sites defies comprehension. And Cochrane has in no manner whatsoever addressed this problem.

This material objection to what Cochrane puts forth may be embarrassing for him, so that he takes a long hard look at what his defense of the established chronology actually requires at these sites. The denudation concept is absurd and Cochrane has not raised a scintilla of scientific-geological stratigraphy to refute Heinsohn. To accept Cochrane's concept requires selectivity that demands miracles.

(2) *At the Persian sites where settlement continued, the Persian level of occupation was severely damaged by intensive building activities in Hellenistic and Roman times.*

Regarding this explanation, we run up against the very same problem just discussed. At the sites where the Old Babylonian stratum is directly beneath the Greeks we again have to deal with all the intervening civilizations supposedly sandwiched between them. The same selectivity and chance come into play and negate this explanation as viable or even logical. As for

(3) *At most of the large sites excavated ... the mound was largely occupied by a palace-fort or other large building,*

the same chance selectiveness is required. Cochrane's analysis tells us nothing and explains nothing.

The most important stratigraphical evidence that neither Cochrane nor anyone else has addressed respecting Old Babylonian second millennium history in terms of stratigraphy is: Did this historical period of time ever exist? With respect to the geological evidence there was in *Pillars of the Past*, vol. I, chapter 9, "Mesopotamian Stratigraphy" a published report of geologist Ulrike Rösner dealing with the stratigraphical evidence of Tell Munbaqa which appeared in *Quartär*, Band 45/46 (1995), with a synopsis of it in *The Velikovskian*, vol. V, no. 4 (2003). The scientific stratigraphical evidence specifically covers the period of time in which the Old Babylonians ruled Babylonia. They supposedly lived sandwiched between the so-called Old Akkadian stratum, dated to around 2300 B.C., and the so-called Mitanni stratum of around 1500 B.C. There must be no 700- to 800-year settlement gap at this point in history if the Old Babylonians actually dwelt in Babylonia from around 2000 to 1600 B.C. But the geological and the archaeological evidence that follows from it is unequivocal, it proves that between these two civilizations there is no settlement gap, which means that the historical time to which the Old Babylonians are relegated by historians, archaeologists, and Heinsohn's critics does not exist, and never has existed!

This scientific evidence has a very profound impact for Heinsohn's chronology as well as for the established chronology. Since the evidence from Tell Munbaqa proves that that time period does not exist, except in the minds and imaginations of historians, archaeologists, and Heinsohn's critics, they cannot then ask about Assyrian palaces at Mari. Mari never existed at that time, either, nor can any historical or archaeological materials from that period be brought forth to negate Heinsohn until they (scientifically) prove that these people existed at that time.

While Cochrane spoke of having all the relevant sites thoroughly excavated at some future date to determine Heinsohn's thesis, to prove Heinsohn invalid requires scientific stratigraphical evidence to disprove the finding at Tell Munbaqa. That evidence simply does not exist. Cochrane has it all upside down and backward. The only site where Heinsohn's stratigraphical thesis was *scientifically* tested proved him correct. To then turn it around and argue, as

Cochrane does, that Heinsohn's stratigraphical theory needs to be tested elsewhere for this time period, begs the question and adroitly evades that evidence. The crux of the matter is that all of Heinsohn's critics refuse to deal with this solid stratigraphical contradiction to the established chronology and have buried their heads in the strata to avoid this profoundly embarrassing problem.

In fact, both Cochrane and the archaeologists at Tell Munbaqa have been adamant in their denial of this evidence.

### ON DARICS, TIN, AND DEAD ENDS

If the Old Babylonians are the Persian rulers of Babylonia living in the second half of the first millennium B.C., the time when coinage came into being, then it would be expected that some sort of evidence of their using coins would exist or be mentioned. On this question, Cochrane claims:

“Ancient coinage practices offer an excellent test for Heinsohn's thesis. As is well-known, archaeologists frequently employ coins in correlating various strata, since distinctive coins from one king or culture serve to provide a secure chronological context for their level of deposit. The practice of minting coins for commerce was first developed by the Lydians in the seventh century BCE. Cyrus the Great, upon conquering Lydia, appears to have begun minting coins of his own. ... Yet it was the coins issued by Darius (c. 515 [B.C.]) that were to become famous throughout the Persian empire. These gold coins became known as *darics*, and the silver ones as *sigloi*.”<sup>74</sup>

Then turning to the work of A. Bivar, Cochrane posits the problem:

“Such coins present seemingly insurmountable difficulties for Heinsohn's reconstruction. For if he is right in identifying Darius with Hammurabi, one would naturally expect to find hoards of gold *darics* in Old Babylonian strata at Mari, Hazor, and Tell el Rimah. Yet such coins are nowhere attested in [these cities'] strata, to the best of my

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<sup>74</sup> Cochrane, *op.cit.*, pp. 71-72



knowledge. Nor is coinage mentioned in any of the 25,000 texts that have been recovered from ... Mari, this despite the fact that these texts provide a wealth of detail on the currency used during this period. One might also expect to find coins showing Hammurabi in garb typical of the ... period. Once again such coins are not to be found. Yet a hoard of Persian coins was found at Babylon itself. How likely is it that Darius only minted coins in his Persian avatar, even when in Babylon?"<sup>75</sup>

Cochrane believes this evidence leads to “deadends” for Heinsohn. He claims “one would naturally expect to find gold *darics* in Old Babylonian strata at Mari,” etc. Why? Did the Persians trade gold *darics* throughout their realm as well as silver *sigloi*?

The fact of the matter is that gold *darics* were not a medium of trade in the Persian empire. As Pierre Briant explains:

“But why [did Darius] create a gold coin whose function was neither to facilitate trade nor to pay soldiers or suppliers? The answer, it is necessary to insist, is the political function of royal coinage. Not only would the royal image circulate widely by this means but also the innovation would in a way crown the achievement of Darius as a new founder of the Empire. This was the basic idea communicated by Herodotus when he wrote: ‘Darius wished to perpetuate his memory ... by something no other king had previously done.’ ... A similar expression is found in Polyclitus, quoted as follows by Strabo...: ‘[Polyclitus] says that in Susa one of the kings built for himself on the acropolis a separate habitation, treasure house ..., and storage places ... for what tribute they exacted, as memorials to his [good] administration ...’ In other words, Darius’s initiative was not fundamentally economic [in the sense we mean today]. It was intended less to pay his expenses than to illustrate his power. ... The Great King used the royal treasures [as] valuable objects that he could use as rewards; darics could also play this role.”<sup>76</sup>

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<sup>75</sup> *ibid.*, p. 72

<sup>76</sup> Pierre Briant, *From Cyrus to Alexander – A History of the Persian Empire* (Winona Lake IN 2002), p. 409

Olmstead, one of Cochrane's own sources, told him that circulation of coins was rarely if ever the case:

“Little of this vast sum [of gold and silver tribute sent to Persia] was ever returned to the satrapies. It was the custom [of the Persians] to melt down the gold and silver [into] ... bullion [to be] stored. Only a small portion was ever coined. Thus, despite the precious metals newly mined, the empire was rapidly drained of its gold and silver.”<sup>77</sup>

Why didn't Cochrane tell his readers this? Further, in another of Cochrane's own sources, we are told:

“In hoards from western Anatolia [in the region of the Persian mint near Sardis] sigloi are reported in enormous preponderance. In Achaemenid [Persian] lands further afield their occurrence is sporadic” [and] “the royal coinage hardly circulated at all in the eastern half of the empire.”<sup>78</sup>

Why didn't Cochrane inform his readers of this as well? At Britannica.com, “Daric”, we learn the same:

“Whereas hoards of [Persian] sigloi have been found almost exclusively in Asia Minor [near the mint at Sardis] and isolated examples have been found only with Greek currency in more distant lands (e.g. Egypt and Afghanistan) darics have been found in Asia Minor but also in Greece, Macedonia and Italy.”

Thus, Persian *darics* and *sigloi* are not found, as Cochrane seems to suggest, all across the Persian empire, but only as “isolated examples” and *darics* were found “only with Greek currency in more distant lands.” Why “only with Greek currency”? The answer seems to be that when Alexander the Great conquered Persia, he seized the gold and silver bullion and the few uncirculated coins, which were carried by some of his troops to Egypt, Afghanistan, Greece, and Macedonia, and later by the Romans to Italy.

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<sup>77</sup> Olmstead, *op.cit.*, p. 298

<sup>78</sup> *The Cambridge History of Iraq*, vol. 2, *op.cit.*, p. 619 and pp. 221-222

What, then, of the “hoard of Persian coins ... found at Babylon”? The fact is, only six *sigloi* were found there, also probably carried by Alexander’s troops. Cochrane further claims that there is no mention of coins in the vast archive at Mari. In order to understand this one must know the different ways that media of exchange were tallied in Persian Mesopotamia. Cook tells us:

“In virtually the whole of the rest of the [Persian] empire silver was used as currency, but generally in the form of bar-ingots or cut-silver [pieces] which had to be weighed for business transaction. ...”<sup>79</sup>

The same would apply to Darius’ *sigloi*. These coins were rarely circulated while other *sigloi* were minted by rulers of the various cities, some of which were found at Babylon and these were weighed and not counted. Hicks *et al.* explain:

“The sigloi differed somewhat from region to region and, to that extent, the practice compromised the uniformity of the imperial coinage. As a result [these coins were measured] ... only by weight. ...”<sup>80</sup>

Olmstead tells us, “the rulers of the merchant cities preferred to mint their own money ...”<sup>81</sup> Georges Contenau enlarges on this:

“The first coins with a wider currency in Western Asia were Persian ‘darics’ [around Sardis], but from the moment they were ‘invented’ these coins exercised the same function as bullion in the Western European economy ... never leaving the state treasury except when used in settlement between two countries for payment of mercenaries.”<sup>82</sup>

Lastly, we were just informed that the Persians paid mercenaries with coins; this was evidently the easiest method of keeping these foreign troops—often Greeks—from deserting. The same is reported by Olmstead: “Only a small portion

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<sup>79</sup> J.M. Cook, *The Persian Empire*, London/New York 1983, p. 70

<sup>80</sup> Hicks *et al.*, *op.cit.*, p. 73

<sup>81</sup> Olmstead, *op.cit.*, p. 189

<sup>82</sup> Georges Contenau, *Everyday Life in Babylonia and Assyria* (NY 1966), p. 89

[of Persian gold or silver] was ever coined. [It was] then usually [spent] for the purchase of foreign soldiers or of foreign statesmen.”<sup>83</sup>

Thus we have clear evidence that the Persians only circulated their coins by hiring mercenaries or bribing foreign statesmen. We now return to the Mari archives. Interestingly Hammurabi as well as his counterparts, the kings of Persia, were also hiring mercenaries and paying them with coins, something impossible in conventional Old Babylonian times. According to France Joannes, Hammurabi paid Mari soldiers with small pieces of silver impressed with a seal, along with other forms of weighted silver.<sup>84</sup> But what is a coin other than a piece of metal stamped with a die? Whether coins were in the form of rings or cut pieces of silver or circular pieces of silver stamped with a die, outside Persia these were all measured by weight. Therefore when a monetary transaction occurred that used coins these were referred to in terms of the forms in which the silver was used. They were referred to not as coins but as medals (Joannes refers to them with the French word *médailles*), or as rondels, circular stamped or cut pieces of silver stamped with a die, but are not referred to as coins, even when coins were used as a medium of exchange.

Daniel C. Snell writes on this issue, “Current views hold that coinage was devised as a way to standardize payments by states to mercenary soldiers.”<sup>85</sup> This clearly implies that the Hammurabi/Darius equation is correct because both use coins to pay mercenaries which could not occur in the second millennium B.C., but was possible in the first, and shows that Cochrane’s criticisms are not all “deadends” for Heinsohn.

To recapitulate what we have covered up to now: Imperial *darics* and *sigloi* were rarely circulated outside Anatolia. Only a small number were ever coined. Thus very few such coins would exist at sporadic sites. Outside Persia in its empire coins and all other forms of currency were weighed. *Darics* are only found with Greek coins which strongly implies these were circulated after Persia fell to Alexander the Great. Hammurabi, like his Persian counterparts, paid mercenaries with “medals” stamped with a die or seal—which apparently were coins. The coin

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<sup>83</sup> Olmstead, *loc.cit.*

<sup>84</sup> France Joannes, “Médailles d’argent d’Hammurabi?”, *Nouvelles assyriologiques brèves et utilitaires*, no. 4 (Décembre 1989), pp. 80-81

<sup>85</sup> Daniel C. Snell, “Methods of Exchange and Coinage in Ancient Western Asia,” *Civilizations of the Ancient Near East*, vol. III, *op.cit.*, p. 1495

hoard at Babylon does not contain *darics*, but only *sigloi*, which may very well have been minted by rulers outside Persia proper or carried there by Alexander's troops or perhaps other Greek mercenaries in the pay of Persia. Coins are mentioned indirectly in the Mari archive.

It is thus rather evident that all these dead end criticisms have run into dead ends. However, there is also another metal in particular that is mentioned in the Mari archive and this metal was actually found there at least 600 years before it ever came into Mesopotamia. That metal was tin used to make tin bronzes. James Muhly reports

“... tin was shipped to Mari in the form of ingots ... and there stored in the various parts of the palace. ... From Mari the tin was sent to a number of well-known ... sites. ... From there according to a balanced account text known as the Mari Tin Inventory, it was shipped to Crete.”<sup>86</sup>

Anyone who has read *Pillars of the Past*, vol. I, understands that tin for the production of bronze, based on the established chronology, does not come into Mesopotamia until after 1100 B.C., that is about 500 years after the Old Babylonian empire fell or over 650 years after Mari was destroyed by Hammurabi. Since tin comes to Mesopotamia after 1100 B.C., the Old Babylonians could only have tin if they existed after 1100 B.C. Thus it makes perfect sense that the Old Babylonians lived in Persian times.

Cochrane has claimed that the lack of royal gold *darics* or silver *sigloi* coins from certain cities, Mari among them, poses “seemingly insurmountable difficulties for Heinsohn.” This was shown to be a criticism with very little if any merit at all. But the evidence of the tin trade and discussion of tin in the Mari Tin Inventory tablet poses truly insurmountable and intractable problems for Cochrane and the various historians he depends upon.

As late as 2001 and 2002 Gwendolyn Leick admitted with respect to the sources of tin from 1900 to 1800 B.C., that is, Old Babylonian times, that “tin

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<sup>86</sup> James D. Muhly, “The Bronze Age Setting,” *The Coming of the Iron Age*, T.A. Wertime, J.D. Muhly eds. (New Haven 1980), pp. 37-38

was brought to Ashur ... from somewhere in the east, probably Afghanistan.”<sup>87</sup> Since she has no real evidence for the source or sources of tin she says it came “probably [from] Afghanistan.” If it was truly known whence the tin came when she wrote and was published, she surely would have informed her readers.

### **THE IRON-CLAD LAWS OF HAMMURABI: FURNACES AND GLASS**

Cochrane states:

“Given Hammurabi’s renown as a lawgiver, Heinsohn is at pains to depict Darius in a similar light. ...

“That Darius was considered the greatest lawgiver of antiquity is little more than a figment of Heinsohn’s imagination. ...

“The primary source for Heinsohn’s position here is Olmstead, who wrote as follows of Darius’ laws:

“‘Darius, however, was determined that he should be ranked with Hammurabi as a great lawgiver. Fortune was not so kind. While tablet after tablet has been unearthed with extracts from Hammurabi’s casebook, the Ordinance of Good Regulations [Darius’s laws] has been so completely lost that it is actually necessary to prove that it ever existed ..., but there is not enough [of this material] for comparison with the treatment accorded in the earlier lawbook [i.e., Hammurabi’s].’

“Heinsohn, needless to say, takes the absence of Darius’ laws as a point in his favor, since he believes Hammurabi’s laws were the laws of Darius. ...

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<sup>87</sup> Gwendolyn Leick, *Mesopotamia – The Invention of the City* (London 2002), p. 200

“Yet, despite the claims of Olmstead, most scholars have expressed doubts about Darius’ standing as a great lawgiver. ...”<sup>88</sup>

And indeed, Cochrane then cites these doubtful scholars who disagree with Olmstead. He cites T. Cuyler Young, Amelia [*sic*] Kuhrt, and J.M. Cook. But this is consensus as proof; majorities, super-majorities, or totalities of historians in agreement do not determine historical truth for this matter or any other. Science determines truth even when everyone disagrees with what it shows. However, Cochrane’s harshest criticism has to do with the fact that other kings who seem to have reigned after Hammurabi and before Darius employed the same wording of the laws as in Hammurabi’s Code.

“It is important to note, however, that even if Olmstead is right as to Darius’ standing as a lawgiver, the upshot of his discussion remains absolutely fatal to Heinsohn’s historical reconstruction. For as Olmstead points out, various ancient kings, including Sargon and Assurbanipal, quoted from Hammurabi’s famous law code. ...”<sup>89</sup>

Cochrane readily admits that these kings are actually identified by Heinsohn as being Persian kings in the footnote on the same page, undermining the point he raised in the text though some readers may not read this. But he disagrees with this. Cochrane goes on to cite Olmstead’s conclusion:

“ ‘In view of all these detailed parallels, there can no longer be any reasonable doubt that Darius and his legal advisers had before them an actual copy of Hammurabi’s lawbook. Quite possibly he used the original stele preserved ... at Susa. ...

“ ‘Continued use of Hammurabi’s collection [of laws] was possible for well beyond a millennium [after him]. ... As such, it was adopted for use by the Persian conquerors. Cyrus [who reigned before Darius], in an Akkadian proclamation intended for Babylonian reading, does sincere homage of the great lawbook by imitating its very

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<sup>88</sup> Cochrane, *op.cit.*, p. 64

<sup>89</sup> *ibid.*, p. 65

phraseology. That this was no mere lip service is proved by a document of his third regnal year which [exactly as Hammurabi] bases the decision on the “king’s judgments”.’

Cochrane adds: “No doubt Heinsohn missed this paragraph.”<sup>90</sup>

The basic argument Cochrane raises is that since there are various rulers who reigned prior to Darius that were using the very same wording or phraseology as in Hammurabi’s Code, that code had to have been originally written by Hammurabi long before Darius lived, which precludes identifying one king with another.

One major problem with Cochrane’s argument is that whilst these kings who lived before Darius as well as their advisers were evidently knowledgeable of Hammurabi and his famous code, while they copied it, they failed to mention Hammurabi. With all their familiarity with Hammurabi’s Code, with its phraseology, its laws, and cases, they never mentioned Hammurabi by name. Is this reasonable? I suggest that if they were so well aware of the code they were copying, they would have known Hammurabi and mentioned him *in Persian times prior to Darius*. To paraphrase what Cochrane wrote above regarding coins:

To the best of my knowledge, there is not a single notation or mention of Hammurabi’s name by any Persian king prior to Darius, or of any non-Old-Babylonian kings after Hammurabi. Nor is his name mentioned in any of the Persian texts that have been recovered from these earlier Persian kings before Darius, this despite the fact that these texts provide a wealth of detail about Hammurabi’s Code. One would also expect to find other Persian documents dated prior to Darius, unrelated to law, mentioning Hammurabi’s name. These are nowhere to be found. Yet Hammurabi’s reputation in Mesopotamia must have been as well-known as his laws. How likely is it that every Persian king prior to Darius who used Hammurabi’s Code as well as the scribes, advisors, etc., did not know of and failed to mention Hammurabi’s name somewhere in their documents, when they had verbatim texts of his code before them, supposedly with his name on them?

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<sup>90</sup> *ibid.*, pp. 65-66



There is a further piece of evidence related to the age of the code that seems to have escaped Cochrane's notice. The fact of the matter is that this form of code goes back to early ancient history even prior to Hammurabi/Darius, based on the established chronology. This was fully discussed by Cochrane's own source, C.G. Gadd's article "Hammurabi and the End of his Dynasty."<sup>91</sup>

"It has been observed ... that the abiding pillar of Hammurabi's fame is that celebrated 'code' of laws, the revelation of which places him among the greatest figures of ancient history. His achievement is ... no longer without comparison and challenge. The existence of Sumerian laws has long been known by survival of examples — these were attributed to Lipit-Ishtar of Isin, and a part of his actual text has now been recovered, having prologue, corpus, and epilogue in complete form of Hammurabi's 'code'. Still more closely comparable, not merely in form but in content and perhaps even earlier are the laws of Eshnunna. These were written in Akkadian scarcely distinguished from the phraseology of Hammurabi; and they [as with Hammurabi's Code] were issued with a short preamble, and probably an epilogue, if the text were preserved. In the portion now extant they deal with prices and tariffs, and are much concerned with valuation especially of damage sustained, have something to do with family affairs, marriage and divorce, and touch upon sales and deposits, slavery and theft. They even include usage of the same three terms 'man', 'subject', and 'slave', as are held to indicate a threefold division of society in the Babylonian code [of Hammurabi]. At about the same time as these various bodies of law were being promulgated, there was reigning in the more distant and supposedly more backward land of Elam a prince named Attakushu, and he too is now known to have set up in the market of his capital a 'stele of righteousness', evidently surmounted by an image of the sun god under which was inscribed a (possibly adjustable) list of fair prices. Nor is this all, for not only is there a legislative act of a special kind issued by Ammisaduqa, the fourth successor to Hammurabi, but it is now clear that similar measures were put in action by a whole succession of kings."

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<sup>91</sup> C.G. Gadd, "Hammurabi and the End of his Dynasty," *The Cambridge Ancient History*, vol. 2, pt. 1 (1973), pp. 187-188

Cochrane has suggested that since Darius copied these laws from Hammurabi as did others before Darius, he could not be Hammurabi. How could he copy a set of laws he had invented? But this process of copying laws from earlier periods and places was nothing new in Mesopotamia. The fact that it was common practice proves nothing with respect to Heinsohn's Darius/Hammurabi equation. As the same figure, Darius/Hammurabi merely used the same legal tradition, but that in no way precludes Heinsohn's equation. The argument that Darius copied these laws is voided by the fact that Hammurabi also copied them. Darius/Hammurabi did not invent these laws; such codes were a tradition in Mesopotamia. Having read Gadd on this point, Cochrane failed to report these facts to his readers. This is the same problem we found regarding Cochrane's critique again and again. With regard to Gadd's material on this question, in Cochrane's own words, "No doubt Cochrane missed this paragraph."

But all this historical evidence pales before the scientific and technological evidence as these impinge upon the Hammurabi/Darius Code, which brings us back to Cochrane:

"As a fellow who otherwise emphasizes physical remains to the point of fixation, however, Heinsohn's position here [on Hammurabi's Code] is hardly consistent. In order to believe in Darius' status as a great lawgiver, one would naturally like to see some physical evidence.

...<sup>92</sup>

This requirement applies not only to Heinsohn but to Cochrane as well. He has produced no scientific or technological evidence in his refutation. To put this in Dwardu Cardona's words, "it should serve to caution people not to be so rash in accepting *anyone's* conclusions without first testing them."<sup>93</sup>

Let us put this question to a scientific and technological test. This is just what is required in terms of forensic historical analysis in order to determine if the famous code was written around 1750 B.C. or around 500 B.C. Cochrane and just about all of the historians agree that it was carved into stone in the early second millennium B.C., but scientific/technological evidence must determine that. If, as

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<sup>92</sup> *ibid.*, p. 65

<sup>93</sup> Dwardu Cardona, "A Return to the Two Sargons and Their Successors," *Aeon*, vol. 5, no. 4 (July 1999), p. 35

Heinsohn, Sweeney, Rose, and I claim, it was carved into stone in the mid-first millennium B.C., the same must apply.

The heart of the question comes down to how the code was engraved in a large tablet of diorite. This has already been discussed in Chapter 10 of *Pillars of the Past*, vol. I. Carving diorite necessitates the use of steel and certain gem stones which are only known to have been used long after the Old Babylonian empire fell.

How could Hammurabi's artisans have carved these laws in diorite, the "steely stone", with copper tools before steel had been invented? This problem has been generally ignored by all historians as well as by Cochrane. No one has ever provided a public demonstration by fully carving a statue of diorite and engraving it with narrow and deeply incised cuneiform or hieroglyphic characters with only copper tools and sand abrasive.

Rather than rehash the evidence from my earlier book, let us concentrate on the problem of manufacturing steel in the Bronze Age. To produce steel one must have a furnace or kiln that can reach the necessary high temperature at which iron ore can be melted. Bronze Age kilns could not generate the requisite temperature that will permit this iron extraction process to work. Gordon C. Baldwin specifically states:

"Pure iron melts at 1530 [degrees] Centigrade [Celsius], too high for Bronze Age furnaces, as compared with copper's melting point of 1083 degrees Centigrade."<sup>94</sup>

Alfred Lucas, along these same lines writes that pottery

"... is baked [in kilns] in order to drive off the chemically-combined water [in the clay], the loss of which is necessary to convert the clay from its original weak friable [easily deformable] state; in which it is softened by water. This reaction takes place between 500°C (937°F) and 600°C (1,112°F)."<sup>95</sup>

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<sup>94</sup> Gordon C. Baldwin, *Inventors and Inventions of the Ancient World*, (NY 1973), p. 195

<sup>95</sup> A. Lucas, *Ancient Egyptian Materials and Industries*, 4th ed. (London 1962), p. 191

The ancient, so-called Bronze Age, Old Babylonians could not produce steel until they had furnaces capable of doing so, and these were not available in Hammurabi's time. In Hammurabi's time, steel was, according to the established chronology, unknown and technologically could not be produced with furnaces that only reached temperatures that could melt copper or bake clay but not melt iron. How then could Hammurabi's Code be engraved in diorite without steel? In Persian times steel was undoubtedly available as well as the gem stones and other hard minerals to cut this exceedingly hard stone. From the forensic evidence it appears rather obvious that no one carved the Darius/Hammurabi Code in diorite in the early second millennium B.C.

But even worse for Cochrane is the fact that iron is mentioned in the archives at Mari, destroyed about 450 years prior to the assumed Hittite first production of iron. Sir Leonard Woolley informs us that the "earliest mentions of iron are in a letter from Mari mentioning an iron bracelet from the king of Carchemish."<sup>96</sup>

In fact, an iron object was recovered, "found at Mari near the pre-Sargonid Temple of Ishtar."<sup>97</sup> Saggs, in discussing Mari, admits "Iron is occasionally mentioned [at Mari] and has even been found in [Old Babylonian] excavations."<sup>98</sup>

No one, I dare say, has evidence to prove that iron was extracted and smelted in the early second millennium B.C. or smelted in furnaces that could attain the requisite temperature.

While tin was discussed earlier, it is interesting to note that in Hammurabi's Code specific statements are found that mention bronze hundreds of year before tin for bronze production was available in Mesopotamia. Olmstead reports "Hammurabi announces that if a physician operates with a bronze knife. ..."<sup>99</sup> How could Hammurabi mention bronze in his code hundreds of years before tin was available in Mesopotamia to make it?

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<sup>96</sup> Hawkes and Woolley, *op.cit.*, p. 563; see also Muhly, *op.cit.*, p. 75

<sup>97</sup> Jane C. Waldbaum, "The First Archaeological Appearance of Iron, *The Coming of the Iron Age*, *op.cit.*, p. 70

<sup>98</sup> H.W.F. Saggs, *Everyday Life in Babylonia and Assyria* (London, NY 1965), p. 70

<sup>99</sup> Olmstead, *op.cit.*, p. 131; see also H.W.F. Saggs, "Foreword," *The Greatness That Was Babylon* (NY 1962), p. XVIII

The theory that the Old Babylonians had steel calls to mind Otto von Bismarck's remark regarding the prime minister of England. He "is only a lath of wood painted to look like iron."<sup>100</sup> How does Cochrane explain these metallurgical, scientific and technological facts?

All this material, taken with astronomy, geology, archaeology, 30-day months for two sets of Persian/Old Babylonian kings, tin bronze and now furnaces and steel to carve diorite, points undeniably to Heinsohn's thesis which grows ever stronger. But there is more.

## GLASS

Not only does Hammurabi have iron and tin bronzes hundreds of years prior to the earliest production of these metals, but the Old Babylonians also possessed glass long before the process of making glass was known. Samuel Kurinsky states:

"Alalakh was at the time under the rule of ... King Hammurabi's son ... in the ruins of whose palace Sir Leonard Woolley [found] glazed ... fritware [the precursor of glass production] ... In the very next level of that residence of Hammurabi's grandson ... a fully matriculated glassmaking technology is unmistakably evidenced by the presence of intricately wrought polychrome true glass objects."<sup>101</sup>

The problem related to finding glass so early is that its production comes out of iron smelting processes and therefore after they had been developed. Here is a description of the astonishment that physical archaeologists and historians felt when George Bass discovered a shipwreck on the Mediterranean sea bottom:

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<sup>100</sup> Ringo, *op.cit.*, p. 198

<sup>101</sup> Samuel Kurinsky, *The Eighth Day* (Northvale NJ; London 1994), p. 131; see also Sir Leonard Woolley, *Alalakh, An Account of the Excavations at Tel-Atchana in the Hataq* (Oxford, UK, 1995), pp. 279-302

“The discovery of ... glass ... and tin ingots on a vessel wrecked in the fourteenth century B.C.E. off the Turkish coast burst upon skeptical scholars as a bombshell. The inclusion of glass ingots was a particularly intriguing and mystifying element of the cargo. The sheer size and quantities of the glass ingots found on board was ... incontrovertible evidence of a ... Mesopotamian [glass] pyrotechnology ... in the fourteenth century B.C.E. Yet no trace of such trade, let alone of the people who were involved in it, had hitherto made its historiographical appearance, as a diligent search through literature and the archival material of museums made plainly evident.”<sup>102</sup>

As we earlier found with iron, “pure iron melts at 1530° Centigrade, too high for Bronze Age furnaces,” etc. To make glass from its primitive ingredients requires approximately the same temperature as copper. However, to make the twenty-five pound ingots of glass found in the shipwreck requires these levels of heat over sustained periods of time:

“[The] melting point of sand ... is about 1,700°C (3,092°F) ... by adding about 25 percent of sodium oxide to silica [sand] the melting point is reduced from 1,723° to 850°C (3,133° to 1,522°F). But such glasses are easily soluble in water. ... The addition of lime (calcium oxide or CaO), supplied by limestone, renders the glass insoluble again, but ... makes the glass prone to devitrification.”<sup>103</sup> [i.e. glass which has been covered by a white scum that will not permit light to go through.]

That is, by adding a flux to the sand one lowers the melting point but the type of glass produced is so-called “water glass,” glass that is easily soluble in water, and thus of little value as a container for liquids. This type of glass cannot be used as can true glass. Even when limestone is added as a stabilizer to the water glass mixture, the glass tends to lose its glassy luster and transparency, becoming devitrified. Nevertheless, the glass found in an Old Babylonian site was in fact “true glass.” That means it was melted from sand without a flux to lower the temperature. The furnace temperature had to be 1,723°C to 1,400°C, which is even higher than the temperature at which iron melts to slightly lower than that temperature. No

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<sup>102</sup> *ibid*, p. 133

<sup>103</sup> “Glass,” *Encyclopaedia Britannica Micropedia*, vol. 5 (Chicago 2002), p. 297

furnaces capable of generating these temperatures existed in that Old Babylonian period.

“The engineering of pneumatically drafted, reverberatory furnaces capable of producing such glass hulks [requires] a sophisticated pyrotechnology that shattered the assumptions of most historians [that such furnaces only came into being in the first millennium B.C.]. A temperature of some 1,100 degrees Celsius (about 2000 degrees Fahrenheit) must not only be attained but unremittingly maintained by pumping a steady draft of air through the flaming furnace by means of one or more bellows for at least four days and nights to produce an ingot such as that found on board the ancient ship.”<sup>104</sup>

Kurinsky further explains:

“The process by which siliceous stone is transformed into glass was invented but once in the course of human history. ... The pyrotechnology required to produce glass is even more advanced than that of smelting iron from its ore.”<sup>105</sup>

The steps required for making glass out of its basic constituents are many and complex ones:

“The production of both glass and iron requires the use of a pneumatically drafted furnace, a technology that goes beyond what is needed to produce copper and bronze. The production of glazes and glass depends on an advanced chemical knowledge including a familiarity with the properties of a variety of unrelated materials that have to be refined from ores mined in distant lands. The knowledge of the minerals, their sources, and the ability to obtain them bespeaks a considerable scientific and commercial capability.”<sup>106</sup>

We are asked to believe that several hundred years before iron technology was introduced into Mesopotamia, the people there developed a more

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<sup>104</sup> Kurinsky, *op.cit.*, p. 139

<sup>105</sup> *ibid.*, p. 174

<sup>106</sup> *ibid.*, p. 34

highly advanced pyrotechnology than that required to make iron in order to allow the Old Babylonians to have a “fully matriculated glassmaking technology” with specimens of “intricately wrought polychrome true glass objects.” And yet they failed to have the ability to produce iron according to the established chronology.

As we know, the sources of tin to make tin bronze are not known to exist prior to 1100 B.C. based on the same chronology. The ship contained both glass and tin ingots which would be impossible at this early period. In the earlier Old Babylonian period, glass, tin, and iron in concert is a triple contradiction to that chronology. In the same way the historians produce tin bronzes from sources that exist nowhere in the early second millennium B.C. They now must invent, or conjure up, methods by which iron, tin, and glass could be produced in the Old Babylonian period. As Colin Renfrew remarked:

“Any theory needs at least a few supporting facts.”<sup>107</sup>

There are no scientific or technological supporting facts to place the Old Babylonians in the early second millennium B.C., but undoubtedly many to place them with the Persians 1000 years later.

## **AGRONOMY, ECONOMIC INFLATION, AND SOCIETAL COLLAPSE IN OLD BABYLONIAN/PERSIAN TIMES**

“When agricultural land and its productivity falls below a certain standard in relation to population ... products become either prohibitively high in price or altogether unobtainable.”

Louis Bromfield, in  
*The New Dictionary of Thoughts*  
(NY 1961), p. 16

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<sup>107</sup> Colin Renfrew, R. Whitehouse, “The Copper Age of Peninsular Italy and the Aegean,” *Annual of the British School of Archaeology in Athens*, vol. 69 (1974), p. 38



Near and dear to this author's heart is determining as closely as possible when the global climatic changes described by Velikovsky happened. By organizing the chronological record into one that is more complete and accurate, we may determine "when" these climatic changes occurred and the effects they must have had on ancient humanity. With the present established chronology that climate record fails to correlate with the historical record. Harvey Weiss, who has long analyzed this, states the problem thus:

"The archaeological and paleoclimatic data [are of] unrepresentative and essentially of ahistorical quality [with regard to] ... the epigraphic [documentary] record, particularly for early historic Mesopotamia. Here the cuneiform record misses the early historic climate change. ..."<sup>108</sup>

Our interest is that of the climate event of the 8th century B.C. described in Chapter 14 of *Pillars of the Past*, vol. I, which discussed irrigation agriculture. There we raised the issue of a climatic change triggered by a pole shift. In volume I, pages 440-443 we further discussed evidence for climatic changes which we date to the first millennium B.C. in Ur, Syria, Greece, Anatolia, and Harappa. This suggests that, based on the short chronology now being discussed, there should also be indications for dating this climatic change around the time of the Old Babylonian era. According to Saggs:

"Something must have triggered the [Old Babylonian] MAR.TU into moving outside their normal range, and there are indications that the main factor was climatic change. Excavations at sites of ancient cities in Syria north of the Euphrates suggest that drier conditions set in about this time. ... The drier period appears to have continued for several centuries. ..."<sup>109</sup>

This would suggest that the climate changed around 1500 B.C. However, the lakes that existed would have taken anywhere from decades for smaller ones to a century or so for larger ones to dry up. Ground water levels would also

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<sup>108</sup> H. Weiss, "Beyond the Younger Dryas Collapse as adaptation to abrupt climate change in ancient West Asia and the Eastern Mediterranean," *Environmental Disaster and the Archaeology of Human Response* (University of New Mexico, NM 2000), p. 99

<sup>109</sup> H.W.F. Saggs, *Babylonia* (hardcover ed.) (Berkeley CA, 2000), p. 91

have begun to fall over decades to centuries, creating the arid conditions that presently persist throughout most of the Near East.

Based on the short chronology this climatic change along with irrigation, salinization evidence well correlates with the rest of the forensic historical evidence.

In Chapter 14 of *Pillars of the Past* vol. I, there was presented an extensive discussion of how irrigation agriculture in southern Mesopotamia—that is, Babylonia—destroyed the soil because of salinization. It was shown there that this climate shift had to have occurred then since it would have been impossible for civilization to be sustained by irrigation agriculture there for well over 3,000 years. The length of time before the region would have had to be abandoned, based on the best estimates of the agronomists, was between 300 and 400 years. From around 780 B.C. and coming forward in time to allow the land to be poisoned by salt requires that the region of Babylonia—mainly south of Babylon—collapsed in Persian times, and more specifically toward the end of the Persian empire. Therefore, if the Old Babylonians are indeed the Persian rulers of Babylonia they had to have lived during the time this salinization process of poisoning the soil became so extensive and catastrophic that southern Mesopotamian society collapsed.

As part of this collapse certain economic conditions must occur that well define this event, as Bromfield pointed out above, namely “prohibitively high prices” for food, or food that was “altogether unobtainable” or at least very difficult to obtain. For the new reader unfamiliar with this material, let us briefly digress to recapitulate how and why this process occurs. In Sandra Postel’s recent book we find:

“All river water and ground water contains dissolved salts. During irrigation, plants take water up from the soil but leave most of the salts behind. In humid climates rainfall percolating through the soil [washes] the salts out of the root zone. But in dry climates [such as that in Mesopotamian Babylonia], farmers must apply extra water to do the job. This additional water can lead to even greater problems, especially in low-lying river valley [plains], where much of [ancient Mesopotamia’s] irrigation [took] place. As more and more water seeps through the soil to the ground-water below, the water table rises. As it nears the surface some of the water vapor evaporates leaving the salts

behind. If the problem is not corrected [as it was not in this ancient region], the buildup of the salts poisons the land, rendering it toxic to crops.”<sup>110</sup>

As the soil became ever more damaged, irrigation canals would have to be lengthened outward to bring water to fresh, new, more distant land on the Babylonian plain. This required that the kings must constantly move farmers farther afield from the lands where salt had made agriculture impossible. And this is just what happened, as Daniel T. Potts explains:

“The year formulae make it clear that the provisioning of their people with sweet water [that was not over-laden with salt], and the opening up of new tracts of land for agricultural exploitation, were of paramount concern for these [ancient Mesopotamian] kings.”<sup>111</sup>

On this question Oppenheim also states “The digging of new canals and the resettlement of the population on new soil formed an essential part of the economic and political program of a responsible sovereign. ...”<sup>112</sup>

In spite of this, over a period estimated to be no longer than 300 to 400 years, there would be almost no suitable land, or very little, available for agriculture. The price for good land would have risen and, over time, as fewer and fewer crops could be cultivated and reaped, the price for these would rise. Along with this price rise would follow interest rates, inflation as well as the desperation of the people caught up in this growing agrarian/economic catastrophe. Based on Heinsohn’s thesis that the Old Babylonians are the Persian rulers over Babylonia, these events should be mirrored in the Persian/Old Babylonian history.

For Persia we are told that early on, well before the salinization catastrophe occurred, Darius’ reign brought great prosperity to Babylonia. Hicks *et al.* explain:

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<sup>110</sup> Sandra Postel, *Pillar of Sand. Can the Irrigation Miracle Last?* (NY 1999), pp. 18-19

<sup>111</sup> Daniel T. Potts, *Mesopotamian Civilization. The Material Foundations* (London 1977), p. 21

<sup>112</sup> A. Leo Oppenheim: *Ancient Mesopotamia* (revised ed., completed by Erica Reiner, Chicago & London 1977), p. 548

“Exploiting their own uncommonly wise statesmanship and managerial excellence, they [the Persians] ushered their newly unified world into an era of increasing trade and improving living standards such as mankind had never before experienced.”<sup>113</sup>

They further show that “all parts of the [Persian] empire shared unprecedented prosperity.”<sup>114</sup> If this was the case with Darius, then, based on Heinsohn’s thesis, this heightened prosperity should also be one of the hallmarks of Hammurabi’s reign. And this, too, is the case. “It remains clear that [Hammurabi’s] reign and time were marked by much higher material prosperity.”<sup>115</sup> Or, as Sabatino Moscati shows, “Under Hammurabi the ... cultures which compose Mesopotamian civilization, [Old] Babylonians [among them] achieve complete and harmonious fusion.”<sup>116</sup>

Interestingly, the Persians/Old Babylonians began to tax their prosperous subjects to the utmost. I repeat how this taxation depleted Babylonia of cash:

“So long as the empire lasted it seems that only a small portion of the gold and silver that went to swell the imperial [Persian] treasury was put back into circulation. There was a chronic shortage of cash in Babylonian Achaemenid [Persian] times.”<sup>117</sup>

When we turn to Old Babylonian times, we find the same high taxation was the rule:

“... under his [Hammurabi’s] reign the provincial administration systematically by-passed authorities and was geared mainly for the enrichment of the distant capital in Babylon.

“It exploited the conquered territories. ...”<sup>118</sup>

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<sup>113</sup> Hicks *et al.*, *op.cit.*, p. 9

<sup>114</sup> *ibid.*, p. 71

<sup>115</sup> *CAH*, vol. 2, pt. 1 (1973), p. 218

<sup>116</sup> Sabatino Moscati, *The Face of the Ancient Orient* (NY 1962), p. 65

<sup>117</sup> *The Cambridge History of Iraq*, vol. 2, *op.cit.*, pp. 221-222

<sup>118</sup> Norman Yoffee, *Collapse of Mesopotamian States and Civilization* (Tucson AZ, 1988), p. 52

The problem or, more accurately, the contradiction involved is that as the amount of money or silver and gold mediums of exchange became scarcer in Babylonia during late Persian times, instead of prices for food, land, interest rates, etc. falling, because less money was chasing the same or similar amounts of goods, inflation followed. Hicks *et al.* wonder about this paradox:

“It is a paradox that a shrinking money supply could cause inflation—it is generally [the over-] availability of money that abets inflation today. But in the economy of Persia scarcity of cash caused by hoarding an opposite effect, largely by forcing up interest rates for those compelled to borrow cash. . . . People required to pay their taxes in cash [mainly silver] had to mortgage themselves to banks to raise money.”<sup>119</sup>

Let us examine this explanation from basic economy theory. The historians have nothing to suggest, from their viewpoint, to show that the amount of crops and woolen goods traded from Babylonia to the rest of the Persian empire decreased. Given a good agricultural base, Babylonia would have abundant agricultural products to trade throughout Persian times. Saggs elucidates:

“The [agricultural] goods Babylonia had available for trade were principally foodstuffs, of which it had a surplus, so long as it kept its irrigation system in order. This was an important resource since there were parts of the ancient Near East dependent upon rainfall, which not infrequently suffered crop failures and famine.”<sup>120</sup>

Hence there would have been a constant influx of silver into Babylonia to pay for these necessities. That influx of cash would circulate in the region and would in turn be available to pay the Persian taxes that were levied. There would not be a chronic shortage of silver under these conditions. If there was a chronic shortage of cash throughout the empire, we would have heard of inflation being endemic everywhere. But such is not the case.

So long as the Babylonians had abundant agricultural goods to sell to the ancient world their income was stable. According to Herodotus, Assyria was the

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<sup>119</sup> Hicks *et al.*, *op.cit.*, p. 84

<sup>120</sup> Saggs, *Babylonia*, *op.cit.*, p. 100

wealthiest satrapy in the Persian empire. As noted, Darius' rule "ushered their newly unified world into an era of increasing trade and improving living standards such as mankind had never experienced." This trade guaranteed that wealth would flow into Babylonia.

Conversely, since the Babylonians through massive irrigation were poisoning their land with salt, over time the great surplus of grain and wool that they sold to the rest of the ancient world would slowly but inevitably diminish. They would have less and less surplus food and wool to trade and their income would fall in tandem with the corrosive loss of productive soil. Hence, they would have less and less silver to pay their Persian overlords in taxes. This economic relationship between the commodities they sold and the income they derived from them is a basic datum of economic theory. When you sell less, your income is lower.

David C. Colander's college text, *Economics*, explains how this form of inflation develops. When demand for goods outstrips supply we develop what is termed "demand-pull inflation." It occurs when there is

"... a gap between the quantity [of goods] demanded and the quantity supplied [which] leads to upward pressure on price. [Even] when the majority of industries [or agricultural producers] are at close to capacity [production] and they experience increases in demand, we say there's demand-pull pressure, and the inflation that results is called demand-pull inflation. ..."

"DEMAND-PULL INFLATIONS ARE GENERALLY CHARACTERIZED BY SHORTAGES OF GOODS. ..."  
[capitalization added]<sup>121</sup>

What created the inflation in Persian Babylonia was the diminished supply of food that the indigenous population still had to buy to live, caused by a dying land that produced fewer and fewer agricultural products. It comes down to the fundamental economic law of supply and demand. People have to have food, but as this necessity for life itself diminishes, people will pay more and more to obtain it. They have no choice. Their demand is inelastic. Robert Claiborne explains: "A rise in the price of grain ... suggests a period of drought [or other

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<sup>121</sup> David C. Colander, *Economics*, 2nd ed. (Chicago 1995), p. 152

agricultural catastrophe].”<sup>122</sup> In fact, Briant admits, “we do not see how inflation could be connected with the lack of liquid assets. Instead stagnation/deflation is a more likely result.”<sup>123</sup>

The other method of creating inflation is an over-supply of money let out in the economy which was certainly not the case in Babylonia. Colander tells us: “Economists of all persuasions agree that large inflations can continue only if the central bank issues large amounts of money.”<sup>124</sup>

Therefore along with the rise in food prices goes the price of land which is also in great demand but in decreasing supply. What follows is a constant rise in the cost of living and so too in interest rates. This too follows from basic economic laws. If a lender before inflation rises received, say, a rate of five percent on the money he lent, but the cost of purchasing commodities then doubled, his income from the loan would be cut in half. To offset this loss, the lender has to double the interest rate he receives to maintain the same real income. Colander describes this further effect of inflation:

“Inflation results when more people on average raise their nominal prices. ... Why [do] people raise their nominal prices[?] The logical answer is that they believe that in doing so they can get a larger slice of the output pie for themselves. But shares of the pie are determined by relative, not nominal prices [which are related to the cost for all other goods] ... say you raise your nominal price by 10 percent but everyone else does, too. So the prices of the goods you sell go up by 10 percent and the prices of the goods you buy go up by 10 percent. Your nominal price has gone up, but your relative price has not, and you’re no better off.”<sup>125</sup>

So, too, with interest rates. Inflation in Babylonian Persia was not the result of an oversupply of money but the result of an undersupply of food and land to grow it.

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<sup>122</sup> Robert Claiborne, “How Has Climate Affected History”, *Mysteries of the Past*, (NY 1977), p. 235

<sup>123</sup> Briant, *loc.cit.*, p. 804

<sup>124</sup> Colander, *op.cit.*, p. 458

<sup>125</sup> *ibid.*, pp. 151-152

Since this is the case with the Persians the same should be the case with the Old Babylonians. Interestingly, the historians who discovered they too experienced inflation do not blame this rise in prices, land costs, and interest rates only on over-taxation. Instead they attribute it to an agricultural crisis. M. Liverani offers this explanation:

“A serious agricultural crisis and the inability to obtain provisions from the north caused an alarming increase in the prices of staple commodities in the capital [where] grain prices increased sixty-fold, fish, fifty- and oil six-fold.”<sup>126</sup>

At first this centralization of irrigation would greatly enhance agricultural production. As Forbes shows:

“The conquest of Hammurabi of the entire Tigris Valley, and that of the Euphrates ... led to strong centralized control of irrigation and resultant prosperity. Several laws in his code deal with irrigation.”<sup>127</sup>

Yet over 300 to 400 years this would hasten the time when salinization would insidiously take its toll as described by Norman Yoffee:

“The agricultural situation in Mesopotamia, in which salinization was an ever-present threat to productivity ..., became exacerbated in the aftermath of Hammurabi’s centralization of the realm. In the period of the last kings of his dynasty, prices for agricultural products rose and seeding ratios per unit of land increased ... the same occurred in Ur III. ... There may have been a decision to abandon or shorten the period of fallow on the lands the Crown controlled, thereby providing short-term fiscal relief, since the lands would initially provide more grain, but ultimately that would result in a loss of productivity. Although this process cannot be observed directly from available sources, we do see

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<sup>126</sup> M. Liverani, “The Amorites,” *Peoples of Old Testament Times*, D.J. Weiss, ed. (Oxford UK, 1973), p. 110

<sup>127</sup> R.J. Forbes, “Extracting, Smelting, and Alloying,” *A History of Technology*, Charles Singer *et al.*, eds., vol. I (London 1954), p. 548



inflated prices, new methods of agriculturally intensive management, and an increased pressure to secure stores of grain.”<sup>128</sup>

This of course led to great hardship for the people who were driven to islands of land that were still arable while the destruction of the economic/agricultural fabric of the region went on around them, to which Yoffee returns:

“The archaeological reconnaissance surveys of Robert Adams (1981) have shown a progressive tendency during the late Old Babylonian period for nucleated settlement patterns to become dispersed into smaller communities that are more evenly spaced along [irrigation] water courses. ... Also many loans were issued by temples ..., a situation that contrasts markedly with that of Hammurabi’s time, when certain prerogatives of the temples, especially judicial ones, were ‘secularized’ by the crown. ... In the times of the fiscal [inflation and public debt] ... the temple [with its stores of grain] seems to have provided a refuge for unfortunate citizens of Babylonia—and managed, of course, to make a profit from the pious debtors.”<sup>129</sup>

This problem would have been most evident in the most southern regions of Babylonia above the marsh lands at the head of the Persian Gulf even in the time of Hammurabi.

“The most important element in farming the soil of a dry country was irrigation ... provided and maintained by the pious care of kings. ... Yet there is some evidence that many of the ancient cities over whom the rule of Lasar then extended were in a state of decay at the time of Hammurabi’s conquest.”<sup>130</sup>

H. Gasche *et al.* further report.

“Not coincidentally, archaeologists have had a difficult time identifying post-Old Babylonian sites. One reason for this is that the

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<sup>128</sup> Yoffee, *op.cit.*, p. 53

<sup>129</sup> *ibid.*

<sup>130</sup> *CAH*, vol. II, pt. 1 (1973) p. 201

principal ... sites—certainly those that have been the most thoroughly excavated—were largely, if not completely, abandoned either on or at the end of the ... period. Already by Samsuiluna year 10 about 140 years before the end of the reign of Sansuditana [the last king of the dynasty], urban centers in southern Babylonia began to be abandoned and the region passed out of the [Old] Babylonian crown. ... This process of deurbanization first struck, among others, Euphrates [River] cities of Ur, Uruk and Lasar. ... But finally during the reigns of Ammisaduqa and Sansuditana Babylon began to lose ... control even of this much diminished realm, and at least some of the northwest cities were abandoned as well.”<sup>131</sup>

These were among the last monarchs of the Old Babylonian era. Postel, using the criteria of the established chronology (which shall be omitted from the following citation), points out:

“One of the key pieces of evidence that salt buildup reached damaging levels ... is the change in the region’s crop mix. Wheat was the preferred cereal for eating, but it was less tolerant of salt than barley is. Grain impressions found in pottery from southern Iraqi sites ... suggest about equal amounts of wheat and barley were grown at [first] ... later wheat apparently accounted for little more than one sixth of the harvest [then] for less than 2 percent in the Girsu area and by [Hammurabi’s time] it was no longer cultivated at all. Moreover, at the same time the crop mix was shifting, yields of barley were declining—another sign that salt had poisoned the land.”<sup>132</sup>

None of these measures worked over the long run and all this culminated in a crushing salinization catastrophe that ended civilization in southern Babylonia. Thorkild Jacobsen concludes:

“Under Samsuiluma and his successors the economic difficulties which had been growing in Sumer came to a head. A major and

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<sup>131</sup> H. Gasche *et al.*, *op.cit.*, pp. 7-8

<sup>132</sup> Postel, *op.cit.*, pp. 20-21

definitive catastrophe—probably a final salting up of the field(s) ... practically depopulated the South and changed it into a wasteland.”<sup>133</sup>

Critics will no doubt argue that this proves nothing with respect to Heinsohn’s Old Babylonian/Persian equation since the Persian records do not, as yet, report all these events. Why should they? These events were happening far away in one of their satrapies and only affected their nation’s tax collection. The rest of the empire was not in such dire condition. Babylonia, though economically important, was only one province out of about 30. But in Babylonia this was their whole world and the catastrophe that befell them was no distant minor event.

Gwendolyn Leick further informs us: “Environmental archaeologists have documented the exhaustion of arable land at certain periods. Nippur, for instance, was almost deserted for generations at the end of the Old Babylonia period.”<sup>134</sup>

Then for some unknown, unfathomable reason the historians, without an iota of evidence, assume that the land around Nippur and the rest of the Babylonian plain was somehow cleansed of its salt and brought back to abundant production. Leick at this concept remarks: “Again and again we see that strongly centralized states collapsed after decades of bad harvests when the carrying capacity of the land had been exhausted.”<sup>135</sup> This, we will see below, correlates with the rest of Heinsohn and Sweeney’s chronology which equates the Neo-Assyrians with the Persians.

However, we do get a glimpse of the devastation Babylonia suffered during Persian times; as William Culican reports, this economic downturn:

“... created ... poverty in Babylonia [during the Persian empire].

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<sup>133</sup> Thorkild Jacobsen, *Toward the Image of Tammuz and Other Essays on Mesopotamian History and Culture*, W.L. Moran, ed. (Cambridge MA 1970), p. 197

<sup>134</sup> Gwendolyn Leick, *The Babylonians: An Introduction* (London, NY 2003), p. 9

<sup>135</sup> *ibid.*

“Many Babylonians dedicated their daughters to be [temple] courtesans when they grew up, it was the only way they could escape the degrading poverty if not outright starvation.”<sup>136</sup>

This surely indicates that economic conditions in Persian Babylonia were driving people to acts of desperation.

Finally, one is left to explain how the Persians were able to extract great wealth from Babylonia after its lands were poisoned by salt, depopulated, abandoned, and left as a great wasteland a thousand years earlier? Since the land would be salted over in about 300 to 400 years, historians cannot then argue for placing the Old Babylonians there and having them prosper agriculturally after the region had been under irrigation cultivation over 2000 years earlier. While they may wish to and do present the concept that the land was revitalized, they do not, and cannot, tell us how this was accomplished. They simply say this is so without a shred of evidence to prove how the land, once salted over, could be revived. A perfect example of this use of words instead of proof is presented by Roux:

“Moreover, it seems that in order to produce more and more cereals [Old Babylonian] landowners violated the rule of fallow, thereby reducing the fertility of the soil and accelerating its salinization. Thus within a century (1700-1600 B.C. in round figures) Babylonia went from political disintegration to economic disorder and ecological disaster. ...

“Ironically, it was the ... Kassites [who followed the Old Babylonians who] apparently took the necessary measures and gradually transformed Babylonia into a prosperous kingdom. ...”<sup>137</sup>

What these necessary measures were that rejuvenated the land we are not told; it is all words backed up by nothing constituting evidence.

Historians then further claim that after 1600 B.C., when the land was completely salted over, it was then revitalized and remained fertile for another thousand years, again based on nothing.

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<sup>136</sup> William Culican, *The Medes and the Persians* (NY 1963), p. 153

<sup>137</sup> Roux, *op.cit.*, 3rd ed. (Berkeley 1999), p. 242

After the Old Babylonian lands in southern Mesopotamia were destroyed by salinization, the historians persist in their belief, based on nothing, that the Kassites, Mitanni, Assyrians, Chaldeans, Medes, Neo-Assyrians, Neo-Babylonians, and Persians, who are assumed to have followed in that order, still exploited the region for its agricultural wealth. This cannot be done, as pointed out in vol. I of this series, without modern techniques which were clearly unknown and therefore never utilized. It is all a house built on salinized sand and it cannot stand.

## LINGUISTICS

“This Aryan family of speech was of Asiatic origin.”

A.H. Sayce 1880

“This Aryan family of speech was of European origin.”

A.H. Sayce 1890

“So far as my examination of the facts has gone it has led me to the conviction that it was in Asia Minor that the Indo-European languages developed.”

A.H. Sayce 1927

as quoted in J.P. Mallory

*In Search of the Indo-Europeans*

London 1989, p. 143

The above citations indicate that linguistics can be understood from different points of view and give different answers and indications about the evidence derived from the various languages. Although linguistics is not as precise as scientific or technological evidence, the changes and mutations that occur with all living languages are taken to be so well understood that it is assumed that one can correctly arrange chronology by its use. Knowing the gradual changes of a language over time, through its stylistic, grammatical and epigraphic form, it is believed one

can properly organize the development of the languages in Mesopotamia and Egypt, that is, which forms of the language came first, second, and so forth. But because linguistics is not a true science, amenable to precise testing, measurement, and falsification, like all other aspects of chronographical materials it must correlate with the forensic evidence to be shown to be valid.

The historians and the philologists, having organized the chronology of the ancient Near East based on fallible documentary evidence, nevertheless claim that the interlocking relationship between history and linguistics which they have painstakingly organized proves that the established chronology of the ancient world is correct and not amenable to major revision. This would seemingly foreclose any possibility that Heinsohn's, Rose's, Sweeney's, and even Velikovsky's reconstructions are tenable. But what is paramount in making this determination is that one must have the correct chronology in the first place before the linguistic evidence itself and by itself can have any standing. If the chronology is in error then the growth, development, and idiosyncratic changes that occur in all living languages must also be in error and must be reconstructed and reorganized along new lines to reflect what the science and technology prove that the chronology actually is, not the other way round.

Historians first unscientifically arranged the chronology of the ancient world for the linguists who then analyzed and accommodated the linguistics into that chronology, believing that their analysis and accommodation is thus a factual, unmovable reality rather than an accommodation. Over time, just as with astronomy, stratigraphy, etc., these interpretative accommodations of the linguistic chronology have become so entrenched, as generations of philologists repeated and refined what they had learned from their teachers and passed it on to their students, that it is taken that their linguistic chronology is carved in stone. Their entire argument is this based on circular reasoning.

Before starting this analysis, I wish to point out a basic axiom that must be applied to linguistics as a method for formulating the chronology of the ancient world. Living languages change with time so that over spans of hundreds of years they become quite different stylistically, grammatically, and epigraphically. Therefore it is highly unlikely that two or more peoples separated in time, say, by 400, 500, or more years would use the very same language in their social intercourse. Peoples separated for such long periods must utilize very different forms, even of the same language, under such a condition. Whenever we encounter two groups that

historians claim lived far apart in time, but use the very same language, it must be that they lived at or around the same time. Cochrane, too, turns to linguistic evidence to deny the validity of Heinsohn's Old Babylonian/Persian equation:

“Darius’ royal inscriptions at Bisitun are written in Old Persian, which he is said to have invented for just that occasion (note that these inscriptions are tri-lingual in nature, Elamite and Babylonian versions standing alongside the Persian). This language is Indo-European in nature and this could hardly be mistaken for Hammurabi’s Akkadian (Old Babylonian) script. Here is what one scholar [J. Wiesehofer] said about the Persian script:

“...‘The Old Persian [cuneiform] script is not a development of the Mesopotamian cuneiform, which was already more than two millennia old by that time, but a new creation influenced by the Aramaic consonantal script and consisting of a mixture of syllabic and consonantal signs.’

“The Aramaic script, I hasten to add, was not yet around at the time of Hammurabi, being first attested around 1000 BCE.”<sup>138</sup>

Cochrane, through his source, argues that Persian “could hardly be mistaken for Hammurabi’s Akkadian” which, according to the established chronology, is one and a half millennia older. Notice that Cochrane’s criticism on this point is circular: since the established chronology is assumed to be correct, he also assumes that Akkadian is much older than Old Persian and thus could not be used concurrently with Persian in Persian times. What Cochrane has failed to discuss from his very own source is that the Persians permitted their Babylonian subjects to continue to use the Akkadian language in their Babylonian homelands. His source, *The Cambridge History of Iran*, vol. 2, p. 53, states:

“One telling revelation of the importance of the satrapy [of] Babylonia is the use of the Akkadian language on a par with the Elamite and the Old Persian [languages]. ...”

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<sup>138</sup> Cochrane, *op.cit.*, p. 60

Of course, Cochrane can argue that the form of Akkadian used in Persian times was the much more modern form. This cannot be known because Persian/Babylonian inscriptions are not found in Babylonia, based on the established chronology. As shown above, the evidence for Persian occupation of this region is skimpy to say the least. Nevertheless Cochrane argues:

“If Hammurabi and Darius were one and the same, one must naturally expect to find inscriptions of Hammurabi written in Old Persian or Aramaic [in Babylonia] and inscriptions of Darius written in Old Babylonian [in Persia]. To the best of my knowledge, however, no such inscriptions have come to light, nor are they likely to be found at any point in the future.”<sup>139</sup>

Darius/Hammurabi wrote inscriptions in Babylonia in Akkadian because the people there who needed to use these could read and understand this language. In Persia, Darius/Hammurabi wrote inscriptions in Old Persian or one of the accepted international languages because the people there who needed to use these could read and understand those languages. Babylonia was, as we were told, the exception for using these other languages. Very little, if anything, was to be gained by writing inscriptions in a language that very few of those who needed to use them could understand and communicate to the populace. There is no problem with this aspect of linguistics, in this case, for Heinsohn.

Cochrane’s real point, however, is that the Akkadian employed in Hammurabi’s age belongs to the late third and early second millennia B.C., but that language over more than a thousand years mutated and evolved into a very different form. The stylistic, grammatical, and epigraphic differences that had to occur over such a long period are taken as proof that they were different forms of that language spoken and utilized at different times. Based on Heinsohn, Rose, and Sweeney’s thesis, all these various languages were spoken around the same time. On this point Cochrane states:

“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. If Heinsohn is right, that

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<sup>139</sup> *ibid.*



Hammurabi and Darius are one and the same figure, 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 ... scripts when celebrating his military accomplishments at Bisitun.”<sup>140</sup>

According to Cochrane the organization (from the established chronology) of Akkadian into its archaic, middle and more modern forms of Babylonian used in southern Mesopotamia and Akkadian [Assyrian] used in northern Mesopotamia means that those successive forms were never utilized at or about the same period throughout that region. The only way to test whether Cochrane is correct or if Heinsohn, Rose, and Sweeney are correct, is to determine this question on foundations of science and technology first, then on linguistic grounds thereafter. Before undertaking this analysis, it is important to learn what the philologists who specifically studied this question have to say. Erica Reiner in her *Linguistic Analysis of Akkadian* discusses the inherent problems related to whether the Old, Middle, and Neo forms of Akkadian and Babylonian were used seriatim—one after the other—as Cochrane seems to suggest, or whether they may have been used in different places around the same times:

“The written records of Akkadian [and Babylonian] form no continuous stream but fall into isolated groups of texts from areas geographically [distant] ... from each other. ... The [philologists 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].”<sup>141</sup>

Reiner fully admits, contrary to what Cochrane suggests, that because there is no continuous set of written records for these languages upon which to establish a linguistic chronology, but rather as that record is sparse, isolated, and contains periods without any written records at all, these have been filled in based on the assumed, established chronology and the assumed evolution of these languages within it. She claims the linguists organized these languages under the

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<sup>140</sup> *ibid.*

<sup>141</sup> Erica Reiner, *A Linguistic Analysis of Akkadian* (London, The Hague, Paris 1966), p. 20

compulsion of the established chronology to fit the historical-chronological framework. She adds without scientific proof:

*“I am inclined to consider 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 stages of Babylonian without at least considerable interference from other Semitic languages.” [emphasis added]<sup>142</sup>*

Here we have the direct assertion that the philologists have no proof regarding the evolution of these languages. In place of proof we are told that they are “inclined to consider Old Akkadian and Neo Babylonian as distinct languages,” and that without proof they “assume the existence of two dialects, Assyrian and Babylonian.” This is so because “This assumption may be considered the statement of negative [i.e., no] evidence ... of the fact that there seems to be no convincing way of deriving the earliest attested Assyrian or Babylonian texts from the preceding stages of Babylonian.” Linguists do not know if they have chronologically organized these languages correctly. Reiner finally admits:

*“While the writing habits changed greatly in the interval [of its assumed evolution] ... the late copies usually preserve rather exactly the tenor of the [earlier] original texts. ... Since, however, the wording of such texts is changed in very rare and exceptional cases only, we have no answer to the question whether such ‘literary’ texts still represent a language intelligible to persons other than the ancient scholars [scribes] ... who copied them, nor a clue to the relation they may have had to the language [then being] spoken.”<sup>143</sup>*

Philologists simply do not know if the assumed old forms of these languages were so different from the more modern forms that they could not have

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<sup>142</sup> *ibid.*, p. 21

<sup>143</sup> *ibid.*, p. 21

been fully intelligible to the assumed more modern speakers and readers. This is the assumption made by Cochrane, namely that the old forms of the language were unintelligible to assumed more modern speakers and readers. In a nutshell, Reiner claims, contrary to Cochrane, that philologists do not really know at the level that science knows, the evolution of Akkadian and Babylonian. After thousands of years, Akkadian/Assyrian spoken in isolation in northern Mesopotamia might well have been a totally different language from the Akkadian/Babylonian form utilized in the south.

In this regard the *Cambridge Ancient History* explains:

“If a language community splits into two or more groups which are subsequently and immediately isolated from one another, the language of each group will continue to evolve. But because there is no fixed direction for linguistic change, these languages will gradually diverge from one another in both form and content until after a suitable time, they will become quite distinct. Some parallel development may occur as the result of the [original] inherited structural features, but this will prove negligible.”<sup>144</sup>

That is almost precisely what we have with northern Akkadian, or Assyrian, and southern Akkadian, or Babylonian. The peoples of these two regions were not in daily contact with each other so that their languages would mutually and closely influence one another. Rather, these two peoples were for the most part—except for diplomats, scribes, and merchants—isolated from the linguistic influences of the other.

Samuel P. Huntington points out: “For more than three thousand years after civilization first emerged, the contacts among [different peoples] ... were with some exceptions either nonexistent or limited or intermittent and intense.”<sup>145</sup> These civilizations lacked mass communication systems and thus were fairly isolated.

Had these two language forms been separated in place and time for thousands of years as the established chronology requires, they would have become two distinct languages unintelligible to speakers and readers of the other. However,

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<sup>144</sup> *CAH*, vol. I, pt. 1 (1973), p. 124

<sup>145</sup> Samuel P. Huntington, *Clash of Cultures and the Remaking of World Order* (NY 1996), p. 48

if, as Heinsohn, Rose, and Sweeney suggest, the history is of much shorter duration, then these languages seemingly of common origin would still be similar in structure, etc. Reiner has assumed that roughly from 2000-600 B.C., over a period of 1400 years, these languages remained so similar that they were two dialects of each other. According to Georges Contenau:

“Akkadian is the name of the language spoken in both Assyria [called Assyrian] and in Babylonia [called Babylonian]. The two forms of the language are practically identical in grammar and vocabulary, and probably differed most in their method of pronunciation. ... But by the late Assyrian [assumed *ca.* 900 to 650 B.C.] and the Neo-Babylonian [assumed *ca.* 650-550 B.C.] Akkadian itself was obsolescent. ... From that time onward the two languages were in simultaneous use. ...”<sup>146</sup>

We will turn to the linguistics of *ca.* 1000 to 550 B.C. below, but for the moment let us concentrate on what Contenau claims for the entire length of the second millennium B.C., namely 2000-1000 B.C. He claims that in all this time the two forms of Akkadian remained “practically identical in grammar and vocabulary; and probably differed most in their method of pronunciation,” and this over a period of around a thousand years!

This fact regarding the close similarity of northern Akkadian (Assyrian) and southern Akkadian (Babylonian) echoes throughout the literature. Lesley Adkins states:

“There were three main Akkadian dialects known today as Old Akkadian, Babylonian and Assyrian and all used slightly different cuneiform scripts.. In reality they were so similar that the terms tend to be interchangeable and today they are studied as a single language.”<sup>147</sup>

C.B.F. Walker repeats these linguistic facts:

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<sup>146</sup> Contenau, *op.cit.*, p. 7

<sup>147</sup> Lesley Adkins, *Empire of the Plain: Henry Rawlinson and the Lost Language of Babylon* (NY 2003), p. 54

“Akkadian is one of the Semitic languages. ... It has three dialects, Old Akkadian, Babylonian and Assyrian, so that by definition anything written in Babylonian or Assyrian can equally be said to be written in Akkadian. Each of these dialects tends to use a slightly variant form of cuneiform script, although all handbooks to cuneiform take them as one [language].”<sup>148</sup>

O. Jesse Lace further states: “The Assyrian and Babylonian languages are so similar that they are often called simply Akkadian. ...”<sup>149</sup> Cochrane is clearly ignorant of this linguistic evidence but speaks about major differences among Old Akkadian, Assyrian, and Babylonian as if he were the linguistic expert. Apparently Contenau, Adkins, Walker, Lace, and Reiner, who are authorities in the field of these ancient dialects, don’t know what they are talking about, but he does. He should be ashamed of speaking out so forcefully on a subject where the true authorities say exactly the opposite of what he proclaimed to be fact.

As we pointed out above, over long stretches of time a living language divided into two or more diverges greatly from its original forms, stylistically, grammatically, and epigraphically, into a totally different form unintelligible to speakers and readers of the other form. The greater the separation in time and distance of the groups from one another, the greater the linguistic change. H.A. Gleason explains: “When the differences [between languages] are small, these are known as dialects. When larger, they are known as [different] languages.”<sup>150</sup>

On this point, J.P. Mallory adds:

“... languages are always in the process of change and therefore, as the area of a given language grows in size, it will be increasingly difficult for all its speakers to intercommunicate and change together along the same lines. Rather, there will be increased tendencies towards regionalization [i.e. dialects] where linguistic change will follow different local paths of development.”<sup>151</sup>

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<sup>148</sup> C.B.F. Walker, *Cuneiform* (Berkeley CA 1987), p. 16

<sup>149</sup> O. Jesse Lace, *Understanding the Old Testament* (Cambridge, UK, 1972), p. 27

<sup>150</sup> H.A. Gleason, *An Introduction to Descriptive Linguistics*, rev.ed. (NY 1961), p. 398

<sup>151</sup> Mallory, *op.cit.*, p. 146

Mallory further informs us that language change

“... will be affected by the size and nature of the geographical area occupied. In the absence of mass media or a written standard [taught to all the people, which did not exist in Mesopotamia], people speaking originally the same language but separated by large distance are unlikely to maintain parallel changes.”<sup>152</sup>

Yet Reiner “assumes” that from “2000-600 B.C.” the Assyrian form of Akkadian of the north was a dialect of the Babylonian form of Akkadian from the south of Mesopotamia. How could the forms of these languages—really dialects—separated by long periods of time and great distances, without mass media and universal education, have remained “practically identical in grammar and vocabulary” and be “assumed to have “probably differed most markedly in their method of pronunciation”? The obvious answer that suggests itself is that the very small differences between them indicate that though dispersed over a broad area of fairly isolated regions, change was small because the chronology was short enough to allow for only small linguistic change to take place, just as Heinsohn’s, Rose’s, Sweeney’s, and to some extent Velikovsky’s theses demand.

There are further problems that beset the chronological length of the Akkadian language, as discussed by Nicholas Ostler in his linguistic history of the world. It is generally taken as a fact that Akkadian was employed from around 2000 and died out about 600 B.C., to be replaced by Aramaic.<sup>153</sup> Then with the supposed collapse of the Old Babylonian Empire *ca.* 1700 B.C.,

“... the dialect of Babylon (which even the Babylonians called *Akkadu*) became established as the literary standard, the classic version of which would be used for official purposes throughout Mesopotamia. This privileged position endured for the rest of the language’s history, essentially regardless of whether Babylon, Assyria or neither of them was the current centre of political power. ...

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<sup>152</sup> *ibid.*, p. 23

<sup>153</sup> Nicholas Ostler, *Empire of the Word* (NY 2005), p. 61

“Besides its use as a native language by most of the inhabitants of Mesopotamia ... Akkadian also came to achieve a wider role as a lingua franca among utter foreigners.”<sup>154</sup>

Ostler goes on to describe the many settings around Mesopotamia where Akkadian was employed in addition to the indigenous language:

“In the second millennium [B.C.] Akkadian was being taught and used in every capital city that surrounded Mesopotamia ... regardless of the ambient [native] language ... It was being practiced in Susa for Elamite speakers, in Nuzi ... for Hurrians, in Hattusas for Hittites ..., in Alalakh and Ugarit near the Mediterranean coast for speakers of other Semitic languages ... and in Akhetaton ... for Egyptians.”<sup>155</sup>

The universality of Akkadian at this time, however, raises a serious problem after 1700 B.C. down to around 1200 B.C. because Akkadian speaking peoples during this period were not ruling Mesopotamia, but people who had their own language, particularly the Kassites, Hurrians, and Mitanni. Ostler explains:

“The middle of the second millennium [B.C.] was not a glorious period for the speakers of [Akkadian or of other] Semitic languages. In 1400 B.C. Babylon had been firmly under Kassite control for two centuries, and Assyria in vassalage to the Mitanni for a century. In northern Syria, established Mitanni control was being disputed by the Hittites. And the rest of Palestine was a collection of vassal states under Egyptian sovereignty.

“It was not ... political influence, then, which made Akkadian the language of convenience at the time. The only [conjectural] explanation is a cultural one ... the culture of the scribal *edubba* [schools].”<sup>156</sup>

There is no evidence to show that scribal schools existed at this time or that each of these foreign entities that dominated Mesopotamia decided to maintain

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<sup>154</sup> *ibid.*

<sup>155</sup> *ibid.*, p. 62

<sup>156</sup> *ibid.*, pp. 62-63

their foreign subjects' language as the mode of communication rather than their own. This would be comparable to the Romans taking over all Gaul and writing and speaking among one another in that ancient foreign language. The "scribal school" explanation indicates that something is linguistically wrong with the established chronology since this linguistic excuse is necessary to accommodate it.

At this point the problem becomes exacerbated because not only at the nadir of Assyrian/Babylonian power did Akkadian become the *lingua franca* of Mesopotamia, but at the zenith and thereafter, during Assyrian domination of these regions, as Cochrane suggests, Akkadian began to be replaced by Aramaic. Ostler fully admits this conundrum, calling it a paradox:

"The paradox deepens the more closely it is considered. Not only was Akkadian, the language, replaced at the height of its political influence; its replacement language, Aramaic, had until [that time] been spoken mainly by nomads. These people could claim no cultural advantage and were highly unlikely to set up a rival civilization [during this period of replacement]. The expectation would have been that like the Kassites eight hundred years before in Babylon, Aramaic [nomad] speakers would have been culturally and linguistically assimilated to the great Mesopotamian tradition. Similar things, after all, were to happen to others who burst in upon great empires—the Germans invading the Roman empire or the Mongol in China."<sup>157</sup>

It is argued that because the Aramaic language used a short, simple alphabet rather than an unwieldy, large, and difficult set of cuneiform symbols, and that the Aramaic speakers were so numerous, this forced Assyrian/Babylonian society to change their language. Roux presents just this explanation:

"Yet to these barbaric Aramaeans befell the privilege of imposing their language upon the entire Near East. They owed it partly to the sheer weight of their numbers and partly to the fact that they adopted, instead of the cumbersome cuneiform writing, the Phoenician alphabet slightly modified, and carried everywhere with them the simple practical script of the future."<sup>158</sup>

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<sup>157</sup> *ibid.*, p. 63

<sup>158</sup> Roux, *op.cit.*, 2nd ed. (1992), p. 276



Ostler looked at this explanation and found it without merit:

“This [supposition of Roux] cannot be right. Writing systems, after all, exist to record what people say, not vice versa. There is no other case in the history of change in writing technology inducing a change in popular speech. And even if it were possible, it is particularly unlikely in a society like the Assyrian empire, where a vanishingly small portion of the population were literate ... how could a mobile and politically subservient group such as the Aramaeans not only spread its language, but also get its writing system accepted among its cultural and political masters, the Assyrians and Babylonians.”<sup>159</sup>

Ostler still argues that when the Assyrians transported great numbers of Aramaic speaking peoples, among others, into their empire, who became in some cases administrators etc., the Assyrians were over time forced to adopt Aramaic and drop their own language. But this is simply illogical. First of all, the Assyrians/Babylonians could have just as easily adopted the Aramaic alphabet and employed it to express their own language. The Greeks did not stop speaking Greek when they switched from syllabic writing to alphabetic. There was no reason for the entire people to give up their mother tongue and learn a new one to incorporate an alphabet. Ostler argues:

“The triumph of Aramaic over Akkadian must be ascribed as one of practical utility over ancient prestige, but the utility came primarily from the fact that so many people already spoke it.”<sup>160</sup>

The fact of the matter is that ordinary people do not, and will not, give up their mother tongue in order to accommodate a foreign people in their homeland and learn a new language unless they are forced to do so. No people will go through the arduous task of learning to speak a new and strangely different language even when surrounded by those who speak it. Immigrants have come to the United States for generations and the older ones as well as middle-aged ones more often than not do not learn English. Their children do learn English where they are a minority when forced to attend school. But in ancient times, there was no such establishment. On

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<sup>159</sup> Ostler, *op.cit.*, p. 64

<sup>160</sup> *ibid.*, p. 67

the other hand, in the Miami area of Florida, Spanish is overwhelmingly spoken but Americans there rarely learn to speak or desire to speak Spanish.

What we are suggesting is that many languages were all spoken around the same time in that region. As Barbara Nevling Porter reveals:

“The late Assyrian empire was a polyglot world. ... The written culture of the Assyrian empire as well was carried on in several languages ranging from the Neo Assyrian, Neo Babylonian, and Standard Babylonian dialects of Akkadian to Sumerian and Aramaic.”<sup>161</sup>

A similar condition also exists with the Mitanni, Kassites, and Minni as explained by Claude Reignier Conder:

“The Names of the Kassites were translated into Semitic speech by Babylonian scribes of the Persian period and from the translations it is clear that the Kassite language was a Mongol dialect similar to Akkadian, to Sumerian and to the languages of the Minni and of the Matiene (Mitanni) further north.”<sup>162</sup>

It seems rather clear that instead of having a very long linguistic history in Mesopotamia, we have instead a great many peoples speaking various dialects of a few languages around the same time, which were only used for international trade but not in the various lands by the indigenous peoples.

Thus far our discussion has been based entirely on linguistic analyses, but, as pointed out earlier, linguistic chronology must follow from scientific and technological evidence. Reiner has told us “that there is no convincing way of deriving the earliest attested Assyrian or Babylonian texts from the preceding stages of Babylonian.”<sup>163</sup> The convincing way of deriving this linguistic chronology is via

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<sup>161</sup> Barbara Nevling Porter, “Language, Audience and Impact in Imperial Assyria,” *Israel Oriental Studies XV Language and Culture of the Near East*, Leiden, The Netherlands; NY; Cologne, Germany, 1995), p. 52

<sup>162</sup> Claude Reignier Conder, *The Hittites and Their Language* (NY 1898), p. 12

<sup>163</sup> Reiner, *op.cit.*, p. 21

forensic historical evidence. Let us get down to the scientific and therefore linguistic heart of these matters.

Old Akkadian was used by the Old Akkadians from around 2300 B.C. and supposedly evolved to a largely different form of the language by around 1500 B.C., a period of some 700 to 800 years. Nevertheless, based on the evidence outlined in vol. I of *Pillars of the Past*, chapter 9, pp. 272-289, an archaeological and geological dig carried out at Tell Munbaqa which spans the same time between the Old Akkadians of 2300 B.C. and the time of the Mitanni of 1500 B.C. showed no scientific-geological evidence of a 700- to 800-year settlement gap. This requires that, based on the established chronology, the Old Akkadians used this supposedly very ancient form of Akkadian without any change whatsoever for 800 years right down to the middle of the second millennium B.C. and that it was unchanged not only for 300 years prior to the rule of the Old Babylonians, but remained unchanged for 100 years after the assumed fall of Babylon in 1600 B.C. To accept this as a linguistic reality boggles the mind. Heinsohn has in fact shown in terms of linguistics that

“Old Akkadian cylinder seals remained in use for business contracts in the Mitanni/Hurrian stratum.”<sup>164</sup>

Do Cochrane and the rest of Heinsohn’s critics expect anyone to accept as a reality of history that for about 750 years the Old Akkadian language remained the same stylistically, grammatically, and epigraphically? This is a whopping contradiction to the linguistics presented by Cochrane, which of course he has completely failed to report, let alone discuss. The fact of the matter is that there is no rational explanation or scientific basis by which to deny Heinsohn’s linguistic evidence. That is probably why neither Cochrane nor any other of Heinsohn’s critics have come forward to address this huge linguistic contradiction to the established chronology. It proves on a fundamental level that the entire structure of linguistics taken as correct by the historians for this period has no standing at all.

Let us furthermore recall that Cochrane claims that Hammurabi destroyed Mari around 1700 B.C. Yet iron, tin, and glass were unearthed there

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<sup>164</sup> Gunnar Heinsohn, “Who were the Hyksos? Can archaeology and stratigraphy provide a solution to the ‘enigma of world history’?” Proceedings of the *Sesto Congresso Internazionale di Egittologia*, vol. II (Torino, Italy, 1993), p. 211

requiring a date for this city in the first millennium B.C. Let us also recall that Cochrane asserted that Aramaic, linguistically similar to Phoenician and Hebrew, developed around 1000 B.C. Thus it would be impossible to find there tablets inscribed with Hebrew names. Kurinsky, nevertheless, explains:

“Professor Parrot, the prestigious archaeologist ... was ecstatic upon discovering significant biblical connotations in the translation of the tablets unearthed from the ruins of Mari. Such biblical names as Abram, Jacob, Benjamin and Zebulun could be distinguished in the Akkadian records.”<sup>165</sup>

This would be extraordinarily unlikely in Old Babylonian times but not in Persian times.

However, Cochrane and Heinsohn’s other critics can still argue that this does not prove that the Old Babylonian in its purest form employed by Hammurabi was ever utilized in Persian times. But that argument can have no merit because of the forensic historical analysis presented in the earlier segments of this book. That is: the 12th Egyptian Dynasty based on unimpeachable astronomical evidence—presented by Lynn E. Rose—must be placed in the mid- first millennium B.C. The 12th Dynasty has been linked and even tied by bonds of archaeology and other forensic historical evidence directly to the Old Babylonians, which places them in the first millennium B.C., in fact, at the time of the Persians. Rose further showed, astronomically and calendrically, that the Old Babylonian/ Persian kings Hammurabi/Darius and Ammisaduqa/Artaxerxes III Ochus share identical 30-day months in identical years of their reigns and identical months of those years. Stratigraphy also shows the Old Babylonians fall in Persian times. The Old Babylonians have iron, bronze, and glass long before these could have been processed in the second millennium.

All these forms of scientific and technological evidence contradict Cochrane’s placement of the Old Babylonians in the early part of the second millennium B.C., but fully directly and indirectly correlate with, corroborate, and converge to place the Old Babylonians in Persian times. Cochrane has produced nothing scientifically or technologically to compare with these forms of solid evidence. Therefore, because the forensic historical evidence places the Old

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<sup>165</sup> Kurinsky, *op.cit.*, p. 26

Babylonians in Persian times, the form of Akkadian that they employ also belongs in Persian times. The linguists follow the science and technology. Cochrane has it all upside down and backward.

Along similar grounds we have shown in the first volume of *Pillars of the Past*, pages 477-479 and 504-510, that the Hittites, assumed to have lived in Anatolia from 1600 to 1200 B.C., used the same language as the Lydians who lived 600 years later, which moves the Hittites linguistically as well as by several other forms of scientific and technological evidence into the first millennium B.C. To that we add the linguistic evidence that in Ramses II's time a scribe named Hori used a form of Hebrew that only came into existence several hundred years later. The Hittites and Ramses II fought a battle which, based on linguistic evidence as well as other forms of forensic historical evidence requires them and the languages they used to be placed in the first and not the second millennium B.C. This, too, is still another immense negation of the linguistic history and chronology of the ancient Near East.

Cochrane has told us that the Aramaic language started around 1000 B.C. or somewhat later and became the international language of the Near East thereafter. Nevertheless, Nicholas Ostler informs us that Aramaic became the standard *lingua franca* of the Near East which stretched "from Hindustan to the land of Kush south in Egypt ... Aramaic ... was to remain essentially unchanged for the next millennium."<sup>166</sup>

That is, from the earliest times of the Neo-Assyrians around 1000 B.C., Aramaic was used as a means of international communications between various countries and for trade. But at the same time, the historians expect one to believe that throughout this lengthy period, Aramaic "was to remain essentially unchanged for the next millennium." This defies linguistic credulity. *No living language* can remain essentially the same for about a thousand years; only dead languages do not change. Living languages change to an immense degree over a thousand years.

This is yet another major contradiction to the linguistic chronology Cochrane has put forth. But it is a major corroboration for the chronology that Heinsohn's, Sweeney's, and Rose's theses demand. In case after case, the linguistic evidence supports the short chronology, while in case after case it contradicts the established chronology.

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<sup>166</sup> Ostler, *op.cit.*, p. 81

Finally, let us turn to linguistics for Egyptian chronology to which historians have tied all the surrounding civilizations. Is their linguistic history as solid as Egyptologists claim? It is assumed by historians that as with Mesopotamia, there is a similarly lengthy linguistic chronology for ancient Egypt. It is accepted that during the third millennium B.C. during the Old Kingdom an ancient archaic form of Egyptian hieroglyphics was written and spoken that evolved to somewhat different forms for the Middle Kingdom, which in turn further evolved slightly into even more modern forms for the New Kingdom, and so on. Barbara Mertz sums up the conclusion of this evolution which hardly changed the epigraphic hieroglyphic expressions of ancient Egyptian writings supposedly for 3,000 years:

“So thorough is modern knowledge of the [ancient] Egyptian language that we can tell the probable date of a manuscript by internal evidence alone—by stylistic, grammatical and epigraphic details—just as a student of English literature can distinguish a work of the fourteenth century from one of the seventeenth.”<sup>167</sup>

Above, as well as in volume one of this series, we have shown via the astronomical/calendrical work of Lynn E. Rose that the 12th Dynasty of Egypt, conventionally dated to the early second millennium B.C., must be placed in the mid- to early first millennium. Thus, the 12th Dynasty belongs in the Persian era. Hence, its late period linguistics and hieroglyphics (style, grammar, and epigraphic expression) also belong in the mid- to latter part of the first millennium B.C. Based on the established chronology these late period forms of written Egyptian would have come after the Middle Kingdom’s 12th Dynasty. But astronomy indisputably proves that the 12th Dynasty of the Middle Kingdom is contemporary with the Later Period dynasties of the Persian era just as was shown for Akkadian and Old Babylonian. Simply stated, there were several forms or dialects of ancient Egyptian in use around the same time, each with somewhat different stylistic, grammatical, and epigraphic forms. They were dialects of Egyptian all used around the onset of the first millennium B.C. and for some centuries thereafter; it is not the case that they were used over the course of some 3,000 years.

The most remarkable thing regarding Egyptian hieroglyphic writing is that although it changed slightly here and there in a fluid fashion, unlike, say, English, it remained largely static once it was formed by the 3rd-4th Dynasties of

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<sup>167</sup> Barbara Mertz, *Temples Tombs and Hieroglyphics* (NY 1978), p. 50

the Old Kingdom down to the time of Alexander the Great and after. John A. Wilson describes this fluid-static condition:

“Writing and literature would be a further example of the static, yet fluid character of Egyptian culture. The Old Kingdom saw the formation of a classical language which was still in relatively successful official use nearly twenty five hundred years later ... writing became fairly common in the Fifth and Sixth Dynasties. ... The classical language called ‘Middle Egyptian,’ which received acceptance in the Old Kingdom, continues with minor change down to the cosmopolitan excitation under the Egyptian Empire and thereafter was maintained for religious and official purposes as long as men carved hieroglyphics on temple walls. And yet it is possible to date inscriptions to their periods of original composition or existing expression by criteria of paleography, vocabulary, ‘spelling,’ syntax, or style; it is possible to point out contemporary colloquialisms in a classical text or archaisms in a relatively colloquial text. To be sure, we have about three thousand years of texts to deal with, and constant change within so long a time would seem inevitable to a modern [researcher], but the extraordinary phenomenon is the broad consistency and continuity [of the almost static, basic structure of Egyptian hieroglyphic expression] over so long a time.”<sup>168</sup>

Here, then, is what historians expect one to believe. The hieroglyphic epigraphy to express all the numerous changes in the spoken language of the Egyptians *was in no major way* reflected in the hieroglyphic script for that language. That would be like people in the United States, England, and elsewhere where English is spoken, to still be using the ancient Anglo-Saxon script which was used for English’s first form a thousand years ago. There were slight hieroglyphic variations among dynasties, but the script was still similar and readable.

This is hardly rational. Over a 3,000-year period even the official and religious hieroglyphic inscriptions should have changed to the point where the language around the beginning would have been as unreadable to later readers as writing forms of ancient European English in Chaucer’s day were unreadable to modern readers of English after only about 500 years. As with Mesopotamia, what

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<sup>168</sup> John A. Wilson, *The Culture of Ancient Egypt* (Chicago 1962), p. 76

is implied by the static expression of Egyptian paleography is that the history—the chronology of Egypt—is much shorter and over this much shorter period the language changed only slightly. Further, as many of these various dynasties were contemporary, they would of course use highly similar forms of hieroglyphics. What we have is similar to what existed in Mesopotamia. The Egyptians in different regions of the country spoke and wrote in dialects of the same language.

Now, according to the established chronology, the 12th Dynasty came about 500 years before the 18th Dynasty. One would hardly expect that the 18th Dynasty would copy its hieroglyphics directly from the 12th. However, Assmann reports:

“Within the cyclical structure of Egyptian history it is of the greatest significance that the early rulers of the Eighteenth Dynasty ... modeled themselves closely on the Twelfth Dynasty in style of inscriptions.”<sup>169</sup>

That is, the 18th Dynasty used the same forms of hieroglyphics as did the 12th Dynasty as if there was no 500-year period between these dynasties. Wilson elucidates:

“In the external manifestations of culture the Eighteenth Dynasty resumed where the Twelfth had left off—or perhaps one should say that there had been no cultural break [between them, as there is assumed to have been] in the Second Intermediate Period. Architecture and art repeated the forms and themes of earlier times. ... The eye detects no major differences between sculptured scenes of the Twelfth Dynasty and those of the early Eighteenth.”<sup>170</sup>

In discussing dynasties that came about a thousand years after the 12th Dynasty, Assmann admits: “In some cases there is still controversy as to whether particular [written] items are from the Twelfth or the Twenty-second [Dynasties].”<sup>171</sup>

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<sup>169</sup> Jan Assmann, *The Mind of Egypt* (NY 2002), p. 199

<sup>170</sup> Wilson, *op.cit.*, p. 166

<sup>171</sup> Assmann, *op.cit.*, p. 341



To explain why after almost a millennium the 26th Dynasty decided to write in the same style, grammar, and epigraphy as the 12th, Assmann argues, as do all other Egyptologists:

“The Twelfth Dynasty represents a cultural apogee in the history of Egyptian civilization. In the New Kingdom, the literature of the [more ancient Middle Kingdom 12th Dynasty] period was elevated to the canonic status of classics; the language of the Middle Kingdom remained in use for sacred purposes until the end of pharaonic history and in art the archaizing style of the late period largely took its bearings from the mature style of the Twelfth Dynasty.”<sup>172</sup>

All this, of course, cannot be correct because in volume I of this series I presented linguistic and other evidence which places the 18th Dynasty in the same period as the 12th Dynasty and the astronomical data presented by Rose show that the 12th Dynasty was contemporary with several of these other dynasties and came after the others. Rather than the 12th Dynasty being the epitome of Egyptian written expression and attainment in the arts, the astronomical evidence indicates these other dynasties did not slavishly copy its forms. This, I suggest, is intellectual, historical, and linguistic gibberish. To the best of my knowledge, ancient Egypt is the only instance in which an entire society reverted to its past form of written expression and to artistic forms that were so far removed in time.

On a similar topic, Velikovsky, who equates the 19th with the 26th Dynasty, writes:

“The Egyptian language and orthography under the Ethiopian and the Twenty-sixth (called also Saitic) Dynasty were so similar to the style and orthography under the Eighteenth Dynasty that experts have often engaged in disputes about the date of a literary relic, with six to eight hundred years [between these dynasties] at stake. One of the cases is that of the Sphinx stele ... of Thutmose IV.

“... A. Erman, an eminent Egyptologist, tried to prove that the text is a product of the Saitic time, especially because of its late spelling. But he was disputed by another equally excellent Egyptologist,

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<sup>172</sup> *ibid.*, p. 118

Spiegelberg, who presented the argument that the ‘late spelling’ is actually not late and that the texts of the Saitic time, seven or eight hundred years after the Eighteenth Dynasty, are conspicuous through their employing a classical orthography; and that thus no marked difference is evident between the texts of these two periods. ...

“... In no language, ancient or new, would eight hundred years have passed without very considerable changes.”<sup>173</sup>

What we have here is the 12th, 19th, and 26th Egyptian Dynasties all writing in an identical hieroglyphic script. The 12th Dynasty is supposedly from 1800 B.C., the 19th from about 1500, while the 26th is from around 800 to 700 B.C. All these dynasties exhibit identical scripts because they all reigned about the same time.

What would be required to show such a total reversion to past written and artistic expression, would be to show another entire society reverting back to a much earlier time in the same way. Such, I dare say, does not exist. It would be the same as the English of King George III having such reverence for the Middle English of Chaucer that the entire British nation felt the compulsion to write in all its newspapers, books, magazines, letters and so forth in the language of Chaucer which existed about 400 years earlier in time. One must assume that the British of George III’s day simply didn’t cherish their past with the same absurdity that historians (based on nothing from the sciences) attribute to the Egyptians. During the European Renaissance, Greek and Latin were used and some of their architectural styles were copied, but the literature of that period also used the vernacular or the language of the people.

Of course, the historians will argue that the Egyptian reverence for their dead and the dead past was an entirely different matter altogether. How, then, does one overcome this argument? It can be overthrown because the Egyptians not only wrote in hieroglyphic script but in other scripts as well that did not remain static for the assumed 3,000 years of Egyptian history.

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<sup>173</sup> Immanuel Velikovsky, “From the End of the Eighteenth Dynasty to the Time of Ramses II,” *Kronos*, vol. III, no. 3 (1978), p. 5

While the Egyptians employed hieroglyphs for official and religious inscriptions, they also wrote in a script known as hieratic. The difference may be understood as that between the block letter printing we learned as children in early elementary school—which would be hieroglyphics, the same as the print that appears in this book and other published materials. Hieratic, on the other hand, is the cursive or hand-written form used for hand-written letters, notes, etc. Hieratic changed much more than hieroglyphs through the assumed lengthy history of Egypt.

However, much later supposedly, hieratic or Egyptian cursive script itself changed into an entirely new form, to facilitate more rapid writing, especially for business transactions, known as abnormal hieratic. Hieratic and abnormal hieratic are so very different that one cannot mistake one for the other. Finally, much later again an even more condensed shorthand form of abnormal hieratic developed, known as demotic. Each of these three cursive forms developed and can also be recognized not only with regard to the kingdom but to the dynasty which employed them supposedly at different times.

That being the case, it is hardly possible that different forms of hieratic, abnormal hieratic, and demotic would ever be used by one dynasty all at the same place. These different forms were used at quite different periods in Egyptian history, based on the established chronology. Nevertheless, Eugene Cruz-Uribe writes of:

“... a great historical anomaly, there survives a Saite [26th] Dynasty document which contains ‘witness copies’ of a business document written variously in Late New Kingdom style Hieratic, Abnormal Hieratic and early Demotic.”<sup>174</sup>

Rather than a great historical anomaly, we have here a great historical-chronological contradiction. Based on the established chronology, the 26th Dynasty was writing a form of hieroglyphs stylistically, grammatically, and epigraphically identical to the 12th Dynasty of a thousand years earlier; so alike are these inscriptions that there is still controversy as to whether particular written items are from the Twelfth or Twenty-sixth Dynasty. At the same time, the 26th Dynasty was using hieratic script of the Late New Kingdom hundreds of years earlier than this dynasty, along with abnormal hieratic which had the taken place of hieratic, and also

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<sup>174</sup> Eugene Cruz-Uribe, “Scripts: An Overview”, *Oxford Encyclopaedia of Ancient Egypt*, vol. 3, D.B. Redford, ed. (Oxford, publ. in NY 2001), p. 192

demotic, which had taken the place of abnormal hieratic. Instead of these cursive scripts developing and evolving from one form into that of another, into the final form, etc., they were all being used at the same time, an impossible condition according to the established chronology.

Now this is carrying reverence for the past too far. Not only was the 26th Dynasty in love with the hieroglyphic forms of the 12th Dynasty, but 12th Dynasty hieratic was not good enough for these lovers of the archaic. Late New Kingdom hieratic was more to their taste; but they couldn't make up their minds if this was a wise choice and being fickle they also decided to use abnormal hieratic of an even later time. But to be absolutely ecumenically safe and snub no one from yet a later time, they embraced demotic to stay in style.

To reconcile all these scripts, all that need be done is follow the evidence of forensic history and allow the science and technology to lead. The ancient Egyptians who used the same forms of their language ruled concurrently or nearly concurrently. There is no great linguistic anomaly based on the short revised chronology.

When we return to Mesopotamia and the Old Babylonians we encounter another linguistic problem, namely: What was the original language of these people before they conquered Babylonia? Laessoe mentions:

“... the tribes from the desert who spoke a Semitic language, thus took over the legacy of the Sumerians; but Amorite, the Semitic dialect spoken by these tribes, is only known in Babylonia from their personal names; once they were established in the cities of Mesopotamia they abandoned their linguistic idiosyncrasies and took over the Mesopotamian script and also the Akkadian language.”<sup>175</sup>

The fact of the matter is that no-one has the slightest knowledge of the prior language spoken by the so-called Old Babylonians. Liverani shows that their names were not necessarily their original names but names given to them by their Babylonian subjects.

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<sup>175</sup> Jorgen Laessoe, *People of Ancient Assyria* (London, 1963), p. 17

“The [Old Babylonian] Martu people from Drehem and Isin generally bear western-Semitic names, while the Martu people from Lagash and Umma [to the south] more often bear Sumerian and Akkadian names, that is names customary in the area in which they had settled and where they worked.”<sup>176</sup>

Therefore, the names given to them by the Babylonians tell us nothing of their language. As Whitney Davis explains:

“Any Semitic name that is non-Akkadian tends to be classified as [Old Babylonian] Amorite, making it impossible (or at least very difficult) to decide if there is one ... language or a cluster of dialects or possibly more than one Semitic language present.”<sup>177</sup>

Whitney Davis makes it clear that the names given or taken by the Old Babylonians cannot be used in any definitive way to learn what their language was since “our evidence [of Old Babylonian names] is compromised by apparent contradictions in the way names and terms are used.”<sup>178</sup> He adds:

“We recognize people as [Old Babylonian] Amorites primarily when the sources add either Martu or Amurru to the names of individuals or tribes they are citing. However, as ancient scribes were not ethnographers [students of other cultures], they were rarely driven to record precise distinctions among people, and in many instances the names of individuals they designate as MAR.TU or Amurru turn out not to be [Old Babylonian] Amorite at all. This casts grave doubt on our ability to establish with precision when these terms referred to ‘Amorites’ and when to ‘westerners’.”<sup>179</sup>

The problem of the language that the Old Babylonians spoke in their original homeland disappears when we pursue the forensic historical evidence. As the Persian rulers of Babylonia their original language is known. It is Persian.

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<sup>176</sup> Liverani, *op.cit.*, p. 109

<sup>177</sup> Whitney Davis, “Amorite Tribes and the Nature of Second Millennium Western Asia,” *Civilizations of the Ancient Near East*, vol. II, *op.cit.*, p. 1235

<sup>178</sup> *ibid.*, p. 1238

<sup>179</sup> *ibid.*, p. 1232

## WHITHER THE OLD BABYLONIANS?

Cochrane tells us:

“Rather than coming from the Persian heartland, as imagined by Heinsohn, the nomadic Martu are clearly represented as coming from the ‘West’—i.e., the Syrian desert region where most scholars have sought their original homeland. Heinsohn’s treatment of the text [regarding this matter of the Old Babylonian Martu] here is shoddy in the extreme if not downright deceptive.”<sup>180</sup>

The reality is that the historians haven’t any reliable evidence to tell us who the Old Babylonians were or where they came from, even though their name also means westerners. Regarding the identification of these people we are faced with an enigma.

“The people we now call the [Old Babylonian] Amorites are at once one of the most important influences on the development of the ancient Near East and one of the most enigmatic. The enigma stems mostly from the fact that [theirs] was never a written language. When people that we know [mention them] they write in Akkadian and it is very difficult to separate out anything that may be specific to [them]... For these reasons, we do not have a literature that we can specifically attribute to [them]: no cosmology, no epics of [their] heroes, no lists of [their] gods and no historiography [of them].

“What we know of [them] comes from reference to them in the written records of other people, primarily from Mesopotamian or Syrian cuneiform documents, but also to a lesser extent from Egyptian and other sources. Non textual evidences are even less rewarding. The archaeological evidence for [them] is scanty and not to be separated from the artifacts of the other ethnolinguistic groups with which they

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<sup>180</sup> Cochrane, *op.cit.*, p. 64

shared the area. No one has yet been able to identify an [Old Babylonian] Amorite pot or weapon with certainty ... Therefore, the reconstruction of [their] ethnolinguistic group ... is based on snippets of information often contradictory.”<sup>181</sup>

This is almost exactly the case we have with the Persians in Babylonia. There is almost nothing there to indicate their presence as discussed in the above unit on stratigraphy. But this aspect of the Old Babylonians makes perfect sense with respect to the Heinsohn, Rose, and Sweeney theses. Where the archaeologists find snippets of Persian evidence, they assign these to the Persians and where they uncover snippets of what might be Old Babylonian material (for no one knows if it is or not) they suggest it may belong to this enigmatic people. Yet both lie directly beneath Hellenistic strata which indicates that they are one and the same people. With respect to the Biblical sources pertaining to them, Liverani reports:

“Nearly all memory of the specific characteristics of the individual [Old Babylonian] population has been lost, and consequently it would not be possible now to construct them within the context of the Old Testament. ...

“This task is not a simple one, as records of [them] are spread over a long period of time; they cover [based on the established chronology] more than two millennia, *and often refer to different entities*. An undifferentiated and uncritical use of the documentation can lead and has too often led to a simplistic leveling of the historical perspective which results in the attribution by the Old Testament of characteristics proper to the [Old Babylonian] Amorites of other groups. ... The picture given is as false as one which would today attribute to the Romans of antiquity [*sic*] characteristics of the contemporary Rumanians or to the Franks characteristics of the French.”<sup>182</sup> [emphasis added]

Liverani is suggesting that one cannot determine the age in which the Old Babylonians lived. Since historians know practically nothing of their origins

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<sup>181</sup> Davis, *op.cit.*, p. 1231

<sup>182</sup> Liverani, *op.cit.*, pp. 100-101

the idea became prevalent among them that they were a nomadic people who migrated into Babylonia from the west. Again, Liverani reports:

“Moreover, the thoroughly nomadic character of the [Old Babylonians] is to be rejected, particularly as regards [their assumed homeland origin in] Syria. If the abrupt change between the Early and Middle Bronze Ages implies some large socio-political upheaval [that would force the Old Babylonians to migrate from Syria to Arabia], it is still impossible to establish a connection between the place and direction of origin of the innovatory elements [and the] material culture and [original] place of the presumed Amorites.”<sup>183</sup>

But none of these identity problems exist using Heinsohn’s, Rose’s, and Sweeney’s theses. We know who they were, what they did, how they lived, etc. References to the Persians in a certain respect are very similar to the descriptions of the Old Babylonians.

“It has been thought that the [early] Persians [just like the Old Babylonians] were little more than a group of uncivilized semi-nomads who upon conquest of Babylon ... greedily swallowed and took over the main elements of Mesopotamian traditions.”<sup>184</sup>

Cochrane, however, suggests that there is an “Interlocking Web of History” that makes it impossible to equate the Old Babylonians with the Persians.

“While no one would claim that conventional history as we have it is completely secure or without difficulties, certain facts seem so well established as to approach certainty.”<sup>185</sup>

Neither this statement nor anything else that Cochrane has written provides us with any solid facts regarding the nature of who the Old Babylonians were, where they came from, what their language was, why they left their homeland, why or how they came to dominate Babylonia. The reason for this is based on the chronology he seems to support. None of this is known. Nothing related to these

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<sup>183</sup> *ibid.*, p. 109. See also J. Oates, *Babylon* (London 1979), p. 6

<sup>184</sup> Heleen Sancisi-Weerdenburg, “Darius I and the Persian Empire,” *op.cit.*, p. 1040

<sup>185</sup> Cochrane, *op.cit.*, p. 71



questions is ever addressed by him. The historians who have addressed and examined these problems have constantly contradicted Cochrane's view that the various facets of history are so well established as to approach certainty. The evidence regarding the Old Babylonians is "completely insecure" and "overwhelmed by difficulties" and is factually unreliable. In no way does Cochrane's argument contain anything that "approaches certainty." Below we will deal with their homeland as it relates to these issues.

The reason that the historians have learned nothing of the origin, language, etc., of the Old Babylonians in the early second millennium B.C. is that they never lived there at that time, nor are they related in any number of ways to events from that period. They are in reality an invention of the historians for that period. None of the problems related to them will ever be resolved so long as historians maintain that the Old Babylonians, who never existed at the time assigned to them, are a historical chronological reality. In reality the historians are seeking to make a fantasy embellished with all the support they can muster into historical truth. As Artemus Ward suggests:

"The research of many eminent antiquarians [historians] has always thrown much darkness on the subject [of the Old Babylonians]; and it is possible [that] if they continue their labour that we shall know nothing at all."<sup>186</sup>

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<sup>186</sup> *Quotes about History* (Internet), presented by Freneq Seasz (Jan 25, 2005)

## DARK AGES

What happens when the chronology of the ancient world is actually lengthened far beyond the limits of what it actually is? This, in fact, was the very same problem that arose when the early historians of the Near East introduced their first chronology which held that civilization using written materials had begun about 5500 B.C. Leonard W. King discusses what had happened when that paradigm was finally explored:

“Considerable changes have recently taken place in our estimate of the age of Sumerian civilization, and the length of time which elapsed between the earliest remains that have been recovered and the foundation of the [Old] Babylonian monarchy. It was formerly the custom to assign very remote dates to the early rulers of Sumer and Akkad, and though the chronological systems in vogue necessitated enormous gaps in our knowledge of history it was confidently assumed that these would be filled as a result of future excavation. Blank periods of a thousand years or more were treated as of little account by many writers. The hoary antiquity as ascribed to the earliest rulers had in itself an attraction which outweighed the inconvenience of spreading the historical materials to cover so immense a space in time. But excavation, so far from filling the gaps, has tended distinctly to reduce them, and the chronological systems of the later Assyrian and Babylonian scribes, which were formerly regarded as of primary importance, have been brought into discredit by the scribes themselves. From their own discrepancies it has been shown that the native chronologists could make mistakes in their reckoning, and a possible source of error has been disclosed in the fact that some of the early dynasties which were formerly regarded as consecutive were actually contemporaneous.”<sup>187</sup>

In order to reduce or get rid of these great blanks or gaps in history, the historians were driven to shorten the chronology of the ancient Near East in the hope that, if the historical period were to begin around 3000 B.C., the problem of gaps, blanks, or more properly Dark Ages, would disappear. That, in fact, has not

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<sup>187</sup> Leonard W. King, *A History of Sumer and Akkad* (NY 1910), 1968 reprint, p. 56

happened. Rather than thousand-year Dark Ages, we will see that these gaps still exist but are of shorter duration.

If the correction made to shorten the 5500 B.C. limit to 3000 B.C. is correct, then residual Dark Ages should have been filled in with additional excavations and light should have been shed upon these dark periods from which the archaeologists and historians could outline the events that took place in them. A good chronology over time would have found the answers with more and more material evidence that would elucidate and explain away any of these remaining lacunae and the problems associated with them. Yet over the past century of archaeological and historical research the Dark Age problems of Mesopotamia (as well as Egypt) have remained intractable and there is no data to bring about closure or even begin to resolve these gaps/blanks/Dark Ages in the history of these ancient regions.

Dark Ages are symptomatic of a chronology that is overly lengthened beyond the bounds of reality. As King pointed out, “dynasties which were formerly regarded as consecutive were actually contemporaneous.” And to some extent making the Old Babylonians come well before Persian times when they were contemporaneous with the Persians has created two such Dark Ages.

What were the events in Babylonia that led to the Old Babylonians’ conquest of that region, and what transpired after they fell from power? The answer given by historians to these queries is that a long Dark Age engulfed Babylonia prior to their coming to power and that when they fell, the region was again overwhelmed by a second lengthy Dark Age. The only way to explain these Dark Ages is by making historical speculations about them. The transition period just before the Old Babylonians came to dominate that region is a blank/gap without a shred of solid evidence to validate what happened, just like the Dark Ages of Greece, of the Hittites, and of others discussed in volume I of this series. Whitney Davis reports on the first of these blank periods:

“A dark age in Mesopotamian history begins at the fall of Ibbi-Sin [the last so-called Akkadian king] lasting a century. When we pick up the thread the [Old Babylonian] Amorites are fully in control at such

cities as Larsa, Kish, Babylon, Sippar, Marad, and Uruk, and large segments of the population own Amorite names.”<sup>188</sup>

As to why the earlier Akkadian empire fell and was taken over by the Old Babylonians, historians haven’t the vaguest notion, let alone evidence. We are expected to believe that a well-organized, powerful empire imploded or was somehow overthrown by migrating desert tribes who came out of nowhere and then failed to record their mighty conquest as did other conquerors. But there is no Dark Age if we employ Heinsohn, Rose, and Sweeney’s thesis. The obvious answer is that the Persians/Old Babylonians under Cyrus the Great overwhelmed the entire region quickly and abruptly. “The Persian conquest in 539 [B.C.] was rapid.”<sup>189</sup> Hicks *et al.*, describe for us how several civilizations fell swiftly before Persian might:

“Starting in 559 B.C., the Persians needed only about 30 years to burst from obscurity and create the first world empire. In that span—little more than a generation—people from Greece to Ethiopia, from Lydia to India came to regard the monarch on the throne of Persia as the only king who mattered.”<sup>190</sup>

Oates reports on what transpired with the collapse of the Old Babylonian empire:

“The history of Babylonia following upon the Hittite attack [that conquered the city of Babylon] is far from clear. Indeed all documentary evidence ceases and Babylonia was engulfed in what our present ignorance leads us to term a Dark Age. There is no general agreement about the length of this period. ...”<sup>191</sup>

The cause of the fall of Babylonia was supposedly a Hittite raid led by Mursilis I who sacked Babylon. Fernand Baudel expresses it thus: “For a brief moment in 1545 B.C. they [the Hittites] surprised themselves by capturing Babylon,

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<sup>188</sup> Davis, *op.cit.*, pp. 1234-35

<sup>189</sup> France Joannes, “Private Commerce and Banking in Achaemenid Babylon,” *Civilizations of the Ancient Near East*, vol. III, *op.cit.*, p. 1475

<sup>190</sup> Hicks *et al.*, *op.cit.*, p. 9

<sup>191</sup> Joan Oates, *op.cit.* fn 183, p. 84

but were so disconcerted by their extraordinary victory that they abandoned it immediately.”<sup>192</sup>

Sidney Smith calls this episode: “One of the major mysteries of [Old] Babylonian history has always been the establishment of Kassite rule over Babylonia as a result of the Hittite raid. ... Explanations [for it] had to be invented.”<sup>193</sup> He adds: “Historians have generally assumed that the campaign of Mursilis I was the same as that which brought the [Old Babylonian] dynasty to an end.”<sup>194</sup> Trevor Bryce explains:

“The Babylonian expedition in particular raises some fundamental questions about its purpose and Mursilis’ expectation of it. He could not have hoped to convert the entire region of Aleppo to Babylon into Hittite subject territory. The vastness of this region, its remoteness from the Hittite homeland and the very capacity of the Hittites to exercise permanent control over conquered territories would have made such a process unthinkable ...”<sup>195</sup>

Walter Sommerfeld specifically states of the supposed Kassite conquest: “It is impossible to say how the[ir] seizure of power took place.”<sup>196</sup>

But the Persians conquered an even vaster territory and ruled it as did the Assyrians even earlier. What made the Hittite ruler so reticent about enlarging his domain as did countless conquerors before and after him? Johannes Lehmann emphatically states that although “It [the conquest of Babylon] was a prodigious feat. Mursilis did not, of course, ‘destroy’ Babylon, but Hammurabi’s dynasty was overthrown.”<sup>197</sup> But there were also several other cities around Babylon that had not been destroyed, with kings, armies, and resources to defend their homeland. These rulers would thereafter have come to Babylon to rebuild and fortify it.

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<sup>192</sup> Fernand Baudel, *Memory and the Mediterranean* (NY 2001), p. 201

<sup>193</sup> Smith, *op.cit.*, p. 24

<sup>194</sup> *ibid.*, pp. 12-13

<sup>195</sup> Trevor Bryce, *The Kingdom of the Hittites* (Oxford, UK, 1998), p. 103

<sup>196</sup> Walter Sommerfeld, The Kassites of Ancient Mesopotamia: Origin, Politics, and Culture,” *Civilizations of the Ancient Near East*, vol. II, *op.cit.*, p. 918

<sup>197</sup> Johannes Lehmann, *The Hittites: People of a Thousand Gods* (NY 1977), p. 199

The point that intrudes itself is: why after conquering one of the richest cities in Mesopotamia and having the kingdom of Babylonia at his feet did Mursilis decide to let go of this vast, rich land? Roux makes it clear that the “fierce Gasgas tribes ... to the north, the Luwians to the west and the Hurrians to the east opposed [the Hittites and were] a triple barrier [to Mursilis’ expansion]. Only to the south was the road [of conquest] relatively free.”<sup>198</sup>

Gasche *et al.* conclude: “The collapse of Samsuditana’s [Old Babylonian] rule is assumed to have been the result of a [Hittite] raid although these two events are nowhere directly connected in ancient sources.”<sup>199</sup>

But all these invented explanations have no basis in scientific fact, none of them are testable or falsifiable. The entire process is scissors and paste history. Nevertheless, these guesses of what actually happened are passed along as historical reality.

The forensic historical evidence regarding Hittite/Lyidian history was outlined in volume I of *Pillars of the Past*, where it was shown that the Hittites existed in the first and not the second millennium B.C. This was based on linguistics, in that the Lydians and Hittites used the same language, though conventionally separated by at least 600 years, an impossible linguistic condition. The Hittites were also shown there to be trading tin and using tin bronze hundreds of years before tin was imported into Anatolia based on the established chronology. The same contradiction is related to the Hittites having iron hundreds of years before they invented processes to manufacture steel, as well as several other forms of historical and archaeological evidence that correlates, corroborates, and converges with the scientific and technological evidence for placing the Hittites in the first millennium B.C. The Hittites—being the Lydians, a first millennium B.C. people—could not have been responsible for overthrowing a so-called second millennium people.

The significant point is that there is a Dark Age that supposedly follows the fall of the so-called Old Babylonians. This problem looms large on the historical landscape. On November 8th-9th, 2002, a special research conference was held in Vienna titled “Mesopotamian Dark Age Revisited” which specifically dealt with this problem.

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<sup>198</sup> Roux, *op.cit.*, 3rd ed. 1999, p. 245

<sup>199</sup> Gasche *et al.*, *op.cit.*, p.6

“[Its] main objective ... was to evaluate sources from Mesopotamia and its so-called peripheral areas shortly before, during and after the ‘Dark Age’ and possibly overcome and define this period of darkness by chronological means. ...

“We believe to have collected several valuable contributions covering different aspects of chronological research in this volume which may clearly show the abundant work still to be done in order to find reliable answers to chronological questions.”<sup>200</sup>

The people who came after the fall were taken to be the Hurrians and Kassites. But of these people historians claim that they know practically nothing. As Baudel reports:

“The Hurrians whose language bears no relation to any other known language apart from Urartian may have come from Armenia. ... Whoever they were they scattered throughout the cities of Mesopotamia ...”<sup>201</sup>

As for the Kassites, he adds:

“If we take another famous example, the Kassites, their origins too are uncertain. They may have come from Iran or the far-off Caucasus ... They are first detectable in the Zagros [mountains], the launch pad for their final emigration. Their language, another non-Indo-European one, might have provided some clues. If these immigrants had not so quickly abandoned it; they adopted Akkadian as soon as they arrived in Mesopotamia in the second millennium [B.C.].”<sup>202</sup>

The origins of the Kassites and Hurrians are unknown. Of the Kassites, Dominique Charpin reports: “This people [the Kassites] ... constitute an enigma for historians: they became so well assimilated in Babylonia that their language is

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<sup>200</sup> Gasche *et al.*, “Preface”, *op.cit.*, p. 7

<sup>201</sup> Baudel, *op.cit.*, p. 136

<sup>202</sup> *ibid.*, pp. 136-137

practically unknown.”<sup>203</sup> How they conquered the Old Babylonian empire is also unknown. Their languages are unknown, their methods of rule, communication, warfare and much else can, at best, be built up based on conjecture. Regarding the duration of this Dark Age and the problem it involves, *The Cambridge Ancient History* shows:

“The removal of the written materials and seals from Late Babylonia (11th to 8th [centuries B.C.]) to Middle Kassite Babylonia (15th to 12th [centuries B.C.]) causes a major archaeological problem. It appears the late Babylonians [of the 11th to the 8th centuries B.C.] had no written records. This problem is referred to as the ‘Dark Age’ of Babylonia. Brinkman writes ‘Babylonian history of the first quarter of the first millennium may be characterized as a period of obscurity or “Dark Age.” Little source material has survived from these turbulent times.’ Brinkman’s figure of 60 texts from the Babylonian ‘dark age’ is reduced to a small number when one considers that the Luristan bronzes represent half the text [material]. They were apparently found not in Babylonia but in the Zagros Mountains. Cuneiform texts from other periods of Babylonian history number in the thousands.”<sup>204</sup>

But what is really known of the so-called Kassite period? Roux says it, too, was a Dark Age:

“Unfortunately, we are not much better off as regards the period of Kassite domination in [Babylonian] Iraq ... all we have at present is less than two hundred royal inscriptions—most of them short and of little historical value—a few letters and a number of economic texts, which is very little indeed for four hundred years ... The bulk of our information derives, in fact, from sources foreign to the kingdom of Babylon. ... This silence makes the Kassite period one of the most obscure in Mesopotamian history, and the words ‘dark age’ and ‘decadence’ come easily to mind.”<sup>205</sup>

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<sup>203</sup> Dominique Charpin, “The History of Ancient Mesopotamia: An Overview,” *Civilizations of the Ancient Near East*, vol. II, *op.cit.*, p. 817

<sup>204</sup> *CAH*, vol. 3, pt. 1 (Cambridge, UK, 1982), pp. 282-283

<sup>205</sup> Roux, *op.cit.* (3rd ed., 1999), p. 228



He adds that “the paucity of our sources makes this long and interesting period one of the least-known in the history of ancient Iraq.”<sup>206</sup> Smith writes that because of this lack of sources, the relationship of the Old Babylonians to the Kassites indicates that “there has never been any decisive proof of the exact relation of this [Kassite] dynasty to later events.”<sup>207</sup>

Thus we supposedly have, with the fall of Babylon around 1600 B.C., a Kassite Dark Age running for 400 years to about 1200 B.C. We furthermore have a Dark Age thereafter covering the Neo- or Late Babylonians down to the 8th century B.C. according to *The Cambridge Ancient History*, cited above. This additional Dark Age was discussed by Velikovsky in his *Dark Age of Greece*. Gordon Childe neatly sums up the situation after the fall of Babylon:

“So the Bronze Age in the Near East ended around about 1200 B.C. in a Dark Age blacker and more extensive than those [two other Dark Ages] that opened our last chapter. Not in a single state alone but over a large part of the civilized world history itself seems interrupted: the written sources dry up, the archaeological documents are poor and hard to date. ...”<sup>208</sup>

Thus, there is no direct connection in the ancient sources that proves that the Hittites brought down the Old Babylonian empire. There are Dark Ages running, according to the established chronology, from before the Old Babylonians and after them, covering over a thousand years. These run from the fall of Babylon around 1600 B.C. down to about 750 B.C. However, by moving the Old Babylonians to the Persian period, all these Dark Ages vanish. There is no 100-year Dark Age prior to the Old Babylonian empire being established, nor is there a 400-year Dark Age for the Kassite period following them, nor is there a 500-year Dark Age following the decline of the so-called Kassites. All these blanks, gaps, etc., in the chronology only derive from a false chronology that is overly lengthened. As C.W. Ceram insightfully remarked:

“Yet it remains astonishing that no one thought of subjecting the established *chronology* of events ... to a searching criticism. Surely

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<sup>206</sup> *ibid.*, p. 242

<sup>207</sup> Smith, *op.cit.*, p. 2

<sup>208</sup> Gordon Childe, *What Happened in History* (NY 1946), p. 178

someone should have guessed, even if he had not been able to prove it at once, that what had gone wrong was the whole system of dating. Surely it should have occurred that ... people's history cannot stop dead for ... hundreds of years."<sup>209</sup>

There is, however, a further outstanding problem regarding these lengthy Dark Ages that exists for the established chronology but not for those of Heinsohn, Rose, and Sweeney, namely, a scientific and mathematical Dark Age running from the Old Babylonians down to the great flowering of the Hellenic Greeks of around 750 B.C. According to the historians, the Old Babylonians had developed very advanced forms of mathematics as well as astronomical tables related to planetary movements and lunar eclipses. Evan Haddingham reports:

“The [Old] Babylonian reliance on numerical methods [for solving mathematical problems] is understandable, considering that they practiced sophisticated arithmetic as far back as 1800 B.C. ... At this early stage, there already existed tables for multiplication, division, square roots, exponential functions and many other mathematical procedures.”<sup>210</sup>

The Venus tablets represent only one of the sets of astronomical tables supposedly produced in these early times. The Greeks in their Golden Age later derived much of their astronomical knowledge and understanding of mathematics from these Old Babylonians ... or so we are told. The problem is: How was this precise information transmitted across these great gaps, these Dark Ages which intervened? As we were informed, little or almost nothing of the writing from one age was passed on to another. We saw above that “the written sources dry up,” or “all we have at present is less than two hundred royal inscriptions—most of them short and of little historical value—a few letters and a number of economic texts,” or “It appears the Late Babylonians [of the 11th to the 8th centuries B.C.] had no written records,” etc. These were supposedly ages of chaos and warfare with little if anything transmitted. Added to this is the 500-year Dark Age of Greece running

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<sup>209</sup> C.W. Ceram, *The Secret of the Hittites*, transl. Richard and Clara Winston (NY 1955), p. 130-131

<sup>210</sup> Evan Haddingham, *Early Man in the Cosmos* (NY 1984), p. 13

from 1200 to around 750 B.C. According to Vincent Desborough, during the Greek Dark Age “the art of writing is forgotten.”<sup>211</sup>

With these Dark Ages in which almost nothing in writing was transmitted from the fall of the Old Babylonians to the 8th century in Mesopotamia, and with a Dark Age where “the art of writing [was] forgotten” in Greece, how was this mathematical and astronomical knowledge transmitted?

“Priority in the development of mathematics belongs to Babylonia where ancient ... numerical, algebraic and geometrical methods existed at least from the Hammurabi dynasty around 1700 B.C. Little is known of the ... later period until Persian times.”<sup>212</sup>

In essence we are told that between Hammurabi’s dynasty and Persian times little mathematical or astronomical data survived. Yes, here and there some odd piece of evidence exists in the literature, but how do we know it was transmitted beyond that time down to Persian times? In terms of Heinsohn’s, Rose’s, and Sweeney’s theses, since there are no intervening Dark Ages between the Old Babylonians and Persians, both being from the same Persian era, transmission of this information to the Greeks poses no problem at all. Peter James *et al.* nicely summarize the problem:

“The scarcity of the documents from [this] period poses yet another riddle. Given the tiny number of surviving texts, how could literacy have been preserved at all? Babylonia, from the 8th century BC onwards, was widely respected by its contemporaries (including the Assyrians, Hebrews and Greeks) as a centre of literature, possessing an immense corpus of written knowledge from mathematics and astronomy to medicine and philosophy. ... How the complex Babylonian [astronomical, mathematical and] administrative or commercial systems could have survived for so long with so few written documents is simply unfathomable.”<sup>213</sup>

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<sup>211</sup> quoted in Peter James *et al.*, *Centuries of Darkness*, (London, 1991), p. 72

<sup>212</sup> *Encyclopaedia Britannica Macropedia*, vol. II (Chicago 1982), p. 640

<sup>213</sup> James *et al.*, *op.cit.*, p. 282

While some researchers may argue that the Greeks did not necessarily obtain their knowledge of mathematics or astronomy from the Babylonians but developed these on their own, the Greeks themselves admit they did. Olmstead speaks of “the priority of so much [Old Babylonian] mathematical knowledge” and notes “... the various admissions by the Greek authors of this [Old Babylonian] priority.”<sup>214</sup>

Heinsohn also equates the Neo-Assyrians with the Persian rule over Assyria. In Neo-Assyrian/Persian strata Old Babylonian mathematical materials were recovered as well. Being that Persia ruled Babylonia and Assyria at the same time, Babylonian works would have been carried to Assyria under the Neo-Assyrians.

Eleanor Robson, Junior Lecturer in Akkadian at Oxford University, on her internet web site, *Mesopotamian Mathematics: Some Historical Background*,<sup>215</sup> points out:

“But perhaps more excitingly, a mathematical problem is known in no less than three different copies, from Nineveh and Nippur. Multiple exemplars are rare in the mathematically rich Old Babylonian period . . .

“It is a teacher’s problem text, for a student to solve, and it is couched in exactly the sort of language known from the Old Babylonian period.”

More evidence regarding the close relationships between the mathematics of the Old Babylonians, Egyptians, and Greeks will be presented in volume III of this series.

All the attempts to explain these “gaps,” “blanks,” or “Dark Ages” are without merit. They are clearly explained by Heinsohn, Rose, and Sweeney’s thesis that the Old Babylonians are the Persian rulers of Babylon. There are no Dark Ages; therefore, nothing of mathematics, astronomy, medicine, philosophy, literature is

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<sup>214</sup> Olmstead, *op.cit.*, 1948, p. 209 fn

<sup>215</sup> *Mesopotamian Mathematics: Some Historical Background* ([www.hps.cam.ac.uk/dept/robson-background.pdf](http://www.hps.cam.ac.uk/dept/robson-background.pdf)) page 7

lost. The end of the Old Babylonians/Persians by Alexander the Great allows for the direct transmission of these scientific materials.

A lot could also have been transmitted to the Greeks in the two centuries or so prior to Alexander, which poses no historical-chronological problem. As with all the other problems discussed thus far, the Dark Age dilemma disappears. Dark Ages are a convenient excuse to maintain the established chronology which is full of these “gaps” and “blanks” that have been artificially placed there by the historians and archaeologists in the same way that they placed a fictitious 700 to 800 year gap in the strata at Tell Munbaqa. Without these artificial Dark Ages intruding in the chronology, the history of Mesopotamia flows simply, logically, and is correlated with, and corroborated by, the forensic evidence as well. All these forms of evidence converge to show that the short chronology is the only one that properly accounts for and makes elegant sense of Mesopotamian history.

## **THE OLD BABYLONIAN/PERSIAN EMPIRE AND PEOPLE**

“Hammurabi and Darius I were separated by differences of blood and religion, and by almost as many centuries as those that divide us from Christ; nevertheless, when we examine the two great kings [and their empires] we perceive that they are essentially and profoundly akin.”

Will Durant  
*Our Oriental Heritage*  
(NY 1954), p. 291

While in the last unit we dealt with evidence that was not specifically scientific or technological, but is related to this type of evidence, we now come to the non-scientific, non-technical evidence of a sociological, administrative, and religious nature. These forms of evidence may be employed at this point to demonstrate similarities, and even unique similarities, for both the Old Babylonians and the Persians. Having first established the scientific, technological, stratigraphical, archaeological, and linguistic foundations that make the real links

between these cultures, we now turn to the historical forms of evidence. First things first, last things last.

It is these materials, which are most susceptible to interpretation, that are also most susceptible to criticism and which critics of the short chronology will attack most vigorously. To do so without fully addressing and answering the forensic historical evidence, that is, by ignoring that evidence and instead turning to documentary materials, shows that the critic has no forensic evidence upon which to build a chronology. To ignore the forensics is merely a ploy to evade responsibility for failing to address these facts. Whatever form that evasion takes, it proves that the critic cannot and will not face scientific and technological facts. Yes, documents cannot be completely ignored either, but they come into play only after forensic evidence speaks. Documents do contain truth, but untestable or unfalsifiable truth as compared to science and technology.

Nevertheless, let us now turn to historical evidence, having first established the rigorous foundations. As is well known, the Persians, after taking Babylonia, instead of forcing their customs, manners, religion, etc., on their subjects, restored local temples and acted as protectors of the local cults; they scrupulously saw to the maintenance of the canal systems. Precisely the same dedication, administration, and communication characterize the Old Babylonians:

“The [Old Babylonian] Amorites ... established themselves everywhere in Babylonia. ... [They] restored local temples and posed as protectors of the local cult; they scrupulously saw to the maintenance of canal systems. ... The principal evidence for conditions at the time of the [Old] Babylonian kings consists of numerous inscriptions giving accounts of the rebuilding or building of temples and city walls, as well as letters exchanged between kings and provincial governors.”<sup>216</sup>

One could argue that this was nothing new and that this practice was followed by other conquerors of Babylonia, but there are further links making this connection. Both Darius and Hammurabi *seem to have* created elaborate espionage systems in order to keep themselves informed of what was happening throughout the empire. Frye reports:

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<sup>216</sup> Laessoe, *op.cit.*, p. 34; see also *CAH*, vol. IV (Cambridge, UK, 1953), p. 188

“To turn now to the centralized bureaucracy of the Achaemenids [Persians], the presumed special agents of the king have attracted considerable attention ... among contemporary scholars. Obviously, so the argument goes, the king of kings needed a corps of officers to keep him informed of events in his empire and keep watch over the many local kings and satraps. We do find references in Greek sources to the ‘eye of the king, [and] ear of the king.’ ... The Ionian sources have no title even similar to ‘eye of the king,’ but in Aramaic papyri from Egypt we find the term *gwshky*’ from the Iranian [Persian] *gaushaka*, ‘listener.’ Here it seems [the] official who represents the central government in legal cases, perhaps a state’s attorney. This is all the firm evidence we have for these offices. One may surmise from later practice in Parthian times [in Persia] and in Armenia that the official known as *bitax*’s represented an older Achaemenian [Persian] office ..., a vice king, who might be identified as the ‘eye of the king,’ and who might have supervised *inter alia* the many state prosecutors, or ‘the ears of the king’.”<sup>217</sup>

The information of a Persian spy system may have been derived from Xenophon, whom many foreign scholars follow regarding this matter:

“Xenophon in his *Cyropedia* insists that ... king [Darius] had a plurality of ‘eyes’ and ‘ears’ in the sense of informers, and elsewhere he speaks of men of trust sent by the king to inspect people at a distance. But we cannot tell whether he is speaking from first hand knowledge . Herodotus would date the use of a network of royal spies and listeners to the foundation of the Median Kingdom.”<sup>218</sup>

Historian Geo Widengren apparently accepts the view that the Persians had well-organized spy networks:

“The famous ‘eyes’ of the [Persian] kings inspected the satraps’ administration without announcing their arrival, and a well-developed system of organized espionage served as an ultimate control.”<sup>219</sup>

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<sup>217</sup> Richard N. Frye, *The Heritage of Persia* (Cleveland & NY 1963), p. 47

<sup>218</sup> *The Cambridge History of Iran*, *op.cit.*, p. 223

<sup>219</sup> Widengren, “The Persians,” *op.cit.*, p. 337

Hicks *et al.* speak of the Persians having a “carefully balanced network of informants.”<sup>220</sup> If, as these historians agree, the Persians had a spy system, there is no doubt that the Old Babylonians also employed such networks. From the omen-texts of Hammurabi we may learn:

“Concerning the affairs of the council of statesmen or council of war certain signs revealed that ‘thy words will be carried to the enemy’. ... The omen texts reveal the range of confidants of whose fidelity the king was anxiously in doubt. ‘The word of the palace’ or ‘thy secret’ was always in danger of ‘getting out,’ there was always a bird in the air to carry the voice, when disclosure to an enemy or to a friend might be equally dangerous. A ruler must be on his guard against the intimates of his court, a barber, a woman, a counselor, a secretary, a chamberlain, a janitor, a noble, even his own son, or even the court-diviner himself. ... Spies are found coming and going between the armies. ... In a general sense the omen-texts bear eloquent testimony to the politics and intrigues of their day.”<sup>221</sup>

Saggs states:

“During his first thirty years he [Hammurabi] generally maintained good relations with the kings of Eshnunna, Larsa and Mari. Each of these kings had ambassadors at the courts of the others, where their duties were to watch over their masters’ interests, in some cases they interrupted this to include espionage on political and military developments by means of a local intelligence network.”<sup>222</sup>

The Persians, of course, would hardly have advertised to the world that their spies were everywhere. That is most probably why so little regarding these networks comes from Persian sources. On the other hand those who were being spied upon were evidently aware of these and discussed them. Thus the general lack of Persian evidence as opposed to Old Babylonian evidence for these intelligence networks is explained.

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<sup>220</sup> Hicks *et al.*, *op.cit.*, p. 60

<sup>221</sup> CAH, vol. 2, pt. 1, *op.cit.*, p. 215

<sup>222</sup> Saggs, *Babylonia*, *op.cit.*, p. 98



A further point related to the administration of the Persian/Old Babylonian empire is how tightly Darius/Hammurabi micromanaged the domain. Of Darius we are told about:

“... measures for ... due control of the satraps. Important was the rapidity of communication [to] diminish the need for a satrap to undertake independent action without reference to [the Persian king at] Susa. Along the greatly improved roads and by means of the rapid government post, communications passed in great numbers and with frequency to and from satraps, to each of whom a royal secretary ... attended to the receipt and dispatch of correspondence between the king and the satrap.”<sup>223</sup>

The very same applied to Hammurabi:

“In general it may be thought that the letters of Hammurabi and his minister[s] hardly give the impression of a strong administration; what appears is a system too much absorbed in day-to-day detail; ... Such compliance is most probably due to conscious insecurity of the régime.”<sup>224</sup>

Along these same lines Saggs reports of the Old Babylonians:

“The governor himself was concerned with the maintenance of order, the execution of justice, and above all with public works, in particular the canals and irrigation system. ... In all these matters the final responsibility for decision rested with the king. ... This made it imperative that governors should make frequent reports. ...”<sup>225</sup>

In discussing Mari during the Old Babylonian period, Saggs reports:

“Perhaps the most striking fact emerging from these documents [at Mari] is the interest taken by the king himself in the affairs of the kingdom. Provincial governors, army chiefs, ambassadors to foreign

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<sup>223</sup> *CAH*, vol. 4 (1953), p. 197

<sup>224</sup> *CAH*, vol. 2, pt. 1, *op.cit.*, p. 187

<sup>225</sup> Saggs, *The Greatness that was Babylon*, *op.cit.*, p. 235

courts, officials of all ranks and even simple individuals constantly wrote to their sovereign, keeping him informed of what was happening in their particular field of activities and asking for advice. In return the king gave orders, encouraged, blamed, punished or asked for more information. A steady flow of letters carried by escorted messengers came in and out of the palace. Military and diplomatic matters and public works naturally formed the bulk of the state correspondence ... but more trivial subjects were also touched upon.”<sup>226</sup>

Not only was there an improved road system linking the capital with the empire to carry letters back and forth for the Persian/Old Babylonian empire but to enhance communications the Persians/Old Babylonians set up a system of fire signals along a string of sites. These apparently were used to report on dire threats to the king or for other emergency purposes:

“Fire signals are said to have been used by the Persians. Warning beacons had of course been used from time immemorial (e.g. in [Old] Babylonia), but the relay of messages [carried across vast distances] was a novelty if Herodotus is correct.”<sup>227</sup>

The very same form of communication was utilized by the Old Babylonians, as discussed by Saggs:

“To raise a general alarm in the event of a serious attack at any point there was a special system [in the Old Babylonian empire]. This involved a series of fire beacons spaced across the country, whereby in [an] emergency a warning could be rapidly flashed to the capital from the danger point.”<sup>228</sup>

Werner Keller also discusses this fire beacon system of the Old Babylonians at Mari:

“The news services in Mari functioned ... quickly and successfully. Important messages were sent by fire signals from the

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<sup>226</sup> Saggs, *Ancient Iraq, op.cit.*, p. 219

<sup>227</sup> Cook, *op.cit.*, pp. 108-109

<sup>228</sup> Saggs, *Everyday Life in Babylonia, op.cit.*, p. 67

frontier of Babylon right up to present day Turkey in a matter of hours, a distance of more than 300 miles.”<sup>229</sup>

As to the dress of the early Persians/Old Babylonians, the reports indicate that they wore leather clothing or skins of animals with fur. Hicks *et al.* report that the enemies of the Persians said that they were “preparing to fight against men who dress in leather—both breeches and everything else.”<sup>230</sup> Olmstead shows that Persian armies were made up of, among others, “Caspians,” who “wore skin coats” and “Pactyans ... clad in skins.”<sup>231</sup> Of the Medes, who may have served with the Persians, Olmstead adds that they wore “Leather trousers and laced shoes.”<sup>232</sup>

As for the Old Babylonians, prior to their conquest of Babylonia, Saggs says the people of that land claimed these foreigners “wore skins.”<sup>233</sup> This makes perfect sense because much of Persia is a mountainous land and can be quite cold. As the readers of volume I of *Pillars of the Past* may recall, the Scythians, who lived in lands of southern Russia which often experience brutally cold winters, also wore leather to keep warm. Neither Syria, the assumed homeland of the Old Babylonians, nor Arabia could be considered as such a cold habitat that its people were forced to wear leather or skins over their entire bodies.

Robert Drews points out that

“... the wise Sandanis counsels Croesus [king of the Lydians] not to attack the Persians, a tough lot who lived a tough life. The first sign of their hardhood that Sandanis produced was that ‘they wore leather trousers.’ We must observe, even though Sandanis and Herodotus did not, what the leather trousers denote: the Persians—like the nomads in the Pontic-Caspian steppe—wore leather trousers because they were often on horseback.”<sup>234</sup>

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<sup>229</sup> Werner Keller, *The Bible as History* (reprint NY 1980), p. 62, 64

<sup>230</sup> Hicks *et al.*, *op.cit.*, p. 9

<sup>231</sup> Olmstead, *op.cit.*, p. 241 and 242

<sup>232</sup> *ibid.*, p. 32

<sup>233</sup> Saggs, *Babylonia*, *op.cit.*, p. 92

<sup>234</sup> Robert Drews, *Early Riders The Beginnings of Mounted Warfare in Asia and Europe* (NY/London 2004), p. 134

As we will see below in Chapter 3, “The Mitannians and The Medes”, horseback riding by mounted warriors does not come into being until the first millennium B.C. Thus not only is the dress of the Old Babylonians like that of the Persians, but leather clothing clearly denoted the Persians.

Let us also recall that the homeland of the Old Babylonians is unknown, as discussed earlier. The question at hand is: How was their original home described by the people who encountered them? Persia, as is well known, is a cool, mountainous land covered by high, snow-clad peaks year-round. Neither Syria nor Arabia can lay claim to such a description for these regions. M. Liverani describes a Mesopotamian king who had a battle with the Old Babylonians “in a mountainous region.”<sup>235</sup> Hicks *et al.* claim the Old Babylonians had settled “in the mountainous back country of Elam.”<sup>236</sup> Olmstead describes the climate of Persia thus: “Nowhere was ... rainfall sufficient to bring crops to maturity, but melting snow fortunately ran down from barrier mountains [to allow for agriculture via irrigation],”<sup>237</sup> and speaks of Persia’s “snow-capped peaks.”<sup>238</sup>

With respect to the homeland of the Old Babylonians, Clay describes it “with its diversified features of snow-capped mountains, table lands, fruitful plains and tropical valleys.”<sup>239</sup> Let us now compare the topography and climate of Persia/Iran with that of Syria to see which of these lands fits the descriptions of the Old Babylonian homeland. For Iran, we are told:

“A series of massively eroded mountain ranges surrounds Iran’s high interior basin. Most of the country is above 1,500 feet (460 metres) [above sea level], with one sixth of it over 6,500 feet. In the north the 400-mile strip along the Caspian Sea more than 70 miles wide and frequently narrowing to 10 falls sharply from 10,000-foot summits to the marshy lake’s [i.e. the Caspian Sea’s] edge, 90 feet below sea level. Along the southern coast, the land drops away from a 2,000 foot plateau, backed by a rugged escarpment three times as high, to meet the Persian Gulf and the Gulf of Oman.

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<sup>235</sup> Liverani, *op.cit.*, p. 104

<sup>236</sup> Hicks *et al.*, *op.cit.*, p. 13

<sup>237</sup> Olmstead, *op.cit.*, p. 20

<sup>238</sup> *ibid.*

<sup>239</sup> Albert T. Clay, *The Empire of the Amorites* (Yale Oriental Series), vol. VI (London 1919), p. 67

“The Zagros [mountain] range stretches from the border of Armenia in the northwest to the Persian Gulf, and thence eastward into Beluchestan. ... The Elburz Mountains ... run along the south shore of the Caspian Sea to meet the border [mountain] ranges of Khorasan to the east. The highest of the chain’s many volcanic peaks [are] snow clad.”<sup>240</sup>

With respect to the Persian/Iranian climate, we learn “it ranges from subtropical to subpolar.”<sup>241</sup> These descriptions well fit Clay’s picture of the Old Babylonian homeland with its “snow-capped mountains, table lands, fruitful plains and tropical valleys.” Syria, on the other hand, where they are assumed to have originated, contains no high, snow-clad mountains, high plateaus, or subtropical conditions. Of the Syrian region we are told:

“The Jabal an Nusayriyah [mountain] range borders the coastal plain and runs from north to south. The mountains have an average width of 20 miles, and their average height declines from 3,000 feet (900 metres) in the north to 2,000 feet (600 metres) in the south. Their highest point is 5,125 feet. ...

“The Anti-Lebanon Mountains ... mark Syria’s border with Lebanon. The main ridge rises to a maximum height of 8,625 feet ... while mean [or average] height is between 6,000 and 7,000 feet. Mt. Hermon ... Syria’s highest point rises to 9,232 feet (2,814 metres).

“Smaller mountains are scattered about the country.”<sup>242</sup>

As for Syria’s climate, “The coast and the western mountains have a Mediterranean climate. ... Snow may occur in winter away from the coast and frosts are common.”<sup>243</sup> This does not square with a land of “snow-capped mountains” as well as “tropical valleys.”

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<sup>240</sup> “Iran,” *Encyclopaedia Britannica Macropedia*, vol. 21 (Chicago 2002), p. 930

<sup>241</sup> *ibid.*, p. 931

<sup>242</sup> *op.cit.*, vol. 28, p. 361

<sup>243</sup> *ibid.*, p. 363

A view of any good atlas such as the *Readers Digest Atlas of the World* *Rand McNally Maps* (Pleasantville NY 1987), page 112, will make it quite clear that Persia/Iran fits the description of the Old Babylonians' homeland, and Syria or Arabia do not. The Old Babylonians are said to have settled "in the mountainous back country of Elam", which is in southern Persia. Therefore, the original homeland of the Old Babylonians apparently was Persia, as the Heinsohn, Rose, and Sweeney theses demand.

A further aspect of the Persians which was quite unusual for their time was their incestuous marital customs. Velikovsky has gone into some detail regarding this in *Oedipus and Akhnaton*:

"The Iranians had an approach to the problem of incest very different from that of other peoples of antiquity. They had an ethical religious concept and practice of *xvaetvadatha* ... which means, according to ancient authors and modern scholars alike, the marriage of parents with their children and of uterine brothers and sisters. The ancient Iranian texts commend and even command *xvaetvadatha*; in certain religious ceremonies only a young man who has undertaken it may officiate. ... Obviously it was not only the royal house that practiced incest but the Persians of various ranks too. [Quintus Curtius Rufus (viii, ii, 19) tells of the Bactrian satrap Simithrites who married his mother.] Marital relations with mother, daughter, and sister among the Persians are reported with odium by Diogenes Laertius, Strabo, Plutarch. ... Strabo declared ... 'These Magi, by ancestral custom, consort even with their mothers. Such are the customs of the Persians'.

...

It is even said to have been prescribed by Zarathustra as the eighth of his ten admonitions to mankind."<sup>244</sup>

According to Herodotus the Babylonians of that age also had an unusual religious custom. A woman had to go to a temple and sit outside until a man—no matter who—came to her and dropped a coin in her lap. She then followed the stranger into the temple to cohabit with him there and then, and only then could she return home. But these were the original Babylonians whom the Persians had

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<sup>244</sup> Immanuel Velikovsky, *Oedipus and Akhnaton* (NY 1960), pp. 99-100

subdued and reigned over. Surely this indicates a certain openness to unusual sexual practices. However, when the Old Babylonians ruled their lands, according to Saggs, the various peoples who had contact with them claimed they “had perverted sexual practices.”<sup>245</sup>

Homosexuality was well known to society since early times and was not considered “perverted” by the Greeks nor, probably, by the Babylonians. It seems highly probable that the Old Babylonians practiced incest. This does not preclude that they also kept intact the laws of the land they ruled over. Since incest was a perverted practice to the Babylonians, Hammurabi in section 154 of his code *punished Babylonians* who practiced it. But the unusual aspect of the law is that while other peoples punished incest with death, the Old Babylonians/Persians merely demanded exile for it. The problem is: Why did the Babylonians call their Old Babylonian rulers’ sexual practices “perverted”? In this case the rulers are being referred to. The Hammurabi Code does punish incest but these laws reflect the Babylonian customs and not those of the Persians. Again, the Persians did allow the local customs of their subject peoples to stand in law. This rendition of his Code was for the Babylonians and not for the Persians.

Lastly, with regard to burial of the dead by the Persians/Old Babylonians, we learn from Saggs that the people of Mesopotamia claimed that “when [the Old Babylonians] died they were not buried according to proper ritual.”<sup>246</sup> This, too, is reflected in the burial customs of the Persians. Herodotus remarks:

“There is a [Persian] practice ... concerning the burial of the dead, which is not spoken of openly and is something of a mystery; it is that a male Persian is never buried until the body has been torn by a bird or a dog. I know for certain that the Magi have this custom, for they are quite open about it. The Persians in general, however, cover the body with wax and then bury it.”<sup>247</sup>

Pierre Muret also writes along the same lines:

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<sup>245</sup> Saggs, *Babylonia*, *op.cit.*, p. 92

<sup>246</sup> *ibid.*

<sup>247</sup> Herodotus, *Histories* (transl. Aubrey de Sélincourt, Harmondsworth UK 1954/1978), Book 1, verse 140 ff.

“It is a matter of astonishment, considering the ‘Persians’ have ever had the renown of being one of the most civilized nations in the world, that notwithstanding they should use such barbarous customs about the dead as are set down in the writings of some historians. . . . If we will give credit to ‘Procopius’ and ‘Agathius,’ the ‘Persians’ were never wont to bury their dead bodies. . . . But, as these authors tell us, they exposed them stark naked in the open fields . . . as they were sooner or later made prey of.”<sup>248</sup>

This, of course, is highly nebulous and does indeed require more evidence before it will stand up to scrutiny. Granted, none of this historical evidence is to be taken as anything other than strongly indicative of the Persian/Old Babylonian equation, yet along with the forensic historical evidence they can be used to support the short chronology of Heinsohn, Rose, and Sweeney. Various aspects of Persian/Old Babylonian culture clearly lend themselves to be held in common.

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<sup>248</sup> Pierre Muret, *Rites of Funeral, Ancient and Modern* (London, 1683), p. 45



## PERSIAN/AMORITE RELIGION

As with the last unit, the historical evidence contained in this section is of a secondary nature because it is not testable or falsifiable as scientific evidence would be. Nevertheless, it does indicate that the Heinsohn, Rose, and Sweeney theses are valid. Regarding this material Cochrane states:

“As is apparent from the famous stele bearing his code of laws which shows the king before Shamash, Hammurabi was devoted to the Semitic sun-god. ...

“Darius ... was renowned for his patronage of Ahuramazda, the leading god of the Zoroastrian religion. ...

“Heinsohn would recognize a parallel between the Semitic god and Ahuramazda. ... Why this should be the case, Heinsohn does not elaborate. Yet the identification of Ahuramazda with the sun ... has long since been abandoned [by modern scholars]. In any case, it is difficult to see what Heinsohn would gain were the identification to hold, since he is still faced with the major problem of explaining why Hammurabi worships a vast Semitic pantheon while Darius tends toward monotheism, worshipping Indo-European gods alone. Until he [or anyone] answers this question, Heinsohn’s argument here amounts to little more than grasping at straws.”<sup>249</sup>

The problem Cochrane poses for Heinsohn’s Darius/Hammurabi equation is that since Darius *tended* to worship one Indo-European god among others, while Hammurabi worshipped a vast Semitic pantheon of gods, they could not be one and the same person. But what Cochrane has failed to discuss, or inform his readers of, is the fact that Darius and other Persian rulers, when they traveled outside Persia, and some of them even inside Persia, appear to have worshipped, and may have been involved with, non-Indo-European gods. They did this outside Persia to curry favor with their conquered subjects and priesthods. By worshipping their subject peoples’ gods and claiming that these gods had conferred kingship on them, they attempted to undercut any rebellious feeling for their domination. Since their

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<sup>249</sup> Cochrane, *op.cit.*, p. 66

subject peoples' own god or gods had given the kingship to the Persian king, that king was no longer a total outsider, especially since he rebuilt their temples and even worshipped at these shrines. Having done this the king appeared not as an outsider but made a powerful connection with the subject peoples and priesthood through their religion. As Henri Frankfort wrote in "The Making of a King",

"The accession of the new king was formally sealed by the ritual of coronation. To view such solemnities as purely symbolic distorts the significance which they had for the ancients. For them the first contact between the new ruler and the royal insignia was but the outward sign of a union in which the unchanging powers of kingship took possession of his person and made him fit to rule. ..." <sup>250</sup>

This may even explain why identical kings have different years of reign. If one was crowned in the conqueror's homeland first but in a subject's homeland at a later date, the scribes of each land may very well have dated the start of the reigns to different times. Let us now investigate whether Darius and other Persian monarchs worshipped non-Persian, non-Indo-European gods outside Persia. Hicks *et al.* discuss Cyrus' policy of worship in Babylonia:

"When Cyrus conquered Babylon he was aided by the Babylonian king failing to show respect for its god Marduk. ...

"If the people did have any fears about the new [Persian] regime Cyrus quickly reassured them and presented himself not as a foreign conqueror but as a king of Babylon personally selected by Marduk. ... Cyrus himself worshipped Marduk daily [and] returned [to them] the foreign god [which had been removed] by the last king [and] returned the gods ... to their legitimate abodes throughout the kingdom." <sup>251</sup>

The same tactful political/religious attitude and behavior is also true of Cambyses who followed Cyrus:

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<sup>250</sup> Henri Frankfort, "The Making of a King" (Internet), Introduction to Mesopotamia—  
www.gatewaystobabylon.com

<sup>251</sup> Hicks *et al.*, *op.cit.*, p. 321

“Rumors of his [Cambyses’] madly cruel acts against the Egyptian religion were soon spread by the priests and later told by Herodotus. However, they probably lack all foundation, for it is obvious that Cambyses in Egypt followed the same policy of [religious] toleration as had his father Cyrus in Babylonia. The Egyptian inscriptions are clear on this.”<sup>252</sup>

Darius, in this respect, was no different:

“Like Cambyses, Darius adopted as king of Egypt a name Sitis-Re that proclaimed his devotion to the [Egyptian, Semitic] god Re. He repaired the temple of [the god] Ptah at Memphis, and built the great temple in the oasis of Khargah. He made offerings to the gods and gifts to the priests. Uzahor in his inscription at Sais describes how Darius commanded him to reestablish the Temple-school there, and concludes eulogistically ‘all this the king did because he knew that such was the best means of awakening to new life all that was falling into ruin, in order to uphold the names of the gods, their temples, revenues and the ordinances of their feasts forever’! Later in his [Darius’s] reign ... the architect Khnum-ab-Re who carried out much work for Darius speaks of him as ‘a friend of all the gods’.”<sup>253</sup>

In fact, contrary to Cochrane’s statement, we are emphatically told:

“Not only were the Persians prepared to be tolerant of the various religions within their empire; they went further and actively supported the temple-worship of the gods of their subjects or contributed to the building of their temples.”<sup>254</sup>

What then is the documented religious record of the Persians in Persia itself on the worship of the gods? Cochrane seems to suggest that this is all well known and understood, but Sancisi-Weerdenburg claims that our present knowledge of the nature of religion is not at all that broad:

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<sup>252</sup> Widengren, *op.cit.*, p. 321

<sup>253</sup> *CAH*, vol. IV (1953), p. 25

<sup>254</sup> *ibid.*, p. 187

“Despite confident statements in the literature, lack of documentation makes it extremely difficult to make sense of the religious situation in Iran in the period [of Darius’ reign]. The primary evidence consists of two altar bases at Pasargadae that lack names or statues of the gods for whom they are erected. There are a number of personal names composed with the name of a god, such as Mithradates, attested for the reign of Darius, but by definition given a generation earlier.

“The issue of the Persian religion has become confused by attempts to identify in the very meager evidence traces of the religion that originated with Zarathustra [who] worshipped Ahura Mazda ... undoubtedly the supreme god in Persian inscriptions. ...

“Much new information on religious matters could be gleaned from the Persepolis tablets. Rations from the [Persian] royal granaries were to be distributed not only to Elamite [but also] Babylonian gods. The testimony on worship and the cult rests on a portrait that is considerably more intricate than that obtained by reading the royal inscriptions in which Ahura Mazda ... enjoys an exclusive position.”<sup>255</sup>

Thus in Persia itself there are documents that say the royal granaries were to supply food to the temples of Elamite and Babylonian gods who were Semitic and not Indo-European gods. Further, Cook reports:

“Herodotus tells us that the Persians made no statues of their gods because they considered it folly to visualize them in human form ... he seems to have been correct in this.”<sup>256</sup>

Yet Sancisi-Weerdenburg described the altar bases for statues that existed in Persia, lacking the statues of the gods for whom they were erected. If statues stood upon these bases, then these were not altars to Persian gods. What then of the Old Babylonians? Did they, as did the Persians, raise Marduk to a high place in the Babylonian pantheon? Roux reports:

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<sup>255</sup> Sancisi-Weerdenburg, “Darius I and the Persian Empire,” *op.cit.*, p. 1042 and p. 1045

<sup>256</sup> Cook, *op.cit.*, p. 149

“In order to legalize his dynasty and curtail any further claim to the kingship over Sumer and Akkad, Hammurabi put the god of Babylon, Marduk—hitherto a third-rank deity—at the head of the pantheon; but he tactfully proclaimed [as did Cyrus] that the divine lordship had been conferred to Marduk by Anu and Enlil [Indo-European gods] and that he, Hammurabi, had been ‘called by the same gods to promote the welfare of the people’.”<sup>257</sup>

Cook further explains that the Persians “were prepared to see in those [foreign deities] the equivalent of their own. . . . Ahura Mazda could be equated with Zeus . . . and with Bel.”<sup>258</sup>

There is a great deal of evidence that contradicts Cochrane’s assertion that Darius tended toward monotheism in that he only ever mentioned Ahura Mazda. But worst of all, some of these citations were in the very same literature Cochrane cited, namely Cook and Roux. Why, yet again, did he fail, at the very least, to report this material to his readers and discuss it? To paraphrase Cochrane in this regard as he wrote on page 64:

As a fellow who otherwise emphasizes the documentary record to the point of fixation, however, Cochrane’s position here regarding the written record he himself had read and then ignored when it was clearly inconsistent with what he was presenting does deserve comment.

Cochrane has also charged that “Heinsohn’s treatment of the text here is shoddy in the extreme if not downright deceptive.” So let us recapitulate and examine for a moment the written record to evaluate how Cochrane dealt with it.

Cochrane used *ad hominem* and guilt by association by comparing me to Leroy Ellenberger. That, I suggest, goes beyond “shoddy in the extreme.” He called my book “virtually worthless,” “wrongheaded,” and biased before he had even read it. This, I suggest, is shoddy in the extreme and downright deceptive. Cochrane also claimed that Darius in two years established himself as the uncontested ruler of the Persian empire by winning a great number of battles. But

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<sup>257</sup> Roux, *op.cit.*, p. 202

<sup>258</sup> Cook, *op.cit.*, p. 148

Cochrane's own source says this took one year and Cochrane's own source told him that "Darius proves himself a liar" regarding the length of time he took to re-conquer the empire. Isn't this shoddy in the extreme and downright deceptive?

Cochrane discussed and compared the lengths of Darius and Hammurabi's reigns to prove that they were not of the same length. This he did, despite the fact that Lynn E. Rose, in a book Cochrane possessed and had written a chapter in, showed him clear calendrical evidence that they and another set of kings reigned for the same length of time because they had identical 30-day months in identical years of reign in identical months of that year. But Cochrane chose to withhold this evidence as if it didn't exist. Isn't this really shoddy in the extreme and downright deceptive?

Cochrane has also spoken about the stratigraphy of Mesopotamia, although Heinsohn has published materials about Tell Munbaqa and discussed the geological work of Ulrike Rösner which proved that there is no geological or solid archaeological evidence for some 700 to 800 years of history into which the Old Babylonians should fall. This was published in *The Velikovskian* in 1993, vol. I, no. 1, a good six years prior to Cochrane's assault on Heinsohn. Heinsohn raised these very same issues in the Proceedings of the *Sesto Congresso Internazionale di Egittologia*, vol. II (Torino, Italy, 1993), p. 211, six years before Cochrane raised the issue of stratigraphy. And lastly, Rösner published her geological work in *Quartär* in 1995, four years before Cochrane mounted his assault. But no one reading his criticisms of Heinsohn in *Aeon* would have the faintest clue from Cochrane that this fundamental contradiction to nearly everything Cochrane was saying about stratigraphy existed. This, too, is downright deceptive and shoddy in the extreme.

Before going on, it must be pointed out that there is evidence that the Amorites came from Persia; as reported in *Eerdman's Dictionary of the Bible*, there is:

"... an ethno-linguistic term used to render Sum[erian] *martu* and Akk[adian] *Amurru*. The designation is imprecise, as the word Amorite was used differently in different times and places ... the word made its first significant appearance during the time of the Akkad kings ... who described Jebel Bishri, in Syria, as the Amorite mountain. This does not mean that this is their homeland or any place of residence, a century

later Gudea would describe two such ‘Amorite mountains,’ Jebel Bishri and Jebel Hamrin, N[orth] E[ast] of Sumer.”<sup>259</sup>

Gudea places the Amorites north east of Sumer which is clearly pointing to Iran/Persia. This placement is related to naming the Amurru or Martu.

And this is the question still to be addressed: Why were the Old Babylonians/Persians referred to in Babylonia as either “Amurru” or “Martu”? I tentatively put forth that the reason relates to their main god “Ahura Mazda” whom their subjects evidently knew to be their major deity. It may be that Amurru or Martu was a Babylonized form of Ahura-Mazda. The evidence seems to lend itself to this interpretation.

According to Clay, “Amurru was not only the name of the [Old Babylonian] country, but also the name of the chief deity of the land.”<sup>260</sup> Alfred Haldor states with respect to Martu “the god Martu is to be regarded as a personification of the Amorites.”<sup>261</sup> It thus appears that Amurru or Martu as Ahura Mazda was the god, or was seen as the god or gods of the Old Babylonians/Persians by the Babylonians. Whitney Davis specifically states:

“We recognize people as Amorites [that is, Old Babylonians] primarily when the sources add either Martu or Amurru to the names of individuals or tribes they are citing.”<sup>262</sup>

This would seem to indicate that Amurru and Martu are two distinctive gods and not one. On this question Clay explains:

“As is well known <sup>mat</sup>*Mar-tu* and *Mar<sup>ki</sup>* are ideograms of or represent the name *Amurru*, <sup>d</sup>*Mar-tu*. This would seem to indicate that *Amar* or *Mar* are related; and this is the fact. As stated [elsewhere], *Amar-Utug* became *Marduk* and *Amar-da* became *Marada*. That the names of the deity <sup>d</sup>*Mar* and *Amurru* are also identical is conclusively

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<sup>259</sup> *Eerdmans Dictionary of the Bible*, David Noel Freedman *et al.*, eds. (Grand Rapids MI 2000), p. 55

<sup>260</sup> Clay, *op.cit.*, p. 67

<sup>261</sup> Alfred Haldor, *Who were the Amorites?* (undated, Leiden Holland), p. 72

<sup>262</sup> Davis, *op.cit.*, p. 1232

shown. ... It seems therefore that no other conclusion can be reached but that *Mâr* and *Amâr* are variants of the same name.”<sup>263</sup>

Therefore the name of the Old Babylonian/Persian chief god, Ahura Mazda, may have been taken as the same name for the same god and Babylonized to Amurru or Martu. But what is most interesting is that the Persians/Old Babylonians raised Marduk, whom they may have recognized as a variant name of their own god, *Amâr* or *Mâr*, as the chief deity of Babylonia.

Above and beyond this tentative identification, it was pointed out to me by Birgit Liesching that both Ahura-Mazda and Marduk/Martu/Amurru are Jupiter deities. This indicates that since the Persians worshipped Jupiter as Ahura-Mazda, where their subject peoples worshipped Jupiter by names similar to their own or Jupiter under another name, they adopted that god by that foreign name. This is less tentative and must be taken into account when dealing with this issue.

Another question that has not been discussed thus far is: What was the original religion of the Old Babylonians? The evidence above seems to indicate that their religion was that of the Persians. We further made mention of the fact that the Persians did not force their god or religion on their subject peoples. Therefore little or nothing of their religion or religious customs such as their marriage via incest or their burial practices, etc., would have been known in Babylonia and it would appear that they had fully adopted the Babylonian pantheon, because to appear as acceptable rulers, they worshipped the Babylonian gods to strengthen their position with the people, cults, and priesthood. To recite Whitney Davis regarding this:

“We do not have a [religious] literature that we can specifically attribute to the [Old Babylonian] Amorites: no [religious] cosmology; no epics of heroes [and their relations with their gods], no list of Amorite gods.”<sup>264</sup>

This of course makes perfect sense in terms of Heinsohn’s, Rose’s, and Sweeney’s theses. Since the Persian/Old Babylonian rulers of Babylonia adopted the political and religious appearance of worshipping these foreign gods they would have left little if any evidence regarding their own religion except the names of their

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<sup>263</sup> Clay, *op.cit.*, p. 68

<sup>264</sup> Davis, *op.cit.*, p. 1231



main god and lesser ones, as well as perhaps indications of incestuous marriages and possible exposure prior to burial. Along with the forensic historical evidence we appear to know that the religion practiced by the Old Babylonians was that of the Persians. As Cook so cogently remarks:

“There was no attempt to force Persian religious beliefs and practices on subject people who had deities of their own. The kings did destroy some sanctuaries of alien gods, but this was by way of retaliation or punishment and not with intent to suppress other religions.”<sup>265</sup>

In addition, since the Old Babylonians are the Persian rulers of Babylonia it is obvious that they would be seen as far less polytheistic than the people with whom they had contact. They would only have worshipped those gods that were sufficient to maintain the appearance of their being Babylonians. Saggs claims that “from the extant evidence polytheism was less developed among the [Old Babylonian] Amorites than in Mesopotamian city-states.”<sup>266</sup> This is yet another strong indication that ties the Persians to the Old Babylonians.

One final point: It may very well be that Hammurabi and certain Old Babylonian rulers were indigenous rulers in Babylonia with their own gods, etc., but Darius and Artaxerxes III Ochus and other Persian kings were their overlords. These historical questions must be resolved but they must be resolved based on the scientific and technological evidence.

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<sup>265</sup> Cook, *op.cit.*, p. 147

<sup>266</sup> Saggs, *Babylonia*, *op.cit.*, p. 93

## CONCLUSION TO THE PERSIAN/OLD BABYLONIAN EQUATION

A history without the support of science and technology is only  
a great emptiness surrounded by an education.

Paraphrased from  
William E. Woodward's  
*Meet General Grant*,  
Chapter 15 (NY 1928)

It is impossible to accept the established chronology of the  
ancient Near East as valid because it is contradicted by scientific and  
technological fact, after fact, after fact, after fact, after ...

Charles Ginenthal

Oscar Wilde wrote three pungent *bons mots* that to my mind, when paraphrased to fit the contents of this book, well describe what historians have done to the chronology and history of the ancient world. One I paraphrase thus: "The 3000 year long history of the ancient world is largely an invention of historians." The second, "The historians have bestowed on the history of Egypt and Mesopotamia the gift of perpetual old age." The third, "There are two ways of learning to distrust the chronology of the ancient Near East, one way is to simply distrust it, the other way is to read the historians."<sup>267</sup>

What, then, are the scientific, technological, stratigraphical, linguistic, archaeological, and historical realities regarding Heinsohn, Rose, and Sweeney's Old Babylonian/Persian equation? Trevor Palmer in his brief criticism of the short chronology, which is entirely based on documentary evidence, has raised an important issue:

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<sup>267</sup> See Ringo, *op.cit.*, pp. 57, 58, 87

“If a plausible case is to be sustained for an *alter ego* [nation or king said to be the same as another nation or king], it is necessary to go much further than that and find points of similarity which are distinctive and numerous to justify [this]. ...”<sup>268</sup>

Palmer, of course, was discussing almost entirely history based on documents, but one cannot deny the application of this method of testing chronology by forensic historical evidence. Let us examine these elements, with first things first. These first things are the scientific and technical facts:

There is no scientific astronomical anchor for the established long chronology, though Cryer claimed such calendrical astronomical dating “is of primary importance.” The Venus tablets of Ammisaduqa have no standing by which their second millennium B.C. chronology could have been originally set up. The only second millennium scientific anchor that did exist prior to the work of Lynn E. Rose, the El Lahun Papyri for the 12th Egyptian Dynasty, demands that the 12th Dynasty be placed in the mid- to late part of the first millennium B.C. The heliacal rising of Sirius corroborated by “distinctive and numerous” lunar festival dates proves that point.

This has repercussions that resound across the ancient world. All the various forms of “distinctive and numerous” archaeological evidence, outlined by Sidney Smith and others, demand that the Old Babylonians be set, in time, almost parallel to the 12th Dynasty, and this requires that the Old Babylonians existed in the same time period as the Persians. Therefore, when Alexander the Great overthrew the 12th Dynasty in Egypt, he just a little later overthrew the Old Babylonians/Persians in Mesopotamia. Furthermore, along these same lines Rose has shown “distinctive and numerous” identical calendrical 30-day months that corroborate and correlate with the archaeological evidence. The Persian kings Darius I and Artaxerxes III Ochus, the *alter egos* of the Old Babylonian kings Hammurabi and Ammisaduqa, have identical 30-day months, in identical years of their reigns and for identical months of those years. How many more “distinctive and numerous” points of such a unique character does one require? These unique points of coincidence are spread across many years; to believe these agreements happened purely by chance is so wildly improbable as to be miraculous.

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<sup>268</sup> Trevor Palmer, “In Search of Alter Egos,” *Chronology and Catastrophism Review* (2004:3+4), p. 12

The technological and geological evidence to allow the Old Babylonians to have, and therefore to have produced, tin bronze, iron, and glass, is also “distinctive and numerous.” All of these materials are being produced by the Old Babylonians hundreds upon hundreds of years before the established chronology says that they could actually be manufactured. Pyrotechnological furnaces of the type required to produce iron and glass did not exist in Old Babylonian times, nor were deposits of tin ore available in Mesopotamia or elsewhere that were worked in the early second millennium B.C. These, too, are “distinctive and numerous” enough points of evidence to justify the Persian/Old Babylonian equation.

The indisputable scientific, geological, and archaeological fact is that the stratigraphic evidence at Tell Munbaqa proves that the period between *circa* 2300 and 1550 B.C. does not exist. But the established chronology requires that the Old Babylonians are dated to that non-existent period. Isn't that an overwhelming “distinctive” evidential point? The fact that Old Babylonian strata at Mari, Der Mushkan-Shapur and Ashur (Ishtar's Temple) lie immediately below Greek or Parthian strata dated to the latter part of the first millennium B.C. requires—for the established chronology to hold—that processes had to remove either some or all of the Kassite, Mitanni, Middle-Assyrian, Neo-Assyrian, Neo-Babylonian, Medish, and Persian strata sandwiched between the Greeks and Old Babylonians and then miraculously stop to uphold that chronology. The fact is that there is not one solitary excavation carried out by a geologist to uphold that miraculous chronology that has ever been presented to prove that these imaginary settlement gaps exist and thus discredit the short chronology.

The linguistic evidence that flows from the science and technology indicates that several forms and/or dialects of the various languages spoken and written in the ancient world were used at the same time. The linguistic chronological tail cannot be allowed to wag the forensic historical dog. Babylonian inflation is not caused by taxation in Persian times. Lack of cash in an entire society never leads to inflation. The Old Babylonians/Persians living 300 to 400 years after the true onset of irrigation farming in Mesopotamia inevitably leads to salt poisoning of the land. This inevitably leads to fewer and fewer crops to sell to the rest of the Middle East and this inevitably leads to economic inflation. While the historians to some extent accept the fact that the Old Babylonian empire collapsed because of salinization of the land, they wave their hands and magically the Kassites fix the salinization problem. And all this is presented by historians without a stitch of evidence to prove one scintilla of it.

Historians admit that they don't know who the Old Babylonians are, or where they really came from. They have no real evidence as to their original language, what kinds of pottery, buildings, or art works they originally used in their homeland. There is not a trace of them in Syria or Arabia, their assumed homeland, that historians or archaeologists can point to as their place of origin. Attestations about them come mainly from unprovenanced sources outside Babylonia. We only know of them in Babylonia by their names and these names were also applied to non-Babylonians. Their original religion is a total blank. They come out of nowhere into history during a hundred year Dark Age and disappear without explanation from history, leaving a 400- or so year Dark Age. Yet their magnificent mathematical and astronomical accomplishments are then miraculously transmitted across this Dark Age where little to nothing is written, and through other Dark Ages in Mesopotamia that follow this one. In a desert without any water great flowering plants do not grow, nor does a desert of illiteracy transmit learning. All these massive contradictions and problems leave little doubt for those to whom forensic historical evidence matters, that the Persian/Old Babylonian identification is not only sensible but is demanded by the evidence.

First things first. Before raising cut-and-paste historical evidence as refutation, the science and the technology must be answered in full. However, when we take an overview of the Persian/Old Babylonian identification in terms of forensic historical evidence, the many facets of history are explained. They corroborate, and correlate with, each other, converging in time and place.

Instead of not knowing who the Old Babylonian rulers of Babylonia were, we know they were the Persians; their unknown language is Persian, their unknown homeland with its "snow-capped mountain peaks" and "tropical basins" is Persian Iran. Their missing archaeological materials are known as well. Their unknown religion is known and the name of their god Amurru Martu may be Ahura Mazda. The hundred year Dark Age that precedes their bursting forth into history is no longer dark and in fact does not exist, as it should not. Their Dark Age disappearance from history, with the problem of the transmission of their mathematical and astronomical achievements across a Dark Age vacuum of perhaps a thousand years, also does not exist. These Dark Ages should not exist because the excavation carried out at Tell Munbaqa proves that the period of time in which it is assumed they lived never existed. Their technological capabilities in metallurgy and glass also fall into time and place. Their being the Persians of the first millennium B.C. correlates with their ability to obtain tin ore; their pyrotechnological knowledge

of furnaces correlates with their ability to manufacture iron and glass (or import glass). Their ability to carve the Hammurabi Code into diorite is no longer fraught with problems, but clearly falls into time and place since, as Persians, they had steel. Iron objects discussed and found at Mari or elsewhere are no longer anachronisms but are in full agreement with their Persian time and place identity. The inflation that comes toward the end of the Persian period in Babylonia is not a paradox nor does it violate the well-understood laws of economics but inevitably follows about 300 to 400 years of irrigation agriculture—as a consequence of which salt poisons the soil. We do not need the Kassites to miraculously, hand-wavingly, turn the salt-poisoned soil back to being fertile.

The Persians'/Old Babylonians' architecture, pottery, and other stylistic relics are completely explained and fall into time and place. These are the styles of the various peoples whom they ruled over. The paucity of their strata throughout Mesopotamia is no longer an enigma that has to be explained by *ad hoc* hypotheses. Because the Persians/Old Babylonians ruled rather unobtrusively, they would naturally leave little of their materials in the ground, at sites only here and there, and that is what is actually found. We don't require unscientific-unrealistic explanations that selectively erode, destroy, and remove ("denude") only the strata sandwiched between the Greeks or the Parthians and the Old Babylonians/Persians. We do not need thousand-year-long settlement gaps that have never scientifically been proven to exist.

The laws of Hammurabi are those of Darius, though we have mostly Hammurabi's rendition. The language of the two where partial copies in Persian exist, is identical because the king had them rewritten from earlier law codes, so they are one and the same. The great hardship of the people of Babylonia toward the end of the Persian/Old Babylonian period also falls into time and place because these events happened to one and the same people. The description of the Persians not burying their dead may relate to a similar practice among the Old Babylonians. The same may apply to the "perverted" sexual customs of both. The reason Hammurabi paid mercenaries at Mari with stamped pieces of silver (which is essentially what coins are), as did Darius, also follows logically. The rise of living standards in Babylonia at the onset of the Darius/Hammurabi kingship follows, as does their meticulous micromanagement of their empire, as well as the possibility for them to gather intelligence from their networks of spies.

There is no clear evidence that the Hittite raid on Babylonia caused the downfall of that empire. As we were informed above, their downfall is not, as Sidney Smith claimed, a “major mystery.” As with the Persians, their empire fell to Alexander the Great. The identical 30-day months found for Darius/Hammurabi and Artaxerxes III Ochus/Ammisaduqa is not a miraculous chance occurrence but happens because they are either the same persons or contemporaneous in each case.

Not every last iota of evidence fits Heinsohn, Rose, and Sweeney’s thesis regarding the Persian/Old Babylonian identification, but look how numerous and distinct the problems are that historians have acknowledged but have no evidence to explain away. These numerous and distinct contradictions were derived from their cut-and-paste documentary approach to history and chronology. And it must be acknowledged as well that there is no scientific way to test and falsify these documentary materials which are fraught with internal contradictions and problems of provenance. Let us recall how Cochrane argued for the validity of the Persian history from documentary sources. However, J.D. Muhly with respect to the Persians states:

“Virtually everything that could be said about the Persian Empire is to be found in the massive history by Pierre Briant, first published in French in 1996 and in an English translation in 2002. This is a monumental work in every sense of that word, but it has to be admitted that it is a history based almost entirely upon [foreign, Greek] Classical sources.”<sup>269</sup>

The Old Babylonians have always been linked directly to the 12th Egyptian Dynasty across Palestine and Crete, based on the very evidence employed by historians and archaeologists, which is now buttressed by science and technology. Are the historians and archaeologists going to argue that their very own evidence, that makes these connections, is invalid if it requires the down-dating of these civilizations to the mid- to late first millennium B.C. but then makes a valid case if it dates these same civilizations to the early second millennium? It is their evidence, based on their methodologies and analyses along with forensic historical data that now validates Heinsohn, Rose, and Sweeney’s identification of the Old Babylonians

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<sup>269</sup> J.D. Muhly, Review of Giovanni Lanfranchi, Michael Roaf, Robert Rollinger, “Continuity of Empire (?)...”, *Bryn Mawr Classical Review*, vol. II, no. 11 (2004) (Internet), p. 3

with the Persians. The entire spectrum of forensic history in tandem with archaeology and documentary history leads to this conclusion.

Wells, in his critique of Rose's astronomical calendrical work on the dating of the 12th Egyptian Dynasty, maintained that "synchronisms [of Egyptian history dovetailing] with neighboring countries cannot be ignored."<sup>270</sup> Other historians, as I recall, raised the same issue with Velikovsky's chronology. But now these foreign interconnections of Egypt with Crete, Palestine, and Old Babylonian Mesopotamia stare them in the face. It is their evidence they see, but the confrontation with these facts is perhaps too overwhelming for them.

Problem after problem, paradox after paradox, enigma after enigma, contradiction after contradiction are cleared up by Heinsohn, Rose, and Sweeney's thesis that these two civilizations are contemporary. A good theory explains more with fewer *ad hoc* hypotheses. Every *ad hoc* hypothesis invented to explain away all these interrelated, interconnected scientific and technical facts is just that, an invention. Rather than jettison or ignore the forensic historical evidence, or twist it with *ad hoc* theories to make it fit the overly lengthy chronology, historians, if they are worth their salt, should embrace the forensic history and use it to rewrite a far more accurate short chronology and the history that follows from doing so.

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<sup>270</sup> Ronald A. Wells, Review of Lynn E. Rose, *Sun, Moon, and Sothis: A Study of Calendars and Calendar Reforms in Ancient Egypt*. *Journal of Near Eastern Studies*, vol. 61, no. 4 (Oct. 2002), p. 315



## CHAPTER 2: PERSIANS, NEO-ASSYRIANS, NEO-BABYLONIANS: ASTRONOMY AND CHRONOLOGY

As with the Old Babylonians being the Persian rulers of Babylonia, Heinsohn and Sweeney hold that the same situation also existed in Assyria when it was ruled by Persia. They maintain that the Neo-Assyrians were various Persian rulers of Assyria. However, there is a difference in their placement of the Neo-Babylonians in Mesopotamian chronology. Heinsohn has the Neo-Babylonians contemporary with the Neo-Assyrians. Sweeney, on the other hand, places these civilizations in the conventional order—Neo-Babylonians follow Neo-Assyrians—but he claims the Neo-Babylonians are the last few rulers of the Persian Empire in Babylonia that ruled there for about 80 years.

These differences will be analyzed below in terms of what the astronomical evidence shows, and the astronomy will be the scientific arbiter for where these civilizations must be placed. Nevertheless, Heinsohn and Sweeney also disagree about which kings of the Persians are alter-egos of the Neo-Assyrians. This historical question will also be touched upon in terms of how the astronomical/forensic historical evidence relates to these disagreements. But the astronomical evidence will, of course, not be supportive of either Sweeney's or Heinsohn's chronologies where it contradicts them! We will go only where the science leads.

In this respect there appears to be a major scientific obstacle facing these short-revisionists' theories. That evidence is astronomy and it seems to stand solidly and irrefutably against their chronologies. This evidence was briefly reviewed by Peter James *et al.*, wherein they purport to show how solidly the chronology of the first millennium B.C. Neo-Assyrians and Neo-Babylonians has become astronomically anchored:

“Indeed, a Greek astronomical treatise from as late as the 2nd century AD contains one of the most valuable chronological sources ever discovered, known as the ‘Canon of Ptolemy’. Claudius Ptolemy, the famous Greek mathematician and geographer, recorded for

posterity the names and reign-lengths of the kings of Babylon from Alexander the Great ... back to Nabonassar, who ascended the throne in 747 BC. How Ptolemy came across documents containing such information is uncertain, but his interest in them lay mainly in their astronomical content. The source available to him, now lost, provided detailed records of lunar eclipses observed by the ancient Babylonians, which Ptolemy dated according to an era beginning with the accession of King Nabonassar.

“Ptolemy’s King List enabled 19th-century scholars to take their first confident step back into the past of Mesopotamia. Following the decipherment of cuneiform the skeleton of history after 747 BC could be fleshed out from the records of the Babylonians themselves. Most important, the next stage of reconstruction could also be achieved—to give precise dates to [Neo-]Assyrian history by linking it with that of [Neo-]Babylonia. ...

“To go back beyond this point [i.e. the year 747 B.C.), Assyrian texts had to be used. As a means of reckoning, the Assyrians delegated different officers of state, [usually, but not necessarily] beginning with the king himself, to be the ‘eponym’ (name giver) for each year. [The officer in question was called the *limmu*.] Thus, if an event was dated to the year of governor X of city Y, a scribe could determine when it happened by referring to a list of such ‘eponyms’ [or *limmus*]. In 1862 Sir Henry Rawlinson was fortunate enough to discover a major Eponym List giving every incumbent [eponym/*limmu*] from 911 BC down to 660 BC. The information from the ‘two Canons’ (those of Ptolemy and the eponyms) could then be combined, with a result proudly described by Sir Henry’s nephew, George Rawlinson:

“These two documents, which harmonise admirably, carry up an *exact* Assyrian chronology almost from the close of the Empire to the tenth century before our era.’

“With only minor adjustments, the system devised by the Rawlinsons is still accepted today ...”<sup>1</sup>

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<sup>1</sup> James *et al.*, *op.cit.*, pp. 265-266

Although Biblical scholars found this chronology in conflict with that of the Bible, James *et al.* explain that “it was Hebrew chronology, not Assyrian, which needed adjustment ...”<sup>2</sup> Nevertheless, according to Robert Newton, there are many serious problems with Ptolemy’s work.

“In 1978 a study by the American astronomer Robert Newton, entitled *The Crime of Claudius Ptolemy*, accused him of having perpetrated a massive scientific fraud, ‘a betrayal of the ethics and integrity of his profession that has forever deprived mankind of fundamental information about an important area of astronomy and history’. According to Newton, all Ptolemy’s claimed astronomical observations were fabricated; even worse, he had fudged, or even faked, Babylonian lunar eclipse data in order to match his own shaky calculations. One reviewer of Newton’s work even suggested that ‘Ptolemy’s forgery may have extended to inventing the lengths of reigns of Babylonian kings’.

“Even so, Newton’s strongly worded attack has done little to tarnish Ptolemy’s reputation ... As more reasonable historians of science have shown, the standards Newton applied to Ptolemy’s work were far too exacting. Ptolemy may have selected from, thereby ‘smoothing out’, the observational data available to him, but this is a very different matter from Newton’s charge of outright forgery.”<sup>3</sup>

Ptolemy was ultimately vindicated, according to James *et al.*, as well as according to most historians, archaeologists, and archaeoastronomers of our time.

“Was Newton right to claim that ‘studies of Babylonian chronology need to be reviewed in order to remove any dependence upon Ptolemy’s king-list’? On this question Mesopotamian archaeology can now fully repay its debt to the old astronomer. Though Newton glossed over the fact, Ptolemy’s figures for the regnal years of Babylonian kings have been completely vindicated by a wealth of cuneiform texts, including chronicles, short king lists and dated business documents [with a limmu/eponym’s or kings’ name]. The dates derived from Ptolemy’s Canon can also be checked using the

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<sup>2</sup> *ibid.*, p. 267

<sup>3</sup> *ibid.*

information from cuneiform ‘astronomical diaries’. The Babylonians, originators of western astrology, were, as Ptolemy knew, meticulous observers of the night sky. Their records of the positions of planets and stars, committed to clay by people who believed that these had a vital influence on earthly matters, are extremely detailed—so much so that the observations given in various tablets from the 7th to 5th centuries BC can be precisely dated to a year, month and even day by modern astronomers.

“Final confirmation of the whole canonical system comes from another astronomically fixed point ... provided by [Neo-]Assyrian records. A solar eclipse is clearly referred to in the Eponym List for the month of Simanu in a year which must, according to the links with Ptolemy’s Canon, have been 763 BC. The fundamental importance of this observation for ancient chronology was stressed by [P.] van der Meer:

““This eclipse of the sun has been astronomically fixed, on grounds that have never been questioned, as having taken place on June 15th 763 B.C., according to modern reckoning. This year is therefore used as a base reckoning for the [Neo-]Assyrian calendar. It is the [absolute] sheet-anchor upon which depends not only the [Neo-]Assyrian chronology but also that of the whole of Western Asia.”<sup>4</sup>

Carl Olof Jonsson, in the “Conclusion” of one of his articles, “The Foundations of Assyro-Babylonian Chronology,” presents this affirmative statement of the evidence that astronomical and other evidence anchors the chronology of the Neo-Assyrians and Neo-Babylonians:

“It has been demonstrated above that the [Neo-]Assyrian chronology as well as the [Neo-]Babylonian chronology from the eighth to the sixth centuries [B.C.] are both firmly established, but independently of each other, by a number of ancient cuneiform documents. Further, both chronologies are—still independently of each other—fixed to the absolute chronology by a large number of

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<sup>4</sup> *ibid.*, pp. 267-268

astronomical observations recorded from these ancient times. But this is not all. Due to the numerous synchronisms between the two countries during this period the two independently established chronologies grip hold of each other like the teeth of two intermeshing cog-wheels. It would be impossible to change the chronology of one country by even one year from Nabonassar [who reigned 747-733 B.C.] and on without changing the chronology of the other country by the same extent. This is the challenge that confronts anyone who would like to revise the chronology of either of these countries in this period.”<sup>5</sup>

In total, the Canon of Ptolemy correlates with and is corroborated by the Neo-Assyrian and Neo-Babylonian king lists. These further correlate with and are corroborated by the limmu/eponym lists and also by a great number of business tablets which are dated by either a king’s or limmu/eponym’s name on them. But, above all, these, in turn, correlate with and are corroborated by precise astronomical data found in diaries and chronicles. Altogether this represents a massive compilation of data that meshes like cogwheel teeth to anchor the established chronology of the Neo-Assyrians and Neo-Babylonians in the first millennium B.C. A. Aaboe states:

“The Diaries occupy a unique position among documents of relevance to the study of ancient history. The ever-presence of the texts, if we can date them at all, to the very day, and in the sheer bulk, continuity and detail and kind of information [they give] they are unmatched.”<sup>6</sup>

This being the case, it seems indisputable that the theories of Heinsohn, Sweeney, Velikovsky, and even Peter James and all the other historical revisionists that clash with this overwhelming evidence for the chronology of the first millennium B.C., must be dismissed as utterly false. This is, in fact, the very same kind of evidence that Lynn Rose challenged in revising the chronology of the 12th Egyptian Dynasty and, as we have seen above, that of the Old Babylonian empire in Mesopotamia, placing it in the first millennium B.C. How then can one argue against this very same kind of evidence when it so clearly supports the established

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<sup>5</sup> Carl Olof Jonsson, “The Foundations of the Assyro-Babylonian Chronology”, “Conclusion,” *Chronology & Catastrophism Review*, vol. IX (1987) p. 22

<sup>6</sup> A. Aaboe, “Observation and Theory in Babylonian Astronomy”, *Centaurus* vol. 24 (1980) p. 24

chronology in Mesopotamia for that same millennium? That would surely be having a double standard of inference, accepting one for Egypt that upholds Heinsohn's, Rose's, and Sweeney's chronologies and then rejecting the same kind of evidence when it contradicts these revisionists' chronologies for the first millennium B.C. That will not be done since it is not only unscholarly but unethical to do so.

It should be noted, however, that this is not the first time historians, archeologists, and archaeoastronomers proved the established chronology to be correct from astronomy, dated commercial documents, king lists, and archeology, only to discover that their chronological edifice was built on sand. Thorkild Jacobsen in 1937 came to the very same conclusion from these same intermeshing forms of evidence for the precise placement of Hammurabi's Old Babylonian Dynasty which he and the other scholars placed in the third to second millennium B.C. They, too, assumed their chronological edifice to be immovable and therefore demanded an irresistible force to topple it. But as we have shown, the astronomical and other evidence outlined in Chapter 1, does require the Old Babylonians to be placed in Persian times. Jacobsen states:

“In choosing [among the dates for Hammurabi's reign] the best criterion is dated [Old-]Babylonian contracts dealing with delivery of dates by date-growers. The days of delivery stipulated in the contracts will fall differently according to which [of] the [various high, middle, or low periods offered by historians as] possibilities [for a reign] is chosen; and since we must assume that the deliveries would follow soon after the harvest, that possibility which will make them fall most nearly in the time after date harvest, ending in the middle of October, has obviously the best chance of being correct. As shown by Fotheringham[’s astronomical evidence] the possibility which gives the best results is the one that places Hammurabi[’s reign] from 2067-2025 [B.C.]”<sup>7</sup>

In addition to this harvest evidence, Jacobsen also turns to a well-documented lunar eclipse and evidence from the Venus Tablets to correlate with and corroborate and converge as proof of the correctness of this chronological structure:

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<sup>7</sup> Thorkild Jacobsen, *The Sumerian King List* (Chicago 1937), p. 196

“The Venus tablets ... afford a possibility of establishing an absolute date in older Mesopotamian history by means of astronomical reckoning. On the basis of observations of a lunar eclipse foreboding the fall of I(b)bî-Sin and the end of the 3rd dynasty of Ur, Schoch has calculated that this omen occurred on the night of February 17/18 (Gregorian) 2283 B.C. Now this is a highly important fact indeed that the two mutually independent astronomical dates, Fotheringham’s [Venus tablet date] for the 8th year of Ammi-Saduga [Ammisaduqa] which places Hammurabi at 2067-2025 B.C., and Schoch’s for the [lunar eclipse] omen foreboding the fall of Ur III, 2281 B.C., just give the [correct] span in time between these two events which from our chronological [king] lists can be seen to separate them.”<sup>8</sup>

Jacobsen goes on to present additional data that came prior to Hammurabi’s Old Babylonian Dynasty which further links these to the 2067-2025 B.C. dates for Hammurabi’s reign with all the foregoing astronomical, lunar eclipse, Venus Tablets, king lists, and date harvest contract evidence to prove how absolutely solid that dating is for the Old Babylonian law giver, and he concludes:

“There is full agreement between (1) the absolute date for the 1st dynasty of Babylon as derived from the best ... chronological [king list] text, (2) Fotheringham’s astronomical dates [based on the Venus Tablets] for the same dynasty, (3) Schoch’s astronomical [lunar eclipse] date for the fall of the 3rd dynasty of Ur, (4) the delivery or harvest contracts for dates, (5) the documentary evidence prior to the Old Babylonian dynasty and (6) the documentary evidence after the fall of the Old Babylonian dynasty which all correlates and corroborates each other for the dating of Hammurabi to 2067-2025 B.C. These [facts] speak strongly for their correctness, and we have accepted them without hesitation.”<sup>9</sup>

Nevertheless, there is great disagreement about these dates and Hammurabi is placed in the history hundreds of years closer to the present by most historians. What, then, was wrong with the evidence Jacobsen presented? It was the astronomical evidence, especially the historians’ acceptance without question of the Venus Tablets of Ammisaduqa. Regarding this data set Otto Neugebauer

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<sup>8</sup> *ibid.*, pp. 196-197

<sup>9</sup> *ibid.*, p. 201

“demonstrated the impossibility of using [the] Venus tablets to date the First [Old] Dynasty of Babylonia.” “One reason that this is impossible is that the extant copies bristle with copying errors.”<sup>10</sup> R.R. Newton makes it clear, “I do not believe that we can draw any valid chronological conclusions from the Venus observations made under Ammizaduga.”<sup>11</sup> Newton adds:

“I am not alone in doubting the conclusions of Langdon and Fotheringham [on the validity of the Venus Tablets as related to chronology]. It is clear that [Sidney] Smith, “Chronology: Babylonia and Assyria,” *Encyclopædia Britannica*, vol. 5 (Chicago 1958) does not accept them. He says that the question is still open, but that to accept their conclusions would be to force the ‘abandonment of a great deal of Babylonian historical writing as worthless.’<sup>12</sup>

“[And] I do not believe that the reign of Ammizaduga can be dated unless it can be done by evidence exterior to the Venus tablets.”<sup>13</sup>

Otto Neugebauer argues that the Venus evidence of Langdon and Fotheringham has “been disproven by subsequent archeological evidence.”<sup>14</sup>

It is the great number of unusual movements described for Venus in these tablets, as discussed in the work of Lynn E. Rose and Raymond C. Vaughan, that makes this astronomical evidence questionable as the conventional sort of chronological material.

What then of the single lunar eclipse foreboding the fall of the Ur III Dynasty offered by Jacobsen? The problem with this eclipse and others like it, as Velikovsky pointed out long ago, is that it cannot be accepted since it lacks the specificity of precise time and place to date it. As Newton properly argues; “We can find a possible eclipse date at average intervals of around 40 years. Thus when the

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<sup>10</sup> H. Hunger, D. Pingree, *Astral Science in Mesopotamia* (Leiden, the Netherlands 1999), p. 37

<sup>11</sup> R.R. Newton, *Ancient Planetary Observations and the Validity of Ephemeris Time* (Baltimore MD/London 1976), p. 95

<sup>12</sup> *ibid.*

<sup>13</sup> *ibid.*, p. 96

<sup>14</sup> Otto Neugebauer, *The Exact Sciences in Antiquity* (NY 1969), p. 139



historical period itself is uncertain we can find an eclipse to fit any preconception about the period involved.”<sup>15</sup>

That is the essential, crucial point. All the checks and cross-checks of business contracts, king lists, etc., depend on the astronomy. If astronomy is demonstrated to be erroneous, the entire chronological edifice collapses. This is just what Lynn E. Rose did when he reexamined the work of major figures in the field of archaeoastronomy, especially the lunar dates as they were expected to mesh with the heliacal rising of the star Sirius for Egyptian chronology. While each of the major players as well as several others claimed that they had made these lunar dates correlate with the placement of the 12th Egyptian Dynasty in the early second millennium B.C., they obtained scores of around 50 percent, which clearly indicated the lunar material did not mesh with the heliacal rising of Sirius. As we well know, Rose’s placement of the 12th Dynasty is such that the lunar data *do* intermesh. Of the 24 New Crescent dates, 23 hit to the day.

Of course, it will be argued, none of the astronomical evidence related to the placement of the Neo-Assyrians and Neo-Babylonians suffers from these lapses of accuracy. But that is what will be refuted below.

Let us further remember that the chronology of Egypt rested on the astronomical anchor for the 12th Dynasty, as outlined in volume I of this series. In spite of generations of scholars building a chronological edifice on that date, Lynn Rose incontrovertibly proved that the 12th Dynasty had to be shifted closer to the present by 1477 years. Therefore, in order to support the short chronology, the astronomical evidence that has been invoked as solid support for the first millennium empires of the Neo-Assyrians and Neo-Babylonians must also give at least acceptably solid support for moving these civilizations closer to the present by about 300 or more years.

Not only must the astronomical data correlate with, and corroborate, this shift, it should in case after case be a match for that of the established chronology but in certain instances it should be dead on. As we proceed, we will discover that the intermeshing gear teeth of the conventional chronology in certain instances not only do not fit into one another properly but make the gears clash so that they grind and break.

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<sup>15</sup> R.R. Newton, *loc. cit.*

This work could not have been accomplished without the generous and excellent work of Lynn Rose, who looked at the astronomical evidence and showed that the short chronology rested on solid empirical grounds. His work will appear in the Appendix of this book.

### THE SOLAR ECLIPSE OF JUNE 15, 763 B.C.

James *et al.* make it clear that this eclipse holds the entire Neo-Assyrian and Neo-Babylonian chronology together:

“The solar eclipse of 763 BC brings us full circle, through [Neo-]Assyrian chronology and its links with Babylonia, to the lunar eclipse records of Ptolemy. The resulting picture, with checks and cross-checks provided by every source from modern astronomy to the Old Testament, is as watertight as one could ask for any period of ancient history. Assyrian history is firmly datable, with a margin of error no greater than a year, as far back as 911 BC when the continuous Eponym [limmu] List began; Babylonian history is equally certain at least as far back as 747 BC ...”<sup>16</sup>

This affirmation echoes throughout the literature. R.R. Newton quotes from the tablets for the solar eclipse:

“-762 [763 B.C.] June 15 ... gives [this statement listing the eclipse] *‘Insurrection in the city of Assur. In the month Sivan the Sun was eclipsed.’* Independent evidence, due in part to the lists of kings and their reigns, prepared by Ptolemy ... allows a close dating of this [eclipse] record, close enough to make the identification virtually certain.”<sup>17</sup>

E.J. Bickerman states: “The whole series of the eponyms [limmus] of the city of Ashur ... is dated, thanks to the mention of the solar eclipse of 15 June

<sup>16</sup> James *et al.*, *op.cit.*, p. 268

<sup>17</sup> R.R. Newton, *Ancient Astronomical Observations and Accelerations of the Earth and Moon* (Baltimore/London 1976), p. 60

763” [and] “Assyrian chronology is pinned down by the mention of the solar eclipse which occurred on 15 June 763 BC ...”<sup>18</sup> Edwin R. Thiele claims:

“Astronomical computation has fixed this [solar eclipse] date as June 15, 763 B.C. This notation is of immeasurable value for [Neo-] Assyrian chronology ... It is thus that we have absolutely reliable dates for each year of [Neo-]Assyrian history ...”<sup>19</sup>

A.T. Olmstead tells us there was a “total eclipse of the sun. To us its occurrence in 763 [B.C.] fixes by exact astronomy the [Neo-]Assyrian chronology.”<sup>20</sup>

With so many highly respected historians saying this total solar eclipse at Assur and Nineveh fixes with absolute certainty, and is the keystone date of, Neo-Assyrian chronology, how can anyone argue? First of all, there was no *total* solar eclipse reported at Assur or Nineveh in 763 B.C. Let us recall that in Bur-Sagale’s year of appointment as the limmu/eponym, the tablet specifically stated “In the month Sivan the sun was eclipsed.”

That solar eclipse was not only dated astronomically by modern retrocalculations, but largely as part of the integrated list of limmus or eponyms. Bur-Sagale was placed at that time with the king from the king list, Assurdan III. It was the eclipse that fixed that date. Jonsson discusses this:

“Prof. Schrader then presents the calculations of Mr. Lehmann, who concluded that the June 15, 763 BC eclipse had a magnitude of about 11.0–11.4 inches (= 91.7–95%) which came very close to Prof. R.R. Newton’s modern calculation of 95.0 %.”<sup>21</sup>

The reader is requested to take note of the fact that this solar eclipse was not total at Nineveh nor at Assur but only about 95 percent of the Sun was covered by the Moon, according to Newton; there could also be as little as 91.7 percent hidden according to Lehmann. (Nonetheless, it must be noted that version 3.1 of the Lange and Swerdlow program does make 763 B.C. total; it also makes

<sup>18</sup> E.J. Bickerman, *Chronology of the Ancient World*, rev. ed. (London 1980), p. 67 and p. 87

<sup>19</sup> Edwin R. Thiele, *A Chronology of the Hebrew Kings* (Grand Rapids MI 1977) p. 29

<sup>20</sup> A.T. Olmstead, *History of Assyria* (London 1923), p. 172

<sup>21</sup> Jonsson, “Foundations ...”, *op.cit.*, pp. 14-15

436 B.C. partial.) The question that must first be asked is: Does a partial solar eclipse of around 95 or 91.7 percent provide sufficient darkness for anyone to notice that a solar eclipse has taken place? The answer to that question is that if 5 to 8 percent of the Sun's disk is visible, the change in the amount of light experienced by an observer on the Earth is hardly even noticeable. R.R. Newton states: "It is unlikely an observer who was not expecting an eclipse would notice one unless the magnitude reaches at least 0.99'."<sup>22</sup> That is, about 99 percent of the Sun must be hidden before a person not expecting or anticipating an eclipse will notice it. This is confirmed by D. Justin Schove: "The average person notices a thin solar crescent of a solar eclipse only when the magnitude reaches 0.99' [or 99 percent of the Sun is covered]."<sup>23</sup> The change from daylight to totality is not a gradual transition, but comes only seconds before the Sun is totally hidden. This is confirmed by F. Richard Stephenson who unambiguously tells us: "MOST OF THE DIMINUTION IN LIGHT LEVEL OCCURS IN THE LAST FEW SECONDS BEFORE THE SUN IS COMPLETELY OBSCURED."<sup>24</sup> [capitalization added] Mark Littmann *et al.* describe via an analogy the difference between a partial and a total solar eclipse:

"Some people see a [predicted] partial [solar] eclipse and wonder why others talk about a total eclipse. Seeing a partial eclipse and saying that you have seen an eclipse is like standing outside an opera house and saying you have [heard or seen] the opera, ..."<sup>25</sup>

Michael Maunder and Patrick Moore report:

"As totality approaches the Moon's shadow can be seen sweeping across the landscape—or even better the seascape. It travels over 1000 mph [1620 km/h] and gives the impression of a vast dark cloud rushing toward you and then enveloping you ... and almost before you have time to appreciate it you find that totality has begun."<sup>26</sup>

Here is a vivid description of people in the last few seconds before totality of the solar eclipse of November 3, 1994, on the Altiplano of Bolivia:

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<sup>22</sup> R.R. Newton, *Medieval Chronicles and the Rotation of the Earth* (Baltimore 1970), p. 70

<sup>23</sup> D. Justin Schove, *Chronology of Eclipses and Comets* (NY 1984), p. XV

<sup>24</sup> F. Richard Stephenson, *Historical Eclipses and Earth's Rotation* (Cambridge UK 1977), p. 50

<sup>25</sup> Jay M. Pasachoff, in Mark Littmann *et al.*, *Totality* (NY and Oxford 1999), p. 1

<sup>26</sup> Michael Maunder and Patrick Moore, *The Sun in Eclipse* (Berlin and NY 1998), p. 53

“‘Here it comes!’ someone shouted. ‘Where?’ ‘Over there,’ pointing to the northwest. ‘Oh yes! I see!’ ‘It’s getting dark now ... it’s getting real dark now ... it’s really getting ... Oh my God!’ Cheers and gasps accompanying the beginning of totality.”<sup>27</sup>

We have gone into this at some length to make it clear that unless a person has an excellent knowledge of solar eclipses, he does not know when to look for it and if it is a partial solar eclipse as the one dated June 15, 763 B.C. at Nineveh and Assur (at least according to most calculations, but not according to version 3.1 of Lange and Swerdlow), it will probably not be observed or even taken note of. Nevertheless, the critics of this explanation will claim that the Neo-Assyrians back in 763 B.C. had astronomers/astrologers that were always on the lookout for this kind of partial solar eclipse and thus surely had to notice it. Since these ancients understood the nature of lunar eclipses and could predict these, they could do the very same with solar eclipses. This is based on a fundamental misunderstanding of the evidence. Lunar and solar eclipses, although related, are singularly different, as explained by Duncan Steel:

“Lunar eclipses are witnessed more often than solar.

“This is because the full moon may be seen from anywhere on the night side of the planet. That implies that half of humanity might see the Moon being eclipsed, but in addition such eclipses last several hours and the globe spins to allow observers elsewhere a chance to note the eclipse ...”<sup>28</sup>

On the other hand, a total solar eclipse, as is well known, is only experienced over a very narrow path on the Earth’s surface and does not last for hours but minutes. The occurrence of solar eclipses on the same place of the globe is (on average) inordinately infrequent. As Steel, citing Rebecca R. Joslin, further explains:

“A total solar eclipse occurs *somewhere* about every 18 months, but as the track of totality is usually less than a hundred miles wide, you

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<sup>27</sup> Mark Littmann *et al.*, *op.cit.*, p. 133

<sup>28</sup> Duncan Steel, *Eclipse* (Washington DC 2001), pp. 54-55

should expect to wait for several centuries in any random location for the next one.”<sup>29</sup>

According to George F. Chambers:

“Take London for instance. From the calculations of Hind, confirmed by Maguire, it [can] be considered as an established fact that there was no total [solar] eclipse visible at London between 878 and 1715 [A.D.], an interval of 837 years. The next one visible at London though uncertain is also a very long way off. There will be a total eclipse on August 11, 1999, which will come as near to London as the Isle of Wight, but Hind writing in 1871 said he doubted whether there would be any other total eclipse visible *in England* for 250 years from the present time [1900].”<sup>30</sup>

Those observers situated nearer to the equator, as compared to those located nearer the poles, are afforded a somewhat longer period of totality:

“Places at or near the Earth’s equator enjoy the best opportunity for seeing total eclipses of the Sun ... the speed imparted to an observer as a result of the Earth’s axial rotation diminishes from the equator towards the poles ... so that the nearer he is to the pole, the slower he goes and therefore the sooner will the Moon’s shadow overtake and pass him, and the less the time at his disposal for seeing the Sun hidden by the Moon ...

“It was calculated by Du Sejour that the greatest possible duration of a total phase of a solar eclipse at the equator would be 7 m[inutes] 58 s[econds] and for the latitude of Paris 6 m[inutes] 10 s[econds].”<sup>31</sup>

Therefore, because lunar eclipses can be seen from more than half the surface of the Earth and last for hours, an understanding that they repeat themselves at fairly regular intervals could be acquired in a relatively short time—a few hundred years—by the ancient Mesopotamian observers. They could make records of these

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<sup>29</sup> *ibid.*, p. 4

<sup>30</sup> George F. Chambers, *The Story of Eclipses Simply Told for General Readers* (NY 1900), p. 34

<sup>31</sup> *ibid.*, p. 33

lunar eclipses over those few hundred years and work out the trend in order to predict and thus observe and record that cycle which is now called the Saros cycle. (Actually, the Saros was a period of 3600 years but it was misinterpreted by Edmund Halley in 1691 who assumed it referred to the lunar cycle of approximately 18 years. That is, lunar eclipses tend to repeat themselves every Saros cycle of 18 years.)

However, because solar eclipses were only observed very, very rarely, in very narrow paths across the Earth, and for only a few minutes, an understanding of their cycle of repetition was not attained until quite late in history. As Steel pointedly states, in “Mesopotamia and environs, only a small fraction of all solar eclipses would be in the records making the discovery of the complex cycle ... a near impossibility.”<sup>32</sup>

Although historians and others suggest that since the Mesopotamian astronomer/astrologers understood the Saros cycle for accurately predicting lunar eclipses, they could then crudely use this data to indicate when a solar eclipse might occur and look for it, this is based on certain unproven assumptions. As Neugebauer explains,

“The myth of the Saros is often used as an ‘explanation’ of the alleged prediction ... of ... solar eclipse[s] ... There exists no cycle for solar eclipses visible at any given place, all modern cycles concern the Earth as a whole. No Babylonian theory for predicting a solar eclipse existed in 600 B.C., as one can see from the very unsatisfactory situation 400 years later, nor did the Babylonians [or Neo-Assyrians] ever develop any theory which took the influence of geographical latitude into account.”<sup>33</sup>

Solar eclipses do come in cycles but they can only be seen—even partial eclipses—at certain locations on Earth at that particular time. Various factors, including the rotation of the Earth on its axis, cause these eclipses to reoccur but not at the same points on the globe. This understanding was probably not available to these ancient peoples until some time around 300 B.C. or much later.

Anatoly Fomenko shows just how unrealistic it is to believe that the Saros cycle enabled the ancient astronomers to predict solar eclipses:

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<sup>32</sup> Steel, *op.cit.*, p. 54

<sup>33</sup> Neugebauer, *op.cit.*, p. 142

“Predicting solar eclipses is a truly formidable task due to the complexity of lunar motion that is defined by a large number of external factors. One might attempt to predict a solar eclipse by the Saros cycle that includes about 43 solar eclipses, 15 of them being partial, 14 annular, 2 belonging to the category of so-called ‘total annular,’ and 12 [being] total. However, the eclipses from the Saros can occur on different areas of the Earth, and so a prediction for a given location is true in one case out of 400 in general. That is to say, the probability of a correct prediction based on the Saros cycle equals 1/400.”<sup>34</sup>

Sten F. Odenwald explains:

“The historian Herodotus (460 B.C.) mentions that Thales was able to predict the year where a total solar eclipse could occur. Details of how this prediction was made do not survive ... Apparently the method used worked only once because what is known of Greek scientific history does not suggest that this method was ever reliably used again.”<sup>35</sup>

As far back as 1875 Henry Creswicke Rawlinson understood that it was not possible to predict solar eclipses in the 6th century B.C. As a note in his translation of Herodotus’s *Histories* explained:

“The prediction of this eclipse by Thales may be classed with the prediction of a good olive-crop or of the fall of an aerolite [meteorite]. Thales, indeed, could only have obtained the requisite knowledge for predicting eclipses from the Chaldeans, and that the science of these astronomers, although sufficient for the investigation of lunar eclipses, did not enable them to calculate solar eclipses—dependent as such a calculation is, not only on the determination of the period of recurrence, but on the true projection of the track of the sun’s shadow along a particular line over the surface of the earth—may be inferred from our find that in the astronomical canon of Ptolemy, which was compiled

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<sup>34</sup> Anatoly T. Fomenko, *History: Fiction or Science? Chronology*, 2nd, revised ed. (Paris/London/NY 2003), p. 95

<sup>35</sup> Sten F. Odenwald, *Back to the Astronomy Caf e ...* (Westview Press 2003), p. 66



from the Chaldean registers, the observations of the moon's eclipses are alone entered."<sup>36</sup>

Just as has been discussed above, there are no known lists of solar eclipses which the ancient astronomer/astrologers of Mesopotamia could turn to in order to organize and predict solar eclipses.

George Sarton, along these same lines, writes of an assumed prediction of a total solar eclipse for 585 B.C. in Anatolia during a battle by the Greek philosopher Thales:

"It is now agreed by historians of ancient astronomy that the Babylonians could not have discovered the period [the cycle for predicting and locating the sites of solar eclipses] before the fourth century [300s B.C.]. Hence, Thales could not have learned it from them ...

"We must conclude that Thales did not predict the eclipse ... because he lacked the necessary knowledge. It is foolish for us to say that he understood the phenomenon. The explanation with which we are familiar [namely the moon blocked the sun's light] would have been incomprehensible to him for he conceived the earth as a disk (not a sphere) floating in the ocean."<sup>37</sup>

Dwardu Cardona, however, tells us that the Babylonians of this same period "knew the earth was a sphere and thus counted it among the planets. This is evidenced by the words of Diodorus who wrote that the Chaldeans had it stated 'that the moon's light is reflected and her eclipses are due to the shadow of the earth'."<sup>38</sup>

Diodorus wrote in the first century B.C. and his statement is not supported by a single Babylonian document. The Babylonians simply did not have a physical model of the sky, as we presently do, from which they could determine that the Earth was blocking the Sun to bring about a lunar eclipse and that the Moon was blocking the Sun for a solar eclipse. Their knowledge was based on tables of

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<sup>36</sup> Herodotus, *Histories*, transl. George Rawlinson (London 1875), p. 199 fn

<sup>37</sup> George Sarton, *Ancient Science Through the Golden Age of Greece* (NY 1952), p. 306

<sup>38</sup> Dwardu Cardona, *God Star* (Victoria BC, Canada 2006), p. 118

earlier eclipses. It was from such tables they could predict lunar eclipses. They had, as Neugebauer pointed out, no such tables for solar eclipses, as far as we know.

Some archaeoastronomers have tried to show that it was possible to predict such occurrences based on certain assumptions, but these have failed to survive criticism. For example, D.R. Dicks' criticism of van der Waerden's support for Thales's ability to predict a solar eclipse shows:

“Van der Waerden draws conclusions that are totally at variance with the available evidence ... in his discussion of Thales' alleged prediction of a solar eclipse. In a desperate attempt to vindicate the historicity of this prediction, he spins a web of inferential reasoning based on wholly improbable suppositions, which force him to assume that the Babylonians and Thales not only knew of a 47-month eclipse cycle (for which there is not the slightest evidence), but also were aware of the moon's movement in latitude [above and below the ecliptic or sun's path through the sky] and recognized that in 47 months the moon returns 51 times to the same node again [on the ecliptic where it can block the sun and permit an eclipse to occur] ... This presupposes not only accurate observations, but also the concept of the ecliptic as a mathematical line from which the moon's apparent path deviates both north and south (the nodes being the intersection points of the two), and the assumption that such comparatively advanced astronomical knowledge was possible in the sixth century B.C. is ludicrous; ... all the indications are (on both Greek and Babylonian sides) that such a stage was not reached until at least 150 years later [350 B.C.]”<sup>39</sup>

Dicks adds in footnote 46 to this discussion that there was no

“... understanding of geographical latitude [from pole to pole] to predict the totality of the eclipse. Such an advanced level of knowledge was not reached by Babylonian astronomy of the Seleucid period (the last three centuries BC), much less that of the sixth century B.C.”<sup>40</sup>

Historian of astronomy A. Pannekoek explains that “although they [the Babylonians] could predict lunar eclipses at short intervals, [they] were not yet able

<sup>39</sup> D.R. Dicks, *Early Greek Astronomy to Aristotle* (Ithaca NY 1970), p. 174

<sup>40</sup> *ibid.*, p. 255

to solve the more difficult problem of solar eclipses.”<sup>41</sup> Alan E. Samuel suggests the Greeks did not derive their understanding of many aspects of astronomy from the Babylonians at all:

“It is certainly wrong to suppose that Greek astronomical theory of the fifth century [400s B.C.] rested on a sophisticated, elaborated and completed Babylonian base, which became known to Greeks of the period, and it is a kind of begging of the question to explain fifth century Greek astronomical and calendric discoveries by postulating prior Babylonian knowledge. Rather, we should be investigating the nature of *Greek* knowledge of the period in hopes of understanding the discoveries in terms of the Greek milieu in which they were made. ‘Babylonian influence’ may be a red herring.”<sup>42</sup>

The ancient Mesopotamians, unlike the Greeks, did not enquire into the causes of eclipses. They were not empiricists. As E.H. Hutten states:

“The philosophers of the [Greek] Ionian school combined theorizing about the universe with knowing some facts and this made their work so unique and fruitful. Eastern [Mesopotamian] sages, too, were speculating about the world, but they were guided by religious ... feelings rather than by a desire to understand eternal reality ...”<sup>43</sup>

R.J. Forbes and E.J. Dijksterhuis put it this way:

“[Mesopotamian] Science, if we can call it such, only formed part of religious and philosophical wisdom. It did not construct a world-picture of its own built solely on the observations of natural phenomena and resting on certain supposed or established laws of nature: Such a concept was totally foreign to pre-classical [Mesopotamian] civilization.”<sup>44</sup>

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<sup>41</sup> A. Pannekoek, *A History of Astronomy* (NY 1961), p. 99

<sup>42</sup> Alan E. Samuel, *Greek and Roman Chronology: Calendars and Years in Classical Antiquity* (Munich 1972), pp. 21-22

<sup>43</sup> E.H. Hutten, *The Origins of Science* (London 1962), p. 13

<sup>44</sup> R.J. Forbes and E.J. Dijksterhuis, *A History of Science and Technology* (Baltimore MD 1963), pp. 15-16

A. Crombie neatly sums up the predicament related to solar eclipse understanding and prediction in Mesopotamia:

“Impressive as are the technological achievements of ancient Babylonia, Assyria and Egypt ... as scholars have presented them to us, they lack the essential elements of science, the generalized conceptions of scientific explanation and of mathematical proof.”<sup>45</sup>

Lacking an understanding of why solar eclipses occurred, the Mesopotamian astronomer/astrologers could not determine from their apparently non-existent solar eclipse tables when one might occur either in the future or in the past. When was the ability developed to make even crude solar eclipse predictions? According to Neugebauer, “It is difficult to say when this phase developed into a systematic mathematical theory. It is my guess that this happened comparatively rapidly and not before 500 B.C.”<sup>46</sup> As late as 1997, Richard Stephenson claims this is the case:

“It appears that the first person to give the true explanation of eclipses is Anaxagoras (500-428 BC) rather than Thales ... At this early period Babylonian astronomers attained very poor success in anticipating [solar eclipses] for a given location.”<sup>47</sup>

This is explicitly explained by Littmann *et al.*:

“In his account Herodotus credits Thales with predicting ... [a solar] eclipse. If so, Thales would have been the first person *known* to have calculated a future solar eclipse ... The Chaldean (or New [Neo-] Babylonian) Empire dating about two centuries later [than this eclipse] shows recognition of an 18-year-11-day rhythm in eclipses—the Saros ... Such a rhythm predicts not just the year but month and precise day of the eclipse. Yet Herodotus seems amazed that Thales could be accurate to ‘the very year in which it took place.’ Was Herodotus so surprised that Thales could predict an eclipse to the day that he simply could not believe that degree of precision and used the more

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<sup>45</sup> A. Crombie in Francesca Rochberg, *The Heavenly Writing: Divination, Horoscopy, and Astronomy in Mesopotamian Cultures* (Cambridge UK/NY 2004), p. 16

<sup>46</sup> Neugebauer, *op.cit.*, pp. 101-102

<sup>47</sup> Stephenson, *op.cit.*, p. 343

conservative ‘year’ instead? That would be out of character for the flamboyant Herodotus ... The problem is to predict a total [solar] eclipse for a particular location on Earth. Could Thales [in the 6th century B.C.] have accomplished this? It is doubtful.”<sup>48</sup>

People living in the Near East and Greece even in the 500s B.C. were seemingly amazed by the idea that any one could determine when and where a solar eclipse would occur because it seems no one at that time had even the slightest understanding of the nature of solar eclipses. Yet we are asked to accept that the people living in the 8th century [700s] B.C. were to convert their knowledge of lunar eclipses into an understanding of solar ones. Because the 763 B.C. solar eclipse is needed for support of the Neo-Assyrian chronology, historians assume just that. If by 763 B.C. they actually had an inkling regarding solar eclipses, why did it take some 500 more years for this inkling to grow to fruition?

Jonsson, like many others, based on consensus and not proof, suggests that the Saros “cycle was used by Babylonian astronomers ‘to predict the dates of possible eclipses by at least the middle of the 6th century [550 B.C.] and most probably long before that.”<sup>49</sup> As for the qualifying statement “and most probably long before that [550 B.C. date]”, not a shred of evidence is produced or exists. It is all assumption and conjecture and not proof. The 585 B.C. eclipse prediction of Thales has no basis in empirical fact nor does one know if the 763 B.C. eclipse of Bur-Sagale and King Assurdan III had been predicted and looked for. In fact there are no other documents for this king, none! According to George Smith:

“The total absence of contemporary dated documents during the reign of Assur-daan III is remarkable. The Assyrian canon is the only proof of the reign of these [Neo-Assyrian] kings.”<sup>50</sup>

Smith further tells us: “Above the date 763 [B.C.] there is no positive proof of any Assyrian canon date.”<sup>51</sup> Thus all we have for the period of Assurdan III is a chasm—a Dark Age—in which he has been ensconced without solid proof other than that based on this solar eclipse.

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<sup>48</sup> Littmann *et al.*, *op.cit.*, pp. 48-49

<sup>49</sup> Jonsson, *The Gentile Times Reconsidered*, *op.cit.*, p. 171

<sup>50</sup> George Smith, *The Assyrian Eponym Canon* (London, undated), p. 83

<sup>51</sup> *ibid.*, p. 152

John M. Steele shows that systematic observations of the sky only go back to some time around the middle of the eighth B.C.:

“Not only were celestial events used for divination in Mesopotamia, they were also systematically observed and recorded in Babylon from about the middle of the eighth century BC, and [only] by the fourth century BC mathematical schemes had been developed that allowed [the accurate prediction of] various astronomical phenomena ...”<sup>52</sup>

In his footnote Steele shows: “There is no firm evidence for similar long-term systematic observational programmes in other Mesopotamian cities.”<sup>53</sup> In terms of the 763 B.C. eclipse there is no evidence for systematic study and recording of the sky at that time or before it. Therefore, there is no real evidence that the ancients could have even understood the Saros for forecasting lunar, let alone solar eclipses. Steele does suggest, however, that the [Neo-]Assyrian astronomers may have recognized that there was a 6-month–5-month solar eclipse cycle by which they may have considered that an eclipse could possibly occur; but he does not think that this assumed possibility could have been known before 650 B.C.<sup>54</sup>

The evidence clearly suggests that the discovery of the Saros for determining solar and lunar eclipses came much later than believed. This, too, would make perfect sense in terms of the short chronology. Since the history/chronology of Mesopotamia is much shorter there is less time available for discovery and thus comprehension of the Saros. In fact, Duncan Steel explains:

“How far back do the eclipse records of Babylon go? Solar eclipse notations that may be unambiguously interpreted and dated [based on the established chronology] start from 700 B.C., but most postdate 350 B.C. On that basis, assuming that at least a century of records would be needed to decipher the Saros, it would seem unlikely

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<sup>52</sup> John M. Steele, *Observation and Prediction of Eclipse Times by Early Astronomers (Archimedes)* vol. 4 (Boston/London 2000), p. 21

<sup>53</sup> *ibid.*

<sup>54</sup> John M. Steele, “Eclipse Prediction in Mesopotamia,” *Archive for History of Exact Sciences*, vol. 54, no. 2 (2000), p. 429

that eclipse prediction based on these records would have been possible much before 250 B.C.”<sup>55</sup>

The records themselves are so sparse prior to 350 B.C. that Duncan Steel, who believes it was possible to figure out the Saros earlier, was forced to admit that the evidence for predicting and then looking for solar eclipses starts some time around 250 B.C. Since it has long been thought that there was no total solar eclipse observable at either Assur or Nineveh in 763 B.C., some sort of suggestion had to be put forth to explain why it was observed. Stephenson offers a tentative solution:

“Fotheringham supposed that it [the 763 B.C. eclipse] must have been total somewhere in Assyria. However, this suggestion is unfounded; the record gives no information regarding magnitude, although the eclipse was presumably very striking. It may have been seen at the Assyrian capital of Assur ... but the report could have come from some provincial location instead.”<sup>56</sup>

The 763 B.C. total eclipse actually took place some distance from Assur and Nineveh. According to Lynn Rose, it occurred in north central Turkey (Anatolia) in the vicinity of Lake Van.<sup>57</sup> The document for Bur-Sagale says the event was “an eclipse” which may mean that it was not partial but total, presumably with day turned to night. This is the only eclipse reported for a very long period; some have presumed that it was seen as total. Why didn’t the document say the Sun was partly covered or obscured or some other words that convey what supposedly happened? As was pointed out above, an eclipse magnitude of 92 to 95 percent does not turn day into night. A. Mosshammer puts the case for why Stephenson’s suggestion lacks credibility. In discussing the Thales eclipse, he states:

“A Babylonian record of a [total] solar eclipse not visible in Babylonia is equally impossible as Neugebauer’s work has shown.

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<sup>55</sup> Duncan Steel, *op.cit.*, pp. 83-84

<sup>56</sup> Stephenson, *op.cit.*, p. 127

<sup>57</sup> Lynn E. Rose, using a computer program, reported to the author in a personal communication. The program that Rose was using was Lange and Swerdlow’s *Planet’s Visibility 2.0*. It should be noted that their later version, *Planetary, Lunar and Stellar Visibility 3.1*, has the -762 eclipse *total* in Nineveh! Apparently the degree of this eclipse remains unsettled.

Lacking the means to predict an eclipse visible in Turkey, the Babylonians would not have sent observers to watch for it.”<sup>58</sup>

The same applies to the 763 B.C. eclipse; lacking the means to predict an eclipse in Turkey, the Assyrians would not have sent observers to watch for it. Stephenson’s suggestion does not exclude that a trusted Neo-Assyrian official in Turkey, having observed the eclipse, reported it by trusted agents to the capital. But this suggestion opens a can of worms, since if this explanation is taken to be valid, then any solar eclipse in any part of the various ancient empires can have the same said of it. Rather than having observed astronomical phenomena as data, we have “hearsay” and “rumor” of these events as documented proof. This allows both truthful reports and hearsay or false reports to have the same standing. This is hardly the kind of science anyone can trust enough to build a chronology with.

Furthermore, why would the professional astrologers accept a foreign report from the outer frontiers of their nation from someone, or even many, obviously untrained in the arts of astronomical observation? They were the priest observers, keepers, and interpreters of omens from the sky. Lastly, Rose conveyed that the 763 eclipse occurred in a region of Turkey that was only sporadically under Assyrian control at that time. See *Past Worlds, The Times Atlas of Archaeology* (Maplewood NJ 1988), p. 57. The editors say that:

“North of Assyria the people around Lake Van, ethnically predominantly Hurrian, coalesced into a confederation of states ... by the 8th century [the 700’s B.C., it was] a serious rival of Assyria for control of Northern Assyria. The two states clashed over control of the horse-rearing regions south of Lake Urmia [which is southeast of the Lake Van region].”<sup>59</sup>

They add that “Assyrian control there was ‘loose or transitory’.”<sup>60</sup> Hence there is no evidence that during 763 B.C. this region was even under Assyrian control. Elsewhere we learn:

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<sup>58</sup> A. Mosshammer, “Thales Eclipse”, *Transactions of the American Philological Association*, vol. III (1981), p. 150

<sup>59</sup> *Past Worlds, The Times Atlas of Archaeology* (Maplewood NJ 1988), p. 56

<sup>60</sup> *ibid.*, p. 56



“In the early 8th century waning Assyrian power allowed neighboring kingdoms to prosper. The Urartians centered in eastern Anatolia around Lake Van greatly expanded their territory, notably to the south.”<sup>61</sup>

Are we to believe an enemy of Assyria saw the eclipse and was captured, brought to Assur and reported this? Of course, I am only joking but these are the solutions we are left with when we move an apparent total eclipse seen in Assur and Nineveh to the edges of the Neo-Assyrian empire. All these explanations regarding the 763 B.C. eclipse are merely conjectures, surmises and belief. We have an absolute solar eclipse date documented by almost nothing!

However, if Heinsohn and Sweeney are correct, this total eclipse had to have occurred in Persian times. Since they equate the Neo-Assyrians with Persians, there had to be a total solar eclipse observed at either Nineveh or Assur or at both cities. And, exceedingly important, that eclipse had to have occurred in the month of Sivan. That is precisely what Rose reported: a total eclipse passed directly over both Nineveh and Assur in 436 B.C. so there cannot be the slightest possibility that no-one saw it, even if it was a cloudy day: everyone there saw that day turned into night! The event occurred in the month of Sivan, which clearly fits the Bur-Sagale document. (As noted before, however, we must reserve judgment; the 3.1 version of Lange and Swerdlow has the 436 B.C. eclipse as a large partial in Nineveh and Ashur, *not* a total.)

In terms of Velikovsky’s theory there was a pole shift in the eighth century B.C. It makes perfect sense that the fifth century or thereabouts was the period when the Moon blocked the Sun to cause solar eclipses at different times than before, and the ancient Babylonians learned much later when solar eclipses might or might not occur. After the pole shift, ancient man could have first learned by observation the regularity of the Moon’s eclipses by the Saros cycle. But it would have taken far longer to understand, predict, and know when and where to observe solar eclipses. Some partial eclipses may have been seen but we can’t prove this. These hard-won achievements grew slowly after the eighth century B.C. The heavens were now stable and orderly so that mankind made these great strides.

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<sup>61</sup> *Oxford Atlas of World History*, P. O’Brien ed. (NY 1999), p. 39

Moreover, this explains why only total solar eclipses could have been reported after the eighth century B.C. Ancient man by around 436 B.C. had at his disposal enough data to begin to unravel the nature of solar eclipse cycles which was finally achieved later, as Neugebauer, Sarton, and Stephenson show. They could plot the motions of the stars first, then the planets and the Moon, but the most difficult problem that took longest to fathom was when and where solar eclipses would take place so that they could actually record partial ones. That the Moon caused solar eclipses at the period of new Moon, on about the last, or next to last, day of the month, if it also came close to the ecliptic and could therefore block the Sun's light, came last after 500 B.C. The 436 B.C. total eclipse may have been instrumental for that understanding. Not only does this analysis support Heinsohn's and Sweeney's theses but gives further support for Velikovsky's eighth century B.C. pole shift as well.

All the scientific pieces fit the Bur-Sagale document in terms of time and place. There is no need to travel to the edge of the empire into disputed territory at just those five to six minutes in the month of Sivan to experience a total solar eclipse and report it home. Whatever population lived in and around Nineveh and Assur in 436 B.C., staying right where they were, observed the four, five, or six minutes of totality in the month of Sivan—on May 31, 436 B.C. And this occurred in Persian times. Therefore the historic date given to Bur-Sagale, the limmu/eponym, should not be 763 B.C. but 436 B.C. This requires that the 436 B.C. eclipse becomes the “sheet anchor upon which depends not only the Assyrian chronology, but also that of the whole of Western Asia,” to use van der Meer's words.

This is extremely important in terms of chronology related to the limmu/eponym list. Because the 763 B.C. eclipse must be moved to 436 B.C., not only must the limmu/eponym, Bur-Sagale, be moved some 327 years closer to the present, but if this list is as accurate as is claimed (though I don't believe this for one moment and will present astronomical and documentary evidence below to prove this), then every limmu on the list must also be moved closer to the present by 327 years. It is taken as true (though I don't believe this either) that the limmu list began about 910 or 911 B.C.; but by moving all the rest of the limmus closer to the present in order to be aligned with Bur-Sagale's eclipse date, we must also move the entire limmu list from its 911 B.C. date forward by 327 years. Therefore the limmu list would not corroborate the King List of Ptolemy or any others that support this chronology. By doing this, the limmu list would then run from 625 down to 334

B.C., and end only three years before Alexander the Great conquered Persia. In that case the limmus would be living almost entirely in Persian times.

Nevertheless, we must remember that the limmu list is also a cogwheel with gear teeth that should also mesh properly with the astronomical cogwheels and their gear teeth and with the king list cogwheel and its gear teeth in order to corroborate the established chronology. However, even without moving any of the dates of the limmu list, the kings list, and the astronomical date for the eclipse of Bur-Sagale, the limmu list cannot be made to properly mesh with these other cogwheels' gear teeth. That intermeshing does not work and for a good reason. The fact of the matter, not often mentioned in such discussions of chronology, is that the limmu list ends in the 20th year of Ashurbanipal dated to 649 B.C., while the Neo-Assyrian empire falls 37 years later in 612 B.C. This leaves a shortfall of 37 limmus, but additional attested limmus were found that overfill this 37 year gap, disrupting that mesh. The reason is that there is a fundamental contradiction regarding Ashurbanipal, and indeed all the rest of the Neo-Assyrian kings' reigns that follow him. The limmu list covers the period up to 649 B.C. or up to the 20th year of Ashurbanipal's reign. Since the Neo-Assyrian empire supposedly fell in 612 B.C. there should be no more than 37 limmus to cover this period. There were, however, subsequent finds of limmus known as post-canonical limmus that could be used to fill in this empty time. Instead of having the required 37 limmus to corroborate the established chronology, the historians have found 50! That is, there are 13 limmus too many for the established chronology. That requires that Assurbanipal, along with the rest of the Neo-Assyrian kings, has to be moved back in time by 13 years to make the limmu list correlate with and corroborate the chronology of these kings' reigns. But to do this would destroy the astronomical data employed in the first place to establish their chronologies. This contradiction is discussed by Alan Millard:

“In the long lists of eponym officials covering the entire Neo-Assyrian period ... the last eponym preserved is that for the year 649 B.C. Thus the eponyms for the years 648-612 [B.C.] when Nineveh was destroyed and the Assyrian Empire came to an end are referred to as post-canonical (p or P.C.) eponyms ... their order is mostly unknown with clues to their order being scarce. The result is a puzzle of monumental proportions ...

“However 648 to 612 [B.C.] requires 37 eponyms while the number of attested is ... [around] 50.”<sup>62</sup>

To attempt to get around this “puzzle of monumental proportions”, that is, to explain away 13 superfluous years for the Neo-Assyrian Empire, Millard suggests:

“There are a number of ... solutions that might be employed to pare down the attested P.C. eponyms to fit the 37 year span ... One is simply to throw away some of them, *assuming* them to be [a] hopelessly garbled version [of the correct list]. Another is to *assume* that there may be a few eponyms that are both post-canonical and post-empire. These solutions may eliminate a few ..., but the most likely solution ... is to *assume* there were a number of eponyms in use at different cities simultaneously. Unfortunately, ... evidence for this *assumption* is not yet available nor is there an obvious spatial distribution pattern [i.e. eponyms living in different cities] among the P.C. eponyms that bear it out.”<sup>63</sup> (emphasis added)

Every solution to this problem offered by Millard is based on *assumptions*. They are simply put forth to salvage the established chronology and for no other reason. Although Millard calls this a “puzzle of monumental proportions”, it is in reality a “contradiction of monumental proportions to the established chronology.” Jonsson claimed one cannot move any of the kings by even one year, but the 50 post-canonical limmus demand the entire chronology be shifted by 13 years. Those who have relied so strongly on the fit of the limmu lists to the Neo-Assyrian king lists and their reigns have not addressed this overwhelming contradiction. Some of course may argue this is only a small discrepancy and will be resolved at some future date. But this is assuming one knows what future events will disclose.

As we can see, this major cogwheel of the Neo-Assyrians–Neo-Babylonians does not intermesh with the established chronology and history based on it but requires revisions to make them mesh; this cannot be done by keeping the chronology as it presently stands. Cryer explains the implications of such a move:

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<sup>62</sup> Alan Millard, *The Eponyms of the Assyrian Empire 910-612* (Helsinki 1994), p. 72

<sup>63</sup> *ibid.*, p. 73

“There is, moreover, a consequence all too few chronographers are willing to acknowledge—namely that the king list [and limmu list] series of ... inscriptions, or other source on which the original chronology was based, may in fact contain faulty data and, if it contains one error, it may contain more. In actual fact, whenever we process duplicate versions of an ancient chronology, they invariably disagree with one another at numerous points. Many scholars see it as a pressing task to reconstruct from such a hypothetical ‘original’ chronology, but in fact, such work is simply textual criticism: the chronographer is rather concerned with the rather difficult problem of finding the best date—or none, if circumstances warrant it—for historical phenomena.”<sup>64</sup>

He adds this highly important comment:

“And should that ‘worst observation’ happen to be a baseline date [as is that of the 763 B.C. solar eclipse] catastrophic consequences would ensue, as this would entail that the entire chronological system would have to be relocated to another segment of the absolute time scale.”<sup>65</sup>

The fact that the baseline date for the established chronology of the first millennium B.C. in Mesopotamia contains a 327-year error suggests that the chronology contains many others. As Cryer points out, “we must beware of Gause’s caution that such a structure is no better than the worst observation contained in it.”<sup>66</sup> Nevertheless, objections will be raised and it may even be argued that this is only one “possible” error because by some highly improbable means the Neo-Assyrians knew where and when to look for the 763 B.C. solar eclipse, saw it, and noted it. It certainly is not impossible, they can say. They will argue that the astronomy must agree with the established chronology. In this way there is not the slightest chance that it would be possible to argue against it or for them to revise history.

This approach is just what will be shown as false in the next unit. If Heinsohn and Sweeney are correct, there will be other disagreements—actually

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<sup>64</sup> Cryer, *op.cit.*, p. 657

<sup>65</sup> *ibid.*

<sup>66</sup> *ibid.*

contradictions that cannot be correct. Astronomical considerations stand above all documents and archaeological finds as well as anything known from history. In this respect we will see just how many other cogwheel teeth grind against the machinery of the astronomical chronology of the ancient world and have no way of being made to fit the established chronology.

## ESARHADDON, ECLIPSES, AND CHRONOLOGY

A fundamental element of first millennium B.C. chronology that Jonsson and all historians seem to uphold is the placement of Neo-Assyrian king Esarhaddon's reign to the dates of 680 B.C. down to 669 B.C. Sidney Smith writes "680 is reckoned as the first year of Esarhaddon."<sup>67</sup> As for the last year of his reign he writes, "Esarhaddon marched toward Egypt in 669 ... before reaching the Egyptian border Esarhaddon fell sick and died in the eighth month of the year."<sup>68</sup>

Georges Roux states "he [Esarhaddon] entered Nineveh, and in March 680 B.C. 'sat down happily on the throne of his father'."<sup>69</sup> On page 303 we are told that "Esarhaddon ... on his way to Egypt ... fell sick in Harran and died (669 B.C.)."

H.W.F. Saggs writes "680 Esarhaddon's first year," and on page 108, "Esarhaddon set out in 669 on a further Egyptian campaign but died on the way."<sup>70</sup>

The astronomical data as it relates to Esarhaddon is one of the cogwheel teeth necessary to the established chronology, and thus must intermesh with the cogwheel teeth of all the Neo-Assyrians and Neo-Babylonians and fit him astronomically into the grand design of first millennium B.C. chronology. Jonsson presents a chart on page 232 which illustrates this arrangement. For many of these Neo-Assyrian and Neo-Babylonian kings Jonsson has presented and documented astronomical evidence that supports established first millennium B.C. chronology. However, for whatever reason, Jonsson did not present any of the documentary evidence as it relates to astronomical phenomena during the reign of Esarhaddon. Esarhaddon being in this sense a cogwheel, the teeth of this cogwheel should also grip hold with the kings who followed him and in intermeshing with the gear teeth of these kings uphold the established chronology. But the fact of the matter is they simply do not. And, as we will see, there is no other way to get around these basic astronomical contradictions to the established chronology than revising it. As Cryer said, if there is one error there may be others, and there are!

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<sup>67</sup> Sidney Smith, "Sennacherib and Esarhaddon," *CAH* (Cambridge UK 1954), p. 79

<sup>68</sup> *ibid.*, p. 86

<sup>69</sup> Georges Roux, *Ancient Iraq*, 2nd ed. (London 1980), p. 300

<sup>70</sup> Saggs, *The Might That Was Assyria* (*op.cit.*), p. 105

Sidney Smith's translation of the Esarhaddon chronicle in *Babylonian Historical Texts* (London 1924) page 14 reads: "In the month Teshri [Sept.-Oct.] the sun darkened [its] light." In the footnote to this citation Smith reports: "Sir Frank Dyson, the Astronomer Royal, has kindly informed me that there were three eclipses in 680 B.C. [Esarhaddon's first year] of which only the first could possibly be visible at Babylon, but as this eclipse fell on Jan. 1, 680 B.C. according to the Julian calendar, this cannot be the phenomenon referred to in the text which is dealing with [around 15] September – [15] October [or no more than about a month from these dates]. The expression therefore [does not refer to a solar eclipse but refers] to some other phenomenon."

Notice how Dyson and Smith have removed this solar eclipse from history by using a double standard of inference. If an eclipse was found that confirmed the document, then there was indeed an eclipse, but since one could not be found, this statement or expression referred to "some other phenomenon." But this form of language has been used to refer to solar eclipses. The only reason that this solar eclipse of Esarhaddon's first year was rejected is that it contradicted the established chronology. When an eclipse confirms the established chronology, it is obviously a solar eclipse; when it contradicts that chronology, then it is "some other phenomenon" being referred to.

A.K. Grayson has explained away Smith's translation thus:

"This line is not at all clear and the parallel in Chron. I [which uses the same terms] is of no help. The interpretations of Smith ... and Landberger and Bauer ... are unsatisfactory."<sup>71</sup>

Grayson's translation of the same lines is "In the month tishri the forecourt ... in the mont[h ...]"<sup>72</sup> Now the real reason that the old translation is "unsatisfactory" is that there was no solar eclipse in the first year of Esarhaddon. If, in fact, the astronomers had found that there was a solar eclipse at that time, there would never have been a question raised that the "expression refers to some other phenomenon" and is "unsatisfactory" and must therefore be retranslated.

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<sup>71</sup> A.K. Grayson, *Assyrian and Babylonian Chronicles* (Winona Lake, IN 2000), p. 125

<sup>72</sup> *ibid.*



Grayson has been shown to alter documents based on his understanding of what they should contain, not on what they actually contain. Jack Cargill pointed this out in his discussion of “The Nabonidus Chronicle and the Fall of Lydia”:

“A.K. Grayson in 1975 published a long-awaited collection of all extant Babylonian chronicles ... for the coming generation, superceding in particular the well-known text of Sidney Smith (1924). Grayson altered some of Smith’s readings [such as that for an eclipse in the first year of Esarhaddon] and interpretations within this chronicle, generally in the direction of what appears to be improvement ... It is particularly unfortunate that Grayson’s acceptance ... (a half-hearted acceptance) of an unsubstantiated interpretation came [to be submitted] ...”<sup>73</sup>

What Grayson did was interpret a partial word to mean “Lydia” when there wasn’t any clear evidence that Lydia was meant. As Cargill shows, “There has never existed any reading of ‘Ly[di]a’ in the Nabonidus Chronicle.”<sup>74</sup> Here is how Grayson explained why he interpreted the signs to read Lydia: “such a reading is suggested by historical probability rather than any clear indication from the traces.”<sup>75</sup> As Cargill explains:

“Grayson erred only in failing to follow the negative consequences of his own observations, being persuaded to restore the obliterated toponym as Lydia by baseless considerations of ‘historical probability.’ The appearance or non-appearance of the toponym ‘Lydia’ on a tablet inscribed in Babylonia is, however, not a question of probability at all, but a question of fact. Probability could only enter the argument if other evidence strongly supported one guess about the fact over the other. No such supportive evidence is known to exist [regarding ‘Lydia’] of the Nabonidus Chronicle, as dissidents have pointed out for almost a century, is not a necessary—and therefore not a proper—restoration.”<sup>76</sup>

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<sup>73</sup> Jack Cargill, “The Nabonidus Chronicle and the Fall of Lydia: Consensus with Feet of Clay,” *American Journal of Ancient history*, vol. 2, no. 2 (1977), p. 97

<sup>74</sup> *ibid.*, p. 109

<sup>75</sup> *ibid.*

<sup>76</sup> *ibid.*

Smith was no less guilty of falsely interpreting the Nabonidus Chronicle in places. The point, however, is that over time it became obvious that no such solar eclipse took place in the month of Teshri so the passage “In the month Teshri the sun darkened [its] light” was changed by Grayson “In the month Teshri the forecourt ... in the mont[h ...].” It is all “unsatisfactory” because it doesn’t fit the established chronology of Esarhaddon. Q.E.D.

Unfortunately for established chronology there was also another attested solar or lunar eclipse that occurred in Esarhaddon’s reign which cannot be construed as anything but an eclipse because it is called an “eclipse.” According to Leroy Waterman<sup>77</sup>, a letter from “Kudurru To King Esarhaddon” said “After the king my lord went to the land of Egypt in the month of Tammuz an eclipse took place.”

According to Roux, Esarhaddon “in 679 B.C. had captured the city of Arzani ‘on the border ... of Egypt’.”<sup>78</sup> But for this year according to van den Bergh no lunar eclipse occurred in the month of Tammuz.<sup>79</sup> Rose, using the computer program *Planet’s Visibility* of Lange and Swerdlow, showed that there was also no solar eclipse in the month of Tammuz for Esarhaddon’s entire reign. [personal communication]

Roux further reports “in the spring of 671 B.C. ... Esarhaddon led his army into Syria ... and [then] entered the green land of Egypt.”<sup>80</sup>

Marc J.H. Linssen gave unequivocal evidence that Esarhaddon had to have an eclipse in 671 B.C. He discusses how:

“Making loud noises and music are ... effective [in warding off the evil effects of lunar eclipses]. The use of the kettle drum during an eclipse is well attested. The lamentation priest playing the kettledrum on the occasion of a solar (?) eclipse is attested in the Assur prayer VAT

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<sup>77</sup> Leroy Waterman, *Royal Correspondence of the Assyrian Empire*, vol. I (1930-1936), p. 187

<sup>78</sup> Roux, *op.cit.*, 3rd ed., p. 327

<sup>79</sup> G. van den Bergh, *Periodicity and Variation of Solar (and Lunar) Eclipses* (Haarlem, Netherlands 1955), p. 220

<sup>80</sup> Roux, *op.cit.*, p. 328

13832. In a Neo-Assyrian letter to Esarhaddon from 671 B.C. the kettledrum is used during an eclipse of the moon (LAS I 278).”<sup>81</sup>

This is indisputable evidence—proof—that there had to have been an eclipse in 671 B.C. if and only if Esarhaddon lived at that time and had invaded Egypt. If there was no lunar eclipse that year in the month of Tammuz, then this would be clear proof that Esarhaddon did not live at that time and must be moved to some other time to accommodate his reign and correct the chronology for his reign and that of all the kings who preceded and followed him.

Again, van den Bergh showed on the same page noted above—page 220—that there was no lunar eclipse in the month of Tammuz of that year. He does have one for July 2 that could perhaps fit the month of Tammuz which roughly coincides with the month of July. Nevertheless, Lange and Swerdlow proved that Tammuz of that year began on July 18 so a July 2 date could not be in the month of Tammuz and thus no lunar eclipse occurred in the month of Tammuz for 671 B.C.

The last time Esarhaddon attacked Egypt in 669 B.C. he died while en route.<sup>82</sup> There was an eclipse that occurred on June 10. But the month of Tammuz that year began on June 25 so the eclipse could not have occurred in Tammuz. There were no solar eclipses, according to Rose, in the month of Tammuz during the times Esarhaddon invaded Egypt, nor were there lunar eclipses in this month when he invaded Egypt. There is no astronomical evidence that supports the placement of Esarhaddon into the established chronology of the Neo-Assyrian empire. This is an immense contradiction to that chronology and has profound effects on all the other astronomical dates of the Neo-Assyrians and Neo-Babylonians called forth by Jonsson and the other historians.

Thus we now have the eclipse of 763 B.C. and the eclipse[s] for Esarhaddon that do not fit the chronology supported by the historians. There is a further form of astronomical data related to the planets at the time of Esarhaddon as reported by David Pingree and Hermann Hunger:

“Esarhaddon in an inscription found at Assur (Borger [1956], p. 2) states that in his first year Venus appeared in the West, in the Path of the stars of Ea, reached its <*ašar*> *niširti* and disappeared, while Mars

<sup>81</sup> Marc J.H. Linssen, *The Cult of Uruk and Babylon* (Leiden, Netherlands 2003), pp. 114-115

<sup>82</sup> Roux, *op.cit.*, p. 329

shone brightly in the path of the stars of Ea. Since the edge of the Path of Ea cuts the ecliptic at about  $210^\circ$  and  $330^\circ$ , a first visibility of Venus in the West occurred on about 20 January -679 with a longitude of about  $299^\circ$ , it reached its *ašar niširti* in the Fish [constellation] in February, and set in the West on about 14 October -679 with a longitude of about  $200^\circ$ . Mars was in the Path of Ea from about 18 December -680 till about 24 June -679; it was retrograde (and therefore bright) in the Path of Ea from about 4 September to 1 November -679. In another inscription, from Babylon (Borger [1956] p. 17), Esarhaddon reports that in his first year Jupiter approached the Sun in [the month of] Simānu, had its first visibility, reached its *ašar niširti* in the month Pēt-bābi, and then had its first station. Jupiter set heliacally in the West on about 24 May -679 (the conjunction of Simānu had occurred on 30 April), rose heliacally on about 26 June -679, and reached its *ašar niširti* in the Crab [constellation] in late September. The conjunction of the month here called Pēt-bābi occurred on 23 September. Jupiter's first station occurred on about 24 October. Thus, the statements fit the astronomical facts well, but are not presented with the [necessary] details of position and date that would make them useful to an astronomer."<sup>83</sup>

Lynn Rose examined the Borger 1956 material from which Pingree and Hunger obtained their data and wrote to the author on April 21, 2007:

“Finally, I found what was wrong. The Venus, Mars, and Jupiter materials are not tied to Year I of Esarhaddon. I had xeroxed ... pages 2 and 17 of Borger, ... but it appears that the Year 1 mentioned on page 17 is from a separate text and may not apply to the Jupiter material on that page. The Venus and Mars materials on page 2 may belong together, but there seems to be no year.”

That is, the documents in Borger where Pingree and Hunger found their data do not refer to the Jupiter, Venus, and Mars observations taking place in Esarhaddon's first year. The Jupiter material is in a separate document and year than that of Venus and Mars which has no regnal year for Esarhaddon. What Hunger and

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<sup>83</sup> David Pingree, Hermann Hunger, *Astral Science in Mesopotamia* (Leiden, Netherlands 1999), p. 138

Pingree did was put the two documents together and because the data fit year one of Esarhaddon, they assumed that their connecting the two documents was appropriate because it fit. Rose, who separated the data from the two documents, found several places where, as separate sets of astronomical data, they did fit between January -420 and January -380. But more work needs to be done to narrow down the correct dates.

This shows that evidence provided by Jupiter, Venus, and Mars cannot be employed to uphold Esarhaddon's conventional regnal date. Before anyone can do that, the two documents must be proven to have referred to year one of Esarhaddon. While the Jupiter material does belong in year one, the Venus and Mars data has no regnal year attached to it and cannot, without clear documentation, be placed in year one.

What we have are two sets of astronomical data; one referring to an eclipse of the Moon, positively dated to 671 B.C., but in this year in the month of Tammuz no such eclipse took place. The other set of data refers to the positions of Venus etc. in the year 680 B.C. that are roughly but not precisely set against certain positions of the sky. The problem is that one cannot accept both the lunar and planetary data belonging to Esarhaddon based on the established chronology because one of these—the lunar eclipses—did not occur. In order to properly place Esarhaddon in the chronology, both the lunar and planetary evidence must mesh with each other. Since they do not, one cannot accept one without the other to establish Esarhaddon's place in history. To accept the planetary evidence and ignore or dismiss the lunar evidence does not constitute proper archaeoastronomical analysis. It is culling data at best and manipulating it, saying only the data that fits our assumption and chronology is valid. Thus the reign of Esarhaddon cannot be kept in its present position and must be moved elsewhere.

What is undeniably true, therefore, is that the placement of Esarhaddon in the established chronology hasn't any responsible astronomical data to support it. He simply could not have reigned from 680 to 669 B.C. and thus the astronomical data for his reign does not intermesh with the gear teeth of the kings who followed him nor with the kings who preceded him. The dates for these kings as well as Esarhaddon are in error and must be revised. Moreover, the limmus/eponyms he appointed for the years 680 thru 669 B.C. are also erroneously placed and their placement must also be revised.

Because the astronomical evidence requires that these kings and limmus/eponyms be moved to a different time, along with that of the 763 B.C. solar eclipse to 436 B.C., the established chronology is faced with devastating contradictions. If we move Esarhaddon back 15 years to 695 B.C., then there would be an eclipse in the first year of his reign in the month of Teshri and another in his sixth year in the month of Tammuz. But were one to proceed along these lines, not only would Esarhaddon have to be moved 15 years back in time, but all the other kings and limmus/eponyms would also have to be moved back 15 years. This would allow Esarhaddon's reign to agree with the eclipse data for his reign but the astronomical data that upholds the positions of all the other kings and limmus/eponyms would be off by 15 years and their reigns would then be in contradiction to the astronomy. The same occurs when we move Esarhaddon forward by even one year.

What do we know from the documents of the period prior to Esarhaddon's reign? James *et al.* show:

“An informative comparison can be made between the figures given by two Assyrian monarchs, Shalmaneser I (conventionally 13th century BC) and Esarhaddon (7th century BC), for the history of the Temple of Assur. This was founded by the early king Ushpia, then successively restored over the centuries by kings Erishu, Shamshi-Adad I, Shalmaneser I and Esarhaddon. The two documents are in accord with respect to the first period mentioned [(from] Ushpia to Erishu), in that no figure [in years] is given. Otherwise they disagree. For the second interval [(from] Erishu to Shamshi-Adad) there are contrasting figures of 126 (Esarhaddon) and 159 (Shalmaneser) years. For the third period [(from] Shamshi-Adad to Shalmaneser), Esarhaddon gave 434 years, while Shalmaneser himself recorded 580 years.”<sup>84</sup>

Therefore, the documentary evidence of the period prior to Esarhaddon's reign is in contradiction to other documents. The astronomical eclipse data for Esarhaddon do not fit the period to which he has been assigned by historians, and to move him forward or back in time to have the astronomical evidence fit these other placements destroys the entire chronological edifice upon which the established chronology is predicated. Keeping Esarhaddon in this present slot

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<sup>84</sup> James *et al.*, *op.cit.*, p. 294

simply defies the astronomical facts. Moving him destroys the *rest* of the astronomical evidence. All these astronomical dates must be moved to a time when they do not contradict each other but are in harmony.

James *et al.* claimed that the chronology of the Neo-Assyrians and Neo-Babylonians “is as watertight as one could ask for any period of ancient history. Assyrian history is firmly datable, with a margin of error no greater than a year, as far back as 911 BC.” Jonsson claims “it would be impossible to change the chronology by even one year ...” Yet here we have attested astronomical evidence and data that demands all the king and limmu/eponyms be moved by years, or even, in terms of the 435 B.C. eclipse, by centuries. To argue, as does Jonsson, that the intermeshing astronomical cogwheel’s gear teeth make any revision—that is, shortening—of the chronology of first millennium Mesopotamia “impossible” must now apply to the established chronology. The machinery of astronomy has ground to a stop at this point.

Nevertheless it may be argued that these are only two forms of eclipse data that are in question. According to James *et al.*, as well as Jonsson, there cannot be even one astronomical discrepancy in the data. But the fact of the matter is we have just begun our excursion into the astronomical data and there are other points that at best are questionable and at worst cannot be accommodated to that long chronology.

We will return to Esarhaddon at the end of the units on Astronomy as his reign relates to the short chronology.

## **SATURN, KANDALANU, AND CHRONOLOGY**

Jonsson states:

“One of the most important texts from the seventh century B.C.E. is the Saturn tablet from the reign of the Babylonian king Kandalanu (647-626 B.C.E.) ...

“The text was first described by C.B.F. Walker in 1983 in the *Bulletin of the Society for Mesopotamian Studies*. A transcription and a translation with a full discussion of the text by Mr. C.B.F. Walker has recently been published.

“... the planet Saturn has a revolution of c. 29.5 years. Due to the revolution of the earth around the sun, Saturn disappears behind the sun for a few weeks and reappears again at [mean] regular intervals of 378 days.

“The Saturn tablet gives the dates (regnal year, month, and day in the Babylonian calendar) and the positions of the planet Saturn at its first and last appearances for a period of fourteen successive years, specifically, the first fourteen years of Kandalanu ... The name of the king, given only in the first line, is partially damaged, but may be restored as [*Kand*]alanu. The name of the planet is nowhere mentioned in the text, but the observations fit Saturn and no other planet.

“As Mr. Walker explains:

“‘The name of the planet Saturn is not given on the tablet ... It is, however, certain that we are dealing with Saturn and Kandalanu. Saturn is the slowest moving of the visible planets, and only Saturn would move the distances indicated between successive first visibilities.’

“The text is damaged in several places, and many of the *year numbers* are illegible. Years 2, 3, 6, 7, 8 and 13 are undamaged, however. Besides this each year is covered by two lines in the text, one for the last appearance of the planet and the other for the first, the total number of lines covering fourteen years ... With this framework there is no problem in restoring the year numbers that are damaged ...

“In other words, the absolute chronology of Kandalanu’s reign is definitely fixed by the Saturn tablet, because the pattern of positions described in the text and fixed to specific dates in the Babylonian lunar calendar *is not repeated again in more than seventeen centuries!* ...



“The astronomical data on the Saturn tablet make ... changes [in the dating of the reign of Kandalanu] completely impossible.

“... the Saturn tablet puts a definite block to the attempt at lengthening [or shortening] the chronology ... The astronomical data on the Saturn tablet makes such changes completely impossible.”<sup>85</sup>

But does the astronomical data inscribed “on the Saturn tablet make [a chronological revision] completely impossible?” Is it a fact that these specific positions of Saturn tied to the lunar calendar of Neo-Babylonia cannot be “repeated again for almost seventeen centuries”? The answer to both questions is “no,” if one allows for a much more accurate relationship of the positions of Saturn to the texts. One can find a definitive point in the chronology of the Persian Era wherein the positions of Saturn fit the data in the Saturn tablet far better than in the seventh century B.C. This occurs because that specific data in the Saturn tablet does *not* fit in the seventh century B.C. as closely as Walker and Jonsson suggest.

According to Lynn E. Rose’s personal communication, the position of Saturn fits much better in Persian times. Using Schoch’s Saturn Tables, he tested all the years of Kandalanu for the positions of Saturn and found that Walker’s positionings were off by an average of about two days. Now because of these discrepancies there is no doubt that the evidence in the tablet is based on observations. It is probable that the observers had cloudy nights or other problems related to seeing Saturn such as having it seen a day or two earlier or later than expected. Because this is observational data, a good fit would add up all the earlier and later days for Saturn’s appearances and disappearances and be off by a few days at most. Rose in comparing this data found he had a +2 overall score. But when he did the same for Walker’s data, he found a score of -31! That is, Saturn was continually appearing and disappearing before it should.

In terms of an algebraic overall score Walker was off *on average* by -1.647 days too early. Rose on the other hand had an algebraic average of 0.059, or *on average* the ancient observers saw Saturn appear and disappear on the very day expected. When we divide Rose’s algebraic score into that of Walker it shows that Rose’s fit is about 25 times closer than Walker’s when the Saturn data is placed in Persian times!

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<sup>85</sup> Jonsson, *The Gentile Times Reconsidered*, *op.cit.*, pp. 169-171

With regard to the fall and death of Kandalanu, W. Dubberstein states: “The date quoted by Oppert would make Kandalanu [in his 22nd or last year] still ruling in October ...”<sup>86</sup> Parker and Dubberstein report that this occurred by extending Kandalanu’s reign after his death one year later.<sup>87</sup> That is, Kandalanu’s extended reign ended in late October. But it is also held that Alexander the Great entered Babylon in late October.<sup>88</sup> That Alexander should enter Babylon at about the time that Kandalanu’s reign ends is an extraordinary fit.

Heinsohn holds that Kandalanu reigned with the Neo-Assyrian king Ashurbanipal who was the *alter ego* of Darius III of Persia. With Kandalanu placed with Ashurbanipal, the astronomical evidence fits Heinsohn’s thesis precisely.

The period of over 1700 years that Jonsson and Walker suggest is required for Saturn to repeat the astronomical positions is simply without foundation; it recurred about 295 years later, but not precisely. Let us also remember that the assumed 763 B.C. total solar eclipse actually took place in 435 B.C. (This is using version 2.0 of Lange and Swerdlow, rather than version 3.1.) It moved the chronology forward by at least 327 years and in turn places most of the limmus in Persian times and ended three years before Alexander the Great took Babylon. Moving Kandalanu forward in time by some 295 years we run into a similar situation because his reign ends with the coming of Alexander to Babylon. This evidence is not fortuitous and not a mere chance possibility.

## THE ASTRONOMICAL DIARY FOR 652 B.C.

The diary of observations of several planets and the Moon for the year 652 B.C. is also one of the major astronomical supports for the established chronology. In it the positions of Mercury, Saturn, and Mars in relation to certain stars clearly determine this date. However the name of “the king, his regnal year

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<sup>86</sup> Waldo H. Dubberstein, “Assyrian-Babylonian Chronology (669-612 B.C.)”, *Journal of Near Eastern Studies*, vol. 3, no. 1 (Jan 1944), p. 40

<sup>87</sup> Richard R. Parker, Waldo H. Dubberstein, *Babylonian Chronology 626 B.C.–AD 75* (Providence RI 1971), p. 11

<sup>88</sup> Michael Wood, *In the Footsteps of Alexander the Great* (Berkeley CA 1997), p. 92

and month names, *are broken away*.”<sup>89</sup> At the end of this diary it states on “‘the 27th’ of the month (the month name is broken away) ... at the site of ‘Hiritu in the province of Sippar troops of Babylonia and of Assyria fou[ght with each] other, and the troops of Babylonia withdrew and were heavily defeated.”<sup>90</sup>

The diary does not name the king, nor the month regarding this battle, but another tablet was found that is taken to fill in these missing elements. The translation of this other document sets the battle in the

“sixteenth year of Shamash-shuma-ukin ... On the twenty-seventh day of Adar [the 12th month] the armies of [Neo-]Assyria and Akkad [Babylonia] did battle at Hirit. The army of Akkad retreated from the battlefield and a major defeat was inflicted upon them.”<sup>91</sup>

The question of the support for this date hinges on a few elements. (1) Did the battle fought at Hirit named in both documents take place at the site of “Hiritu in the province of Sippar”? (2) Was the battle fought between “troops of Assyria and Akkad [Babylonia]”? (3) Can we assume that the battle was fought in the month of Adar? and (4) can we assume the king, unnamed in the diary, is the same king, Shamash-shuma-ukin, named in the other tablet? The answer to the first two questions is no; that is, the documents contradict one another. (1) The battle of Hiritu is not fought in the province of Sippar. (2) The battle fought was not between the troops of Assyria and Akkad (Babylonia).

Grant Frame shows:

“The exact location of Hiritu, however, is unclear. Up until now it has been assumed that the Hiritu mentioned in the Akitu Chronicle [not the diary] was located in southeastern Babylonia, but [the diary] states that it was in the province of Sippar. This would place it in northern [not southern] Babylonia, close to the Assyrian border, unless one wishes to assume either an error in the text or a second and otherwise unattested province of Sippar in the south—both undesirable assumptions ... Hiritu means ‘ditch,’ ‘canal,’ or ‘moat’ and it is thus not surprising that several places have this name, making it difficult to

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<sup>89</sup> Jonsson, *op.cit.*, p. 166

<sup>90</sup> *ibid.*, pp. 166-167

<sup>91</sup> *ibid.*, p. 167

connect the Hiritu of our texts with any other place of a similar name. We may note, however, that a Hiritu is attested in the Ur III period in the province of Urum (possibly located just north of Sippar). Millard has suggested that the Hiritu of the chronicle is to be connected to Harutu ... a border fortress between Babylonia and Assyria situated east of the Tigris in the time of Tukulti-Ninurta II and Ashurbanipal II, and possibly to Hararatu ... a town destroyed by Sennacherib in 702.”<sup>92</sup>

This is like saying a battle was fought at a town called Bergamo in the province of Sicily, Italy, while in fact Bergamo is located in northern Italy at the foothills of the Alps. Thus it is quite clear that the diary and the chronicle contradict one another on this major point of the location of the battle. If there was clear agreement between these documents Millard would not have attempted to construct theories to make them correlate with each other.

Was the battle, as the document states, fought between the Assyrians and the Babylonians? Again Frame explains:

“The identification of the battle mentioned in ... the Akitu Chronicle, and the astronomical diary does, however, present at least one problem. The Babylonian chronicle and the diary state that it was Babylonians (troops of Akkad) who fought with the Assyrians, while edition B [the Akitu Chronicle], an Assyrian document, only refers to Elamite forces on the opposing side. One could argue that the difference is due to the different orientations of the texts (i.e., Babylonian versus Assyrian). Similar conflicting reports about who took part in fighting (as well as who won) are preserved about the battle of Der in 720 [B.C. ], where scholars give greater credence to a statement found in a chronicle—regrettably not the same chronicle as the one in question here—than to those found in Assyrian (and Babylonian) royal inscriptions. However, in view of the detail involved in edition B [of the Akitu Chronicle] (e.g., the names of various Elamite officials taking part in the battle [and not Babylonians] and their fates), it seems certain that a battle [at Hiritu] between Elamite and Assyrian forces actually took place.”<sup>93</sup>

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<sup>92</sup> Grant Frame, *Babylonia 689-627 B.C. A Political History* (Istanbul Turkey 1992), p. 290

<sup>93</sup> *ibid.*, p. 292

Explanations to deal with this further contradiction between the two documents are suggested<sup>94</sup>. However, none of these are other than untestable or unfalsifiable and, in fact, are only assumptions. Frame does support the view that the documents correlate with, and corroborate, one another but has no clear-cut evidence to support his belief.

Not only is the battle fought in the wrong province but one of the combatants is from the wrong nation. One cannot assume that the king, whose name is also broken away, is Shamash-shuma-ukin. The suggestion that one has unimpeachable evidence to connect these two documents cannot be sustained. One must make too many assumptions, and raise them to the level of facts, to have these documents correlate with, and corroborate, one another. These documents also hang in the air and cannot be employed as clear-cut evidence against Heinsohn and Sweeney's thesis that the Neo-Assyrians are the Persian rulers of Assyria.

One, of course, can argue that it is quite probable that these texts fit together, but in dealing with empirical scientific evidence, one does not use probabilities as definite proof. One can also suggest that the vast majority of historians accept this equation between these texts, but majorities have nothing to do with truth.

Battles were constantly being fought between kings in Mesopotamia, and having similar types of battles fought cannot be taken as proof against Heinsohn or Sweeney. One of the principal arguments employed by critics of their short chronology is that when Heinsohn and Sweeney turn to close similarities between kings of different times and empires to show they are one and the same person, there is invariably other contradictory documentary evidence that shows the identification between these kings cannot be upheld. The critics argue that if Heinsohn and Sweeney turn to documents to show the identity is valid, when other documents contradict the identity, that is proof that the associative identification falls apart. Therefore, based on the same principle, these critics cannot disregard the fundamental contradictions between the two documents which have been used to show that the battle discussed in both is one and the same, and therefore one can use this as evidence for the long chronology.

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<sup>94</sup> *ibid.*

The critics cannot have it both ways. To do so would be to use a double standard of inference. That is the reason this author has generally avoided this kind of evidence and required instead empirical evidence to argue for Heinsohn's and Sweeney's theses. The attempt to force these two documents to corroborate one another is merely text editing.

### NABOPOLASSAR, LUNAR DATA, AND CHRONOLOGY

In our discussion of "Saturn, Kandalanu, and Chronology", above, it was shown that Kandalanu, who is either the brother of Ashurbanipal or the name that Ashurbanipal used in Babylon, reigned up to the time Alexander the Great entered that city. According to the established chronology, Nabopolassar comes to reign one year after Kandalanu.<sup>95</sup> Therefore, the astronomical data that places Nabopolassar back between 625-605 B.C. should actually fit—and fit far better—after Alexander the Great, making him a vassal of the Macedonian Greeks who ruled after Alexander.

Nabopolassar is the first king of the supposed Neo-Babylonian empire, but coming after Alexander cannot have been the ruler of a great empire. He would be a minor king who was kept as a vassal in Babylon to administer that region or that city for the Macedonian Greeks who came to control Mesopotamia after Alexander's death. This evidence therefore requires that Emmet Sweeney's thesis, that the Neo-Babylonian kings were the last rulers of the Persian empire, cannot be correct.

Nabopolassar is conventionally believed to have reigned from 626 to 605 B.C. Lynn Rose examined the lunar data related to his reign in a paper with a similar title to this unit. If, as archaeoastronomers maintain, he reigned at that early period, then this would be the *coup de grace* to Sweeney's chronology for the Neo-Babylonian empire. In order to determine where Nabopolassar belongs in terms of astronomical data, Rose examined the attested month-lengths assigned to this king.

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<sup>95</sup> Jonsson, *op.cit.*, p. 107

If these months occurred just where the established chronology would put them, then Nabopolassar was properly placed. But Rose recognized an additional form of evidence that should coincide with Nabopolassar's reign, namely when the New Year should begin. According to Rose's manuscript,

“The [ancient] astral compendium known as the *Mul Apin* is usually considered highly idealized, insofar as it repeatedly puts the vernal [spring] equinox on day 15 of month I [first month of the year, Nisanu] and leaves us with the distinct impression that it *always* occurs on that same date! Perhaps Nisanu 15 was thought of as an approximate *mean* date. In any case, the *Mul Apin* [astronomical compendium] does constitute an early precedent for having the vernal equinox within [the first month of the year,] Nisanu. As I see it, the *Mul Apin* [documentary evidence] is all the justification we need for assuming that the [Neo-]Babylonians were trying, for the most part, to keep the [spring or] vernal equinox within Nisanu ... Nabopolassar's own traditions would clearly have included the *Mul Apin* tradition that the vernal equinox was supposed to occur within Nisanu.”<sup>96</sup>

Rose used the only possible two sets of sequences to see which best fit the conventional chronology. These sequences began in Year 2 of Nabopolassar's reign, on March 13, -623, and on April 11, -623. The March set had the better fit with either 29-day or 30-day lengths for the established chronological placement of Nabopolassar. Rose found that 19 were properly placed and 8 were not, which gave a score of 70.37%. While this number of accurately placed months is well above 50%, a 50% score would mean the data surely cannot support Nabopolassar's reign, but 70.37% is not all that good. For a correct chronology, one would have “expected a score somewhere between 20 and 26.”<sup>97</sup>

But in terms of having the spring or vernal equinox contained in the first month—Nisanu—of the year where Nabopolassar is conventionally placed, Rose discovered for the March sequence that after year 5 of his reign there were “*seven* cases of Nisanu ending *before* the vernal equinox.”<sup>98</sup> That is, over 31% of the time for that period, the first month of the year ended before the vernal equinox occurred or was only around 69% accurate. On the other hand the April sequence

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<sup>96</sup> Lynn E. Rose, “The Lunar Data from the Reign of Nabopolassar”, p. 3

<sup>97</sup> *ibid.*, p. 13

<sup>98</sup> *ibid.*, p. 12

had the month of Nisanu beginning in “seven cases ... *after* the vernal equinox.”<sup>99</sup> In the March sequence one of the Nisanu months ends 13 days before the vernal equinox and in the April sequence one of the Nisanu months begins 15 days after the vernal equinox.

In terms of the calendar, the Babylonians knowing this should have added extra months (intercalary months) in certain years to make the month of Nisanu fall in such a way that it contained the vernal equinox, but the fact of the matter, as shown by Rose, is that this was not done. There were indeed intercalary months added but they were not placed to permit the vernal equinox to fall within the month of Nisanu:

“What this amounts to,” Rose states, “is that the intercalation practices [of the Neo-Babylonians] make no sense: there should have been at least one more intercalary month [added somewhere]. And the intercalary months [that had been added] should have been *much* more effectively placed. (If they were really trying to keep Nisanu starting *after* the vernal equinox, matters would of course have been even worse!)”<sup>100</sup>

Above and beyond these problems is the year 19 of Nabopolassar’s reign which contains three consecutive months—the 9th, 10th, and 11th—each 29 days long. This creates another major obstacle to having Nabopolassar dated where the conventional chronology has put him. Rose is here given space to speak for himself regarding the way these three 29-day months clearly prove that Nabopolassar could never have been placed where the historians and archaeoastronomers require him to be:

“Notice that there are three consecutive 29-day months reported in Year 19: these are months IX, X, and XI. That sort of thing does happen — every six or seven years, on average (see Huber, 1982, pages 24-25) — and is thus not all that unusual. There is a general consensus among scholars that when there was bad seeing at the end of day 29 of a lunar month, the ancients would recognize the new day as day 30 of the old month, but that if they *saw* the New Crescent, they would of

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<sup>99</sup> *ibid.*

<sup>100</sup> *ibid.*, p. 13



course count that new day as day 1 of the new month. (This generalization does not apply to the Maya, however, who used a completely different procedure.) Let us ask ourselves how a month that *we* retrocalculate as having been astronomically a 30-day month might nonetheless have been counted by the Babylonians as a 29-day month. The simplest way in which this might happen is if there were an astronomical sequence consisting of a 29-day month followed by a 30-day month. Suppose that months VIII and IX of Year 19 constituted such a sequence. If there was bad seeing as the 29-day month VIII ended, they would have counted it as a 30-day month, and their day-count would consequently have been running one day late all through the astronomical 30-day month IX that followed. When they got to the end of what they counted as day 29 of that month IX, they might have *seen* the New Crescent and immediately started a new month. Thus an astronomical sequence of 29-30 would have been reported as a sequence of 30-29. That would explain how the *first* of the three reported 29-day months might be wrong. But how could they get the next two wrong? Scribal error might be involved, but I find that unlikely at best. Presumably they did not count a month as having 29 days unless they *saw* the New Crescent. I grant that they might have counted it as having 29 days, in *spite* of bad seeing, if there had been a number of days of invisibility and if the New Crescent had consequently been considered a sure thing; this circumstance might have been considered almost the equivalent of an actual sighting. But our best bet is to assume that if they cut the month off at 29 days, it was because they *saw* the New Crescent. Besides, at least if we look at what is now our only surviving sequence, the March one, the three relevant invisibilities in -606 and -605 were *each* of two days only! (The April sequence runs into a 3-day invisibility at the end.) Now I ask the crucial question: How could they be wrong about the final two months of the four-month sequence, namely, months X and XI? If the astronomical sequence of VIII-IX-X-XI was 29-30-29-29, and if there was bad seeing after the initial 29, it would be very easy for them to mistake this for a sequence of 30-29-29-29. But they would not have called the second one a 29 unless they *saw* the New Crescent that ended it (or had some other astronomical assurance that that must indeed have been the time when the New Crescent would become visible). Similarly, they would not have called the third and fourth ones 29-day months unless

they *saw* the New Crescents that ended them (or had some other appropriate astronomical assurance regarding the matter). In other words, except for the remote possibility of scribal error, *I do not see how they could have been wrong about the lengths of the third and fourth months in this sequence.* It seems to me that they must have *seen* those two consecutive 29-day months, namely months X and XI of Year 19. Yet retrocalculation clearly shows New Crescents on 11/26/-606, on 12/26/-606, on 1/24/-605, and on 2/23/-605, thus the March sequence features a definite 30-29 for months X and XI of Year 19, and the April sequence features a definite 29-30 for months X and XI of Year 19. I cannot for the life of me see how it is possible for either month X or month XI of Year 19 to have an astronomical length of 30 days. And what that amounts to is that I cannot for the life of me see how the conventional dating of Nabopolassar can be correct.<sup>101</sup>

In other words, there is no way other than scribal error, a very convenient excuse that historians use to fix data to fit their chronology which is contradictory, for Nabopolassar to have three months—the 9th, 10th, and 11th months of his 19th year of reign—to each contain 29 days! Thus there are three levels of evidence that are contradictory to having Nabopolassar dated where the historians have placed the founder of the Neo-Babylonian dynasty:

(1) There are fewer of the appropriate 29 and 30-day months than would have been called for.

(2) The only two possible sequences in which Nabopolassar could be placed in the seventh century do not have the vernal equinox occurring sufficiently often in the month of Nisanu.

(3) It is not possible to place Nabopolassar in either of these possible sequences and at the same time allow him to have, in the 19th year of his reign, the 9th, 10th, and 11th month each 29 days in length. As Rose shows, what works there is that in the 8th, 9th, 10th, and 11th months, 29, 30, 29, and 29 day sequences would be satisfactory.

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<sup>101</sup> *ibid.*, pp. 14-15

If, however, Sweeney is correct in some manner, then Nabopolassar's reign should fall at around the end of the Persian era and have none or at least far fewer of these problems than his present placement contains. On August 7, 2006, Rose did find this far better fit at just around the end of the Persian era and into Hellenistic times: By moving Nabopolassar's reign forward to March 23, 340 B.C., all the later Nisanu months contain the vernal equinox except one which has it fall "only about 21 hours, not even a full day," before the vernal equinox. That is, nearly every single month of Nisanu after year 5 of Nabopolassar's reign contains the vernal equinox, while in the conventional time, when he supposedly lived, 7 out of 22 years do not accommodate the vernal equinox within Nisanu and in one instance for each of these sequences it is off by 13 or 15 days.<sup>102</sup>

As for the 19th year, according to Rose's calculations there are no problems with there being 29-day months in the 8th, 10th, and 11th months of that year and a 30-day month in the 9th month.

Lastly, of the 27 attested month-lengths, instead of having 19 that fit, a score of 70.32 %, Rose's chronology has "20 correct month lengths giving a score of just over 74 percent."<sup>103</sup> In two areas of this question Rose's placement of Nabopolassar is perfect or nearly perfect. In one area his placement is somewhat superior to the established chronology. That is, in that area Rose's attested month-lengths are "just as respectable" as those of the historians and archaeoastronomers, but in the other two areas the dating of this king is near perfect, while theirs is unsupportable. Rose concludes:

"All this constitutes very strong evidence that Nabopolassar reigned from -341 to -320. The repercussions of those new dates are quite interesting. Nebuchadnezzar and Nabonidus and the other Neo-Babylonians would be vassal kings under the Macedonians. Nabopolassar himself would have begun under the Persians and continued under [and in the service of] the Macedonians."<sup>104</sup>

Now according to all historians the Neo-Babylonians follow the Neo-Assyrians. In the case of the short chronology, the Neo-Assyrians belong in the fifth and fourth centuries and are presumably vassal-kings under the Persian Great Kings.

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<sup>102</sup> *ibid.*, p. 16

<sup>103</sup> *ibid.*

<sup>104</sup> *ibid.*, p. 14

That being the case, the Neo-Babylonians coming at the very end of the Persian era makes perfect sense. It follows from the astronomical evidence that agrees with the Neo-Assyrians being Persians. It agrees with the limmu list used to establish that chronology and with the rest of the scientific and technological evidence which, as we will also show, support this down-dating of both the Neo-Assyrians and Neo-Babylonians.

## NABONIDUS'S LUNAR ECLIPSE AND CHRONOLOGY

According to the established chronology Nabonidus comes after Nebuchadnezzar and some short-reigned minor kings; he also comes after Nabopolassar. Since that is the case, he should reign well after Nabopolassar and leave the correct spacing in the number of years for Nebuchadnezzar and these minor kings to rule during the Macedonian rule of Mesopotamia. While at a working visit to Lynn E. Rose's home I found in his library a paper by Erica Reiner titled "Babylonian Celestial Divination"<sup>105</sup> which begins thus:

"On the thirteenth of the month of Ululu, the moon became eclipsed and set while [still being] eclipsed. It was a sign that the moon god requests a high priestess.' So speaks Nabonidus, the last king of the [Neo-]Babylonian empire before its conquest by Cyrus, in his account of the selection and installation of his daughter as high priestess of the moon god Sin. The date of the eclipse can be established as 26 September 554 B.C., and its interpretation was given to the king by the scholars in his entourage who had at their disposal a compendium of celestial omnia in which they were expected to look up the significance and the prediction associated with the event observed."

Paul-Alain Beaulieu places the eclipse in "Nabonidus's second regnal year (September–October) ... a lunar eclipse [occurred] on 26 September 554 ..."<sup>106</sup>

Rose examined this eclipse in Georg van den Bergh's *Periodicity and Variation of Solar (And Lunar) Eclipses (op.cit.)* which showed such an eclipse occurred at that time. The question, of course, is: was the Moon eclipsed when it set? When Rose used the *Starry Night* and *Planetary, Lunar and Stellar Visibility* programs to evaluate this question, he found that the Moon, at the very moment of setting, was emerging from a partial eclipse and one could see the almost complete full Moon except for a very tiny part covered by the Earth's shadow, just as it set at the horizon. Where the Moon touched the horizon is the point where that tiny area of the Moon was still in shadow. Any observers who witnessed this event could not

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<sup>105</sup> Erica Reiner, "Babylonian Celestial Divination," in *Ancient Astronomy and Celestial Divination*, Noel M. Swerdlow ed. (Cambridge MA 1999), p. 21

<sup>106</sup> Paul-Alain Beaulieu, "King Nabonidus and the Babylonian Empire", *Civilizations of the Ancient Near East*, *op.cit.*, p. 974

truly know, during such conditions, whether or not the Moon was still eclipsed at the moment that it set. If the *Starry Night* and *Planetary, Lunar and Stellar Visibility* programs are correct, then this eclipse cannot be correctly placed where historians claim it belongs. The evidence is dubious in the extreme.

The question as it relates to the short chronology: Is there a date in the 3rd century B.C. wherein the Moon is eclipsed while setting, and where the evidence is unambiguous, as it is with the *Starry Night* and *Planetary, Lunar and Stellar Visibility* programs? According to Rose there was a total eclipse of the Moon in the month of Ululu while it was setting, as shown by *Starry Night* and *Planetary, Lunar and Stellar Visibility*. That is, it occurred on October 2, 294 B.C. but unlike the one given in *Starry Night* and *Planetary, Lunar and Stellar Visibility*, dated to September 26, 554 B.C., the Moon was totally eclipsed above the horizon as it was setting and remained totally eclipsed while setting! According to Rose this 294 B.C. “eclipse is the *only* relevant eclipse anywhere within several decades,” forward or back in time.

Now this eclipse happened during Nabonidus’s second year on the throne. Therefore, Nabonidus would have begun his reign in 296 B.C. With Nabopolassar’s reign ending in 321 B.C., as shown above, there is a period of about 25 years into which the reigns of Nebuchadnezzar and the few, short-reigning kings must fit. Critics can raise the objection that Nebuchadnezzar II ruled for 43 years and it would be impossible to squeeze his reign into 25 years, even if he were made a co-regent with Nabopolassar the day he was born. Then one could also add where is there a place for the short-lived kings who followed him, namely Awel-Marduk, 2 years or less; Neriglissar, 4 years or less; and Labashi-Marduk, 2-3 months.<sup>107</sup> 50 years in all does not fit into a 25-year framework.

There is a further reason that makes it impossible to place Nebuchadnezzar II in this time slot. And that is based on astronomical dating. As Jonsson informs us:

“The most important astronomical diary for our discussion is designated *VAT 4956* ... This diary is dated from [the month of] Nisanu [day] 1 of Nebuchadnezzar’s thirty-seventh regnal year to Nisanu 1 of his thirty-eighth regnal year, recording observations [and positions]

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<sup>107</sup> Jonsson, *op.cit.*, p. 121

from five months of his thirty-seventh year (months 1, 2, 3, 11 and 12).

...

“Among the many observed positions recorded on *VAT 4956* there are about thirty which are so exactly described that modern astronomers [who retrocalculate all this data] can easily fix the precise dates when they were seen. By doing so they have been able to show that all these observations (of the moon and the five then known planets) must have been made during the year 568/67 B.C.E.”<sup>108</sup>

These thirty precisely-dated and positioned points in the sky of the Moon, Mercury, Venus, Mars, Jupiter, and Saturn unquestionably place Nebuchadnezzar II's 37th year between 568 and 567 B.C. and nowhere else! Yet I maintain that Nebuchadnezzar does fit into the 25-year time-slot between 321 and 296 B.C. even without moving Nebuchadnezzar II. The fact of the matter is that there were two major Nebuchadnezzars as well as minor ones. In ancient times they were not designated Nebuchadnezzar I and Nebuchadnezzar II as is done, say, with English kings such as George the first, the second, the third, and so on. The astronomical data on the tablet refers to one of these two Nebuchadnezzars, not necessarily the one customarily called Nebuchadnezzar II. Therefore the other major Nebuchadnezzar must have had a reign length that clearly fits into the 25-year time-slot between Nabopolassar and Nabonidus and must also allow for the 5 to 6-year reigns of Awel-Marduk, Neriglissar, and Labashi-Marduk.

According to *Nebuchadrezzar I information* from *Answer.com* on the Internet Nebuchadnezzar I “was king of the Babylonian Empire from about 1125 to 1104 BC”, or 21 years. According to Grant Frame, Nebuchadnezzar I's reign length is based only on one of the copies of the “Babylonian King List [which] indicates that he reigned for twenty-two years.”<sup>109</sup> Further dates for Nebuchadnezzar I are “(ca. 1124-1103)” or 21 years.<sup>110</sup> That is, Nebuchadnezzar I only ruled for about 21 years. Therefore, he and Awel-Marduk, Neriglissar, and Labashi-Marduk do fit quite well into the 25-year time-slot between Nabopolassar and Nabonidus.

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<sup>108</sup> *ibid.*, pp. 157-158

<sup>109</sup> Grant Frame, *Rulers of Babylonia From the Second Dynasty of Isin to the End of the Assyrian Domination (1157-612 B.C.)* (Toronto 1995), p. 11

<sup>110</sup> Roux, *op.cit.*, p. 277

On the other hand it can be argued that moving Nebuchadnezzar I forward by about 900 years creates an unfilled slot in the second millennium B.C. Yet the fact of the matter is that Nebuchadnezzar I's reign falls within Dark Ages discussed in volume I of this series. Brinkman stated that during this Dark Age

“Babylonian history during the first quarter of the first millennium B.C. [directly after Nebuchadnezzar I] may be characterized as a period of obscurity or ‘dark age’ ... Little source material has survived from these ... times and this little is sometimes quite difficult to date.”<sup>111</sup>

James *et al.* add:

“The term ‘Dark Age’ seems like an understatement when the archaeological remains from Babylonia usually dated between *c.* 1050 and 750 BC are examined. Even the most important cities show little trace of activity over this long period. After flourishing under the Kassite kings of the 15th to 14th centuries BC, the great city of Ur waned a little in importance ... The last remains from this period consist of some inscribed bricks of King Adad-apla-iddina (1068-1047 BC). Then the documentary record become a complete blank over a period of something like 350 years; archaeological remains are equally elusive ...

“A ... bleak picture is related by Brinkman for the whole country [of Babylonia]:

“‘Archaeological sources are ... meagre. Architectural remains which may belong to this time are usually minor repairs on older structures, with no inscription left to record the identity of the repairer. (In fact, no buildings have yet been excavated in Babylonia which can be dated with certainty to the time of any ruler between 1046 and 722 B.C.)’”<sup>112</sup>

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<sup>111</sup> quoted in James *et al.*, *op.cit.*, p. 279

<sup>112</sup> *ibid.*, pp. 279-280; also see Ginenthal, *op.cit.*, Chapter 15, “Dark Ages Based on Dark Scholarship”



Oppenheim further informs us:

“The victory of Nebuchadnezzar I (1125-1104 BC) over the Elamites ushers in that half millennium through which Babylonia ... slowly rose to power [and] culminates in Nabopolassar ... (625-605 BC) ... Much of that span of time is as dark an age as the Dark Age itself.”<sup>113</sup>

That is, after Nebuchadnezzar I there is a Dark Age. What happened before Nebuchadnezzar I reigned, was there a Dark Age? Yes. This can be read on page 484 of *Pillars of the Past* vol. I: Nebuchadnezzar I hangs in the chronological air preceded by a Dark Age and followed by one. When we incorporate all the scientific and technical evidence into this scheme, we have nothing to keep Nebuchadnezzar I where he is conventionally now placed.

The chronology in terms of astronomy allows Nabopolassar to be followed by Nebuchadnezzar's 21-year reign, then by Awel-Marduk for almost 4 years; by Neriglissar for almost 2 years, by Labashi-Marduk for 2-3 months, followed by Nabonidus. The fit is almost as close to perfect as one could wish. Therefore, the 25-year slot between Nabopolassar and the arrival of the Macedonians fully allows for this chronology. The astronomical fit is the most important evidence and it fits the short chronology almost perfectly but not the long chronology. What this forensic evidence shows is that the long chronology “is impossible” as Jonsson said of any revision of first millennium Mesopotamian history.

Anatoly T. Fomenko, the Russian historian, has devoted some space to archaeoastronomical dating in his book *Empirico-Statistical Analysis of Narrative Materials and its Application to Historical Dating* (Dordrecht/Boston/London 1994). His comments on page 137 regarding eclipses, stellar and planetary positions deserve consideration:

“A method of independent dating was suggested by [Nikolai Alexandrovitch] Morozov ... namely that all possible eclipse characteristics are extracted from a text under investigation, and when the dates of all these eclipses with characteristics are mechanically extracted from the astronomical tables (canons) ... he discovered that

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<sup>113</sup> Oppenheim, *op.cit.*, pp. 154-160

under the pressure of established traditional chronology, the [archaeo]astronomers did not consider the whole spectrum of dates obtained; they took only those dates which fit the time interval *a priori* dictated by historical tradition. It turned out this practice often led the astronomers to the impossibility of discovering in the required century an eclipse precisely answering to the description in the document, while being forced, in most cases, and still not questioning the whole system of chronology, to resort to doubtful solutions, for example to indicate an eclipse no more than partly satisfied by the description [found in the diary, canon, etc.]”

This is just what we have found to be the case repeatedly. The 763 B.C. eclipse of Assur Dan III did not fulfill its characteristics. It was a partial eclipse over Assur and Nineveh, while the 435 B.C. eclipse fulfilled the characteristics completely. None of the eclipses of Esarhaddon fit in the chronology of his reign.

The eclipse of Nabonidus also did not fulfill the characteristics stated in the document when it was dated to September 26, 554 B.C., but fully conformed to the document when it was dated to October 2, 294 B.C.

We have found the very same thing to be the case for the planetary positions of Saturn as they relate to the dating of Kandalanu/Assurbanipal. We have found the very same thing to be the case for dating Nabopolassar as the data relates to the vernal equinox. And finally, we have shown that the data related to the astronomical diary for 652 B.C. is not at all fulfilled in that the historical data in it does not correlate with, or corroborate, the astronomy.

Fomenko claims all this data fits around the 15th-16th centuries A.D. The challenge Fomenko must address is Lynn E. Rose's work regarding Sothic dating of the 12th Egyptian dynasty in which the retrocalculations fit the characteristics of the El-Lahun Papyri! Fomenko, in order to uphold his chronology for moving ancient history beyond the time of the Middle Ages A.D. must have a Sothic date for the 12th Dynasty, but not only that, he must fit the lunar data in these papyri such that they accommodate the heliacal rising of Sirius. To my knowledge, no-one has found a fit for this data other than Rose. If Fomenko can find a fit in his chronology, let him produce it. That would require about 40 pieces of information regarding precise days of the lunar month spread across about 50 years of history correlating with the heliacal rising of Sirius. It is my contention that neither

Fomenko nor anyone else will be capable of finding a place in the entire spectrum of historical times where this material will fit other than the place—time—that Rose has found for them. And I look forward to Fomenko’s response to this challenge. Neugebauer shows that

“In conclusion one may say that chronology is not only the backbone for the writing of history, but that [astronomical] facts belong to the very few [scientific, rigorous] elements of history which can be established objectively.”<sup>114</sup>

As can be seen, the astronomical facts are the backbone of the short chronologies of Heinsohn, Sweeney, and Rose, and in part of Velikovsky. They are scientifically and mathematically rigorous and objective and cannot now be called upon as support for the long, established one.

## TWO CONTRADICTORY SETS OF ASTRONOMICAL DATA

After the above material had been written, Lynn Rose, in a letter dated August 10, 2007, reminded this author that there was another set of astronomical data points that appeared to be in total contradiction to the short chronology and that had to be accounted for. This material was strongly emphasized by Carl Olof Jonsson in *The Gentile Times Reconsidered: Chronology and Christ’s Return*, 3rd ed. (Atlanta GA 1998), pages 147-178. The material was first presented by Abraham J. Sachs in *Late Babylonian Astronomical and Related Texts* (1955), page 223, number 1417, which presents four lunar eclipses, each separated by a little over 18 years. The first one occurred in the reign of Sennacherib, the second and third in the reign of Shamash-shuma-ukin, and the fourth in the reign of Kandalanu.

No matter how Rose worked with these four eclipses, they would not fit in with the short chronology, and thus this astronomical data clearly appeared to be in contradiction to the short chronology. Furthermore, the dates of these four eclipses worked *for* the established chronology. Thus this counter-evidence had to

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<sup>114</sup> Otto Neugebauer, *A History of Ancient Mathematical Astronomy* (NY 2004), p. 1073

be accounted for or shown to be without real merit. In this regard, Rose also informed me of a vital difference between the evidence presented above, which clearly fits the short chronology, and these four eclipses.

The astronomical data that fit the short chronology were contemporary, primary sources; these were written to the king by astrologers or other dignitaries, either predicting an eclipse or other astronomical event, or explaining or describing it. On the other hand, the four eclipses that fit the established chronology were from secondary sources from Seleucid/Hellenistic times (no earlier than the late fourth century or the third century). They were written long after these eclipses occurred. They were not written by observers of these eclipses at all. And this is the weakness of this data. Herbert Niehr explains the difference between primary and secondary sources for doing historical, as well as archaeological, research:

“When working within the realm of ... history [or archaeoastronomy] ... it is necessary to be aware of the specific peculiarity of the sources used. A distinction between primary and secondary sources ... must be made. Primary sources or evidence consist of ‘those texts that were produced in the course of the events as they were happening’ and secondary ones (or tertiary etc.) by ‘those texts that were produced after the events in an attempt to clarify for future generations how things were thought to have happened’.”<sup>115</sup>

Most historians regard all the ancient writers as primary source evidence, but this is simply false. Jonathan M. Hall explains why this assumption by historians about these sources for the ancient world is untrue:

“The common tendency [by historians] to regard ancient authors [or other documents] as primary sources is not entirely accurate. Herodotus was not an eyewitness to the great war between Greece and Persia that constitutes the central theme of his work. The *Histories* were written around the start of the Peloponnesian War [between Athens and Sparta] in 431 [B.C.], almost fifty years after the Persian War, so it is clear that Herodotus’ account—which actually includes plenty of commentary and interpretation—is reliant on the reports

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<sup>115</sup> Herbert Niehr, “The Rise of YHWH in Judahite and Israelite Religion: Mythological Religio-Historic Aspects,” in Diana Vikander Edelman, editor, *The Triumph of Elohim: from Yahwisms to Judaisms* (Kampen, the Netherlands 1995), p. 47

[written or oral] of others, and [it is these reports that] technically count as Herodotus' primary sources. Strictly speaking, ... the *Histories* [by Herodotus] are a secondary source."<sup>116</sup>

Thus the eclipse material that upholds the established chronology is also clearly a secondary source. Hall further explains that treating secondary sources as primary "can sometimes endow ancient [secondary] sources with an aura of infallibility that they do not always deserve."<sup>117</sup>

This, however, is the crux of the matter: the primary sources written at the time of the astronomical events fully support the short chronology while the secondary sources for these four eclipses do not, but fully support the established chronology. These two sets of astronomical data give contradictory evidence. Clearly, both cannot be correct.

Hall offers three tests to determine whether or not a set of data can be shown to be valid.

"The first test is that of *temporal proximity*. Was our informant an eyewitness to the events he (very rarely she) describes, or was he at least contemporary to them ...? If not, how long after the events that are described did he write?"<sup>118</sup>

Undoubtedly the astrologers or others who predicted or reported the various astronomical events to their kings were contemporary with the astronomical events that uphold the short chronology. Undoubtedly the scribes who wrote about these four eclipses were not contemporary with the astronomical events that uphold the established chronology.

Hall's second test "is that of *contextual fit*. How well does the source fit ... the ... context ...?"<sup>119</sup> In this instance, both sets of data astronomically fit the events they describe. But as we understand, since each data set fits two totally contradictory chronologies, one must be false.

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<sup>116</sup> Jonathan M. Hall, *A History of the Archaic Greek World* (Malden MA 2007), p. 18

<sup>117</sup> *ibid.*

<sup>118</sup> *ibid.*, p. 19

<sup>119</sup> *ibid.*, p. 20

Hall's third test "is that of *intentionality*. What is it that our source deliberately wants to communicate and what prior knowledge or presuppositions are casually assumed?"<sup>120</sup>

Again, this is the crux of the matter. The astrologers or others who wrote reports for their kings had the intention of being as accurate as possible. A false prediction or erroneous report could result in losing one's life. Therefore, the primary sources for the short chronology were going to be as accurate as possible. Their intention was founded on their not losing their lives. The scribes who were the secondary sources for the four eclipses had no such fear. In fact, their rulers clearly wished to be seen as the kings of a very long and illustrious history. Their intention, as we understand from Berossus and Manethon and even Josephus, was to show posterity how ancient the world they ruled over was. Therefore, there was cause and intention to make history longer than it was. If this meant moving various kings back in time that, too, was acceptable. Surely, as we will see shortly, the evidence of Nabonidus indicates just this.

Clearly, the best, most accurate evidence is that of the primary sources, written contemporaneously with the astronomical events and accurately describing these events, thus giving a good contextual fit with the astronomical realities. But, above all, they were written with the intention of being as accurate as possible. Equally clearly, the least secure evidence is that of the secondary sources, written long after these astronomical events took place. These events could have been accurately described in reports that were for other rulers of the past and were then given to a more recent king to make history longer and thus more venerable.

The question is: Why is it that the very best forms of primary source/contemporary data that agree with the astronomical realities should obviously be in stark contradiction to the established chronology, but obviously be in full agreement with the short chronology, while the non-contemporary secondary sources or materials written much later, from many decades to hundreds of years after the astronomical events, constitute the evidence for the established chronology?

That would be comparable to having a person accused of a crime filmed on a dated videotape at a meeting with dozens of witnesses to attest to this fact, but accused of having been at the scene of a crime somewhere else at the same time.

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<sup>120</sup> *ibid.*, p. 21

Yet those people who would accuse this person were neither at the meeting nor at the crime scene. Saying he must have been at the scene of the crime because there is other evidence that places him there does not prove the case against him. He could not have been in both places at the same time. The primary source evidence—the video tape—contemporary with that man being at the meeting negates the secondary source evidence. In terms of the present discussion, that would be comparable to having dated video tape evidence of the various astronomical phenomena that upholds the short chronology, contemporary primary source evidence, but then turning to astronomical reports written several decades to hundreds of years after these events to say these other lunar eclipses prove that the primary source, video-taped evidence, is invalid.

No! One cannot invalidate or override the very best primary sources of astronomical evidence with later secondary source materials. To the contrary, the primary source astronomical evidence invalidates and overrides the later secondary source of evidence. That best primary source evidence places the Neo-Assyrians in Persian times. What the proponents of the established chronology must explain is: how the primary source contemporary documents are inaccurate while the much later secondary sources should be trusted in the face of these primary accurate sources. Those who uphold the established chronology have it all upside down and backward. They are forced to argue that the less reliable secondary source evidence takes precedence over the primary sources. But, alas, that is what I believe the proponents of the established chronology will be forced to do, namely reject the primary contemporary sources for the secondary later ones. How they do this with a straight face should be interesting. What they must argue is that the secondary and *later* sources are primary and must be treated as primary.

## ASTRONOMICAL CONCLUSIONS

Altogether Jonsson has presented several astronomical data sets or points that support the established chronology, but nearly all do not stand up to the astronomical facts:

1. The eclipse of Bur-Sagale dated to 763 B.C. was partial and, at that time, would hardly be predicted, let alone looked for. The Bur-Sagale total eclipse for 763 B.C. occurred in Turkey in an area around Lake Van that the Neo-Assyrians may not have controlled. But the eclipse of 435 B.C. was total over both Assur and Nineveh and this clearly was seen by everyone. Because the 763 B.C. event has no dated or observed solar eclipses prior to and after it, this indicates it was a total eclipse, but it simply wasn't.

2. The eclipses dated to the reign of Esarhaddon, whether lunar or solar, never occurred. In a certain sense, Esarhaddon, who precedes the other monarchs, dated astronomically by the historians, must be moved to a place where these eclipses fit his reign. But once we move Esarhaddon either forward or back in time, we must also move all the rest of the Neo-Assyrian kings who came before and after him. When we do this none of the astronomical data of these rulers will fit their reigns in the established chronology.

Rose has found several places in Persian times which will fit the eclipse data. He informed me that the Esarhaddon planetary data was too vague to be useful for chronology. He is seeking other forms of astronomical data that are not vague or ambiguous to finally nail down Esarhaddon's place in the Persian era.

3. The Saturn data relating to Kandalanu seems to fit this reign in the established chronology, but not very well. When we move his reign to the place that Heinsohn suggests, the Saturn data fits his reign far better. Amazingly, in terms of Heinsohn's thesis, Kandalanu who reigned with, or is, Assurbanipal in Babylon, the *alter ego* of Darius III vanquished by Alexander the Great, dies (or his year-count ceases) in the very year and month that Alexander enters Babylon. This astonishing result is reminiscent of Rose's Sothic evidence, presented in volume I of this series, wherein Rose moves the 12th Egyptian Dynasty into the first millennium B.C. and it ends when Alexander the Great conquers Egypt. Correlations of this exactness are not and cannot be related to chance.

4. The Astronomical Diary for 652 B.C., which dates the Neo-Assyrian king Shamash-shuma-ukin via a battle at Hiritu, is not corroborated by the Akitu Chronicle. The battle is not fought in the north in the province of Sippar, but in the south. The Akitu Chronicle B edition is explicit with regard to the battle. It says the battle was fought between Assyrians and Elamites, while the diary claims the foes



were Assyrians and Babylonians. The chronicle is explicit in that it gives the names of various Elamite officials who took part in the battle.

Therefore the placement of Shamash-shuma-ukin cannot be fixed to fit the established chronology, nor can it be employed to discredit Heinsohn or Sweeney's thesis that the Neo-Assyrians were the Persian rulers of Babylonia; the correlation and the corroboration between the 652 B.C. Astronomical Diary and the Akitu Chronicle has major incongruities and cannot be used for a good fit in the established chronology.

5. The astronomical data employed to fix the reign of Nabopolassar to the conventional chronology suffers from three major problems which do not exist in the short chronology. First, the number of 29- and 30-day months necessary to fit Nabopolassar into the orthodox chronology are fewer than would be called for. Second, between the two workable sequences for dating Nabopolassar—either in the conventional or short chronology—the vernal equinox is expected to fall in the month of Nisanu. But in the conventional chronology the vernal equinox does not fall in the month of Nisanu 31 percent of the time while in the short chronology it falls in Nisanu except in one case—where it misses by less than one day. Third, it is not possible to place Nabopolassar's reign in the orthodox chronology and during his 19th year of reign have the 9th, 10th, and 11th month each 29 days long. In the short chronology these month-lengths work, having the 8th month of Nabopolassar 29 days long, the 9th month 30 days long, and the 10th and 11th months 29 days in length, with bad seeing at the end of the 8th month.

Rose's placement of Nabopolassar's reign from -341 to -320 has far fewer problems than the orthodox placement for this monarch. In other respects his attested month-lengths are just as respectable as those of the historians and archaeoastronomers.

6. Nabonidus's lunar eclipse conventionally dated to September 26, 554 B.C.—in the ancient calendar to the 13th of the month Ululu—in no way can be considered a proper astronomical fit. The Moon, according to the document, was completely eclipsed above the horizon and stayed totally eclipsed when it set below the horizon. The fact of the matter is that Nabonidus's reign in the established chronology does not have the Moon fully eclipsed before it set. The Moon is only covered by about one or two percent at the point where it touches the horizon as it begins to set and is fully illuminated by the time it actually sinks below the horizon.

In the short chronology this eclipse occurs not in 554 B.C. but on October 2, 294 B.C., and the Moon becomes completely eclipsed while still above the horizon and remains totally eclipsed when it sets below the horizon. There is no other eclipse answering to this description within several decades forward or back in time.

The time period between the reigns of Nabopolassar and Nabonidus is 25 years. This allows for Nebuchadnezzar's 21-year reign and the short reigns of Awel-Marduk, 2 years or less; Neriglissar, 4 years or less; and Labash-Marduk, 2-3 months, to fit as close to perfect as one could wish.

When we add to all this astronomical data Rose's 29- and 30-day month dates that fit Hammurabi and Darius I with those that fit Ammisaduqa and Artaxerxes III Ochus, for the Old Babylonians and Persians, along with his Sothic down-dating of the 12th Egyptian Dynasty to the first millennium B.C., it becomes clear that the depth, scope, and power of astronomical evidence is immense.

Lynn Rose, however, told me there may be some problems that we have not answered (personal telephone discussion February 23, 2007), such as the names of the parents of some kings which may pose contradictions, for example the case of Nebuchadnezzar II whose father supposedly was Nabopolassar.

Further, Nebuchadnezzar II was famous for fighting cruel, bloody wars. But if he was placed in the short chronology and reigned in Babylon under the Macedonians or their followers, the Seleucids, none of this would occur and further, there would be little written by him of his great wars. In these regards, we are told:

“On receiving the news of his father's death [in 605 B.C.], Nebuchadrezzar [II] returned ... to Babylon. In his numerous inscriptions he tells but rarely of his many wars. The Babylonian chronicle is extant only for the years 605–594 [B.C.], and not much is known from other sources about the later years of this famous king.”<sup>121</sup>

Why wouldn't this supposedly mighty king boast of his exploits as did the rest of the kings of Mesopotamia? Why do we only have information of his reign in the Babylonian Chronicle for about 9 years, 605-594 B.C.? Again, even the

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<sup>121</sup> Wolfram Th. von Soden, “Nebuchadrezzar ... II”, *Encyclopaedia Britannica Macropedia*, vol. 21 (Chicago 1990), p. 930

documentary evidence seems to correlate with, and corroborate, the short chronology.

Before critics of this archaeoastronomical evidence raise questions regarding what has been presented, I suggest that it behooves them to discredit Rose's astronomical evidence in full. There may still be astronomical problems ahead for those of us who are in general proponents of Heinsohn's and Sweeney's short chronologies, but there are clearly and undeniably major astronomical problems confronting the orthodox chronology. That chronology is not built on cogwheel astronomical gear teeth that mesh. Their chronology, at this point, is a mechanism where the cogwheel gear teeth not only don't mesh with one another but rip one another to pieces and the whole contraption doesn't operate at all. Rose has further written to me on March 16, 2007:

“Hermann Hunger's *Astrological Reports to Assyrian Kings*, Helsinki 1992, Report 104, pages 63-64, describes an eclipse of the Sun on the 28<sup>th</sup> of Nisan at 2½ double-hours of the day. The signature is missing, but Hunger assigns it to the scribe Akkullanu, without saying why (the handwriting, perhaps?). The Report has no date on it. 2½ double-hours of the day would be about 11:00 a.m. — or a little earlier than that, assuming that we are after the vernal equinox. Lines 2-4 are only partially readable, but there are fragmentary references to the west, to covering, and to 2 fingers. (Normally, 100% coverage would be counted as 12 fingers.) A late morning eclipse would not be in the west, but all solar eclipses do feature the Moon coming from the west, and thus any covering of the Sun (to whatever degree) is also more or less from the west, and that is probably all that is reflected here. The reference to fingers suggests that the eclipse was partial. At this point, I do not know whether it means that 2 fingers of the Sun were covered or that 2 fingers of the Sun were left uncovered. The latter *might* be what is meant here, but I would have to find some comparable texts to have any confidence about that.

“Hunger assigns this Report to April 15, -656. Lange and Swerdlow have a first visibility on March 18, as does *Starry Night*, so the late morning of April 15 would indeed be on the 28<sup>th</sup> day of the lunar month, and that month presumably could have been Nisan. (We do not know exactly how the Neo-Assyrians were intercalating here or

just when they were trying to start the year.) Lange and Swerdlow have the eclipse at 57% at Babylon at 8:34 UT, at 67% at Ashur at 8:37 UT, and at 69% at Nineveh at 8:40 UT. That is a *lot* more than 2 fingers covered. And there would be more than 3 fingers left uncovered. Depending upon the site, there could even be as much as 4 or 5 fingers left uncovered! (UT is about 3 hours behind Babylon; [roughly speaking,] at 8:00 UT in England, it is about 11:00 a.m. local time in Babylon. I should also mention that the 2½ double-hours of the day might have referred to the *beginning* of the eclipse rather than to the maximum, in which case those retrocalculated times of the maximum might not be running late at all.)

“If we hope to replace the entrenched chronology, this is the kind of result that we must duplicate in the fourth century. We may not have to answer *all* such results, but we should be prepared to deal with *most* of them, if we are to be taken seriously.

“I searched from -410 to -330. Nothing worked at all, except May 2, -378. Lange and Swerdlow have a first visibility on April 4, -378, as does *Starry Night*, so the late morning of May 2 would indeed be on the 28th day of the lunar month, and that month presumably could have been Nisan. (Some might say that that is a bit late for Nisan, but it must be remembered that we do not know exactly how they were intercalating here or just when they were trying to start the year.) Lange and Swerdlow have the eclipse at 89% at Babylon at 8:30 UT, at 80% at Ashur at 8:33 UT, and at 78% at Nineveh at 8:36 UT. That looks to me like all but 2 fingers and some part of a third were indeed covered, at least at Ashur and at Nineveh, but I do not yet know whether the Neo-Assyrians ever spoke of the matter in that way. It would help if we could find a clear use of the “all but” locution somewhere else. In any case, we can hold our own against Hunger’s date.

“Even though it is very encouraging, I do not regard this particular finding as anything that will in itself change any minds. It is merely one of a great *many* things that we will be expected to do if we are to set aside the accomplishments of the entrenched chronology.”

One of the ironic aspects of Rose's archaeoastronomical work, which lowers the chronology of Egypt by 1477 years or more and also lowers the First Babylonian Dynasty by about that same amount, is that Isaac Newton in his *Chronology of the Ancient Kingdoms*, published after he died, came to quite similar conclusions regarding chronology based largely on astronomical grounds. According to Ayval Leshem,

“Newton devised a chronology of [the ancient] kingdoms relying on astronomical data from the ancient world and comparisons between different ancient historical narratives. [Frank] Manuel defined Newton's approach as one of adapting data already known from astronomy (such as precession) to the domain of historical chronology. The aim of Newton's project was the establishment of a relationship between observed movements of the earth with respect to the fixed stars and ancient political events, so that the past might be ‘predicted’ backward, so to speak.”<sup>122</sup>

Anatoly Fomenko explains how Newton worked and how the historians responded to his opus:

*“Newton made a radical revision of the ancient chronology based on natural scientific ideas ...*

*“The new chronology offered by Sir Isaac is a lot shorter than the consensual [or established] chronology ... Newton moved most of the events dated as preceding the epoch of Alexander the Great forward in time, closer to us ...*

*“Contemporary historians have this to say about the works of Newton's: ‘They are the fruit of forty years of labour, diligent research and a tremendous erudition. Basically, Sir Isaac Newton studied all of the major literary works on ancient history and all the primary sources beginning with ancient and oriental mythology.’*

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<sup>122</sup> Ayval Leshem, *Newton on Mathematics and Spiritual Purity* (Cambridge UK 2003), p. 8

“Modern commentary invariably comes to the conclusion that Sir Isaac was wrong when they compare his conclusions to the consensual ... chronology. They say that:

“Naturally without deciphered cuneiform and hieroglyphic writing, having no archaeological data due to the *non existence of archaeology in that age*, bound by the presumed veracity of the Biblical chronology and the belief in the reality of what was told in the myths, Newton’s errors weren’t measured in mere tens or hundreds of years—he was thousands of years off the mark, and his chronology is far from true even in what concerns the very reality of the events described. W. Winston wrote in his memoirs: ‘Sir Isaac often saw the truth in mathematics intuitively, without even needing proof ... But this very Sir Isaac Newton compiled a chronology ... However this chronology isn’t any more convincing than the most ingenious historical novel, as I have finally proved in my refutation thereof. O how weak, how utterly weak even the greatest of mortals can be in some regards.’

“What did Sir Isaac suggest exactly? Basically he had analyzed *the B.C. chronology* of Ancient Egypt and Ancient Greece ...

“For instance, the contemporary consensual version of chronology ascribes the first years of the reign of the [first] Egyptian Pharaoh Menes to approximately 3000 B.C. ... Newton suggested this event could be given a date as recent as 946 B.C. ... Thus the shift forward in time comprises about 2000 years ...

“The famous Trojan War is dated to roughly 1225 B.C. today ... but Newton claims this event to have occurred in 904 B.C. The shift forward [in time] here is one of approximately 330 years. Et cetera ...

“Newton also moves some fundamental dates of ‘ancient’ Egyptian history about 1800 years forward in time ...

“The mid XVIII century [1700’s] press saw a multitude of responses [to Newton’s short chronology]. Most of them were made by historians and philologists, and had voiced such negative opinions as ‘the blunders of the honoured dilettante’ in regard to Newton’s work.

Only very few articles appeared that expressed support of his opinion. After the initial responses subsided, the book was de facto hushed up and withdrawn from scientific circulation.

“In the XIX century [1800’s], François Arago ... presumed Newton’s chronological research unworthy of more than the following flippant remark: ‘By and large, Newton failed to come up with correct judgments in anything except mathematics and its applications ... Apart from his theological opuses, the chronology [of the ancient world] that he compiled is there to confirm our opinion—the very chronology that [Nicolas] Fréret refuted immediately upon publication.’ Most probably Arago decided not to get involved in the issue and [simply] quoted Fréret’s opinion without thinking twice about it.

“Cesare Lombroso tries to bring the issue to conclusion in his notorious *Genius and Insanity* in the following manner: ‘Newton whose mind amazed the entire humanity, as his contemporaries rightly state, was yet another one to have gone senile in his old age, although his symptoms ... weren’t ... grave ... That must have been the time when he had written his *Chronology* ...’ ”<sup>123</sup> [emphasis in the original]

Nicolas Fréret’s criticisms of Newton’s chronology came down to two, namely that Sir Isaac had gotten the position of the stars in the vernal equinox in the wrong position and that his counting of the generations in the Bible was wrong by about a third. David Brewster outlines Newton’s response to Fréret:

“To all the observations of M. Fréret Sir Isaac returned a triumphant answer. This celebrated writer [Fréret] had ventured to assert, ‘that he believed he had stated enough concerning the epoch of the Argonauts, and the length of generations to make people cautious about the rest; for these are the two foundations of all the new system of [Newton’s] chronology.’ He [Fréret] founds his arguments against the epochs of the Argonauts as fixed by our author, on the supposition that Sir Isaac places the vernal equinox at the time of the Argonautic expedition in the *middle of the sign of Aries* whereas Sir Isaac places it

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<sup>123</sup> Fomenko, *History, Fiction or Science?*, 2nd ed. (Bend OR 2003, 2006), pp. 11-13

*in the middle of the constellation*—a point corresponding with the middle of the back of Aries or  $8^{\circ}$  from the first star of Aries. The position of the colure [a great circle in the sky that passes through the celestial poles and the equinoxes, and which enables one to analyze the position of a celestial body] is assigned on the authority of Eudoxus, as given by Hipparchus, who says that the colure passed over the back of Aries. Setting out with this mistake, M. Fréret concludes that the Argonautic expedition took place 532 years earlier than Sir Isaac made it. His [Fréret's] second objection to the new [Newtonian chronological] system relates to the length of generations, which he says is only eighteen or twenty years. Sir Isaac, on the contrary, reckons a generation at thirty-three years or three generations at 100 [years]; and it was [really] the length of the reigns of kings he made eighteen or twenty years. This deduction he found in the reigns of sixty-four French kings. Now the ancient Greeks and Egyptians [contrary to Fréret] reckoned the length of a reign equal to that of a generation; and it was by correcting this mistake and adopting a measure [of time] founded on fact, that Sir Isaac placed the Argonautic expedition forty-four years after the death of Solomon and fixed some of the other points of his [chronological] system."<sup>124</sup>

Fréret apparently read Newton's work carelessly and published his largely error-ridden critique, which the historians eagerly accepted, even though Newton answered it in the "revised" edition published after his death. As with Velikovsky, the historians banded together to misrepresent, stifle, and then ignore Newton's short chronology. Later critics argued that the astronomy in the ancient records was simply not suitable to build a revision of chronology upon. Brewster writes:

“[M. Foumerd was] to show the uncertainty of the astronomical argument, arising on the one hand from the vague account of the ancient sphere as given by Hipparchus, and, on the other, from the extreme rudeness of ancient astronomical observations. Delambre has taken a similar view of the subject. He regards the observations of ancient astronomers as too incorrect to form the basis of a system of

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<sup>124</sup> David Brewster, *Memoirs of the Life, Writings and Discoveries of Sir Isaac Newton* (London 1855), pp. 305-306



chronology; and he maintains that, if we admit the accuracy of the details in the sphere of Eudoxus and suppose them all to belong to the same epoch, all the stars which it contains ought at that epoch to be found in the place where they are marked, and we might thence verify the accuracy [of these positions]; and ascertain the state of the observation ...”<sup>125</sup>

The fact of the matter is that if Newton was correct, then the Establishment historians were all wrong and that was what drove the criticism of his chronology. As Scott Mandelbrote shows:

“[Arthur Bedford’s] initial criticisms were directed at the astronomical methods of dating that Newton’s chronology had deployed. He pointed out that Newton’s findings disagree with those of the most prominent orthodox writers on chronology—James Ussher, William Lloyd, Richard Cumberland and William Beveridge—all of whom were in agreement about the major dates in secular and sacred history ...

“Bedford was perceptive in noticing that Newton’s conclusions about sacred [or Biblical] history created doubts over the authority and antiquity of scripture ...”<sup>126</sup>

This, in large measure, is how Lynn E. Rose’s down-dating of ancient chronology was criticized as well as that of Velikovsky. Yet if Newton’s short chronology had not been misrepresented and then suppressed, historians in the 1800’s could have held to a view that the chronology of the ancient world was far shorter and could have placed the various civilizations, as Heinsohn, Rose, Sweeney, and Velikovsky did, into this far shorter framework. Specifically, instead of using the astronomical canons and chronicles with such data in many of their present, clearly incorrect places in the chronology, the archaeoastronomers would have attempted to place them closer to the present, as Rose has done. They might then have reached the conclusions that Rose presented above regarding Mesopotamian chronology and the proper placement of the 12th Egyptian Dynasty based on the

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<sup>125</sup> *Ibid.*, pp. 309-310

<sup>126</sup> Scott Mandelbrote, “Newton and Eighteenth Century Christianity,” *The Cambridge Companion to Newton*, Bernard Cohen, George Edwin Smith, eds., (Cambridge UK 2002), pp. 411-412

data in the El Lahun Papyri. Because the historians failed to heed even the possibility that Isaac Newton may have been onto a correct solution of the chronology of the ancient kingdoms, they missed a golden opportunity to discover a much more accurate chronology of these civilizations.

At this point I must note that I am extremely grateful for the work of Carl Olof Jonsson because it is highly readable and well researched. Thus I have a high regard for him even though, on the evidence that I have presented above, I strongly disagree with his thesis regarding the conventional chronology being fully supported by astronomical data.

Lastly, I wish to return for a moment to the work of Claudius Ptolemy, which Robert R. Newton castigated and concerning which one of the reviewers of Newton's book, *The Crime of Claudius Ptolemy*, noted above, suggested that "Ptolemy's forgery may have extended to inventing the lengths of reigns of Babylonian kings." This accusation, based on the evidence in this unit, is thoroughly justified. The most generous explanation one may give for this behavior on Ptolemy's part is that he was really not interested in chronology but rather wanted the eclipses of the past that he wrote about to fit his epicycles and all the rest of the paraphernalia of his geocentric system. By simple retrocalculation he placed these eclipses where they fit his system and then placed a king he might have known about in that time frame to give support to his great opus. In so doing he had a complete explanation of the physical, astronomical universe, corroborated by the eclipses, which he evidently invented, of the historical universe. His work is thus a double tragedy for knowledge. Most historians and archaeoastronomers believe what he did was "smooth" the evidence, as has been said. But this smoothing has led both astronomy and historical chronology down the wrong paths.

Having read this unwelcome evidence, I feel sure the proponents of the established chronology and supporters of Ptolemy will no doubt be as unmoved as Ptolemy's unmoving Earth.

## FINAL NOTE

As pointed out above, the 3.1 astronomical retrocalculation program clearly indicates that a 763 B.C. eclipse did occur at Nineveh, and thus confirms this aspect of the established chronology as well as the 135 B.C. total eclipse at Babylon not related to the short chronology. However, it does not change much else for the short chronology.

1. It does not change the Sothic dating in any way whatsoever for Lynn E. Rose's dating the 12th Egyptian Dynasty by 1477 years closer to the present.

2. It does not change the astronomical facts that the 20-day months of the Old Babylonian kings Hammurabi and Ammisaduqa correlate with the 30-day months of the Persian kings Darius the Great and Artaxerxes III Ochus, placing these Old Babylonian kings in Persian times.

3. It does not in any way support the lunar or solar eclipses for placing Esarhaddon where the established chronology requires him to be placed nor does it do the same for the Venus, Mars, and Jupiter data for this same king.

4. It does not change the fact that Rose's evidence for the data regarding Saturn is far superior for placing Kandalanu/Assurbanipal roughly in the time of the Persian king Darius III.

5. It does support the astronomical diary for 652 B.C., even though no kings are thereby dated.

6. It still supports the chronology of Nabopolassar that Rose found fits a short chronology.

7. It also supports the lunar eclipse data for Nabopolassar that Rose found also fits a short chronology.

8. It does appear to contradict the 435 B.C. solar eclipse for Ashurbanipal III. Still, it must be pointed out that that eclipse covered about 97 percent of the Sun; but this 435 B.C. date is far closer to the times when solar eclipses were understood. The Persians/Neo-Assyrians of *this era* could have known or suspected a solar

eclipse would have taken place and thus would have looked for it, noting it as the one for Ashurdan III. So the short chronology is still probable with respect to this eclipse.

How, finally, I ask, can the established chronology of the Neo-Assyrians be correct based on only one eclipse from a primary source but contradicted by so many other primary source materials for lunar eclipses, as well as planetary and calendrical forms of data? Though all this, in time, may be clarified with a new program for working out these astronomical materials and data, the short chronologies of Heinsohn, Sweeney, and Rose are still in excellent shape while the established chronology still is terribly wanting in terms of the astronomical support so absolutely essential for its survival.

## FURTHER ASTRONOMICAL EVIDENCE

Lynn E. Rose has been looking into the astronomical eclipse material presented by Hermann Hunger. The following letter was sent to this author on March 19, 2007, in which Rose deals with one such text:

“Hermann Hunger’s *Astrological Reports to Assyrian Kings*, Helsinki, 1992, Report 207, pages 116-117, describes some ‘Observations of the Moon’ over a period of some 17 days. There is no signature, and there is no date. Hunger warns us in a footnote: ‘Restorations conjectural.’ Nonetheless, despite the various difficulties and obscurities, there are some important pieces of information here that may be useful for dating purposes. I quote lines 2 and 3 from the obverse, and then lines 2 through 8 from the reverse.

<sup>2</sup> [On the 29<sup>th</sup> day], a *possible* solar eclipse.

<sup>3</sup> [In the month ..., on the 1<sup>st</sup> day], the moon became visible.

<sup>2</sup> The night of the 13<sup>th</sup> day [*was cloudy*,  
in] the morning [watch] the moon came out.

<sup>4</sup> [The daytime of the 1]3<sup>th</sup> day was cloudy, (the moon) was not visible;

[... *an eclipse* of] the moon passed by.

<sup>6</sup> The night of the 14<sup>th</sup> day [*was cloudy*,  
in the mor]ning [watch] the moon came out; the disk did not  
wane.

<sup>8</sup> [... On the 1]5<sup>th</sup> day the setting of [the moon] was not visible;  
*solstice*.

“The text itself skips days 2 through 10. I have not quoted days 11 and 12, which are described in lines 4 through 8 of the obverse and in line 1 of the reverse. There is little of interest there, except that, as Hunger says: ‘it is remarkable that on four successive days there were clouds which let the moon come out in the morning watch.’ (The morning watch was the last third of the night.) Hunger tells us on page XXIV of his Introduction: ‘Broken portions of the text and all restorations are enclosed within square brackets. Parentheses enclose items omitted by ancient scribes. ... Uncertain or conjectural translations are indicated by italics.’ Almost all of Hunger’s restorations are obvious, or at least plausible. Later, however, I shall reject his ‘29<sup>th</sup>’.

“The expression ‘passed by’ was generally used with regard to events that were believed to have happened, but were not seen. Often this involved lunar eclipses occurring in the daylight hours, and therefore below the horizon.

“The most important information in Report 207 is that such a lunar eclipse ‘passed by’ two days before a solstice. I searched for such a situation between -425 and -325, looking for either summer or winter solstices. The only good fits that I found were in the year -363, when there was a lunar eclipse early in the morning of December 23 and a winter solstice late in the afternoon of December 25, and in the year -344, when there was a lunar eclipse early in the morning of December 23 and a winter solstice in the early morning of December 25.

“A third possible fit that I explored in some detail did not work out. That was -382, when there was a lunar eclipse early in the morning of December 23 and a pre-dawn winter solstice on December 26. This was only a partial eclipse; far more importantly, the winter solstice was

on day 16, which is *three* days rather than two days after the eclipse. Goldstine has '23DE 7;50'. According to van den Bergh, there was no eclipse on December 9. According to *Starry Night* for Babylon, the winter solstice was about 5:54:57 a.m.

“Let us look at the two good fits in chronological order.

“The -363 Fit

“According to *Starry Night*, the lunar eclipse of December 23, -363, actually began *before* Sunrise, but did not reach the midpoint or ‘maximum’ until *after* Sunrise. (The Moon first contacted the penumbra at about 5:19 a.m., and first contacted the umbra at about 6:15 a.m.) With cloudy weather, the Neo-Assyrians may not have been able to tell precisely when the eclipse would have occurred, anyway; in other words, they may not even have known exactly how *much* of the phenomenon occurred after Sunrise. According to *Starry Night* (for Babylon), Moonset was at 7:06:31 and Sunrise was 7:05:50; the ‘maximum’ was at about 7:55. (Goldstine has '23DE 7;49' for the Full Moon.) At Ashur, Moonset was at 7:19:39 and Sunrise was at 7:18:19; the ‘maximum’ was at about 7:52. At Nineveh, Moonset was at 7:21:24 and Sunrise was at 7:19:59; the ‘maximum’ was at about 7:51.

“The report that ‘the disk did not wane’ on day 14 is probably honest, and is at least fairly close to the truth. *Starry Night* does not show any blatant indications of the onset of waning until some rather radical magnification is used, and the Neo-Assyrians were presumably not capable of such magnification.

“According to *Starry Night*, the winter solstice was at 4:47:21 p.m. Babylon local time on December 25, -363. Assuming for the moment that December 23 was day 13, the Neo-Assyrian scribe would have been correct in putting the winter solstice on day 15, two days after the lunar eclipse that passed by. The Sunset did not occur until 5:05:57 p.m. that afternoon. At Ashur, the winter solstice was at 4:47:20 p.m. local time, and Sunset was at 5:02:56 p.m. local time. At Nineveh, the winter solstice was at 4:47:13 p.m. local time, and Sunset was at 5:01:41 p.m. local time. This winter solstice comes very *close*

to missing day 15, but it is a *hit* notwithstanding. The Moonset that was ‘not visible’ on that day 15, due to the continuing bad weather, would have been at 9:16:12 in Babylon, at 9:29:06 in Ashur, and at 9:30:49 in Nineveh.

“One thing that we can definitely say here is that if the lunar eclipse was on December 23, -363, somewhere around dawn, and if that was day 13, and if the solstice was in the afternoon of December 25, and if that was day 15, then day 1 would have begun on December 10. But from there on it gets messy. We are now running into one of those occasional cases where the retrocalculations vary, and there is nothing very definite that we can say about the length of that previous month. According to *Starry Night*, there were New Crescents on November 10 and December 10, -363. Both according to Lange and Swerdlow and according to Schoch’s Tables, however, those New Crescents were on November 9 and December 9. The Lange and Swerdlow program is fast and convenient, but it is notoriously prone to recognizing excessive visibility. Schoch’s Tables were specifically designed for Babylon, and I have found over the course of many years that they are usually on target. Of course, there is always the possibility of bad seeing on November 9 and December 9, which could put us back on November 10 and December 10, anyway.

“If the lunar month delimited by those two New Crescents lasted a full 30 days, then the new lunar month would have begun on what was already being recognized as day 1. If the old lunar month had only 29 days, then the new lunar month would have begun on a truncated day 30 that would have been quickly changed to day 1 when the New Crescent appeared.

“If the previous month contained 30 days, the eclipse ‘possibility’ would have been on day 28. And if the previous month contained only 29 days, the eclipse ‘possibility’ would have been on day 27. I cannot for the life of me see how it could have been on Hunger’s ‘29<sup>th</sup> day’!

“In any case, we should realize that solar eclipses may occur just before lunar eclipses, or just after them, or even *both* before them *and*

after them. (Such occurrences, of course, may not all be visible from one and the same location.) Occasionally, a solar eclipse — for example, the total eclipse of August 6, -281 — will appear without any adjacent lunar eclipse. In this case, there was no solar eclipse at all in December of -363. A solar eclipse did occur on January 7, but it was not visible in that part of the world. It was only a partial eclipse, anyway, not central anywhere on Earth, and it was visible only at extreme southern latitudes, including the South Pole. The text as we have it does not extend beyond the solstice, anyway. The scribe would of course not have been wrong to mention the *possibility* of a solar eclipse late in the previous month, whether it was on day 27 or on day 28; that was an excellent time to be on watch, especially if they did not yet have the ability to calculate that a solar eclipse would definitely *not* occur at a given eclipse station. (Numerous Reports mention fruitless watches on several successive days late in the lunar month. The Neo-Assyrians seem to have known approximately when solar eclipses were *possible*, but not when they were definite.)

“The -344 Fit

“According to *Starry Night*, the lunar eclipse of December 23, -344, also began *before* Sunrise, but did not reach the midpoint or ‘maximum’ until *after* Sunrise. (The Moon first contacted the penumbra at about 5:12 a.m., and first contacted the umbra at about 6:17 a.m.) With cloudy weather, the Neo-Assyrians may not have been able to tell precisely when the eclipse would have occurred, anyway; in other words, they may not even have known exactly how *much* of the phenomenon occurred after Sunrise. According to *Starry Night* (for Babylon), Moonset was at 7:09:06 and Sunrise was 7:06:01; the ‘maximum’ was at about 7:54. (Goldstine has ‘23DE 7;46’.) At Ashur, Moonset was at 7:22:31 and Sunrise was at 7:18:29; the ‘maximum’ was at about 7:53. At Nineveh, Moonset was at 7:24:49 and Sunrise was at 7:20:09; the ‘maximum’ was at about 7:52.

“The report that ‘the disk did not wane’ on day 14 again is probably honest, and is at least fairly close to the truth. *Starry Night* still does not show any blatant indications of the onset of waning until some rather radical magnification is used.



“According to *Starry Night*, the winter solstice was at 7:28 a.m. Babylon local time on December 25, -344. Assuming for the moment that December 23 was day 13, the Neo-Assyrian scribe would have been correct in putting the winter solstice on day 15, two days after the lunar eclipse that passed by. At Ashur, the winter solstice was at 7:28 p.m. local time. At Nineveh, the winter solstice was also at 7:28 p.m. local time. The Moonset that was ‘not visible’ on this day 15, due to the continuing bad weather, would have been at 9:14:27 in Babylon, at 9:27:36 in Ashur, and at 9:29:55 in Nineveh.

“Fortunately for us, the -344 fit is free of retrocalculation problems. Both *Starry Night* and Lange and Swerdlow have the New Crescents on November 9 and December 9, -344. Schoch’s Tables give the same results, though November 9 is a close call, with less than half an hour to spare. In order for December 23 to be day 13 and in order for December 25 to be day 15, there would presumably have to have been bad seeing on October 10, November 9, and on December 9, causing a 29-30-30 sequence to be read as a 30-30-30. That is not really a problem; such things do happen. This Report 207 itself documents the considerable amount of bad seeing that the Neo-Assyrians sometimes had to deal with!

“With November 10 and December 10 as the recognized dates of the New Crescents, the ‘possible solar eclipse’ would have been on day 28 of that previous month. Hunger’s reconstructed ‘29<sup>th</sup>’ would be incorrect, though his reconstructed ‘1<sup>st</sup>’ would be correct.

“The -344 fit is much better than the -363 fit. The main reason is that the winter solstice is safely placed just past the mid-point of lunar day 15, rather than precariously close to the Sunset that brought day 15 to a close. A second factor of some weight is that this time there really was a solar eclipse in the previous month. It was partial rather than total, and it was visible only at the North Pole and over the northern half or so of North America, but the Neo-Assyrians were right that it was a good time to be on the watch for a solar eclipse. Still another consideration — not the Neo-Assyrians’s fault — is that -363 is plagued by New Crescent retrocalculation problems that -344 escapes.

Nonetheless, the -363 solution might still be correct. We have an acceptable fit and an excellent fit. Take your choice.

“And What of the Entrenched Chronology?”

“All that has been discussed above concerns our fifth/fourth century placement of the Neo-Assyrians. What is there in the eighth/seventh century? I have looked in the eighth/seventh century for a similar fit, which is what the entrenched chronology would require, but I have found *nothing*. My search ran from -725 down to -625. The one and only eclipse close enough to check in detail was December 27, -651. This was an eclipse that ‘passed by’ at about mid-day. Unfortunately, or, for us, fortunately, the winter solstice was not two days later, but was that very same afternoon! The lunar date of *both* events was *day 14*. Thus the entrenched chronology has *no* lunar eclipse that would have ‘passed by’ two days before a solstice. That must be why Hunger fails to offer *any* date for this Report, not even a ‘conjectural’ one, despite the fact that he has already dated a great *many* such Reports on the basis of their astronomical content.

“It is like the Egyptian Middle Kingdom. People are so sure where something goes that they tug and tuck until they convince themselves that they have a fit. But they do *not* have a fit. The whole society belongs somewhere else entirely. Yogi Berra comes to mind: it is ‘*déjà vu* all over again.’

“Since Report 207 fits (twice!) in the fourth century, but not at all in the eighth/seventh century, we have won another skirmish.”

Rose’s letter of March 24, 2007, is devastating to the work of Hunger and those who uphold the established chronology based on astronomical eclipse data:

“Hermann Hunger’s *Astrological Reports to Assyrian Kings*, Helsinki, 1992, Report 300, pages 161-169, describes an ‘Eclipse of the Moon in Evening Watch’. It is signed by Zakir. There is no date. I

quote lines 2 and 16 from the obverse, and then lines 5, 6, and 12 from the reverse. (Plate VII contains photographs of both sides of this tablet.)

<sup>2</sup> ... Venus until ...

<sup>16</sup> [If] there is an eclipse in Sivan (III) on the 14<sup>th</sup> day ... ,

<sup>5</sup> and the god, in his eclipse,  
[becomes dark on the] side [east above,

<sup>6</sup> and] clears [on the side west] below, the north wind (blows,  
and the eclipse) passes the evening watch [and ‘touches’ the  
middle] watch.

<sup>12</sup> [*If in its eclipse*] Jupiter stands there ...

“The evening watch was the first third of the night, and the middle watch was the middle third of the night. Remember that Hunger tells us on page XXIV of his Introduction: ‘Broken portions of the text and all restorations are enclosed within square brackets. Parentheses enclose items omitted by ancient scribes. ... Uncertain or conjectural translations are indicated by italics.’ Notice that *none* of the material that I quoted here is in italics, except in line 12. One reason for that is that there is a very similar text, Report 336, pages 192-193, that obviously describes the same eclipse and that contains some of the key language that is obliterated in Report 300. A critically important instance of this is that Report 336 (which is signed ‘[From Asar]edu’) preserves intact the key words ‘touches the middle watch’. (Unfortunately, however, Report 336 does not mention either Venus or Jupiter, which is why I have preferred to quote from Report 300.)

“Hunger blithely dates both Report 300 and Report 336 to ‘-677 May 22’. According to *Starry Night*, however, the Sunset was at 6:47:50, and the Sunrise the next morning was at 4:59:26. If they had accurately trisected the hours of darkness, the evening watch would have ended at 10:11:15.333 p.m. The ancients did have such things as water clocks, and presumably the evening watch did end somewhere in that vicinity. The eclipse had its mid-point around 7:06 p.m. (Goldstine puts the Full Moon at ‘19;12’.) From the *Starry Night* screen, it appears

that the first contact was around 5:18, the first totality was around 6:20, the last totality was around 7:51, and the last contact was around 8:42. That falls a long way short of ‘passing’ the evening watch and then ‘touching’ the middle watch. Indeed, how can Hunger seriously expect anyone to believe that this -677 eclipse actually ‘passes’ the evening watch and then ‘touches’ the middle watch? There is no way that they could talk that way about a lunar eclipse that had its last totality at 7:51 and its last contact at 8:42. I do not comprehend Hunger’s reconstruction according to which the Moon ‘[becomes dark on the] side [east above, and] clears [on the side west] below’. (Whenever the Moon passes through Earth’s shadow, it does so from west to east, and thus *any* lunar eclipse *both* darkens *and* clears more or less from the *east* side of the Moon’s disk!) But how could they have seen the Moon *becoming* dark, in *whatever* manner, if first totality was around 6:20, and if Moonrise was not until 6:43:11? If the Moon rose totally eclipsed, why did they not say *that*, instead of talking about the pattern of darkening, a process that they could not have seen, anyway, because the Moon was below the horizon the entire time?

“The reference to Venus is of course obscure. Since Venus set at 7:03:59, however, the eclipse could (for barely twenty minutes) have been ‘witnessed’ by Venus until Venus itself set. Jupiter remained in the western sky all evening, until Jupiter itself set at 11:35:04.

“*Planet’s Visibility* puts the last totality at about 7:47 p.m., and the last contact at about 8:46 p.m., which is once again long before the start of the middle watch.

“Let us now look in the fourth century. On June 6, -380, according to *Starry Night*, the Sunset was at 6:59:45, and the Sunrise the next morning was at 4:50:10. If they had accurately trisected the hours of darkness, the evening watch would have ended at 10:16:6.667. The eclipse had its mid-point around 9:43 p.m. (Goldstine puts the Full Moon at ‘21:45’.) From the *Starry Night* screen, it appears that the first contact was around 7:51, the first totality was around 8:46, the last totality was around 10:35, and the last contact was around 11:36. I am not certain whether they were focused on first and last *contact*, or on

first and last *totality*. Either way, the eclipse did indeed ‘pass’ the evening watch and then ‘touch’ the middle watch.

“This time, much *more* of the eclipse could have been ‘witnessed’ by Venus until Venus set at 10:03:18. Jupiter did not set until 11:24:58.

“*Planet’s Visibility* puts last totality at 10:37 p.m., and the last contact at about 11:40 p.m., which certainly would ‘pass’ the evening watch and then ‘touch’ the middle watch.

“At best, Hunger’s dating here is desperate, wrong, disappointing, and pathetic. At worst, it is deceptive, dishonest, and fraudulent. The -677 lunar eclipse simply does *not* fit Reports 300 and 336, but the entrenched chronology that Hunger embraces seems to *compel* him to come up with *something, anything*, here, whether it actually fits or not.

“It has become increasingly clear that Gunnar Heinsohn was correct in placing the Neo-Assyrians in the fifth and fourth centuries, rather than in the eighth and seventh centuries. This is the first time, however, that I have ever found anything in the various scholarly studies of the surviving astronomical materials that might be regarded as an entrenchment *scandal*.”

In these two letters, Rose shows that the established chronology suffers from serious astronomical eclipse problems. He has only been using eclipses that contain sufficient data to allow for an exact dating. Other eclipses which are merely labeled as an eclipse could be made to fit in many places prior to the 8th and 7th centuries B.C. or after.

Rose sent me the following letter dated April 19, 2007. In it he examined a report from Akkullanu to Esarhaddon. Aspects of this fairly precise report caused Rose to examine it to see if it fit the established dating of the reign of Esarhaddon, 680-669 B.C., or if it fit in Persian times. He discovered that the unique astronomical data concerning a close conjunction of Mars and Saturn only fit and

made sense in the Persian era and not in the conventional reign of Esarhaddon. These planets' positions fit correctly in -391 (or 392 B.C.). Here Rose explains this evidence:

“Hermann Hunger’s *Astrological Reports to Assyrian Kings*, Helsinki, 1992, Report 102, pages 61-62, deals with circumstances that Hunger describes as ‘Mars Near Saturn, Full Moon on 16<sup>th</sup> Day’. As is so frequently the case with Hunger, this Report does not even mention Full Moon, but speaks instead of the Sun and the Moon being seen together for the first time, which is not the same phenomenon at all, even though it does occur shortly after Full Moon. (The Moon *might* still *look* full, of course, but ‘Full Moon’ is now usually defined as an instant in time, when the Moon is directly opposite the Sun, its longitude differing from that of the Sun by exactly 180 degrees. To speak otherwise only invites confusion.) I quote lines 1, 5, and 7 from the obverse; lines 4-6 and 8-11 from the reverse; and line 5 from the side, which is the signature.

<sup>1</sup> Observation of the moon which [was seen] on the 16<sup>th</sup> day together [with the sun].

<sup>5</sup> If the moon [is not seen] in Adar (XII) either on the 14<sup>th</sup> or [on the 15<sup>th</sup>] day ...

<sup>7</sup> If on the 16<sup>th</sup> day the moon and the sun [are seen together ... ]  
...

<sup>4</sup> As to Mars which [*moved*] towards [Saturn],  
it will absolutely not come close [ ... ].

<sup>6</sup> A planet flared up and surrounded the sun [ ... ].

<sup>8</sup> Concerning that it cam[e near] in front of it, I have  
[*already*] sent its explanation to the king my lord [ ... ].

<sup>10</sup> Concerning the burnt offerings about which the king wr[ote to me], I opened [my hands] and prayed for the king, the crown prince, and Samas-[sumu-ukin. ... ].

<sup>5</sup> From Akkullanu.

“It is line 6 of the reverse that permits Hunger to reconstruct the missing ‘Saturn’ in line 4 of the reverse. A planet like Mars could not be seen flaring up and surrounding the Sun, nor could it be seen doing anything else in close proximity to the Sun, except possibly during a total eclipse of the Sun, but Mars *could* have been seen moving towards *Saturn* at some point. Obviously, this is one of a great many places where ‘Sun’ means ‘Saturn’. Indeed, as Report 95 puts it: ‘Saturn is the star of the sun’. When one body was regarded as being ‘the star of’ some other body, it also came to share the name of that other body, a practice that one can only deplore.

“Line 1 of the obverse and line 4 of the reverse are observations. Line 5 of the reverse is a prediction. Lines 5 and 7 of the obverse and line 6 of the reverse are the standard sorts of quotations from Assyrian scripture, notably the large body of omens and rules pertaining to astrological prognostication. We are not told why the passage being quoted in line 6 of the reverse used the colorful language of flaring up and surrounding. Perhaps it is a relic from earlier times.

“The unnamed king would be Esarhaddon, the unnamed crown prince would be Ashurbanipal, and the one partially named individual here would clearly have been Ashurbanipal’s older brother, Shamash-shum-ukin, who must have been displeased by all of the favor that was being showered upon young Ashurbanipal. Akkullanu was currying both colts. After Esarhaddon’s death, Ashurbanipal was made king of Assyria and Shamash-shum-ukin was made king of Babylon.

“Dating this Report astronomically seemed to be something of a long shot, but I wanted to try, because of the rare opportunity that this Report provides for obtaining at least a rough dating of Esarhaddon’s reign. If I can manage to find a *unique* dating for this Report somewhere in the fifth or fourth centuries, that will mean that Esarhaddon’s reign has at least been tied to a particular point in time, even if we do not yet know exactly when the reign started or ended.

“Hunger’s note on this Report 102 is ‘Date: -668 Mar. 15 (see LAS II App. J).’ *Planet’s Visibility* puts the New Crescent on ‘29.02.-668’. *Starry Night* likewise shows it on February 29, and shows

the first seeing-together of the Sun and the Moon on March 16, which would be day 16 of the lunar month.

“Goldstine puts the Full Moon on ‘15MR 2;12’, which may be a clue as to why Hunger gives that date instead of March 16. Hunger’s obsessive interest in Full Moons has been noted before.

“*Starry Night* also shows Saturn and Mars with identical right ascensions of 11h 52.933m at 7:43 a.m. on March 16. Saturn and Mars were below the horizon then, but they would have been visible in near conjunction during almost the entire night, from shortly after Sunset right until Sunrise on the morning of March 16. (They would have set together in the west not long after Sunrise.)

“In those hours before dawn, Mars and Saturn were already virtually as close as they would get. With good seeing, the pass would have been taking place right before the viewers’ eyes, and they would have seen virtually all that needed to be seen. The difference between 7:43 a.m. (when Mars and Saturn were below the horizon and could not be seen) and, say, 4:43 a.m. (when they were still above the horizon) would have been imperceptible. For all practical purposes, they *saw* Mars passing Saturn, and they saw that Mars and Saturn were *not* coalescing into one point of light.

“Why then would Akkullanu make a big deal out of this? Why would he use such strong language when a routine and mater-of-fact account of what had happened would do? It seems unlikely that he was involved in any debate with his rivals here. Who would have taken seriously someone who tried to gain credit for making a bombastic and competitive prediction about something that had most likely been observed all night by everyone, anyway (seeing permitting)? That would be like predicting the outcome of a football game that has already been played! The closest approach of Mars to Saturn that would have been visible in those longitudes had presumably already been witnessed, just before dawn; when they were next seen, the following night, the separation of Saturn and Mars would have become about fifty percent greater. For all practical purposes, then, Mars had already *missed* Saturn. That would never have been an issue.



“One thing that bothers me here is the very boldness of Akkullanu’s claim in line 5 of the reverse that ‘it will absolutely not come close’. The word ‘absolute’ is not in the text, but Akkullanu in effect expresses the idea of ‘not’ (*la*) *twice*, for emphasis. (Line 5 begins ‘*la-ás-sú la*’.) In formal English, double negatives are regarded as canceling each other out, but in Akkadian and various other ancient languages they pile up and serve to express emphasis. I do not criticize Hunger’s decision to use the word ‘absolutely’ here, but I have spent far too much time in Departments of Philosophy to be comfortable with that word; I would much prefer to quote Akkullanu’s remark as ‘it will emphatically not come close’ or something of the kind. Nonetheless, I shall follow Hunger’s translation. After all, even Akkullanu himself might have preferred Hunger’s ‘absolutely’!

“For the moment, what is important about Akkullanu’s prediction is not whether it was correct or not, but that it was a prediction, a prediction whose accuracy had not yet been verified at the time of the Report. Hunger, of course, does seem to have Akkullanu making a *prediction* about something that has already *happened*.

“In any case, it seems to me that Mars *does* come fairly close using Hunger’s dating. With those identical right ascensions of 11h 52.933m at 7:43 a.m. on March 16, the declination of Saturn was 3° 53.729, and the declination of Mars was 4° 12.203. The difference in declination was only 18.474 minutes of arc. That is less than one-third of a degree. Nonetheless, it is more than half of the angular diameter of the Moon or of the Sun, and that much separation between Mars and Saturn would be easily discernible. Maybe Akkullanu merely wanted to stress that Saturn and Mars would not even come close to appearing to *coincide* with each other. Or maybe Akkullanu was wrong. Or maybe Hunger was barking up the wrong century. All of these questions will have to be addressed as we proceed. In the meantime, Akkullanu’s emphatic prediction makes no sense to me.

“Another thing that bothers me at this point is that line 8 of the reverse says that Mars was coming towards Saturn from in front, that is, from the west. How could this be the case, since both Saturn and Mars were retrograding, that is, moving to the west against the

background of fixed stars? Surely the fast-moving Mars would then have approached the slow-moving Saturn from the *back*, that is from the east. Notice also that Hunger's restoration of 'near' in line 8 of the reverse seems to contradict Akkullanu's pronouncement that 'it will absolutely not come close' in line 5 of the reverse, just three lines earlier. I think that something like 'from' would have been a better restoration. Mars would have been moving toward Saturn *from* 'in front of it', that is, from the west. In any case, the fact that these two planets were retrograding constitutes still another major problem with Hunger's dating.

"I have been called a nit-picker, by no less a scholar than Professor Richard A. Parker. He is of course correct; a nit-picker is exactly what I am. But not all nits are insignificant. These 'nits' that I have found in Hunger's dating of Report 102 are in fact fatal flaws.

"Let us now look at the fifth and fourth centuries.

"Upon examining Tuckerman's tables, I found that Mars sometimes has about the same longitude as Saturn, in what could conceivably be the month of Adar, and that this situation is then repeated about every two years or so. This may continue over a fairly short stretch of years, but then the conjunctions tend to move to later positions in the year. There are a lot of irregularities. Among the complicating factors here are the retrogradations of Saturn, and especially the wide-swinging retrogradations of Mars. (Actually, *any* situations that have Mars retrograding will have to be rejected, for one of the same reasons that Hunger's dating had to be rejected: the text has Mars approaching Saturn from the *west*.)

"I have not been able to determine whether it is astronomically possible for the Sun and the Moon to be seen together for the first time on day 17 of a lunar month. If it is, and if bad seeing has retarded the day count, that phenomenon would be attributed to what they saw as day 16. Unless and until this rather improbable possibility of a seeing-together on day 17 can be eliminated, however, I feel obliged to look for that phenomenon on day 16 *or higher*.

“Akkullanu does *not* say that there was bad seeing on day 14 and on day 15. As I see it, however, the tone of his remarks is such as to suggest that observations *were* conducted on those two days and that the phenomenon of the Moon and the Sun being seen together did *not* occur. Otherwise, he might have been considerably less confident about attributing that phenomenon to day 16.

“In somewhat analogous situations, ancient observers often remark that when they finally were able to see what they were looking for, the process in question was well advanced. Thus they *might* have said — although I know of no such remark pertaining to *this* phenomenon — that the Sun and the Moon were by day 16 *well* above the horizon and could *easily* be seen together, the implication being that the Sun and the Moon might well have been seen together one or two or even three days earlier if not for bad seeing.

“I take it that there was no bad seeing at the beginning of the month that might have retarded the day count. That would have caused them to count a day 15 phenomenon as day 14, and a day 16 phenomenon as day 15. Neither of those things happened here. Only an astronomical day 17 would be consistent with a retarded day count here; the day 17 would be reported as a day 16.

“I decided to make a list of the times when Mars passed Saturn in what could conceivably be Adar. Theoretically, however, the pass could have been *after* Adar. But since Mars was already moving toward Saturn on Adar 16, it seemed likely that the Martian passage of Saturn would not be very long after Adar 16. Nonetheless, since I did not know where the proper cut-off point would be, I decided to allow an additional month, after Adar, and then check each such passage for the circumstance that the Sun and the Moon were first seen together on Adar 16 (or even later). Since it is *relatively* infrequent for the Sun and the Moon to be first seen together on day 16, there is still at least *some* chance of finding a *unique* solution here. That extra month amounts to quite a *lot*. In a month, Mars can cover more than twenty degrees of arc. At that rate, in a month and one-half, Mars could *easily* cover an entire constellation. Presumably this also means that Mars could not

have been all that far away from Saturn on Adar 16. Otherwise, Mars and Saturn might even have been in different constellations on Adar 16.

“I searched through Tuckerman from -450 to -320. The first six plausible passes were in the years -449 to -439; another eleven were in the years -429 to -409; another eight were in the years -397 to -383; another four were in the years -363 to -357; and another two were in the years -331 and -329.

“All but one of these clearly had the Sun and the Moon first seen together no later than day 15. The exception was -391, which was a close call. In March of that year, it difficult to say whether the Sun and the Moon were first seen together on day 15 or on day 16. If this *is* the Adar 16 that Akkullanu was referring to, that would have been on March 14, -391, just ten days before Mars passed Saturn.

“Akkullanu does not say that Mars actually reached Saturn. He merely says that ‘Mars ... [*moved*] towards [Saturn]’ (as Hunger has it), and Akkullanu then *predicts* that ‘it will absolutely not come close’. If he had for all practical purposes *seen* Mars reach Saturn, he would presumably have reported his *observation* of that event, and he would presumably *not* have spoken in the language of prediction. (I do not know which metaphor is more appropriate, but, yes, I do keep beating a dead horse and I do keep attacking a straw man. I shall not stop until everyone acknowledges that the horse *is* dead and that the man *is* nothing but straw.)

“Let me give some further details about the -391 situation. The New Crescent of interest occurred on February 26. Thus day 16 would indeed have been March 14. In Babylon, Ashur, and Nineveh, the Sunrise on March 14 preceded the Moonset by more than half an hour, and the Moon and the Sun could easily be seen together.

“What is much less clear, however, is the status of day 15, which was March 13. At Babylon, according to *Starry Night*, Sunrise was at 6:23:38 a.m. on March 13, and then Moonset was at 6:32:02 a.m. At Ashur, Sunrise was at 6:29:44 a.m., and then Moonset was at 6:35:58 a.m. At Nineveh, Sunrise was at 6:30:11 a.m., and then Moonset was

at 6:36:00 a.m. All of these are rather close. The graphics for day 15 at all three sites do show a little piece of the Sun and a little piece of the Moon peeping above the horizon at the same time, but that is probably not enough to enable us to say that both of them were seen together. I am going to proceed on the tentative assumption that Ashur and Nineveh, at least, would not have seen the Moon and the Sun together until day 16.

“At all three cities, the right ascensions were exactly the same at 21h 17.789m at about 9:04 p.m. on March 24, and the declinations differed by just over one-quarter of a degree.

“There is nothing in Report 102 that requires Mars to have reached its nearest approach to Saturn no later than day 16. Hunger should *not* have assumed that the near approach of Mars and Saturn had already occurred when the Report was written. Akkullanu did *not* say that, and his making a *prediction* about the matter speaks very strongly against any such assumption on the part of Akkullanu himself.

“Notice Hunger’s ‘[*moved*]’ in line 4 of the reverse. The brackets connote that that is only a restoration, and the italics connote that it is uncertain or conjectural, anyway. (See page XXIV of Hunger’s Introduction.) I think that ‘has been moving’ or even ‘is moving’ might have been a better restoration. The process had *not* been completed.

“The more I think about it, the clearer it becomes that Mars passing Saturn could not have happened on the same morning as, and within only a couple of hours of, the seen-together phenomenon. The passage had to be *expected* to occur at some unspecified *later* time, *after* the Report was submitted. That is why Akkullanu is *predicting* a future event, rather than *reporting* a recent event.

“In -391, both Saturn and Mars were prograding on their orbits, not retrograding. Thus Mars would have been approaching Saturn from *in front* (as line 8 of the reverse explicitly states), that is, from the west — and *not* from behind, that is, from the east (as Hunger’s date has it).

“Once we have opened the door to having Mars pass Saturn sometime after Adar 16, why can’t it be after *Adar* itself? It does seem quite unlikely that Akkullanu would make his confident prediction a long time before the actual event, but would that justify my excluding passes that were later than Adar — passes, that is, that were in an intercalary Adar or in a Nisan? To be safe, I *should* perhaps list all of the passes in that next month as well. But I would not want to go much farther than that: Akkullanu’s prediction does seem to concern something *later* than the time of writing — just not all that far away.

“I had a big break-through on April 11, 2007. I had been wondering how Akkullanu could have been able to predict that Mars would not come as close to Saturn as other royal stargazers may have expected. (After all, his emphatic prediction does suggest that one or more of his rivals may have been toying with a contrary scenario.) Mars was definitely going to pass Saturn sooner or later. The only questions are ones like these: Will it pass above or below Saturn? How close will it come? Will the two planets appear to have coalesced into one point of light?

“The answers to all such questions will depend upon the latitudes of the two planets. The Neo-Assyrians may not have thought in terms of latitude exactly as we do, but they were dealing with the same sort of phenomena that we now handle in terms of latitude. Anyway, it was by pondering the tables of Tuckerman, especially his table for -391, that I finally discovered what Akkullanu was probably doing here.

“The mid-line through the constellations of the zodiac is of course the ecliptic, which (for us) is also the path of the Sun, by definition. It is called the ecliptic because eclipses of the Sun or the Moon occur only when the Moon is on or almost on the ecliptic. The Sun is of course already there. We would say that a planet that is north of the ecliptic has a positive latitude, and that a planet that is south of the ecliptic has a negative latitude. Akkullanu and his precursors need not have used our concept of latitude, but they would at least have known that a planet might pass through the upper half of a constellation of the zodiac, or through the lower half, or somewhere through the

middle. And they *did* keep *very* close track of the planetary movements.

“Mars has a minimum latitude when it is at that point on its orbit that is farthest below the plane of the ecliptic. About 1.88 years later, on the average, Mars will once again be at its minimum latitude. This pattern varies considerably, due to the relatively high eccentricity of the orbit of Mars, and also because of the very wide angle of the Martian retrogradation, which is of course a reflection of Mars’s nearness to Earth. The previous low latitude was on about July 20, -393. On that and all previous such occasions, for as long as Mars and Earth had been on their current orbits, Mars would have had a decreasing latitude that gradually leveled off over several months toward a minimum value and then gradually began to rise again.

“From Tuckerman’s figures, it is clear that Mars did indeed take several months to bottom out in this way. Throughout February of -391, the rate of decrease in latitude was roughly one-tenth of a degree ( $0.10^\circ$ ) every ten days. At the beginning of March, the latitude of Mars still exceeded that of Saturn by just over one-third of a degree, but it might have seemed to Akkullanu’s rivals and competitors that if Saturn continued its very slow reduction in latitude and if Mars continued its very rapid reduction in latitude, they would soon come very close and perhaps even coalesce. Through March and April, with the ten-day intervals that Tuckerman uses, Mars’s rate of reduction in latitude dropped to  $0.08^\circ$ , then  $0.07^\circ$ , then  $0.07^\circ$ , then  $0.05^\circ$ , then  $0.04^\circ$ , then  $0.02^\circ$ , then  $0.01^\circ$ , and then it reached the minimum value of  $-1.43^\circ$ , which it held for much of the first half of May. (This reduction would in fact have been smooth; Tuckerman’s three consecutive reductions of  $0.10^\circ$  and his two consecutive reductions of  $0.07^\circ$  are obviously due to rounding.) In the meantime, Saturn continued its own reduction in latitude, until it finally reached a minimum of  $-2.02^\circ$  in August, which was Saturn’s lowest latitude in that entire year of -391. (Saturn had started to retrograde in early June, and did not begin prograding again until late October.) By August, in angular terms, Mars was up and away. Saturn was still in Aquarius, but Mars was already leaving Taurus and entering Gemini. The latitude of Mars itself would already have risen to almost  $-0.4^\circ$ , and by mid-September it would be

crossing the ecliptic and acquiring a *positive* latitude. By then, Mars was in mid-Gemini, four constellations beyond Aquarius. (According to *Starry Night*, Saturn spent the entire year of -391 in Aquarius, and, according to Tuckerman, Saturn did not attain a positive latitude until -381, ten years later.)

“Observations of small differences in latitude are admittedly rather difficult. Even  $0.1^\circ$  is only about one-fifth of the diameter of the Moon or of the Sun. Akkullanu was getting fairly near the limits of naked-eye observation. But for this project Akkullanu would not have needed either the skill or the equipment of a Tycho Brahe. Stargazing for purposes of astrological prediction was a large and well-financed royal bureaucracy, practically a national industry. Whatever he needed would have been available.

“A simple and stable cross-staff or similar device (like the Egyptian merkheth?), carefully calibrated, could have been used to measure quite small angles. The background stars could also have been used, as reference points. A skilled stargazer like Akkullanu could easily have prepared star-charts of the background constellations, on which the changing positions of Mars — or of any other planet — could be entered, thus not only plotting its recent course but also making it possible to extrapolate its future course.

“In addition to being a stargazer and prognosticator, Akkullanu also served as ‘temple-enterer’ for the king; he could enter any temple in the land, and no priest could stop him. (As a rule, ancient temples were entered *only* by priests, and not even by all of *them*.) If Akkullanu did not like what he saw, there might be changes. In an absolute monarchy of the Sargonid variety, where severity and harshness were routine, it was not unusual for reputations, fortunes, careers, and even lives to hang in the balance. Akkullanu would have known as well as anyone how easily and how quickly a priest’s or courtier’s circumstances could change. All it required was a nod from the king. Akkullanu was not very likely to have bet *his* reputation, fortune, career, and life on a risky astronomical prediction that could be used against him by rival soothsayers who wanted his job and were ready to pounce.



“If by mid-March Akkullanu had already noticed Mars’s slowing rate of reduction in latitude, and had anticipated the bottoming out, he could have deduced that Mars would *not* move very much closer to the latitude of Saturn, and of course that Mars and Saturn would *not* coalesce into one point of light. The year -391 provides the *perfect* astronomical situation for enabling Akkullanu to predict of Mars that ‘it will absolutely not come close’ to Saturn. My recent discovery of this unexpected but all-important circumstance removes any remaining doubts that I might have had about -391. I am now convinced and confident that Esarhaddon was on the throne of Assyria in the year -391. (I do not yet know when he came to the throne or when he died.)

“Thus it does appear that I finally have at least an approximate dating for Esarhaddon.

“(When I use precise figures from Tuckerman, from van den Bergh, from Lange and Swerdlow, or from *Starry Night*, it should be understood that while these figures may not be entirely accurate, and while they are certainly subject to further refinement, they do nonetheless reflect the general situation, and they are more than accurate enough for our purposes here.)”

Other data has yet to be explored as it relates to this specific dating of Esarhaddon. But for now, the only precise astronomical evidence requires that Esarhaddon be placed around 392 B.C. That is, Rose has an astronomical date for Esarhaddon and there is none for Esarhaddon in the conventional chronology.

This evidence along with all the other astronomical evidence presented earlier indicates quite clearly that those who uphold the established chronology based on astronomical data lack support. If they now wish to raise any other eclipses, etc., as evidence against the short chronology, they must also show that it cannot fit in the Persian era. They must do their homework properly, not as Hermann Hunger did. He attempted to make the data fit the 8th and 7th centuries B.C. when he could obviously see they did not. The astronomical evidence for Heinsohn’s chronology now makes the long-established chronology impossible!

**ADAD-GUPPI'S BIOGRAPHY:  
HISTORY OR FICTION?**

“The ancient historians gave us delightful fiction in the form of fact ...”

Oscar Wilde  
*Collins Complete Works  
of Oscar Wilde*  
Glasgow 1999, p. 1073

While the astronomical evidence clearly fits the short chronology, it changes the actual reign lengths of all the kings from Ashurbanipal through to Nabonidus by at least 22 years because the length of the reign of Nebuchadnezzar is shortened by those 22 years, as well as an additional several years in which Nabopolassar's reign overlapped with Ashurbanipal/Kandalanu (whom Heinsohn equates with Darius III, overthrown by Alexander the Great). The biography of Adad-guppi, the mother of Nabonidus, has, however, been promoted as being in full conformity with the more lengthy chronology, where these years are not dismissed.

What we have then is a biographical sketch that supposedly conforms with the established chronology and in a certain sense—in terms of years—contradicts the short one. I have repeatedly pointed out that such documents are subject to many forms of error because they were employed to give the king in power a high standing and longer background than existed in reality.

This is clearly the case with Nabonidus. To repeat from volume I of this series, James *et al.* point out:

“There is ample evidence to show that the ‘dates’ given by the ancient Mesopotamians for their own history were often wide of the mark. An inscription of the 6th-century Babylonian King Nabonidus states that the famous King Hammurabi reigned 700 years before Burnaburiash, while modern reckoning separates them by some 300 years. Nabonidus also claimed that 3200 years elapsed between his

own time and that of King Naram-Sin of the Akkad Dynasty. This would place Naram-Sin *c.* 3750 BC, a full 1500 years older than any modern estimate!<sup>127</sup>

Whether one agrees with the short chronology or not, this undoubtedly shows that Nabonidus wanted to prove to those who were his subjects and to posterity that he had come after an extremely long and illustrious history. Even in terms of the long chronology he was expanding the history well beyond what the historians could swallow. This is also the same Nabonidus who supposedly has this Adad-guppi biography written in his own reign. When historians turn to use the document of Adad-guppi to magnify or enlarge the history/chronology preceding Nabonidus, they take it at face value as honest and above board. When these same historians turn away from what Nabonidus wrote about the length of time separating his reign from Hammurabi or Naram-Sin to magnify or enlarge the history/chronology preceding him, they do not take it at face value. Jonsson, who has written extensively on the chronology and history of Nabonidus, sadly and unfortunately failed to mention this problem which lies at the heart of Nabonidus's propaganda campaign to enlarge and magnify his importance and the length of the chronology to which he was the heir.

Now this motivation does not disprove the assertions in the Adad-guppi tablets but, as in a case of law before a jury, it indicates that one must be highly distrustful of what is being presented to bolster the case for the established chronology, when in fact it may be tainted evidence, or, more accurately, highly tainted evidence. As with Nabonidus's statements regarding Hammurabi and Naram-Sin, he did not so much change the history, rather he distorted the length of the chronology. This will be described in detail below. Here we will be doing what historians traditionally have done, subjecting this documentary evidence to an agonizing appraisal, just what historians should have done in the first place before putting Adad-guppi's biography on the witness stand. As we discussed in the Preface, this document will, like so many others, be burdened with such numerous problems as to make its evidential value negligible, not to say dubious in the extreme. Jonsson, nevertheless, turns to this biography as additional support for the established chronology:

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<sup>127</sup> James *et al.*, *op.cit.*, p. 293

“[It] exists in two copies. The first was discovered in 1906 ... in south-eastern Turkey, in the ruins of the ancient city of Harran ... [it] is a grave inscription evidently composed by Nabonidus for his mother, Adad-guppi’.

“The text not only includes a biographical sketch of Nabonidus’ mother from the time of [Neo-]Assyrian king Ashurbanipal and on to the ninth year of Nabonidus (when she died), but also gives the length of reign of each of the Neo-Babylonian kings [who followed the fall of the Neo-Assyrian empire] except, of course, Nabonidus himself, who was still living. Unfortunately, in the first copy the portion of the text setting out the reigns is damaged.

“However, in 1956 Dr. D.S. Rice discovered three other stelae at Harran from the reign of Nabonidus, *one of which bore a duplicate inscription* ... Fortunately, the sections ... containing the chronological information were not damaged. The first of these sections reads as follows:

“‘From the 20th year of Ashurbanipal, king of Assyria, when I was born, until the 42nd year of Ashurbanipal, the 3rd year of his son Ashur-etil-ili, the 21st year of Nabopolassar, the 43rd year of Nebuchadnezzar, the 2nd year of Awel-Merodach, the 4th year of Neriglissar, during (all) these 95 years ...’

“It should be observed that the first two kings, Ashurbanipal and his son Ashur-etil-ili, were [Neo-]Assyrian kings, while the following kings were *Neo-Babylonian* kings. This indicates that Adad-guppi first lived under [Neo-]Assyrian rule but then ... under [Neo-]Babylonian rule. Nabonidus’ mother lived to be a centenarian ... a complete summary of her long life is given.

“‘He [the Moon god Sin] added (to my life) many days (and) years ... and kept me alive from the time of Ashurbanipal, *king of Assyria*, to the 9th year of Nabonidus, *king of Babylon*, the son whom I bore, (i.e.) one hundred and four ... years ...’

“Interestingly, the same information is also given in the *Nabonidus Chronicle* ...

“‘The ninth year ... on the fifth day of the month Nisan the queen mother [Adad-guppi] died ...’

“... the lengths of the reigns are *in complete accordance with the Royal Canon* [of Ptolemy]—a very significant fact, because the corroboration comes from a witness *contemporary with all these ... kings*, and intimately connected with all of them! More so than the individual testimony of any one source, it is the harmony of all these sources which is most telling ...”<sup>128</sup>[emphasis in the original]

Henry Zemel on his Internet website “Caeno” has also raised this very same documentary criticism of Heinsohn’s and Sweeney’s revised chronologies.

Having shown that the astronomical data related to fixing the dates of these kings contains contradictions, we will now show that there are numerous contradictions in the text of the Adad-guppi stele. In his “Appendix,” Jonsson deals with one of these:

“Ashurbanipal is generally believed to have begun his reign in Assyria in 668 B.C.E. His twentieth year, therefore, is dated to 649/48 B.C.E. If Adad-guppi’ was born in that year, and if she lived on until the beginning of Nabonidus’ ninth year 547 B.C.E., she would have been 101 or 102 years old at her death, not 104 years as stated in the inscription. Scholars ... have concluded the stele contains a miscount of about two years.”<sup>129</sup>

Jonsson offers that the theoretical explanation of Dr. Joan Oates for this 2 year discrepancy “has been accepted by other scholars as most probably the correct one.”<sup>130</sup> Oates assumes, “from the inscription,” that she lived first in Assyria under Assyrian kings until the 3rd year of Assur-etil-ili, then lived in Babylon. Jonsson tells us that Oates believes that

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<sup>128</sup> Jonsson, *op.cit.*, pp. 113-116

<sup>129</sup> *ibid.*, p. 331

<sup>130</sup> *ibid.*

“this does not mean Assur-etil-ili’s third year was his last. If Assur-etil-ili began his rule in Assyria after his father’s death in 627 B.C.E., his third year was 624/23 B.C.E. His second and third regnal years in Assyria, then, overlapped the first and second years of Nabopolassar in Babylon (625/24 and 624/23 B.C.E.). In calculating the age of Adad-guppi’, Nabonidus (or the scribe who made the inscription) simply summed up the regnal years without taking into account this overlapping of Assur-etil-ili’s reign with that of Nabopolassar.”<sup>131</sup>

But this cannot be proved, only assumed, and Jonsson quotes Erle Leichty as admitting that although “Oates’ chronology will probably turn out to be the correct one ... final judgement must await the rest of the evidence.”<sup>132</sup> In spite of this discrepancy–contradiction, Jonsson claims “the error in the inscription is a minor problem that does not affect the reigns of the *Neo-Babylonian* kings as given in the Adad-guppi’ inscription.”<sup>133</sup> He adds this final point:

“[The problem] arose in the attempt [at that time] to establish Adad-guppi’s age, which had to be *calculated*, because, as pointed out by Rykle Borger, the Babylonians (like Jehovah’s Witnesses today!) ‘never celebrated their birthdays, and hardly knew how old they were themselves’.”<sup>134</sup>

This being the case, the entire stele is based entirely on calculations made after the death of Adad-guppi and in no way could she be considered a reliable eye-witness. This brings us back to Cryer who suggested (I repeat):

“... inscriptions ... may in fact contain faulty data and, if it contains one error, it may contain more. In actual fact, whenever we process duplicate versions of an ancient chronology, they invariably disagree with one another at numerous points. Many scholars see it as a pressing task to reconstruct from such a hypothetical ‘original’ chronology, but in fact, such work is simply textual criticism ...”<sup>135</sup>

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<sup>131</sup> *ibid.*

<sup>132</sup> *ibid.*, p. 332

<sup>133</sup> *ibid.*

<sup>134</sup> *ibid.* (Others spell Borger’s first name Riekele)

<sup>135</sup> Cryer, *loc.cit.*

That is precisely what Oates's theoretical explanation amounts to, "textual criticism." Cryer has informed us that, if a text "contains one error, it may contain more." Furthermore, although Jonsson claims this is "a minor problem that does not affect the reigns of the *Neo-Babylonian* kings ... in the Adad-guppi' inscription," it is not really minor. Since the Neo-Assyrians come before the Neo-Babylonians, a two-year discrepancy—contradiction in the length of the Neo-Assyrian kings indicates that the Neo-Babylonian kings must be moved two years back in time. Though, as Jonsson suggests, it does not affect their reign length, it most definitely affects the dates of their reigns. That is, each Neo-Babylonian king must be moved back in time by two years, which would create havoc with the astronomical data. All astronomical data would then be off by two years, which can hardly be considered a minor problem. Let us therefore examine these other problems.

How do we know the Adad-guppi stele was written, as Jonsson stated, by "Nabonidus" or a "scribe" at his behest? Saying it was "evidently composed by Nabonidus for his mother" or by a "scribe" for him, is an *ex cathedra* statement, that is, it is evidently so only if it is evidently so. The same applies to the work of Dr. W. Schramm whom Jonsson cites, who writes "it cannot be doubted that an authentic *Vorlage* on the story of Adad-guppi" [was used to make the inscription].<sup>136</sup> Saying "it cannot be doubted" is also an *ex cathedra* pronouncement, unsupported by testable, falsifiable proof. As T. Longman III states:

"However, there remain issues that need to be discussed [related to such documents as the Adad-guppi stele]. Primary among these is the extent to which each text reflects actual historical events. On the basis of preliminary study, one may conclude that the stories presented in these texts are not devoid of historical information. This position is supported by those cases in which there are additional and contemporary witnesses to historical—literary texts ... The assumption that the texts contain a kernel of historical truth is behind their use as modern aids for the reconstruction of ancient history."<sup>137</sup>

Longman suggests that in order for a document to be regarded as historically authentic and true, it ought to be "supported by ... cases [of other] contemporary witnesses." But according to Paul-Alain Beaulieu there is no

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<sup>136</sup> Jonsson, *op.cit.*, p. 116

<sup>137</sup> Tremper Longman III, *Fictional Akkadian Autobiography* (Winona Lake IN 1991), p. 209

corroboration for the Adad-guppi stele from contemporary witnesses because, as he shows, “this passage ... remains the sole source on Adad-guppi’s life.”<sup>138</sup> The evidence contained in the Adad-guppi stele has no contemporary witnesses for corroboration. It is its own sole source for its historic validity. Longman continues:

“The question arises, therefore, as to the relationship between the ‘historical’ and the ‘literary’ in these texts. To what extent do these compositions accurately reflect historical events? No precise answer to this question may be given.”<sup>139</sup>

Longman makes it quite clear that one cannot prove the events in such documents as that of Adad-guppi are part of a truly correct historical account rather than a literary, fictional one. The reason is that not only is there no eye-witness corroboration of this document but these texts were created as propaganda tracts to extol the king or others. As Longman further shows,

“Recent study by both Assyriologists and biblical scholars has exposed the political/propagandist function of many literary compositions. Frequently literature was composed in order to justify a political act that had taken place in the past.”<sup>140</sup>

With respect to the Adad-guppi stele in particular as it relates to propaganda, Longman adds:

“A dual function may be seen in Adad-guppi: on the one hand Adad-guppi’s recounting of her life promotes the worship of the moon cult (her own example ... suggests that such a course of action leads to a prosperous and long life): on the other hand the text also glorifies her son Nabonidus. The text ... was probably composed by a pro-Nabonidus group that supported his religious program.”<sup>141</sup>

He thus concludes: “the Adad-guppi autobiography is an example of a text that has a religious and a political [propaganda] function working side by

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<sup>138</sup> Paul-Alain Beaulieu, *The Reign of Nabonidus King of Babylon 556-539 B.C.* (New Haven CT and London 1989), p. 70

<sup>139</sup> Longman, *loc.cit.*

<sup>140</sup> *ibid.*, p. 210

<sup>141</sup> *ibid.*, p. 212



side.”<sup>142</sup> While Jonsson suggests the stele was “evidently composed by Nabonidus”, Longman suggests that it was “probably composed by a pro-Nabonidus group.” Longman goes on to say that such texts were produced by an anonymous author who assumed Adad-guppi’s name as a pseudonym:

“A pseudonym is ‘a false or fictitious name, esp. one assumed by an author’ ... A pseudonymous literary work, therefore, is one written by someone other than that named in the text as author ... Further, autobiography [such as that of Adad-guppi] is a type of composition in which the narrator claims to be the author.”<sup>143</sup>

Here are the reasons offered for presenting such a text:

“B.M. Meltzer has demonstrated the use of pseudonymity. Among others he lists financial gain, malice, respect, modesty and convenience ... Roland states, ‘The only way open to those who wanted to convey to the people the communications they believed they had received from God was to use the device of pseudonymity’.”<sup>144</sup>

In the main text itself, the main god, Sin, is made to say “Through you [Adad-guppi] I will bring about the return of the gods (to) the dwelling in Harran by means of Nabonaid [Nabonidus] your son.”<sup>145</sup> All this shows that no-one knows who wrote the stele and for what reason. But more important is the problem of when it was composed. Longman gives this general overview: “Close examination, however, reveals that these compositions were written years—at times even centuries—after the death of the purported narrator–author.”<sup>146</sup>

He further conjectures that the Adad-guppi text was composed “(10 years?)” after her death.<sup>147</sup> The question mark added to the estimate makes it clear that the date of the text is purely conjectural and can in no way be known.

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<sup>142</sup> *ibid.*, p. 103

<sup>143</sup> *ibid.*, p. 204

<sup>144</sup> *ibid.*, p. 106

<sup>145</sup> *ibid.*, p. 226

<sup>146</sup> *ibid.*, p. 102

<sup>147</sup> *ibid.*, p. 209

Related to this is the fact pointed out by Raymond Philip Dougherty<sup>148</sup> that the word used by the chronicler refers to the parent of Nabonidus not in the feminine but in the masculine form, as though Adad-guppi was a man. This indicates that the chronicler who copied the *Nabonidus Chronicle* did so long after the events recounted and edited it based on his incorrect understanding of the text. Dougherty further shows:

“An apparent quandary arises ... concerning Nabonidus’ stay ... in Arabia [based on] available texts ... he was not in Babylonia in the ninth year of his reign when the death of his mother occurred. ... The intimation of the *Nabonidus Chronicle* is that he took part neither in the three-day period of mourning ... nor in the general mourning after her death during the month of Sivan of the same year. On the other hand the Eski-Harrân inscription column III, lines 19b-32, attributes to Nabonidus the performance of extensive burial rites which were common in antiquity.”<sup>149</sup>

To explain this obvious contradiction, textual editing is employed by Dougherty. It is assumed that Nabonidus issued orders that “all appropriate rites should be performed in his name ... [therefore a]nything done at the behest of a distant sovereign was credited to him.”<sup>150</sup>

All of this editing and textual criticism cannot be tested; it is all assumed and then the assumptions are taken as valid historical chronology of this period.

Therefore, we have no idea who wrote the Adad-guppi inscription, when it was written, or if the work is a valid basis upon which one may depend as evidence for the established chronology. Herbert Butterfield discussed these foundation inscriptions

“in the Babylonia of the time of Nabonidus ... and indeed during the whole of what is called the Neo-Babylonian epoch. There was a sense for the past, and a great desire to restore ancient temples; but it was necessary to follow the rules that had been established in each

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<sup>148</sup> Raymond Philip Dougherty, *Nabonidus and Belshazzar* (New Haven CT 1929), pp. 23-24ff

<sup>149</sup> *ibid.*, p. 26

<sup>150</sup> *ibid.*, p. 27

case—to discover the *temena* which had authenticated the original building and had shown how the god had intended it to be constructed. A breach of this divine decree might bring tragedy, and there were occasions when a temple was pulled down because it was disclosed that it did not correspond with the basic document. If the text could not be found, some other document might be used to authenticate tradition at a given place; though it was liable to be superseded if something still more ancient emerged. The *temena* was attached to the original building and if the temple was in ruins it might be necessary to institute something like a dig [to find it]. Mention is made of specialized workers who took part in this investigation. In the process, varied kinds of texts were likely to be uncovered [such as the Adad-guppi stele]; they would be transcribed and studied and if they contained the name of a ruler he would be located in the king-lists and the date would be worked out.”<sup>151</sup>

We thus have no idea if this stele was found tens or hundreds of years later in the area of a temple that was either destroyed or left in ruins. We have no idea if elements in it were missing and then replaced by scribes who attempted to give it the correct translation and meaning. A.K. Grayson describes what happened to these inscriptions:

“[Neo-]Assyrian royal inscriptions are one of the major sources of this period. The few extant Babylonian inscriptions of this era have little relevance to Assyrian history. Among the Assyrian royal inscriptions the commemorative texts [like that of Adad-guppi] are the largest and most important group. They consist of annals [etc.]. The annals were commonly re-edited many times during a reign and the historian should give priority to the earliest version for a given campaign. Even the modern scholar must be very critical, for most of the texts now extant are products of considerable editing, selecting and conflating of various sources. Moreover, the Assyrian royal inscriptions are notoriously biased and occasionally untruthful, and one must constantly watch for deliberate omission, distortions, and falsification.”<sup>152</sup>

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<sup>151</sup> Butterfield, *op.cit.*, pp. 116-117

<sup>152</sup> A.K. Grayson, “Assyria: Ashur-Dan II to Ashur Nirari V,” *CAH*, vol. III, pt. 1 (Cambridge UK 1991) p. 241

All these problems must also be involved in either small or large measure with the Adad-guppi stele. How can one know for certain that this is not the case with the Adad-guppi inscription, since there are no other corroborating eye-witness accounts to determine its validity? An example of how this falsification occurs is reported by Joan Oates and David Oates:

“The interest of ... historical [reality] for the archaeology of Nimrud is that Sargon later substituted his own ‘improved’ version of events, as a blatant piece of propaganda in a document directly modeled after that of [the original one by] Merodach-Baladan and [placed it] in the same temple in Warka. Indeed the final lines of the ‘substitute’ cylinder read ‘copy of a foundation-text sent (?) to/from the palace in the land of Assur; *copied and revised.*’ Sargon retained the original in his archives where it was found by Mallowan in 1952, literally over a period of three days, the cylinder itself having been broken—deliberately?— into three pieces.”<sup>153</sup>

In this case the archaeologists and historians were fortunate to discover the original and have the falsifier admit his revision. But we cannot depend on this being the case with other documents such as the Adad-guppi inscription.

Thus far, we have dealt with this inscription in a general way, but it is time to get into its other contradictions and falsifications of substance.

For example, the stele exists in two copies. In the first copy, the claim is made that Adad-guppi gave thanks to Sin “from the time of Ashurbanipal to the 6th year of Nabonidus ... for 104 happy years.” But the 6th year of Nabonidus found in the first copy reads as the 9th year of Nabonidus in the second copy. Here is how Sidney Smith explains why the 9th year of Nabonidus was accepted instead of the 6th year for her death:

“The position is complicated by the fact that the reading of the year in which the parent [Adad-guppi] is stated to have died is uncertain. Langdon reads the number as 6, when the parent intended would be stated to have died in 549 [B.C.], while Pognon and Dhorme read 9, to make the inscription agree with the fact known from the

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<sup>153</sup> Joan Oates, David Oates, *Nimrud, An Assyrian Imperial City Revealed* (London 2001), p. 199

Nabonidus Chronicle II 13 that Nabonidus' mother died in the year 546 [B.C.]”<sup>154</sup>

That is, the historians accepted the number that agreed with their chronology and with another document by a witness who was not there to see Adad-guppi's death and funeral. Though this was a very weak form of corroboration, they suggested this was the case. These, it is assumed, were turbulent times of warfare and what was conveyed may not have been accurately remembered. Jonsson has told us that the Nabonidus Chronicle corroborates the 9th year of his reign for her death<sup>155</sup>, and actually cites it. However, J.A. Thompson informs us that this text may not have been written under the direction of Nabonidus at all but under the order of Cyrus. “The Nabonidus Chronicle ... was almost certainly prepared under the direction of Cyrus.”<sup>156</sup> Even this is uncertain. Whoever prepared the Nabonidus Chronicle is unknown, and yet it is taken, in this case, as an untestable, unfalsifiable assumption elevated to a fact.

In addition, the Nabonidus material contains another fundamental problem discussed by J.A. Thompson:

“One of the texts of Nabonidus found at Haran [with the Adad-guppi stele], and dated to 546 B.C., refers to the ‘king of the Medes.’ This opens up the whole question [of the chronology] afresh. D.J. Wiseman asks whether the phrase may not have been another name for Cyrus [the Persian]. Certainly the people [as opposed to the king] spoke of the King of the Medes in 546 B.C. But by this time there was no such king [because the Medes had been conquered by the Persians], his place had to be taken by Cyrus the Persian who [then] went on to conquer Babylonia. Wiseman suggests [based only on assumption] that we should translate Daniel 6:28 ‘in the reign of Darius [the Mede], even into the reign of Cyrus the Persian’.”<sup>157</sup>

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<sup>154</sup> Sidney Smith, *Babylonian Historical Texts: Relating to the Capture and Downfall of Babylon* (NY 1975), pp. 37-38

<sup>155</sup> Jonsson, *op.cit.*, p. 116

<sup>156</sup> J.A. Thompson, *The Bible and Archaeology*, 3rd ed., fully revised (Grand Rapids MI 1973), p. 175

<sup>157</sup> Thompson, *op.cit.*, p. 200

Again, the documented evidence related to the Adad-guppi and Nabonidus inscriptions has a contradiction that is again solved by assumption. There is yet a further problem with the Adad-guppi stele. With respect to Nabonidus' father, Adad-guppi's husband, Beaulieu shows:

“Adad-guppi refers to Nabonidus' father as ‘Nabu-balatsu-iqbi’ whose name often closes Nabonidus' titulary [such as ‘Nabonidus, king of Babylon, son of wise prince Nabu-balatsu-iqbi’] in building inscriptions, in which he bears the titles *rubu enqi* ‘wise prince’, *rubu gitnalû* ‘perfect prince’ and *sakkanaku gitrudu* ‘heroic governor.’ However, no person called Nabu-balatsu-iqbi who could reasonably be identified as the father of Nabonidus appears in Neo-Babylonian documents.”<sup>158</sup>

However, Beaulieu shows other kings of the time and region also had titularies attached to their names, but in these cases the fathers of these monarchs are actually documented from other sources.<sup>159</sup> Since neither Nabonidus nor Adad-guppi were of royal descent—“Nabonidus ... did not belong to the royal family by descent”<sup>160</sup>, “Adad-guppi ... [d]espite being from a modest background ... achieved high position at the court ...”<sup>161</sup>—they could falsely elevate their status and thus justify Nabonidus' taking control of the country by conquest. Though this is a minor point, the evidence all taken together clearly shows the Adad-guppi inscription is rife with misinformation. Jonsson states:

“Evidently, Dr. Paul-Alain Beaulieu in his discussion of these problems [related to the two-year discrepancy in the Adad-guppi stele] was not aware of Oates' solution. His comments, therefore, are confusing and his questioning of the accuracy of the chronological data of the stele clearly is unwarranted.”<sup>162</sup>

When I wrote to Jonsson regarding this statement he replied that Beaulieu had not cited Oates's theory for the 2 year discrepancy and that was his reason for making the claim. [Jonsson, personal communication.] But this does not

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<sup>158</sup> Beaulieu, *op.cit.*, p. 68

<sup>159</sup> *ibid.*

<sup>160</sup> Tomoo Ishida, *The Royal Dynasties in Ancient Israel* (Berlin, Germany/NY 1977), p. 76

<sup>161</sup> Hennie J. Marsman, *Women in Ugarit and Israel* (Leiden, the Netherlands 2003), p. 348

<sup>162</sup> Jonsson, *op.cit.*, p. 331

explain or prove anything. Jonsson's claim is merely his opinion put forward as if it were a fact, which may not be true. Such an *ex cathedra* statement is clearly unwarranted. In fact, part of Beaulieu's comment unrelated to the problem of the 2-year discrepancy does merit citation because Oates' theory is not the *sine qua non* for its solution. It is merely an unproven theory. Beaulieu states directly:

“In fact, the succession of kings provided in the inscription of Adad-guppi does not fit any known Assyrian or Babylonian city ...”<sup>163</sup>

This statement did merit a reply from Jonsson.

Therefore, despite the Adad-guppi biography having numerous problems, Jonsson and Henry Zemel extol its virtue for resolving the chronology of the Neo-Assyrians through to the Neo-Babylonians. The problem with this analysis is that it offers an uncorroborated document with several problems as proof for the established chronology. This approach must be denied because of the astronomical data that now contradicts that established chronology. Nabonidus's lunar eclipse that occurred on the thirteenth day of the month of Ululu required that it was eclipsed as it set. But Rose showed that the Moon was barely, if at all, eclipsed when it set in the established chronology for Nabonidus; he also found a more suitable case, where the Moon did set totally eclipsed, not on September 26, 554 B.C. but October 2, 294 B.C., some 260 years later!

How can the Adad-guppi document be acceptable as proof for the established chronology when it is fundamentally contradicted by this basic scientific/astronomical/empirical fact? The documents must follow the science, not the science the documents. That is why the Adad-guppi stele is invalid as a source of proof for the established chronology. Besides being ridden with problems, it is contradicted by the science. How can Nabonidus be placed where the astronomy forbids such placement?

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<sup>163</sup> Beaulieu, *op.cit.*, pp. 139-140

## NABONIDUS/NABONASSAR, CYRUS/ALEXANDER

Finally, we come to a most relevant problem related to Nabonidus and Hammurabi. As we have shown earlier, there are several forms of evidence that indicate that Hammurabi is the *alter ego* or, more likely, a contemporary, of Darius the Great who reigned from c. 521 to 486 B.C. On the other hand, Nabonidus supposedly reigned from 555-539 B.C. or some 34 years prior to Darius I's reign. Based on the established chronology, Nabonidus could not have known of Hammurabi, he could not have been aware of his existence, except insofar as Sweeney's thesis requires that Nabonidus and the rest of the Neo-Babylonian kings were the *alter egos* of the last few kings of Persia who ruled over Babylonia. However, as pointed out by James *et al.*, "An inscription of the 6th-century Babylonian King Nabonidus states that the famous King Hammurabi reigned 700 years before Burnaburiash ..." <sup>164</sup>

W.H. Stiebing, Jr., suggests that both Heinsohn and Sweeney's theories are impossible because:

"... if Heinsohn [and Sweeney are] right, a truly amazing feat would have been accomplished by Nabonidus, the ruler of Babylon whose reign began seven years after Nebuchadnezzar's death. At Larsa Nabonidus found an inscription of Hammurabi. Since Heinsohn identifies Hammurabi with Darius I of Persia, Nabonidus succeeded in excavating a royal inscription of a man who would not come to the throne until eighteen years after Nabonidus himself died!" <sup>165</sup>

Cochrane on his own objects to this identification of Darius and Hammurabi:

"Heinsohn's most vocal defender, Clark Whelton, in apparent recognition of the force of Stiebing's objection, proves no less reckless in attempting to rewrite history. Pointing to an unpublished article ... which purportedly supports a radical downdating of Nabonidus, Whelton likewise holds out the possibility that Nabonidus lived *after* Darius ...

<sup>164</sup> James *et al.*, *op.cit.*, p. 293

<sup>165</sup> W.H. Stiebing, Jr., "Heinsohn's Revised Chronology," *Aeon* vol. II, no. 5 (1991), p. 47



“The mind boggles at this sort of historical ‘reconstruction’.”<sup>166</sup>

Let us remember that Nabonidus, based on solid astronomical evidence, did not reign from 556-539 B.C. but after Alexander the Great: he reigned long after Darius I/Hammurabi. While Darius/Hammurabi reigned very early in the Persian era, Nabonidus reigned after the end of the Persian period. Nabopolassar was followed by Nebuchadnezzar and later by Nabonidus. That is, Nabonidus had to have reigned after Darius/Hammurabi and therefore it is obvious that he would have known of Darius/Hammurabi who lived about 200 years earlier. With good forensic historical evidence, the science and technology come first and the documentary evidence comes after. In this case the science and technology correlate, corroborate, and converge to confirm that Nabonidus lived after Darius/Hammurabi. This will again be shown to be the case in the next unit which deals with stratigraphy. Stiebing, Cochrane, and probably all historians of the ancient world are evidently willing to hold the historical documentary evidence superior to scientific and technological evidence with respect to the dating of the Neo-Assyrians and Neo-Babylonians. All these vocal defenders of the conventional historical chronology are oblivious to the great force of the scientific and technological objections which disprove it and it is *they* who are reckless, in attempting to uphold the conventional historical chronology. The mind boggles at this sort of historiographical intransigence.

To discredit this conclusion that Nabonidus came after Darius/Hammurabi, Cochrane raises the following additional criticism:

“Nabonidus ... was conquered by Cyrus the Great in 539 BCE. [prior to Darius/Hammurabi’s reign]. How do we know this? Because there are letters and inscriptions from Nabonidus himself which confirm his intimate relationship to Cyrus the Great. Indeed, it is well-known that a cylinder text of Nabonidus from c. 554/53 or 550/49 BCE describes the impending threat posed by the Persian leader upon the latter’s defeat of the Median army of Astyages:

““And indeed in the third year [of Nabonidus’ reign] came to pass ... Cyrus scattered the numerous Umman-manda [Medes] ...’

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<sup>166</sup> Cochrane, *loc.cit.*, p. 72

“Cyrus himself , in turn, provides an account of the capture of Babylon and its heretical king—the so-called Cyrus cylinder. Here’s a brief sampling from that document: ‘Without any battle, he [i.e., Marduk] made him enter his town Babylon ... He [Marduk] delivered into his (i.e., Cyrus’) hands Nabonidus, the king who did not worship him (i.e., Marduk).’

“In short, aside from completely ignoring the historical record, it is difficult to see how Heinsohn [or Sweeney are] going to succeed in downdating Nabonidus nearer to the time of Cyrus the Younger [or after Darius I’s reign].

“Given these apparently insurmountable problems, one can’t help but wonder why Heinsohn [or Sweeney] ... would seek to ... downdate [Nabonidus] ...”<sup>167</sup>

It certainly looks as though Cochrane had placed Nabonidus squarely in the time of Cyrus the Great well prior to the time of Darius/Hammurabi *based on the documents*. In this regard there is an answer to this seemingly insurmountable obstacle. The name Nabonidus is almost identical to the name of another Neo-Babylonian king who not only reigned prior to Nabonidus, but who, as the astronomical evidence shows, reigned just prior to and just after Alexander the Great. That king’s name has almost exactly the same meaning as Nabonidus, and that king’s name is Nabonassar. Nabonidus means “Nabo protects me”<sup>168</sup>. The other name, Nabonassar, means “Nabo is protecting me.”<sup>169</sup> In English, both names sound different; however, in Akkadian the spelling of both names is almost identical. Sweeney, in fact, spelled all this out long ago for Cochrane, who evidently completely ignored what Sweeney presented.

Emmet Sweeney deals with this question in an article titled “Did Artaxerxes III Despoil the Temple in Jerusalem?”, wherein he states:

“On the face of it there can be no denying that ‘Neo-Babylonian’ documents do seem to provide a formidable defence of accepted chronology; the present writer was compelled to dig deep into the

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<sup>167</sup> *ibid.*

<sup>168</sup> Sweeney, *op.cit.*, p. 100

<sup>169</sup> *ibid.*

fundamentals of Neo-Babylonian and Persian historiography to unravel the truth. Five or six major historical inscriptions of this period are known; all are presented in Pritchard's *Ancient Near Eastern Texts*. One, from the time of Nabopolassar, describes the fall of Assyria and the rise of the Neo-Babylonians under the latter ruler. Another, written as a eulogy for Adad-Guppi, mother of Nabonidus, describes this woman's life and touches also on the circumstances surrounding the rise of her son to the kingship.

“Only two inscriptions describe the fall of Nabonidus and the conquest of Cyrus. These are the so-called Nabonidus Chronicle and the famous Prism of Cyrus. The Nabonidus Chronicle, the lengthier of the two, describes how Cyrus ... became master of the Mede Empire after defeating [its king] Astyages ... and chronicles his victory over Lydia and his conquest of Babylon. To all intents and purposes these documents [as Cochrane claims] appear to put conventional chronology beyond question. However, two points should be considered.

“(a) None of these inscriptions, possibly excluding the Cyrus Prism and the Stela of Adad-Guppi, were actually written contemporary with the events they describe. This is important, for reasons that will become apparent.

“(b) Neither of the inscriptions dealing with Cyrus's conquest of Babylon (the Nabonidus Chronicle and the Cyrus Prism) refer to any Neo-Babylonian or Neo-Assyrian king other than Nabonidus. This is important because, as will be seen, these documents do not belong with the others loosely grouped together as ‘Neo-Babylonian’. In fact, there were two [Neo-]Babylonian kings named Nabonidus. The first, the man defeated by Cyrus, lived 2 centuries before the second, who was the same man as Darius III. The first Nabonidus is usually known as Nabonasser ...

“The names ‘Nabonasser’ and ‘Nabonidus’ look very different. However the Babylonian versions [of these two names], Nabu-natsir and Nabu-na-id, tell a different tale. They mean, respectively, ‘Nebo protects’ and ‘Nebo protecting’ and are rendered more fully into English by the translators as ‘Nebo protects (me)’ and ‘Nebo (is)

protecting (me)’. Thus the only difference in meaning is one of verb tense; one uses [the] present simple [tense] ‘Nebo protects’, the other [the] present continuous [tense] ‘Nebo protecting’. Therefore, the names are virtually identical.

“For the reign of the earlier of these, commonly called Nabonasser [by the historians], there is hardly a brick or inscription, though we know he was an important king whose lifetime was viewed as marking the start of a new epoch. The Ptolemaic Canon begins with his reign ...

“The confusion of Nabonidus I and II has bedevilled Near Eastern historiography for over 2,000 years.”<sup>170</sup>

According to Sweeney, Nabonidus was not conquered by Cyrus but by Alexander the Great. In Babylon he was known as Nabonidus while in Persia he was Darius III. Sweeney explains:

“Ancient monarchs had numerous royal titles, often being known by different names in the different cultural and linguistic regions over which they reigned – e.g. it is widely accepted that Ashurbanipal was known as Kandalanu as ruler of Babylon, whilst his predecessors Tiglath-Pileser III and Shalmaneser V [*sic*] were known, respectively, as Pul and Ululai in their capacity as rulers of the same city. This is common sense: ancient kings derived their legitimacy from posing as temporal representatives of the gods, with their royal titles reflecting their exalted position. Are we to believe that the Assyrian subjects of Xerxes (Khsharshya) knew him by this title, which was for them meaningless and probably unpronounceable?”<sup>171</sup>

A further question related to this identification of the Neo-Babylonians with the last three major kings of the Neo-Assyrian/Persian empire is: Did the Persians toward the end of their empire become greatly influenced by Babylon, and were they thus largely identified as Neo-Babylonian monarchs? As Sweeney further remarks:

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<sup>170</sup> Emmet J. Sweeney, “Did Artaxerxes III Despoil the Temple in Jerusalem?”, *Chronology & Catastrophism Review* (2002-2), pp. 19-20

<sup>171</sup> *ibid.*, p. 15

“Is there then any evidence to suggest that the influence of Babylon became paramount during the time of the last three or four of the [Persian] Great Kings?

“As it happens, there is very good evidence. According to our sources, Darius II was the son of Artaxerxes I by a Babylonian concubine, in virtue of which he was popularly termed Nothus, 'the Bastard'. His accession is thus normally held to mark the end of the pure Achaemenid [Persian] line and a period of increasing Babylonian influence is recognised in the years following. Babylon was rebuilt and re-endowed after the massive destruction wrought by Xerxes and in many ways the city was to supplant Persepolis as the true capital of the Empire.”<sup>172</sup>

This makes perfect sense. Since the last three or four Persian Great Kings enjoyed the luxury and cosmopolitan life in Babylon, they spent much to rebuild it and adorn it with buildings, art, and inscriptions, to the detriment of their own capital, for about 80 years.

How then is one to explain Alexander conquering Nabonidus/Darius III instead of Cyrus conquering Nabonidus? This is all strictly in terms of Sweeney’s chronology which the astronomy does not support regarding the placement of the Neo-Babylonians. This was kindly pointed out to me by Birgit Liesching who caught this error in my work. What I am now showing is not that Sweeney is correct *vis-à-vis* this particular phase of chronology, but rather that he has presented historical evidence which challenges the established way of examining that evidence. Thus, although I defer to his chronology to explain that the established approach is invalid, I use it only as an example, an explanation of how this problem can be approached, and shall do so in certain places below that deal with this same material. In this respect Alexander did not conquer Nabonidus but, as shown above, he conquered Ashurbanipal who is Darius III in terms of astronomical evidence and Heinsohn’s chronology. While I agree with much of Sweeney’s chronology, this material clearly supports Heinsohn.

According to Sweeney on this problem:

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<sup>172</sup> *ibid.*

“Thus whilst I claim that it was Alexander (the conqueror of Nabonidus II), who freed the Jews from their Babylonian captivity, Jewish tradition insists it was Cyrus. The identities of Cyrus and Alexander have been confused in much of Near Eastern tradition. Even Islamic scholarship is affected. To this day, Iranian scholars and clerics hold the name Cyrus in contempt. They [at the same time] honour [Cyrus’s prophet] Zoroaster and the monotheistic religion of Achaemenid times but Cyrus is not identified with this noble faith: he is identified with the Macedonian destroyers of Iran and her culture — with Alexander no less. One of Islam’s holy books specifically links Alexander with Cyrus:

“Alexander had a hairstyle curved like two horns, as did Cyrus. In Daniel 8:6 [we read]: ‘and he came to the ram with two horns’... Many Arab and Persian scholars have referred to Zelranain [‘horned’ in Arabic] as Alexander who destroyed Persian and Indian culture’. ...

“[The Hebrews in Palestine referred to the kings of Persia] by their Semitic names, whereas the chroniclers of the history of the exiled Israelites, living in the Persian homeland, used the Persian names. Thus two parallel histories, one compiled by the free Jews, the other by the exiled Israelites, seem to have developed. When the Scriptures were being written in their final form, during the 1st century BC, the two distinct traditions were available. Clearly the use of totally different names for the same kings must have caused profound confusion and eventually the parallel traditions were put in sequence rather than, as they should have been, contemporary.

“The misplacement of Persian history also had the effect of throwing Hebrew history into chaos. Commentators frequently note that the Jews, most assiduous of record-keepers, left not a single document or even note from the time of Ezra (supposedly 5th century BC) until the time of the Maccabees, in the mid-2nd century BC. In a period when we should have expected a rich tradition to have survived, there are 250 years of Hebrew history totally unaccounted for. The only Jewish writer to cover the 3rd and 4th centuries [B.C.] is Josephus but his sources are entirely Hellenic; he tells us virtually nothing about the

Jews themselves in this epoch. (He does however mention that Alexander came to Jerusalem and honoured the Jewish God — rather in the way Cyrus honoured the same God. ...

“This confusion can be demonstrated in the Jewish literature — e.g. Book of Daniel. It is agreed that Daniel began to prophesy during the time of a Neo-Babylonian king named Nebuchadrezzar [II] and he remained active into the reigns of the early Persians. He was thrown into the lion's den by a king Darius (universally believed to be Darius I), who is said to have inherited the kingdom of Belshazzar, Nabonidus' nephew and acting regent. From this information Daniel is placed in the late 6th century BC.

“However, contradicting this date, the writings of Daniel are filled with references to the Macedonians, who are mentioned by name (Dan. 8:21), and to the early Ptolemies and Seleucids, who are mentioned by implication (the latter two are known, respectively, as the 'king of the north' and the 'king of the south' but no biblical authority doubts that the prophet is referring to the Ptolemies and Seleucids). He also refers to the Romans, who send their ships to the aid of one of the kings of the region. (Dan. 11:29-30). This Roman intervention in the Levant is unlikely to have greatly preceded the alliance [that the Romans had] with Ptolemy II, concluded in 273 BC.

“Because of such conflicting dates, commentators have been forced to abandon the idea that Daniel ever existed or, at least, that he had anything to do with the book named after him. Alternatively, it has been suggested that there were two Daniels, or a Daniel and a pseudo-Daniel, one living in the late 6th and early 5th century [B.C.], at the start of the Persian Empire, and another living 200 years later, at its end.”<sup>173</sup>

Why didn't Cochrane even make an attempt at dealing with this evidence, let alone make his reader aware that Sweeney had presented this information?

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<sup>173</sup> *ibid.*, p. 19

The most important question in this respect is: Does the scientific/astronomical evidence place Nabopolassar squarely in the time of Alexander the Great? Here the evidence, as Rose has shown above, is unequivocal. In the unit above titled “Nabopolassar Lunar Data and Chronology,” Rose proved the astronomical evidence placed Nabopolassar just prior to and just after Alexander the Great’s taking of Babylon. As Rose stated, “All this constitutes very strong evidence that Nabopolassar reigned from -341 to -320.”

Therefore the Cylinder of Cyrus and that of Nabonidus, as documents, must again be made to be in accord with the scientific evidence and not the other way around. As for Alexander and Cyrus, both created great empires in only a few years and administered them in much the same way. Therefore, given Sweeney’s evidence respecting the ancient identities of, or similarities between, Cyrus/Alexander noted by the ancient historians—not that Alexander the Great of Macedonia is Cyrus the Great of Persia—it seems that their careers have been confused in these documents.

What we do now find related to the established chronology is that because of these various identities we run headlong into a Dark Age related to the Neo-Assyrians.

## **THE NEO-ASSYRIAN DARK AGE**

What we have learned from Sweeney that is most striking related to this discussion is that in order to keep Nabonidus in the 6th century B.C., in terms of the conventional chronology, a 250-year Dark Age descends upon Israel, a period wherein Hebrew history is totally missing.

On the other hand, by following the short chronology of Heinsohn and Sweeney, this Dark Age disappears. It is inconceivable that in the latter part of the first millennium B.C., where the records are fullest and most numerous for the ancient Near East, there should exist a period in Israel of 250 years that has simply vanished. As I have repeatedly stressed, Dark Ages are a symptom of chronology



gone awry, and to have such a major one in Israel at this time defies credulity, as well as historical reality.

But this is only one aspect of Dark Ages that are created by keeping the Neo-Assyrians and Neo-Babylonians in the conventional chronology. There are other Dark Ages that are further related especially to the Neo-Assyrians when the established chronology is followed. Let us examine these:

The downfall of the Neo-Assyrian empire is conventionally dated to 612-608 B.C. and thereafter the Neo-Babylonians supposedly reigned to around 539 B.C. when Cyrus the Great conquered Babylon. Assyria was one of its provinces which paid a high annual tribute to Persia. However, historians have found a comparable situation with Hammurabi's Old Babylonian empire. That is, archaeologists can find almost nothing, after the Persians conquered Assyria and administered its lands, to indicate that Assyria was a thriving community. Let us recall that with the Old Babylonians there was a 100-year Dark Age prior to their rule in Mesopotamia and a several hundred year Dark Age after they fell. Let us now begin with the fall of Neo-Assyria. Sidney Smith describes the situation thus:

“The disappearance of the Assyrian people will always remain a unique and striking phenomenon in ancient history. Other similar kingdoms and empires have indeed passed away but [their] people have lived on. Recent discoveries have, it is true, shown that poverty-stricken communities perpetuated the Old Assyrian names at various places, for instance on the ruined site of Ashur ...”<sup>174</sup>

Smith goes on to say:

“In another way the fall of [Neo-]Assyria is unique in that after centuries of military domination in Mesopotamia, and after decades of imperial power, it is almost impossible for the modern historian surely to trace any lasting Assyrian influence on the history of succeeding ages.”<sup>175</sup>

J. Joseph states that after Assyria fell,

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<sup>174</sup> Sidney Smith, “Ashurbanipal and the Fall of Assyria,” *CAH*, vol. 3 (Cambridge UK, 1961), p. 131

<sup>175</sup> *ibid.*

“There was nothing ‘Assyrian’ to be read and remembered, ... the language of the cuneiform documents, Akkadian, had ceased to exist as carrier of ancient Assyrian culture *even before the fall of the Assyrian empire.*”<sup>176</sup> (emphasis added)

Will Durant in his popular histories echoes these commentaries:

“Nineveh was laid to waste ... the population was slaughtered or enslaved ... At one blow Assyria disappeared from history. Nothing remained of her except certain tactics and weapons of war ... not a stone remained visible of all the temples.”<sup>177</sup>

However, it was previously stated that Dark Ages are an indication that the chronology is in marked error. In this case, when we place the Neo-Assyrians under Persian rule, they do not disappear from history for almost 400 years but continued to exist and carried forward their contributions without interruption into the age of the Greeks. In fact, some historians question the concept that Assyria was wiped off the face of the Earth.

Simo Parpola of the University of Helsinki asks:

“What happened to the [Neo-]Assyrians after the fall of Assyria? This is a question that is not easy to answer for two reasons. Firstly, the issue has hardly been touched [upon] by Assyriologists. Most of them seem to tacitly agree with the idea of a more or less total wipe-out, as suggested by Sidney Smith in 1925 ... ‘No other land seems to have been sacked and pillaged so completely as was [Neo-] Assyria.’

“Secondly, in contrast to the abundance of information from the imperial period, information on post-empire [Neo-]Assyria and Assyrians is scanty and scattered. The near-total lack of information from Assyria itself would seem to support the idea of a genocide, which also seems to be supported by ancient eye-witness testimonies. When the Greek historian Xenophon 200 years after Nineveh’s fall passed through the Assyrian heartland and visited the sites of two great

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<sup>176</sup> John Joseph, “The Bible and the Assyrians, It Kept Their Memory Alive,” *Journal of Assyrian Academic Studies*, vol. 12, no. 1 (1998), p. 72

<sup>177</sup> Will Durant, *Our Oriental Heritage, op.cit.*, pp. 283-248

Assyrian cities, he found nothing but ruin and could not retrieve much about them from the nearby villagers. The territory where these deserted cities lay was now Median, and the Greeks assumed that their former inhabitants had likewise been Medes.

“Yet it is clear that no such thing as a wholesale massacre of all Assyrians ever happened. It is true that some of the great cities of Assyria were utterly destroyed and looted—archaeology confirms this—, some deportations [of the population of these cities] were certainly carried out, and a good part of the Assyrian aristocracy was probably massacred by the conquerors. However, Assyria was a vast and densely populated country, and outside the few destroyed urban centers life went on as usual. This is proved by a recently discovered post-imperial archive from the Assyrian provincial capital Dur-Katlimmu ..., which contains business documents drawn up in Assyrian cuneiform more than a decade after the fall of Nineveh. Apart from the fact that these documents are dated to the regal [*sic*] years of a Babylonian king, Nebuchadnezzar II, nothing in their formation or external appearance would suggest that they were not written under the Assyrian Empire. Another small archive discovered in Assur, written in a previously unknown Mannean variety of cuneiform, proves that Assyrian goldsmiths still worked in the city in post-empire times, though now under Median command.

“Moreover, over a hundred Assyrians with distinctively Assyrian names have recently been identified from many Babylonian sites, dated between 625 and 404 B.C. [or well into Persian times].”<sup>178</sup>

Now all this documents that the Assyrians were not annihilated when their empire fell supposedly to the Neo-Babylonians. It further proves that the Neo-Assyrians existed well beyond the fall of their empire. One is left to wonder why they clearly survived into Median times but why they supposedly vanished from Persian times. And that is the problem for the established chronology: although there is supposedly extremely little in the stratigraphical record of Neo-Assyrian material in the Persian period, we nevertheless have a great wealth of Neo-Assyrian material directly beneath the Hellenistic Greeks which undeniably places them in Persian

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<sup>178</sup> Simo Parpola, Assyrians after Assyria, Internet site  
[<http://www.nineveh.com/Assyrians%20after%20Assyria.html>], pp. 1-2

times. Parpola discusses the fact that the Persians had elevated Neo-Assyrians to important administrative offices:

“Many of these names contain the divine [Neo-Assyrian] name Ashur, and some of the individuals concerned occupied quite high positions: one Pan-Ashur-lumur was the secretary of the [Persian] crown prince Cambyses under Cyrus II in 530 BC.

“Distinctively Assyrian names are also found in later Aramaic and Greek texts from Assur ... and continue to be attested until the beginning of the Sasanian period [after the Seleucid Hellenistic Greeks].”<sup>179</sup>

Above and beyond all this, Parpola shows that in Persian times

“[u]nder the Achaemenid [Persian] Empire, the western areas [of Assyria] annexed to Babylonia formed a satrapy [province] called Athura (a loanword from Imperial Aramaic Athur, ‘Assyria’), while the Assyrian heartland remained incorporated in the [Persian] satrapy of Mada ... Both satrapies paid yearly tribute and contributed men for the military campaigns and building projects of the Persian kings.”<sup>180</sup>

While these Neo-Assyrians were obviously a vibrant and important part of the Persian empire, Parpola has told us that in the period after their fall there is a “near-total lack of information from Assyria itself ...” One cannot have it both ways—stratigraphically and historically. The stratigraphy fully supports Heinsohn and Sweeney’s findings that a great deal of Neo-Assyrian material lies directly beneath that of the Greeks but somehow all these numerous materials, including documents, have nothing to do with this fact.

A further problem that we earlier discussed regarding the Old Babylonians is that there is evidently nothing to explain their downfall. This is precisely the same problem we have for the downfall of the Neo-Assyrians: let us recall that J. Joseph informed us that “cuneiform documents ... had ceased to exist

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<sup>179</sup> *ibid.*, p. 2

<sup>180</sup> *ibid.*

... even before the fall of the Assyrian empire.”<sup>181</sup> Saggs states about the downfall of the Neo-Assyrians:

“Within forty years of the rampages of Assurbanipal’s army through Elam, the Assyrian empire was at an end. Hard facts for the detailed sequence of events by which this came about are few. All we have are scattered clues.”<sup>182</sup>

Peter Bedford of Edith Cowan University in Perth, Western Australia, reports:

“The rapid demise of [Neo-]Assyria from its zenith ... to its defeat at the hands of the Medes ... has long confounded historians. The sources [that document this] are scant and problematical.”<sup>183</sup>

Saggs outlines the chronology for the collapse of the Neo-Assyrian empire, but adds the following *caveat*:

“This provides the material for an elaborate chronological jigsaw puzzle, which in view of the large number of pieces missing—scholarly ingenuity may assemble in various ways.”<sup>184</sup>

Olmstead further tells us:

“With startling suddenness our records [of the Neo-Assyrians] cease about 640 [B.C.]. No longer do we enjoy the detailed narrative of the last few years [before the empire fell]. We must [therefore] patch together with much theory the few fragments time has spared ...

“The long reign of Ashur-bani-pal [Assurbanipal] finally came to an end in 626 [B.C.]. Of the circumstances of his death and the

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<sup>181</sup> Joseph, *loc.cit.*

<sup>182</sup> Saggs, *The Might That Was Assyria*, *op.cit.*, p. 117

<sup>183</sup> Peter Bedford, *Empires and Exploitation: The Neo-Assyrian Empire*, Internet site [[www.stanford.edu/group/sshi/Conferences/1999-2000/empires/bedford.pdf](http://www.stanford.edu/group/sshi/Conferences/1999-2000/empires/bedford.pdf)], p. 9

<sup>184</sup> Saggs, *op.cit.*, p. 218

events [leading to the fall of Neo-Assyria] which followed we are entirely ignorant.”<sup>185</sup>

Saggs adds: “We have no knowledge of what was happening for Ashur-etillu-iti or his eventual fate.”<sup>186</sup> This king is one of those who followed Assurbanipal. Saggs further discusses how the Neo-Assyrians rebelled in alliance supposedly against the Neo-Babylonians who were forced to withdraw:

“There is a problem here: how could the Assyrians deploy an army on the Euphrates, powerful enough to compel the retreat of [the Neo-Babylonian king] Nabopolassar, when the previous year they had been so hard-pressed in metropolitan Assyria that Medes had taken cities at both ends of the country? There are two possibilities. Either Assyria made a truce with the Medes (a purely speculative suggestion, without independent evidence), or the Medes had to withdraw to face some other threat.

“The [Neo-]Babylonian records of this time say nothing of the Scythians but do mention a people called Ummanmanda. This term probably denotes tribal hordes from the north of which the Scythians were a part ... Nabopolassar must have been a party to this for in 612 [B.C.] he joins the Ummanmanda and the Medes in besieging Nineveh. The city fell within three months, a surprisingly brief period in view of the fact that the comparable city of Babylon withstood the Assyrian army, masters of siegecraft, for well over a year.”<sup>187</sup>

But as Jonsson points out, “The term used for the Medes ... ‘Ummanmanda’ has often been taken to refer to, or at least include, the *Scythians*. This hypothesis appears to be untenable in the light of recent research.”<sup>188</sup>

The entire chronology of the fall of the Neo-Assyrian empire is a mountain of guesses, assumptions, and unsupported theories, a fiction passed on by the historians as historical reality.

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<sup>185</sup> Olmstead, *History of Assyria*, *op.cit.*, p. 627

<sup>186</sup> Saggs, *loc.cit.*

<sup>187</sup> *ibid.*, pp. 119-120

<sup>188</sup> Jonsson, *op.cit.*, p. 233, fn. 54

With regard to the period prior to the Neo-Assyrian empire, we encounter the same problem that we discovered with that of the Old Babylonians. It, too, is a Dark Age [Ginenthal, *op.cit.*, pp. 484-487]. The Neo-Assyrian empire was plucked out of thin air and placed into the ancient historical chronology with Dark Ages preceding its rise to prominence and following its disappearance into oblivion when it fell. The facts surrounding the collapse of this empire do not exist and are woven out of pure assumption upon assumption. This is hardly the stuff of history.

If indeed the Neo-Babylonians are the last group of Persians in Babylonia, then it is just as it was with the Old Babylonians and Neo-Assyrians: they would have left almost nothing of their material goods because they, too, did not greatly interfere in the state or have great administrative contingents of their own people there, but allowed as far as sensible that their Babylonian agents would tend to their interests. This is just the case; as Amelie Kuhrt shows,

“... artifacts, texts and excavated sites that can be dated to the Achaemenid [Persian] period (or, indeed, the Neo-Babylonian one) ... are extremely sparse, making it difficult to present even a sketchy picture of this enormous and crucially important area.”<sup>189</sup>

In terms of stratigraphy this is yet another problem for denudation processes. And isn't it interesting that the identical problem of missing stratigraphy should also be found for the Persians in Babylonia and Assyria where the Persians are rarely found in the strata while their *alter egos* are always found in the strata directly beneath the Greeks?

By placing the Neo-Babylonians after the Neo-Assyrians, in Persian times and after, all the Dark Ages associated with them vanish as well. With the disappearance of all these unnecessary Dark Ages the great literary, scientific, and mathematical achievements of the Babylonians and Neo-Assyrians are well preserved to be learned by and passed along to the Greeks without interruption.

What is unique in this regard is the work of David Brown, *Mesopotamian Planetary Astronomy—Astrology* (Groningen, Netherlands 2000). In this work Brown demonstrated something quite astonishing if we keep the Neo-

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<sup>189</sup> Amelie Kuhrt, “Achaemenid Babylonia: Sources and Problems,” *Achaemenid History IV, Centre and Periphery*, H. Sancisi, A. Kuhrt eds. (Leiden, the Netherlands 1996), p. 177

Assyrians in the 8th-7th centuries B.C. He indicates that the astronomers/astrologers were capable of predicting the planetary positions of the then known bodies well into the future, and therefore also of retrocalculating well into the past. However, it is generally believed that this ability did not develop until around Persian times; Jonsson tells us:

“Finally in the Persian and Seleucid eras, they had developed a very high level of scientific and mathematical astronomy that had never been reached by any other ancient civilization.”<sup>190</sup>

But with Heinsohn and Sweeney’s thesis which places the Neo-Assyrians in the Persian era, this knowledge is not at all as anachronistic as it seems. During that period they would have been privy to all the astronomical knowledge that came before them and would then be capable of making this kind of momentous discovery. Again, as with so much else, this makes perfectly good sense when we move the Neo-Assyrians into Persian times.

At this point it is appropriate to show in greater detail that there are many problems associated with the fall of Neo-Assyria and the reign lengths of the kings during this Dark Age. Amelie Kuhrt has elaborated on them:

“The sudden disappearance of the [Neo-]Assyrian empire, which seemed impregnable and solid in the period of the seventh century, is a phenomenon as yet poorly understood ... The change is sudden and abrupt, the process and circumstances, and possible underlying causes, all remain obscure.

“One reason for the difficulties in understanding Assyria’s collapse is that the sources are very problematical ... the chronicles do not provide any direct information on the situation within [Neo-] Assyria or in the rest of the empire.

“Second, [Neo-]Assyrian royal inscriptions and archival material are very sparse for this period. This must partly be due to succession problems in [Neo-]Assyria ... Where we should date these struggles [for power] in the period ... is debated ...

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<sup>190</sup> Jonsson, *op.cit.*, p. 155



“Third, a reasonable number of contemporary dated documents from Babylonia exist which can provide information, from their date-formulae, as to who was recognized as king in what Babylonian city at any one point. By drawing up tables of these dates, shifts in political control, civil wars, struggles for power, and the existence of several different claimants to the throne can be charted. But fixing exact dates is by no means easy to do, as the texts date by regnal years, but we are not certain from what point a particular ruler’s regnal years are to be counted. As a result, the reconstruction of events in the 620s is so speculative that there are at least three different hypotheses in existence, all of which have arguments in their favour (as well as against), and not one of which is wholly satisfactory. ... A further problem is created by some texts from Nippur, dated while the city was under siege ... —it looks as though different factions (pro- and anti-Assyrian) dominated the city at various times, which created such confusion that the citizens were unable to date documents according to a specific king and instead simply dated by ‘siege years’. Again there is no precise indication as to when to date the siege. Late Babylonian king-lists (and other material) only add to the confusion: in their attempt to present an orderly roster of kings for this period of chaos, they plump for certain rulers and ignore others, and they fill chronological gaps in a variety of ways which cannot reflect reality. There is absolutely no way of harmonizing all the available evidence, and the chronological problems remain insuperable.”<sup>191</sup>

More instructive from the chronological viewpoint is what Kuhrt has to say about the reigns of Kandalanu and Ashurbanipal:

“Another contentious issue is the identity of Kandalanu. He appears as king of Babylonia after the death of Ashurbanipal’s brother, Shamash-shum-ukin, in 648, and he definitely continued to rule in Babylonia until 627 ... The obvious assumption is that Kandalanu was a subject-ruler installed by the [Neo-]Assyrians. But some scholars have argued that Kandalanu is an alternative name for Ashurbanipal ... If this is right, then Ashurbanipal’s reign ended in 627 [B.C.], and we have to try to accommodate four different claimants to power in

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<sup>191</sup> Amelie Kuhrt, *The Ancient Near East ...*, *op.cit.*, pp. 540-542

Babylonia in as many years (627-623 [B.C.]). This is by no means impossible ... But if Kandalanu is not another name for Ashurbanipal, as has been strongly, and convincingly, argued by Brinkman ..., then we can assume that Ashurbanipal died *c.* 630 [B.C.] ... no documents are dated by him after 631 [B.C.]. [There are other problems related to the timing of Ashurbanipal's death concerning the accession to the throne] ... None of these questions [of accession] can be resolved conclusively at present. Another added difficulty is that we only know about select events inside Babylonia, and hardly anything about what was happening elsewhere in the empire."<sup>192</sup>

Thus, we have Ashurbanipal dying either in 627 or *c.* 630 B.C. That is a period of three years and if the 630 B.C. date for his death is correct, then all astronomical data are problematical because these require that Ashurbanipal died in 627 B.C. So when does the Neo-Assyrian empire fall? The only evidence we have is the agreed-to assumption that in spite of all the problems related to dating its fall, it had to have fallen in 609 B.C. which, according to Carl Olof Jonsson "is the prevailing view among leading authorities today."<sup>193</sup> And dutifully he presents these, giving the reader the impression that this is apparently true:

"In 610 [B.C.] the Babylonians and their allies took Harran, and Ashur-uballit with the wreckage of his forces fell back across the Euphrates into the arms of the Egyptians. An attempt (in 609 [B.C.]) to retake Harran failed miserably. Assyria was finished.'—Professor John Bright, *A History of Israel*, 3rd ed., (Philadelphia ... 1981), p. 316.

"In 609 B.C.E. 'Assyria ceased to exist and her territory was taken over by the Babylonians.' — Professor D.J. Wiseman in *The New Bible Dictionary*, J.D. Douglas (ed.), 2nd ed. (Leicester, Eng. ... 1982), p. 101 [etc.]"<sup>194</sup>

What we have is a "prevailing view among leading authorities," not testable, falsifiable evidence. Kuhrt's comment regarding this evidence is:

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<sup>192</sup> *ibid.*, pp. 542-543

<sup>193</sup> Jonsson, *op.cit.*, p. 233

<sup>194</sup> *ibid.*, pp. 233-234

“What are we to make of this jumble of information? The only things that can be said with certainty are, first, that Ashurbanipal was definitely ruling in 631 [B.C.], but the date of his death is unknown. Second, Kandalanu certainly ruled in Babylonia until 627 [B.C.].”<sup>195</sup>

Nevertheless, based on accepting the established chronology, it is taken that “from 608 [B.C.] onwards do all traces of the [Neo-]Assyrians disappear from the record.”<sup>196</sup>

These Dark Ages are Black Holes that have been invented by historians to swallow up and explain away the chronological messes created by the established chronology.

### PERSIAN, NEO-ASSYRIAN, NEO-BABYLONIAN STRATIGRAPHY

As with the Old Babylonians, Heinsohn and Sweeney hold that the Neo-Assyrian strata located directly beneath the Hellenistic Greeks are those of the Persian rulers of Assyria. An example of this stratigraphical arrangement is found in Joan and David Oates’s work. “After the disappearance of the final remnants of the [Neo-Assyrian] city ... of Kahlu [Nimrud] there is very little surviving evidence until the Hellenistic period.”<sup>197</sup> Sweeney shows:

“A typical Assyrian stratigraphy (as for example at Tell Hamadiyah, Munbaqa, Barak, Balawat, and Nimrud) looks thus:

“Hellenists (after 330 BC)

“[below them] Middle and Neo-Assyrians ...”<sup>198</sup>

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<sup>195</sup> Kuhrt, *op.cit.*, p. 544

<sup>196</sup> *ibid.*, p. 545

<sup>197</sup> Joan Oates, David Oates, *op.cit.*, p. 258

<sup>198</sup> Sweeney, *Ramessides ...*, *op.cit.*, p. 6

Here we run into an important problem. According to the established chronology, the Neo-Assyrians ruled from about 1100 to 612 B.C. or about 280 years before the Greeks instead of over 1000 years for the Old Babylonians. What must therefore be explained away is that both the Old Babylonian and Neo-Assyrian strata lie directly beneath the stratum of the Greeks. Some form of denudation has to be at work to accomplish this. Here, then, is the problem for the established chronology: While for the Old Babylonians, as Cochrane has argued above, denudation processes removed as much as 1000 years of archaeological materials sandwiched between the Old Babylonians and the Greeks, for the Neo-Assyrians and the Neo-Babylonians these identical denudation processes removed only about 280 years of these materials in order to leave the identical arrangement —Neo-Assyrian strata directly below Greek strata. The Neo-Assyrian stratum was closer to the surface than the Old Babylonian stratum and therefore would, as Cochrane proposed, be more prone to be denuded, dug into by the Greeks in their rebuilding program, etc. Why in the case of the Old Babylonians did these denudation processes work so vigorously as to remove up to 1000 years of strata, but for the Neo-Assyrians and Neo-Babylonians work so weakly as to remove only about 280 years, and in both cases miraculously stop at only these particular civilizations? No-one has explained why these highly selective denudation processes only halted when they reached either the Old Babylonian or the Neo-Assyrian strata, nor, I dare say, can such selectiveness of denudation be explained away except by miracles or culling processes.

Surely, if these denudation processes were a reality and were actually at work then at some sites we would find Sumerian/Chaldean strata directly below the Greeks or Akkadian strata directly below the Greeks, or Mitanni/Hurrian strata directly below the Greeks. But such is not the case. In fact, if these denudation processes were actually doing what Cochrane suggests, then no-one should ever use archaeological stratigraphy because any stratum found above another could have been the result of denudation processes removing other strata that were originally sandwiched between them. If one can say denudation processes removed strata, then they could and would destroy strata everywhere at sites in Mesopotamia and thus one could never trust there to be a true representation of the archaeological chronology left in the ground by these ancient civilizations. Cochrane's denudation suggestion is not only selective but contradictory. This denudation concept is what one must accept in all these cases, not in order to "save the phenomena," or to account for the evidence, but only in order to save the established chronology. These denudation arguments are beyond rational discussion because the entire concept

leads to stratigraphical chaos. They stand only as long as one is willing to suspend one's logical faculties.

More importantly, how does one scientifically test the manner in which these processes worked so vigorously in the case of the Old Babylonians and so weakly with respect to the Neo-Assyrians, but more moderately with respect to the Akkadians or Mitanni, and most vigorously with respect to the Sumerian/Chaldeans? That is the crux of the argument. While geological stratigraphy gives scientific proofs regarding the stratigraphy, archaeological stratigraphy does not; it gives interpretations, and these are subject to all the problems of bias, misunderstanding, etc.

There is one further point that must be stressed before proceeding. One cannot accept the stratigraphical evidence that the Old Babylonians are the *alter ego* rulers of the Persians in Assyria, who left a wealth of materials directly below the stratum of the Greeks, and then reject the stratigraphical evidence of the Neo-Assyrians who also left a wealth of materials directly below that of the Greeks. One may not employ a double standard of inference, saying "Yes, in the case of the Old Babylonians it is obvious because of astronomical/calendrical data, evidence of iron, tin bronze, glass, etc., that they must be Persians followed by the Greeks. But no, the Neo-Assyrians and Old Babylonians found directly beneath Greek strata, who also left a wealth of materials there, ruled at some other age." Once the science has spoken, one may not selectively ignore or dismiss the very same kind of evidence, in this case scientific stratigraphy, when it says the same thing! The Old Babylonians are the Persian rulers in Babylonia because of scientific, technological, stratigraphical evidence, therefore in terms of stratigraphy the Neo-Assyrians must also be brought down to late Persian times.

To disprove all this requires scientific, stratigraphical excavations. I believe these scientific, stratigraphical excavations will never be carried out, so that the established chronology cannot be tested or falsified and can stand forever without testing and without scientific proof.

## **IRON AGE AND NEO-ASSYRIAN/PERSIAN CHRONOLOGY**

In terms of the established chronology, the Hittites created hardened iron implements in the 13th century B.C. and this innovation then spread through

the rest of the ancient Near East. In terms of the short chronology the Hittites were the Lydians living well within the first millennium B.C., and thus the invention and implementation of iron weapons, armor, etc., has its introduction around 700–600 B.C. The Neo-Assyrians as close neighbors of the Hittites would surely have learned the value of this metal from its inception. But if the Neo-Assyrians are, or are contemporary with, the Persians as per Heinsohn, Sweeney, and Rose, then they would only have adopted the use of this important metal very, very late in terms of the long, established chronology. However, in the short chronology they would have employed iron almost as soon as it became well known. According to Radomir Pleiner and Judith K. Bjorkman,

“A short summary of facts leads to the following conclusions: In the thirteenth century [B.C.] the intentional extraction of iron took place under the Hittites, northwest of Assyria. But after the fall of the empire, no ... explosion of iron industry, as suggested by scholars, took place, at least in Assyria. The use of iron remained more or less limited to the royal court.

“Its first practical use was attested for military purposes, including the first type of iron tools. These gradually replaced bronze implements and at the end of the eighth century [700 B.C.] they were made in well developed forms. An earlier period, at least the second half of the eighth century [750–700 B.C.] saw the introduction of several additional types of iron objects, many of them for household use ...

“Without any doubt, there was a full-fledged iron civilization in the seventh century [600s B.C.] in Assyria ... There are neither allusions in written records, nor discoveries of smelting sites, nor dated iron slags among the archaeological remains. The only information on the mode of supply of iron (together with other metal tools, too) consists of lists of spoils or tributes in royal inscriptions of the ninth to mid-eighth centuries [800–750 B.C.]. Then any records of that type cease. Later trade in iron must have developed outside the palace ...

“Two turning points can be observed in the use of iron in Assyria. Shortly after 900 [B.C.] what is called the Iron Age began [in Assyria]. About two centuries later [700 B.C.], a well-developed and

full-fledged civilization based on iron had been established. It is interesting that in Greece the development was more rapid; a similar statement can also be made about the Syro-Palestinian coastal area.”<sup>199</sup>

What we have been told is that the Hittites were able to smelt and carburize iron around 1300 B.C. and then, after they fell, the widespread use of the metal so important to civilization did not greatly develop. “The use of iron remained ... limited to [trinkets in] ... the royal court.” Although its military value was inestimable it only replaced bronze in large measure around 750 to 700 B.C. and the Neo-Assyrians entered a full-fledged iron civilization around 600 B.C. Thus, we have a 200-year iron Dark Age after iron was created around 1300 B.C. and only limited use of this metal is known until 800–750 B.C.; and finally by 700 to 600 B.C. Assyria enters the Iron Age. That is, the Neo-Assyrians took about 700 years after iron was invented for this civilization to become a full-fledged iron civilization!

It is clearly unimaginable that a civilization that knew the Hittites, and had on-going trading relations with them—in fact establishing trading posts in that kingdom—should take 700 years to adopt such a significant metallurgical innovation. On the other hand Saggs informs us:

“But whether the delay in the spread of iron production was due to a Hittite attempt at retaining a monopoly, or to production difficulties, the metal was certainly beginning to be available to Assyrian kings in small quantities from the thirteenth century [B.C.], when we find an iron dagger mentioned. In the twelfth century [B.C.] we find mention at the [Assyrian] court of a blacksmith, to whom two sheep were given at the order of one of the kings; the blacksmith had a good Mesopotamian name, indicating that not only iron but also the knowledge of the working of iron had been exported ... to Assyria. It may have been this court blacksmith or his son who made the iron arrowheads which Tiglath-Pileser I (1115–1077 [B.C.]), a generation later, boasted of using with devastating effect against wild bulls. But it was not until the ninth century [800–700 B.C., some 225 years later] that iron became sufficiently available to equip sizeable numbers of troops with iron daggers. From the same century onwards iron was

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<sup>199</sup> Radomir Pleiner, Judith K. Bjorkman, “The Assyrian Iron Age,” *Proceedings of the American Philosophical Society*, vol. 118, no. 4 (1974), pp. 307-308

used for making scale armour (worn not only by soldiers but also by war-horses), for helmets ...<sup>200</sup>

That is, while iron was supposedly invented by the Hittites around 1300 B.C. and the Assyrians had blacksmiths that could make it about 1125 B.C., the Assyrians failed to equip their army with iron daggers until around 885-875 B.C. or almost 250 years later. This, too, is thoroughly unimaginable.

In terms of the short chronology, the Lydians/Hittites exist in the earlier to mid-first millennium B.C. and invent iron around 700–600 B.C. As the Neo-Assyrians are, or are contemporary with, the Persians, they come into possession of iron technology almost immediately after its development. There is no iron Dark Age separating its creation from its wide-range spread and utilization by the Neo-Assyrians/Persians. The Egyptians also came into the Iron Age around this time which indicates the same development and chronology. It is only because of the established chronology that we have iron Dark Ages just as with so many other aspects of historical development.

Again, we ask: Why did the Phoenicians, who knew of iron and used it, fail to trade iron goods to the Assyrians? We were informed that iron had come to Syro-Palestine earlier than to the Assyrians. Therefore the Phoenicians in Syro-Palestine had to know how to manufacture iron, but they then failed to trade it with Assyria. Since the value of iron as a material for arrow-heads was known to Tiglath-Pileser I (1115-1077 B.C.), it is impossible to believe it took around 225 years for the Assyrians to arm their soldiers with iron daggers and another hundred years to create iron armor for these men and their war horses. This would be comparable to Thomas Edison inventing the light bulb and then having this invention only come into wide use hundreds of years later.

Only with the short chronology do these enormous paradoxes, really contradictions, disappear as do all the others. In the unit on the Mitanni and Medes we will run into this same situation, but also in several areas that say that the “technological age” of the ancient Near East occurred in the first millennium. It will be shown that from around 750 to 300 B.C. the ancient Near East had its own Industrial Revolution just as modern western civilization had one, starting in the early 1800’s.

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<sup>200</sup> Saggs, *op.cit.*, p. 184



## AGRONOMY, INFLATION, FAMINE

As was pointed out above, the end of Hammurabi's Old Babylonian/Persian empire was accompanied by a great period of inflation and starvation. This was attributed to the salt poisoning of the southern plain of Babylonia. The very same is then described for the end of the Persian Empire. If, as presented by Heinsohn and Sweeney, the Neo-Assyrians are the Persian rulers of Assyria, then toward the end of that period there should have begun a gradual salting over of the lands of Babylonia which culminated during the Neo-Assyrian period with the complete destruction of its soil, incredible levels of inflation, starvation, and all the horrors of a society in the grips of a catastrophic famine. Early on, the land should have begun to become somewhat less productive. Regarding this condition, Saggs shows:

“Despite the agricultural wealth of the temples, the Neo-Babylonian empire was probably in a considerably less healthy state economically than the [Neo-]Assyrian empire had been ...”<sup>201</sup>

This in turn was followed by famine and is described with all its ramifications which took place at the end of the Persian/Neo-Babylonian era. The description of the beginning of these events in Persian/Neo-Assyrian times is described by James *et al.*:

“A joint study by meteorologist J. Neumann and Assyriologist S. Parpola developed the theory that the decline of both Assyria and Babylonia in the early 1st millennium was due to a protracted drought. Their primary evidence is literary—from the meagre records ... and later texts referring to this period. The documents contain several references to bad harvests, high grain prices, and famines—according to a late chronicle, the Assyrians were even driven to cannibalism during the reign of Tiglath-pileser I. As a contrast, they have assembled numerous [Neo-]Assyrian texts of the 8th–7th centuries BC which describe heavy rainfall, snow, swollen rivers and floods.

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<sup>201</sup> Saggs, *The Greatness that was Babylon*, *op.cit.*, p. 267

“Physical traces of this climatic upheaval were sought by Neumann and Parpola in palaeoclimatic studies, which suggest a widespread period of drought towards the beginning of [this period]. In most cases, however, it is difficult, if not impossible, to correlate the data with the historical record.”<sup>202</sup>

The reason for this is that the Neo-Assyrian chronology is in error and that the cause for this largely lay in the Old Babylonians/Persians failing to send sufficient food supplies to Assyria.

One does not need a prolonged drought to bring about a famine in Assyria. One or two years are sufficient to bring the land to its knees. As Saggs explains:

“The agricultural season in Assyria started (as it still does today) with ploughing and sowing in late October or November, ready for the rains. In good years the first showers come in November, though substantial rainfall can occur up till April. But the rains do not always arrive in November, and if they are delayed too long crop failure can ensue. H.A. Layard ... encountered this in 1847:

“It frequently happens that the season passes without rain. Such was the case this year. During the winter and spring no water fell. The inhabitants ... looked in despair upon the cloudless sky. I watched the young grass as it struggled to break through the parched earth; but it was burnt up almost at its birth. Sometimes a distant cloud hanging over the solitary hill of Arbela, or rising from the desert in the far west, led to hopes, and a few drops of rain gave rise to general rejoicing. ... But disappointment always ensued. The clouds passed over, and the same pure blue sky was above us.’

“For ancient [Neo-]Assyria (as indeed for the same region [in the] last [19th] century) this was a catastrophe, threatening famine. Consequently the prospects for the harvest were a matter of

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<sup>202</sup> James *et al.*, *op.cit.*, pp. 288-289

considerable concern throughout the land, sufficiently important for administrators to report to the king on the current situation as to rains and crops.”<sup>203</sup>

But as we understand, Assyria could have bought or traded food stuffs from food prosperous Babylonia to cover its losses during years of drought. The apparent reason for the Neo-Assyrians’ inability to do so points strongly to the situation that had begun to prevail in Babylonia. The land there was evidently still in production but the levels of the harvest had begun to decline and the Babylonian population would not have permitted a foreign province of Persia to take away this precious resource and create a situation in which its own people would starve. Each province was thus in large measure dependent on its own agriculture to provide sustenance for its population. This could be supplemented in years of drought by trade with Babylonia, the food basket of Mesopotamia before that land began to lose its fertility from irrigation salinization. The amount of arable land in Babylonia was reduced in size during the last many decades that ultimately led to the complete destruction of the soil. “Despite the great agricultural wealth of its temples, the Neo-Assyrian empire suffered severe economic constraints. Inflation was rampant, with prices rising 50 percent. Babylonia suffered from ... famine.”<sup>204</sup>

In Babylonia itself at the end conditions became impossible:

“Famine now appears in Babylon, attributed ... to the impiety of the people, though more soberly traceable to general economic conditions. The ... building activities of Nebuchadnezzar and Neriglissar were producing inflation, and the level of prices rose by up to 50 percent [early on] ... (the trend was not halted by subsequent developments, and the price rise [at the end] ... amounted to 200 percent). The results are seen in commercial documents. One such, dated in the first year of Nabu-na’-id [Nabonidus] relates to a loan of corn made by a herdsman whose cattle were starving. Other texts concern the handing over of children to the temples as slaves, an evident result of extreme indigence. The withdrawal of manpower for years at a time from productive labour on canals to unproductive temple-building or war also contributed to a decline in the land’s productivity,

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<sup>203</sup> Saggs, *op.cit.*, pp. 163-164

<sup>204</sup> Karen Rhea Nemet-Nejal, *Daily Life in Ancient Mesopotamia* (Westport CT/London 1998), p.

whilst the economic situation was further aggravated by Median control of the routes to the east and north. ...

“It has been pointed out that in his inscriptions Nabu-na’-id scarcely mentions the economic aspects [and repercussions] of his undertakings ... However, commercial and administrative documents ... prove beyond doubt, if only incidentally, that Nabu-na’-id was *au fait* with economic problems.”<sup>205</sup>

Again, it is presented that inflation was created by heavy taxation as with the Persian rulers of Babylonia. As Roux suggests:

“Increased expenditures [on building and maintaining a large military] and reduced income [brought about by competition from Greek, Ionian, Lydian commerce, etc.] drained heavily on the royal treasury and deeply affected the general economy of Babylonia. A study of the hire and sale contracts reveals a marked increase in prices between the beginning and the end of the Neo-Babylonian period. ... Under Nebuchadrezzar 1 shekel could buy from 2 to 4 *qa* of cultivated land, but only 1 or 2 *qa* under Nabonidus. A similar increase affected foodstuffs and clothes and other daily necessities ... The average monthly salary of an unskilled labourer, for instance, was about 1 shekel; with this he could purchase 2 bushels of grain and 3 bushels of dates, just enough for him to feed his family. In consequence, people took to borrowing money on a long-term basis, and credit inflation rendered the [Neo-]Babylonian economy even more unhealthy.”<sup>206</sup>

These economic problems, as with the Old Babylonians and Persians, are related to the destruction of the land by salt poisoning via irrigation agriculture. The historians have invented imaginative scenarios to explain this problem. Only Jacobsen understood that the destruction of the land was brought about by salinization. No-one has explained how, considering the Old Babylonians had rendered the land a salt desert, there was a method employed by any of the other empires which supposedly followed it to restore the land. Thus historians have been

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<sup>205</sup> Saggs, *op.cit.*, pp. 147-148

<sup>206</sup> Roux, *op.cit.*, 3rd ed., p. 403

forced to deny the very facts of scientific agronomy in order to sustain their elongated chronology.

When we apply and correlate the evidence of agronomy, inflation, and famine to Babylonia as these relate to Heinsohn's short chronology, we find convergence of all three of these elements with climatology, one of the principal points of this book as well as volume I:

After around 750 B.C., because of a major pole shift which moved Babylonia about 10 degrees south into the desert belt, people of the Babylonian plain began to increasingly irrigate their lands with water, principally from the Euphrates River. This led to abundance and, through trade with the rest of Mesopotamia, to great wealth.

By around 450 to 400 B.C., the land available for agriculture began to gradually diminish so that within 100 years the entire plain was a wasteland. It was during *this* period that areas of drought outside the plain would begin to have difficulty obtaining food through trade with Babylonia, because Babylonia itself needed this commodity. In the final phases of the salinization of the plain, there would be inflation that gradually overwhelmed the people's financial capacity to pay for food, and led to the selling of children into slavery or girls to the temples as temple prostitutes by starving families to keep themselves and their children alive. Ultimately this led to the almost total abandonment of the plain except for nomadic herdsman, and to the collapse of cities, civil society, and all the rest that followed.

None of these correlations was known to me when I began this research. Just as with the stratigraphy wherein all these civilizations' strata are always located directly under those of the Greeks as a corroboration for the short chronology, we have a unique set of circumstances wherein at the end of all these civilizations, Persian/Old Babylonian, Persian/Neo-Assyrian, and plain Persian, just by coincidence each documents at the end inflation and famine in Babylonia. In terms of the short chronology, especially that of Gunnar Heinsohn, this is not a coincidence at all, but an expectation that must follow inexorably from the chronology. Although Heinsohn never raised the issue for his readers, the occurrence of all three periods of inflation and famine strikes a rather harmonious chord with his chronology. That form of correlation, corroboration, and confirmation for all three civilizations is exactly what one would expect to occur with a chronology that is based on valid forensic historical evidence. No *ad hoc* hypotheses are required to make this

evidence agree with the chronology; the agreement is a natural fall-out that must occur with a correct reorganization of Mesopotamian history. But this is in no way the case for this inflation and famine evidence as it relates to the established chronology.

For the established chronology, historians had to invent *ad hoc* explanations for this event once they disconnected the chronology from the agronomy and climatological facts. Inflation in Babylonia rose because of taxation under the Persians while lack of money and a stable food supply leads to exactly the opposite, deflation. Inflation in Babylonia rose because the kings took farmers away from the land to build temples, etc. Yet even when we have mass deportation, people left behind could certainly still farm the land to feed themselves if it was not poisoned by salt. The historians had the key to the question of Babylonian inflation and famine when Jacobsen correctly proposed that in Old Babylonian times salinization had destroyed the soil of the southern plain. However, they then closed their eyes and their minds to the science and held on to the established chronology with the unproven and unprovable claim that the Hurrians for some enigmatic, unknown reason revitalized the land so that civilization could continue and prosper on the plain. But none of this fits the science and thus it makes no sense. What the historians have done to salvage their chronology is best described by James Henry Robinson: “Most of our so-called reasoning consists in finding arguments [however unscientific] for going on believing as we always do.”<sup>207</sup>

The only chronology that gives a clear-cut scientific answer to all three inflations and famines in Babylonia, as these relate to agronomy and irrigation salinization, is provided by Heinsohn’s and Velikovsky’s theses. We do not have to invent any of these *ad hoc* explanations that are unrelated to science or reality. What the historians have presented is not science but assumptions, rationalizations, and deductive reasoning (as opposed to induction). Rather than follow the inductive evidence of irrigation agronomy they have embraced fantasy. As Bertrand Russell said of Aristotle:

“Aristotle maintained that women have fewer teeth than men. He was married twice, but it never occurred to him to ask either of his wives to open her mouth so he could count her teeth.”<sup>208</sup>

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<sup>207</sup> James Henry Robinson in *Leo Rosten’s Carnival of Wit*, *op.cit.*, p. 413

<sup>208</sup> Bertrand Russell, *ibid.*, p. 431

Historians maintain that irrigation farming lasted 3000 years or more in southern Mesopotamia. Some major researchers have shown that this form of agriculture could only last about 400 years before the soil was poisoned by salt. It never occurred to them to open their minds to this scientific fact and count the length of time that ancient civilization could be sustained in southern Babylonia before collapsing. Thus they failed to see that the correlation of inflation, famine, and all the rest for the Persians, Old Babylonians, and to a small extent also the Neo-Assyrians, fits the science and a short chronology.

Let us then examine this problem of salinization in terms of the established chronology of the Neo-Assyrians and Neo-Babylonians. Based on the chronology, these empires ruled from 911 to 539 B.C., or 382 years. *Since irrigation agriculture clearly goes back to the early start of civilization on the Babylonian plain and can only have been carried on for about 400 years*, there could not have been any other empires in this region prior to these two nations. Thus, Neo-Assyria would have to be the first and only major empire to exist in Mesopotamia and no others could have come before it.

To keep the Neo-Assyrians in their established place requires that they were the first society to rule in Mesopotamia! There would be no nation or tradition before the Neo-Assyrians in existence before 911 B.C. It would demand that civilization in terms of archaeology, linguistics, pottery dating, etc., did not exist prior to the Neo-Assyrians. That is the great and stunning contradiction created by the established chronology. Since the Neo-Assyrians and Neo-Babylonians ruled Mesopotamia for 382 years and irrigation agriculture can only be carried on for about 400 years, that established chronology cannot stand.

In terms of the short chronology, this problem does not exist, and to add to its support, irrigation agriculture ends in southern Mesopotamia with inflation, famine, and the desertion of the major cities and towns at the end of the Persian/Neo-Assyrian period. As George Santayana justly wrote: "History is always written wrong, and so always needs to be rewritten."<sup>209</sup> The established chronology of Neo-Assyria and Neo-Babylonia does not conform with the science of agronomy, etc.

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<sup>209</sup> Santayana, *ibid.*, p. 224

## THE NEO-ASSYRIAN/NEO-BABYLONIAN/PERSIAN EMPIRE AND PEOPLE

Because the Neo-Assyrians lived in the first millennium B.C., as did the Persians, the various technological developments, such as iron, tin bronzes, glass production, cannot be invoked as support for their placement. The major scientific evidence for putting them in the first millennium is stratigraphy, technology, and to a lesser extent agronomy as this relates to irrigation agriculture, inflation, and famine. But astronomy does place them in Persian times. We thus now turn to the non-scientific, non-technological evidence of a sociological, administrative, and religious nature. And I reiterate, these materials are the ones most susceptible to criticism. However, to do so without fully addressing and answering the evidence of stratigraphy, technology, and agronomy, and especially astronomy which places them in Persian times, that is, by ignoring that evidence to resort to documentary materials, shows that the critic has no forensic evidence upon which to build a chronology.

As we reported above, the Old Babylonians/Persians did not interfere in matters of religion, etc., and allowed, as far as was reasonable to them, the various provinces to act as rulers and agents on their behalf. Nevertheless, there was an active administrative bureaucracy that ran the empire from day to day in close touch with the central government. If that is so for the Old Babylonian/ Persian empire, it should also be the same for the Neo-Assyrians/Persians.

Therefore, just as the Persians were known to have given the satrapy of Babylonia special status, so too should this be the case with the Neo-Assyrians who were Persian rulers. In this respect, van de Mieroop reports:

“The question of ... control of Babylonia was never solved by the [Neo-]Assyrians. While they could not accept the existence of a disobedient neighbor at their southern border, there seems to be reluctance to take over the country openly. Probably an acknowledgement that Babylonia had fundamentally influenced Assyria’s culture and religion led to a sense of respect that prevented similar treatment to that meted out to other regions.”<sup>210</sup>

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<sup>210</sup> Marc van de Mieroop, *A History of the Ancient Near East*, 2nd ed. (Oxford, UK 2006) p. 236



With respect to finding installations of Neo-Assyrians in Babylonia, these would surely be absent since the Persians, not the Neo-Assyrians, ruled this southern region. Again van de Mieroop shows: “No installations where such [Neo-Assyrian] military troops could have been housed are visible in the archaeological record before the appearance of arsenals in Assyria ...”<sup>211</sup>

It has generally been presented that the Neo-Assyrians were a violent people devoid of administrative skills whose empire was simply out to destroy and loot the nations it defeated. On the contrary, however:

“Byron’s celebrated couplet –

““The Assyrian came down like the wolf on the fold,

““And his cohorts were gleaming in purple and gold–’

“does little justice to the Assyrian system of military government [according to Saggs]. The Assyrians were not simply out for loot. The lands which came under their control they not only attempted to administer, but succeeded in administering, by ancient standards, with admirable efficiency.”<sup>212</sup>

Just as with the Old Babylonians/Persians who raised the merchant class to great importance because trade was an instrument for producing wealth which could be taxed, we are told in the Bible by Nahum, III:16, who taunts Assyria: “Thou has multiplied thy merchants above the stars of heaven.”<sup>213</sup> As for the level of efficiency, their administrative skills were identical to the Old Babylonians/Persians.

“Far from being a despotic militarism holding down conquered races by mere brutal harshness, Assyrian imperialism owed much of its success to a highly developed and efficient administrative system, and to the attention of an energetic bureaucracy to the day-to-day trifles of government.”<sup>214</sup>

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<sup>211</sup> *ibid.*, p. 235

<sup>212</sup> Saggs, *op.cit.*, p. 238

<sup>213</sup> *ibid.*, p. 239

<sup>214</sup> *ibid.*, p. 240

“Administrative reforms [were] undertaken by Tiglath Pileser [III] in reorganizing his civil service and the provinces ... The reorganized provinces ... were subdivided into smaller areas under the control of lesser officials who were generally speaking responsible to the governor but who had the right to make complaints and representations directly to the king ...”<sup>215</sup>

While it is admitted: “Although the Royal annals of the [Neo-] Assyrian kings [are] our main quarry for knowledge of Assyria in the first millennium, [they] give but scattered and scanty hints of the administrative machinery.”<sup>216</sup> “The letters ... show that provincial governors were in constant day-to-day touch with the central government.”<sup>217</sup> As with the Old Babylonian/Persian empire the Neo-Assyrian/Persian was described thus:

“... loosely bound there were those states whose rulers had, from motives of prudence, brought tribute to the Assyrian king as the token of a friendly attitude which depending upon circumstances, might properly be regarded as lying somewhere between alliance and vassaldom. The rulers of such states would receive in return for their tribute the concrete advantage of an Assyrian guarantee ... against aggression.”<sup>218</sup>

As with the Old Babylonians/Persians, if the vassal ruler rebelled or refused to pay tribute, he

“... was liable to call down [upon himself] military action ... an attack on the recalcitrant territory with a small force, pruning it back by annexing the outlying parts to a more reliable subject state or province. The [Neo-]Assyrians would then intervene in the internal affairs of the state to replace the unreliable ruler by another ... acceptable to Assyria, or ... to reinstate the Assyrian protégé or member of his family.”<sup>219</sup>

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<sup>215</sup> *ibid.*, p. 106

<sup>216</sup> *ibid.*, pp. 244-245

<sup>217</sup> *ibid.*, p. 249

<sup>218</sup> *ibid.*, p. 241

<sup>219</sup> *ibid.*, pp. 241-242

With regard to Babylonia the Neo-Assyrians/Persians just as the Old Babylonians/Persians gave it special status:

“In some cases, particularly inside Assyria proper *as well as Babylonia*, the municipal administrations ... retained some measure of autonomy ... granted or confirmed by royal charter in return for support at times of internal crisis, particularly rebellion or disputed succession ...”<sup>220</sup>

Related to the good and efficient administration of the empire the Neo-Assyrians organized identical forms of rapid communication as the Persians:

“Communication was by means of professional messengers ... The normal method of communication with the central government was ... by means of an official messenger ... who traveled along the ‘royal roads’. These royal roads were the main arteries of the empire, and though they were probably cleared tracts rather than made-up roads in the modern, or Roman, sense, they were certainly well enough defined to be used as boundaries between provinces, and level enough to permit the passage of large armies accompanied by chariots. Beautifully made and completely level roads of stone blocks and cobble stones have also been found, but these are restricted to the streets of the metropolis or the immediate approaches thereof. Along the roads across the empire about a day’s journey (twenty to thirty miles) apart, were posts permanently manned by government troops, containing a change of horses or mules for the use of messengers to the next stage. A *mar shipri* [a messenger] would thus travel mounted from stage to stage, with a small escort of troops, carrying the tablets containing his governor’s dispatches or the king’s replies. The post-animals were restricted to particular routes, no doubt to ensure that each station should always have beasts available ...”<sup>221</sup>

Now this no doubt is exactly what existed for the Persians as well as the Old Babylonians/Persians. Saggs goes on to say:

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<sup>220</sup> *ibid.*, p. 248

<sup>221</sup> *ibid.*, p. 249

“One may recognize that the famous Persian system of post-roads throughout the Persian empire was not a Persian innovation but the development of a system employed some centuries earlier by the [Neo-]Assyrians.”<sup>222</sup>

Of course, Saggs and his colleagues could not accept that these communication post-road systems are identical because they were created by identical entities. Furthermore, not only did the Persians and Old Babylonians/Persians employ fire signals to send urgent warnings, but so, too, did the Neo-Assyrians. According to Saggs, the Neo-Assyrians

“In the event of military emergency ... [used] another method of communication ... This involved the use of fire beacons. By the use of a chain of bonfires, a warning could be transmitted rapidly over the whole land.”<sup>223</sup>

We learned that there was an elaborate system used by the kings of the Persians and the Old Babylonians to investigate high officials suspected of malfeasance, sometimes known as the private ear or eye of the king. So, too, this existed with the Neo-Assyrians:

“Another circumstance in which the employment of an ordinary [messenger] *mar shipa* was impracticable was when news for the private ear of the king or a governor required to be communicated. In such a case use was made of a special envoy, carrying a letter of introduction attested by his principal’s seal: his credentials having been presented, he would then pass on his confidential message with the requisite secrecy. The king had a number of such agents called *qurbuti* (‘intimates’), traveling on his business, and when a complaint was made of the conduct of a senior official, it would normally be one of them who was dispatched to investigate and report to the king. Even a governor was liable to receive a visit of inspection from the king’s confidential agent.”<sup>224</sup>

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<sup>222</sup> *ibid.*, p. 250

<sup>223</sup> *ibid.*, p. 236

<sup>224</sup> *ibid.*, p. 250

We also learned that the Persians and Old Babylonians utilized a system of spies throughout the empire, and again this was the case with the Neo-Assyrians:

“An intelligence system was also built up. From this time there is abundant evidence of Urartian spies in [Neo-]Assyrian pay—we have mention of reports in some cases—and it is not unreasonable to assume that this was not limited to Urartu. By the time of Sennacherib’s attack on Jerusalem ... the [Neo-]Assyrian officials certainly knew—if the biblical narrative is to be believed—a good deal about internal developments in Judah.”<sup>225</sup>

Roux sums up these aspects of Neo-Assyrian administration thus:

“We must add that the Assyrian army had other major assets, notably a well-organized system of communications by fast couriers or sometimes fire signals, and a state security and espionage system that would compare favourably with that of modern nations.”<sup>226</sup>

Since the Neo-Babylonians came at the very end of the Persian era into Macedonian rule of the region there should be very little related to these forms of administration. And this is just what has been found. Michael Jursa points out:

“We do not have the Neo-Babylonian state archives, or rather, what remnants of them have been found remain for the most part unpublished, and Neo-Babylonian royal inscriptions tend to be far less eloquent about political and military matters than their [Neo-]Assyrian counterparts. For this reason our knowledge of Neo-Babylonian governmental and administrative [operations] remains unsatisfactory ... Dandamayev ... claim[s] that ‘[t]he Chaldean [Neo-Babylonian] kingdom was also a comparatively primitive empire’ compared to the [Neo-]Assyrian empire (and the Achaemenid [Persian] empire, one presumes). This judgement is debatable, to say the least. What information on the royal administration can be gleaned from private and temple archives suggests on the contrary a well-administered state and well-established bureaucratic procedures. A glance at Nebuchadnezzar’s unfortunately incomplete ‘Court Calendar’ may

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<sup>225</sup> *ibid.*, pp. 85-86

<sup>226</sup> Roux, *op.cit.*, 3rd ed., p. 348

suffice. The list starts with an inner circle of high officials, headed by the *mašennu* and followed, after a break [in the document], by lesser palace officials, the ‘magnates of the land of Akkad.’ These include dignitaries from smaller Babylonian cities. The enumeration ends with vassal kings from the west. A lot of magnates at the court of, say, Esarhaddon, would not have differed structurally and the titles, if not demonstrably in all cases the responsibilities, of many officials would have been the same.

“The dependency of Neo-Babylonian governmental structures on [Neo-]Assyrian predecessors (or possibly the mutual interdependency of the two traditions) can be demonstrated by reference to a peculiar feature of protocol: the invocation (for self-defence or for denunciation of others) of the so-called ‘word of the king,’ as a means to be granted direct royal judgement. Postgate, who was the first to describe this practice known so far only from the Neo-Assyrian court (although it is attested both in [Neo-]Assyrian and [Neo-]Babylonian texts from this period), wondered if one might consider this an originally [Neo-]Babylonian custom, but opted for an Assyrian origin after all. Be that as it may, one can show that this legal institution [of the Neo-Assyrians] survived into Neo-Babylonian times.”<sup>227</sup>

Of course Jursa is speaking about Nebuchadnezzar II and not Nebuchadnezzar I, whose reign of barely two decades we place between Nabopolassar and Nabonidus.

He further remarks:

“What is known of the internal organization of the Neo-Babylonian empire [though sparse] suggests a state as complex as and not dissimilar to the Neo-Assyrian, which, from the point of view of its structure and its institutions, could just as well have served as a model for the Achaemenids [Persians].”<sup>228</sup>

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<sup>227</sup> Michael Jursa, “Observations on the Problems of the Median ‘Empire’ on the Basis of Babylonian Sources,” *Continuity of Empire (?) Assyria, Media, Persia*, Giovanni B. Lanfranchi *et al.*, eds., (Padova, Italy 2003), pp. 175-176,

<sup>228</sup> *ibid.*, p. 178

The possibility that all these empires were one and the same of course could hardly be envisioned. The aim of the book just cited was to try to explain the continuity of empires from the Neo-Assyrians through the Neo-Babylonians, through the Medes into the Persian era. Because this could not be done, the title of the book, *Continuity of Empire (?)* ... had to have a question mark inserted because the historians could not explain how and why Neo-Assyrian administrative and other forms were utilized almost a century after the Neo-Assyrians were wiped off the face of the Earth. But if the Neo-Assyrians and Neo-Babylonians are lowered to late Persian times, it is obvious that they all shared identical and/or highly similar institutions.

## ART AND ARCHITECTURE

If, as presented, the Neo-Assyrians are in reality the Persian rulers of Assyria, this would be reflected in the unique and highly stylized forms of the art of the courts of both nations. Styles do change over relatively short periods and it would be improbable that the Persians would have art forms that are extraordinarily similar to those of the Neo-Assyrians who had supposedly been wiped out about 80 years earlier. However, if the Neo-Assyrians were the Persian rulers in Assyria, one would expect them to have almost duplicate forms of art, because their artistic expression would be the same.

This is one of the great problems that historians have been unable to explain. As Michael Roaf shows in discussing the “transmission of [art and architectural, as well as court] traditions from Assyria to Persia”,

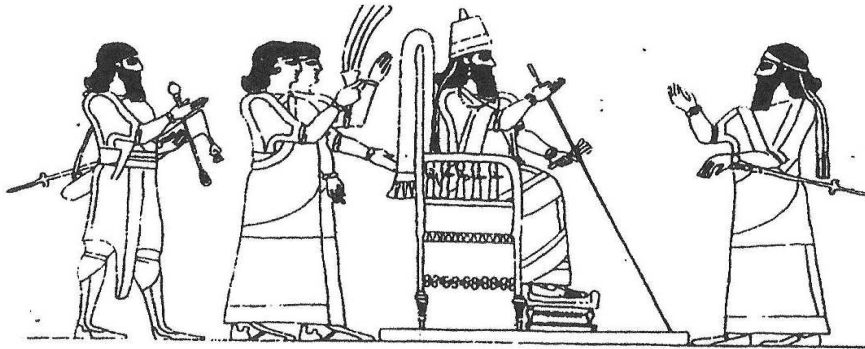
“There is much in the Achaemenid Persian Empire that can be traced back to [Neo-]Assyria. The inheritance is most obvious at the imperial court.

“A comparison of the wall painting of the Assyrian king on his throne ... with the similar scene carved in stone found in the [Persian] Treasury at Persepolis ... shows that the times and manner of palace decoration, as well as the composition and ceremonial of the Persian

court followed the tradition established in Assyria.”<sup>229</sup> (see Figure 1, A and B.)

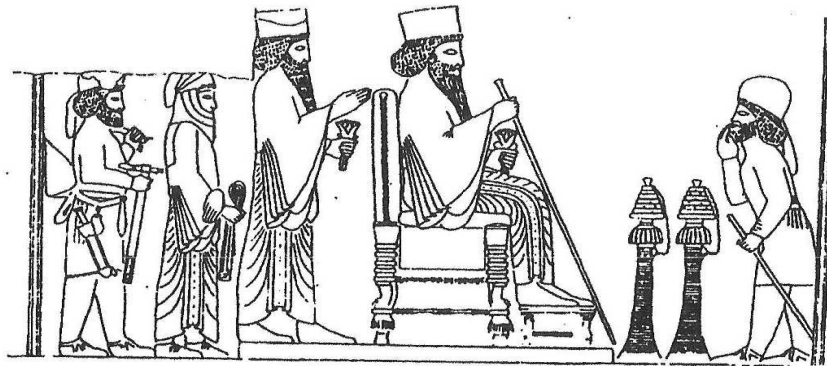
Figure 1

A.



Reconstructed drawing from Neo-Assyria  
(after Thureau-Dangin – Dunand, 1936,  
plate XLIX)

B.



Drawing of Persian panel at Persepolis  
(after Roaf 1983, fig. 122, on page 115)

<sup>229</sup> Michael Roaf, “The Median Dark Age,” *Continuity of Empire (?)*, *op.cit.*, p. 13



Roaf goes on to characterize the nature of the two highly similar drawings:

“In each depiction the king sits on a similar throne with upright back, his feet on a footstool placed on a low dais; ... [each] holds a staff in his right hand and a stylized flower (lotus) in his left; behind ... [each] stand one or two beardless (eunuch) servants and a bearded weapon bearer and in front a high official ... ushers in groups of foreigners. There are also significant differences: a prince is depicted behind the Persian king and incense burners in front, the Persian scene takes place under a canopy, the dress and weapons of the figures are quite different, and the gestures do not correspond. The similarities and the differences show that this is not merely a formal borrowing but that the actual ceremony depicted took place both in [Neo-] Assyria or Persia.”<sup>230</sup>

Heinsohn has produced similar depictions in his book *Assyrekönige gleich Perserherrscher!*<sup>231</sup> (see Figures 2, A and B)

Figure 2

A.



B.



<sup>230</sup> *ibid.*, pp. 13-14

<sup>231</sup> Gunnar Heinsohn, *Assyrekönige gleich Perserherrscher! Die Assyrienfunde bestätigen das Achämenidenreich* (Gräfelfing 1996) p. 133

These are Neo-Assyrian drawings depicting Sennacherib. This ceremony and the artistic nature of the depictions show a direct link between Persia and Neo-Assyria. Roaf goes on to say:

“This is not a unique case and there are many other elements in Achaemenid Persia for which a [Neo-]Assyrian origin can be proposed. These are perhaps most obvious in art and architecture. Some features belong to a common Near Eastern tradition, such as the artistic conventions of rendering each element in its most characteristic view ... But there are other features which, according to our present knowledge, point directly to [Neo-]Assyria.”<sup>232</sup>

In this respect I will present other such forms of art to be found in both traditions. See Fig. 3 below. The upper lines in each group of two lines are Neo-Assyrian cultural and artistic forms, the lower lines are Persian cultural and artistic forms.

As one can see, the artistic depictions exemplified by the Persians were almost identical to those of the Neo-Assyrians. The range of artistic forms and cultural equivalents runs from horses and camels to swords and ships, from winged lions and umbrellas that shade their kings to shields and drinking saucers or bowls, from architectural forms to arrow holders to winged genies, etc.

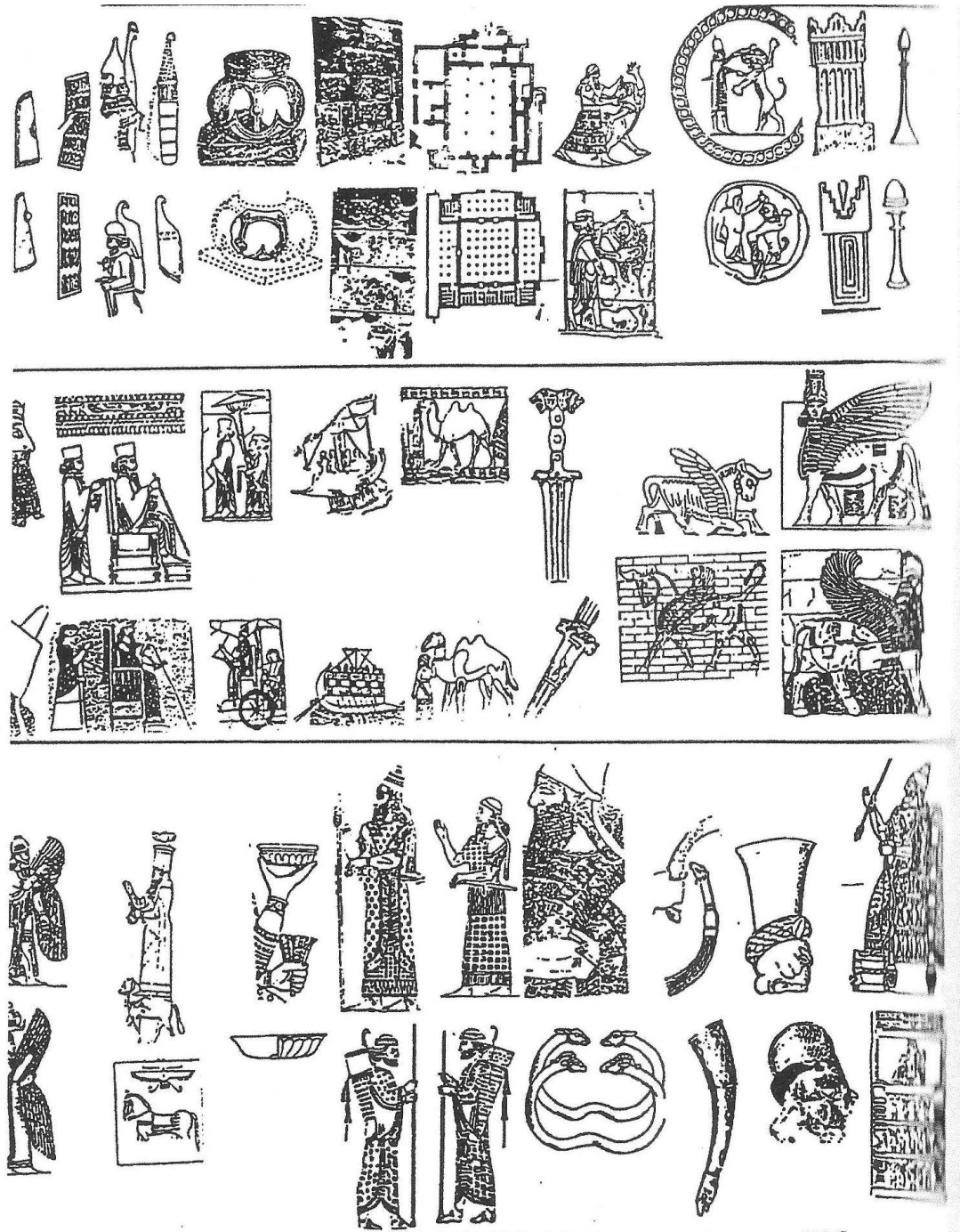
All this material was presented by Heinsohn in *The Velikovskian*, vol. III, no. 1 (1997), p. 36. Heinsohn had earlier presented this material in his book *Assyrikerkönige gleich Perserherrscher!* There he was able to give much more space to this artistic and cultural material, and far greater depth. Emmet Sweeney presented a brief sketch of Heinsohn's evidence in English:

“Can such [artistic and cultural] comparisons be shown to exist [between Persians and Neo-Assyrians]?”

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<sup>232</sup> *ibid.*, p. 14

Figure 3



“As a matter of fact, the evidence linking the late Neo-Assyrians to the Persians is abundant, and covers virtually every field of knowledge. The achievements of the [Neo-Assyrians] match closely those of the Persians, whilst the[ir] artistic and technological achievements ... find their closest parallels in Achaemenid Persia.

“It is possible to identify literally scores of very precise parallels between the latter Neo-Assyrians and the Achaemenids. These may be very broadly classified as follows: (a) Identical monumental architecture and sculpture; (b) Identical religious iconography; (c) Identical types of royal insignia and trappings; (d) Identical military equipment and weaponry; (e) Identical domestic utensils, pottery styles, etc. Many of these have already been highlighted by Professor Heinsohn ...

“In addition to these, there are varieties of parallels that are very specific. Thus for example, the evolution of Neo-Assyrian art precisely matches the evolution of Achaemenid [Persian] art. Early [Neo-Assyrian] work ... finds its closest parallels in early Achaemenid art whilst late [Neo-Assyrian] work is precisely matched by that of the late Achaemenid period. Late [Neo-Assyrian] work does not resemble early Achaemenid work, which it however should if textbook chronology is accurate. Both [Neo-Assyrian] and Achaemenid figurative art begins with highly-stylised forms and gradually develops an increasing realism — so much so that much of the later [Neo-Assyrian] and Achaemenid work could almost be Greek ...

“It is impossible that the artists of the Near East, after developing a realistic style very comparable to that of classical Greece, should then return, a century later, to a primitive and abstract style long ago abandoned, only to repeat a second evolution toward Greek-style realism. ...

“... one point relating to royal symbolism needs to be mentioned here. The Persian royal symbol *par excellence* was the feathered crown. This is part of common knowledge and fully accepted by all authorities. Strangely, however, and incomprehensibly, the feathered crown also appears in Neo-Assyrian art. ... Thus for example from the

time of [Neo-Assyrian] Tiglath-Pileser III onwards the winged bulls who guard the entrances to Neo-Assyrian palaces are shown wearing a tall cylindrical-shaped headdress, of Persian style, surmounted by a feathered crown. Achaemenid [Persian] royal palaces in Persia proper are guarded by identical beasts.

“As Heinsohn has emphasized, such close correspondences can scarcely be explained by simple copying, especially not after a gap of two centuries.”<sup>233</sup>

Roaf in dealing with these almost identical parallels states:

“Outside the field of art and architecture one may note that the nature of the court with the important role played by eunuchs, the [same] methods of provincial and central administration, the road and messenger system, etc., seem to be based on [Neo-]Assyrian practice. [Neo-]Assyrian influence was not the only foreign influence active in Persia but it is certainly the most obvious.

“This immediately presents us with a problem. In 612 [it is taken as a fact that] the Medes and [Neo-]Babylonians succeeded in defeating the Assyrians and destroyed their cities and palaces, and although some members of the [Neo-]Assyrian court may have held out for a few years in Harrān and some Assyrian buildings may have survived and been used by the conquerors, the overwhelming impression both from textual and from archaeological sources is that [Neo-]Assyria was wiped off the map. How then did [Neo-]Assyria influence Persia since there is a gap from the latest mention of an Assyrian polity in 609 and the establishment of the Persian kingdom in about 550 B.C.”<sup>234</sup>

Roaf suggests four possibilities: 1. Neo-Assyrian art and other forms came to Persia prior to the destruction of that empire. Roaf, however, admits that “The little that we know about the Persians before Cyrus [who conquered the empire] suggests that their cultural background was closer to the Elamite than to the [Neo-]Assyrian.”<sup>235</sup> 2. Neo-Assyrian monuments, etc., were not destroyed but survived

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<sup>233</sup> Sweeney, *op.cit.*, p. 80

<sup>234</sup> Roaf, *op.cit.*, p. 14

<sup>235</sup> *ibid.*, p. 15

until the Persians had time to see and copy them. This is a possibility, but Roaf admits: “The overwhelming evidence is that very little, if any of it, would have been visible.”<sup>236</sup> He adds, however, that if a complete Neo-Assyrian

“... palace remained in use after the fall of [Neo]-Assyria, it would have been used by Median rulers or governors and its decoration would have provided the setting for the Median court and it is likely to have been included in the Median artistic repertoire. And *it is likely that the Assyrian elements in later Persian palace decoration were taken from a living artistic tradition and not just copied from an antique model.* The close similarities in court ceremonial noted above ... suggest that this motif was not just an artistic borrowing or a revival of a dead tradition, but was a depiction of a custom which was still carried on at the Persian court.”<sup>237</sup> (emphasis added)

3. It is suggested that Neo-Assyrian artistic influence, etc., was carried to Persia from Media, Neo-Babylonia, Elam, etc. To answer this, Roaf states:

“Darius explicitly recorded that his palace at Susa was built using the resources—both human and material—of different regions of his kingdom. Scholars studying Achaemenid [Persian] art have explored the debt which it owed not only to the arts of the [Neo-] Assyrians, but also that of the [Neo-]Babylonians, Greeks, Phoenicians, Egyptians, Scythians, Elamites and Uartians.

“None of these latter peoples are plausible vehicles for transmitting [Neo-]Assyrian influence. The arts of the Greeks, Phoenicians, Egyptians and Scythians are quite different from Assyria. More recent research suggests that Uartian artistic production ceased before [Neo-]Assyrian and, although [Neo-] Babylonia and Elam were certainly influenced by [Neo-]Assyria (as were other regions) ... [they] had their own distinctive characteristics. ...

“This leaves Media as the most plausible conduit through which [Neo-]Assyrian influence traveled to Persia. This sequence ... is clearly not unreasonable. Furthermore certain features of Achaemenid

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<sup>236</sup> *ibid.*

<sup>237</sup> *ibid.*, p. 16

[Persian] architecture appear to have been derived from earlier Median practices ... there was only one living tradition of monumental art available to the Medes, namely the Assyrian. This might have been modified by the local styles of the [Median] Zagros courts, but they too were heavily Assyrianised.

“Geographically and chronologically Media links [Neo-] Assyria and Persia and [thus] there is a clear means of transmission. The Medes (as far as the limited available evidence shows) had no tradition of monumental art before they conquered the [Neo-] Assyrians. At that time they would have had the opportunity not only to examine at first hand [Neo-]Assyrian art in its original context, but also to appropriate both works of art ... and craftsmen, who could have been employed to decorate the Median palaces ...

“Unfortunately when we come to examine the surviving evidence for such artistic production in Media between 612 and 550 [B.C.] there isn’t any! In fact there is so little evidence that even the existence of a Median kingdom or empire has been doubted.”<sup>238</sup>

Roaf makes many suggestions but ultimately is forced to state:

“I realize that many of the suggestions I have made in this paper are speculative and unsubstantiated by solid evidence, but the hypothesis that Media was the crucial link between [Neo-]Assyria and Achaemenid Persia does offer an explanation for the close links between [Neo-]Assyria and Persia, which are difficult to explain in any other way. Clearly this proposal will only be proved or disproved when further evidence from sites dating to between 612 and 550 in Media has been recovered.”<sup>239</sup>

Roaf has informed us above that these artistic, architectural, and ceremonial forms “were taken from a living ... tradition and not just copied from an antique model.” He added that this “was a depiction of a custom which was still carried on at the Persian court.” In terms of Heinsohn and Sweeney’s thesis, the Neo-Assyrians and Persians, living at the same time in Persia and Assyria, shared

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<sup>238</sup> *ibid.*, pp. 16-17

<sup>239</sup> *ibid.*, p. 22

that “living tradition” and both were therefore still carrying it on in Persia and Assyria. There is no problem related to the transmission, over time, of these artistic, architectural, and ceremonial traditions between Neo-Assyria and Persia because there was no intervening time between these two entities. They lived at the same time, shared the same rulers, and therefore shared these same artistic traditions.

With regard to the artistic tradition of the Neo-Babylonians, Jursa states that “Neo-Assyrian artistic traditions were known and occasionally put to use for Neo-Babylonian royal monuments.”<sup>240</sup>

In a certain sense we have an artistic, architectural, and ceremonial Dark Age intervening between these entities—Neo-Assyria and Persia—once one accepts the established chronology. But by removing it and accepting that the Neo-Assyrians were contemporary with the Persians, the Dark Age shroud is removed and the living tradition sheds light on this problem. Because the historians and archaeologists cannot explain this with “solid evidence,” they must resort to unsubstantiated speculation and suggest that in the future these speculations to maintain the established chronology will bear fruit. And that is all they have, unsubstantiated speculation as opposed to the solid evidence as it upholds Heinsohn and Sweeney’s Neo-Assyrian/Persian identification.

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<sup>240</sup> Jursa, *op.cit.*, p. 174, fn. 36



**NEO-ASSYRIAN/PERSIAN RELIGION AND ICONOGRAPHY**

As was pointed out above, Persia in its rule allowed foreign states to worship their own gods and keep their own cults. Thus we would expect to find the same tolerance to be exhibited by the Neo-Assyrians. In this respect Grayson states:

“*Ashur* ruled the gods, and the universe as sovereign ... He was not a deity of the people [of Neo-Assyria] at large and his presence was manifest only on state occasions in the official documents. The official [Neo-]Assyrian attitude towards foreign gods and cults was one of tolerance, and Assyria did not attempt to impose upon conquered peoples the worship of Ashur.”<sup>241</sup>

Why, then, didn't the rulers of Neo-Assyria worship the same god or gods as the people but only their own god on “state occasions” or in “official documents”? It is unlikely that the rulers and the people of Neo-Assyria should worship different gods or in this case one particular god. Let us recall that in Persia itself there was primarily one god, Ahura Mazda, and that he, like Ashur, “ruled the gods and the universe.” Yet Grayson tells us he was not the “deity of the people” of Neo-Assyria.

If, as Heinsohn and Sweeney maintain, Ashur was the god of the king who ruled Neo-Assyria, then he was privately worshiped by the kings and was therefore the Persian god Ahura Mazda. That is, one would expect to find clear-cut evidence that the Neo-Assyrians worshiped a god identical to the Persian god. This, in fact, is specifically mentioned by Sidney Smith:

“It is not difficult to understand why [the Neo-Assyrian god] Ashur never gained willing adherents among other nationalities. It should be remembered, however, that [when] the [Persian] Zoroastrian religion prevailed in the land which had once been Ashur's, the [iconographic] symbol of the god still remained [in use by the Persians] to testify to his former glory, for that symbol was adopted [by the Persians] to represent the great and good *Ahura-mazda*, and together with the symbol, rites and ceremonies once connected with the worship

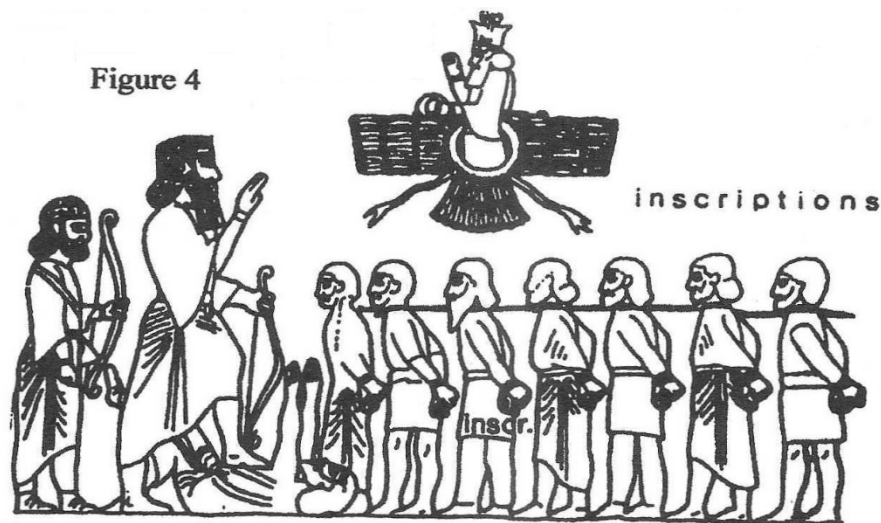
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<sup>241</sup> A.K. Grayson, “Assyrian Civilization ... Religion,” *CAH*, 2nd ed., vol. III pt. 1 (Cambridge UK 1991), pp. 223-225

of Ashur, must have passed [after at least a hundred years] to the Zoroastrian faith.”<sup>242</sup>

Smith has specifically informed us that both the Neo-Assyrians and the Persians worshiped a god with an identical iconographic form, and that both the rulers of Persia and Neo-Assyrian did so employing identical “rites” and “ceremonies.” That is, we supposedly have two different empires worshiping two different gods, separated by several hundred miles and about a century in time, but portraying them with an identical iconographic form, and performing identical “rites” and “ceremonies.” These people in their homelands spoke different languages, had different local customs, dress, and manners, yet when it came to their kings’ religious practices, these were not different but identical.

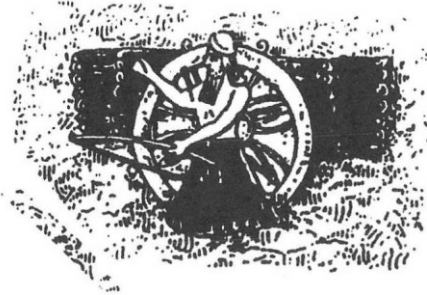
In his book *Assyrekönige gleich Perserherrscher!* Heinsohn displays several of these iconographic forms of Ashur and Ahura Mazda. On page 171, he offers a depiction of the Persian god Ahura Mazda as portrayed on the Behistun rock. The Persian king, Darius the Great, stands facing prisoners of war with his foot on a prisoner and raises his hand to greet and do homage to Ahura Mazda in the sky above the prisoners. See Fig. 4.



<sup>242</sup> Sidney Smith, “The age of Ashurbanipal. The Assyrian religion, worship of Ashur,” *CAH*, vol. III (*op.cit.*), p. 91

On page 180 Heinsohn displays a depiction of the Neo-Assyrian icon of that god. See Fig. 5.

Figure 5



Heinsohn's book gives many other such depictions of this form and similar ones. In this regard Sweeney shows:

“The religious culture of the Neo-Assyrian epoch displays striking correspondences with that of Achaemenid Persia.

“In one sense, the Persian Empire can be regarded as the world's first monotheistic state [with] Ahura Mazda as the one true god ... and by the time of Cyrus [the Great] the new religion had become dominant. In later years it was to become the official religion of the [Persian] Great Kings.

“Monuments from the Achaemenid [Persian] epoch reveal that Ahura Mazda was portrayed as a regal figure seated within or above a winged disc. The winged disc was originally a Mitannian, therefore [based on the short chronology] Median, motif which was promulgated throughout the Near East after the [Mitannian/Median] conquest of the Old Assyrian Empire. ... [A]s rulers of Assyria, we might expect that the Persians would simply have equated the greatest Assyrian god with Ahura Mazda. The supreme and tutelary god of Assyria was of course Ashur. Does the cult of Ashur, in the Neo-Assyrian epoch, resemble that of Ahura Mazda?

“Countless artefacts from Assyria portray Ashur: his image is in fact virtually identical to that of Ahura Mazda, a royal figure inside a winged disc. The precise parallels between these two have of course not gone unnoticed by scholars: yet it is generally assumed that the iconography of Ashur was simply copied by the Persians ...

“In this way the very precise correspondence between Ashur and Ahura-Mazda is explained away. But what is not explained, what is not even mentioned, is that in the whole of Assyria, indeed the whole of Mesopotamia, which was under Persian rule for two centuries, archaeologists have not recovered a single image that could unquestionably be identified with Ahura Mazda. The complete absence of Ahura Mazda is however explained if he is one and the same as Ashur. ...

“Now it is well-known that Xerxes was the first Persian monarch to enforce Zoroastrian monotheism, and as part of that project issued a famous proclamation outlawing what he described as the ‘foreign devas’. He also carried off from Babylon the golden image of Bel-Marduk — a blasphemous act long recalled in the latter city.

“In precisely the same way, [the Neo-Assyrian king] Sennacherib issued a proclamation outlawing the Babylonian deities, and he too carried off the golden statue of Bel-Marduk, [the same] outrage [as] repeated ... by Xerxes.

“Ahura Mazda’s symbol par excellence was fire; and, sure enough, fire altars of a very particular design are found portrayed throughout Iran. How strange then that fire altars of exactly the same type were known in ... [Neo-]Assyria ... supposedly two centuries earlier.”<sup>243</sup>

These identical forms of religious iconography, rites, and ceremonials, as well as identical forms of court ceremony and artistic expression, as well as the administrative organization of empire, espionage networks, post roads with inn stops, fire signals, etc., are quite like what we encountered with the Old Babylonians,

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<sup>243</sup> Sweeney, *op.cit.*, pp. 81-82

especially in that case with the transmission of mathematics to the Greeks and Persians. In both cases numerous technical and non-technical aspects of the Old Babylonian and Neo-Assyrian culture are found in Persia, but without a route of transmission. Yet once again the short chronology which equates the Persians with the Old Babylonians, Neo-Assyrians, and Neo-Babylonians shows that there was no need for a route of transmission of all these aspects of development and culture. Since these empires are all of the same time, but situated in Persia, Assyria, and Babylonia under Persian rule, they shared these developments. The problems with the transmission of these phenomena disappear as a natural outcome of the short chronology.

A last note on dating is offered related to all these cultures. Since the Persians, Neo-Assyrians, and Neo-Babylonians are interconnected as either *alter-egos* or as close contemporaries of the Persians, their exported goods should resemble one another and be closely dated to one another, or have identical dates in the strata or in other instances. Ephraim Stern reports in the *Archaeology of the Land of the Bible* that at Beth-Shemesh in a tomb

“... for example, three types of clay lamps were found together. One is the Judaeen high-footed lamp, typical of the last stage of the Judaeen monarchy, which perhaps continued to the mid-6th century B.C.E. [550 B.C.]. The second type is the closed [Neo-]Babylonian lamp with a long nozzle, which in Mesopotamia was the emblem of the god of light Nusku. It is very rare in Palestine. This lamp appears here for the first time, but continues down to the early part of the Persian period ... The third lamp is ... the most common type in the Persian period. Here, it makes its earliest appearance. The mixture of the three types could occur only in the mid-6th century B.C.E.”<sup>244</sup>

Stern cannot prove the mid-6th century B.C. date but assumes that these lamps could only appear mixed together in the same stratum, though an even later date is required based on the archaeoastronomical evidence and other forensic evidence presented above. For example, in terms of pottery Stern reports:

“Even more important was the influence of the new [Neo-] Assyrian style on local artisans [of Palestine], who began to imitate it

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<sup>244</sup> Ephraim Stern, *Archaeology of the Land of the Bible*, vol. II (NY/London *et al.* 2001), p. 343

from then on and through later periods even years after [Neo-] Assyrian domination had ceased.”<sup>245</sup>

Stern also shows that

“It is now clear that the [Neo-]Assyrian conquerors [of Palestine] brought some of their Mesopotamian burial customs with them: i.e., the use of clay ossuaries ... This Mesopotamian custom continued thereafter in Palestine ... through the domination of the subsequent Babylonian and Persian empires.”<sup>246</sup>

But all this clearly suggests that the burial customs of the conquering peoples, Persians and Neo-Assyrians and in part Neo-Babylonians, were all one and the same. This is explicitly shown by the fact that these three cultures left artifacts in the Holy Land that are “almost impossible” to distinguish from one another. Again Stern speaks:

“[Neo-]Babylonian domination of Palestine lasted for approximately seventy years, a period of time roughly equal to that of the [Neo-]Assyrian rule. A clear distinction must be made between the influence of [Neo-]Babylonian occupation ... and that of their [Neo-]Assyrian predecessors and the Persians after them. Although the [Neo-]Babylonians ... differ[ed] ... from that of their [Neo-Assyrian] predecessors, *this did not leave any clear traces in the country’s record* ...

“During the [Neo-]Babylonian period, the main foreign influence in Palestine remained Mesopotamian culture. *In many cases, this makes it impossible to determine if a certain artifact with [Neo-]Babylonian parallels should be dated to the late [Neo-] Assyrian period, to the [Neo-]Babylonian period, or even to the early Persian period.* During all three periods, Palestine absorbed influences from the same remote Mesopotamian center.”<sup>247</sup> (emphasis added)

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<sup>245</sup> *ibid.*, p. 19

<sup>246</sup> *ibid.*, p. 31

<sup>247</sup> *ibid.*, p. 308

Although each center did apparently have its own unique artifacts, they were melded in a way into a fairly common identity. Therefore, they were not artifacts of three distinct periods of time but rather of three cultures for one distinct period of time.

## A SHORT CONCLUSION

The entire panoply of evidence correlates with and corroborates the short chronology which clearly proves that the Neo-Assyrians were contemporary with the Persian rulers of that land. The astronomy agrees with this thesis. The Dark Age prior to and after the fall of Neo-Assyria vanishes once one embraces the short chronology. The stratigraphy also corroborates this revision; no unique denudation processes are necessary to only halt erosion, or other actions to only leave Neo-Assyrian strata directly beneath that of the Greeks. Neo-Assyria does not take 700 years to begin to fully enter the Iron Age. There is no Iron Dark Age. Agronomy, inflation, and famine follow in this same regard at the end of the Neo-Assyrian times as they do in Persian and Old Babylonian times. The administrations of the Persian, Old Babylonian, and Neo-Assyrian era are extraordinarily alike because they represent the same entity—Persia. The art and architecture follow suit and there is no artistic or architectural Dark Age between the Neo-Assyrians and the Persians. Being the same entity, they employ the same forms of art and architecture. The tolerance of foreign gods and religions is also identical with the Neo-Assyrians and Persians.

When astronomy, and the technology of iron, along with stratigraphy and agronomy agree with the administrative measures, art, architecture, and religious toleration for these entities, the facts speak for themselves that the Neo-Assyrians have to be the Persians. As in the previous chapter about the Old Babylonians, the evidence is compelling except for those who do not want to see.

## CHAPTER 3: MEDES AND MITANNI

Heinsohn and Sweeney maintain that the Medes and Mitanni are one and the same people. The Mitanni are taken to have lived in northern Mesopotamia from around 1500/1600 down to about 1260 BC. On the other hand it is accepted that the Medes lived in the early to mid first millennium B.C., also in northern Mesopotamia. If this is indeed the case then all the materials, historical and archaeological, given to the Mitanni would be missing from the Medes.

For example, historians say the homeland of the Medes is situated “in the Zagros mountains of western Iran, hundreds of miles to the east of Baghdad, while Mitanni was centered on the Khabur triangle in northern Syria hundreds of miles to the west of Baghdad.”<sup>1</sup> Ev Cochrane, in fact, presented a map that depicts this homeland situation as evidence for this fact. But what that map is based on scientifically we are not told. However, as I pointed out in volume I of this series, Michael Roaf showed that, at best, “a fortress at Tell Gubba in the Hamrin region and a building with columned halls at Tille Höyük on the Euphrates in Southern Turkey has been attributed [but not proven to belong] to the Medes.”<sup>2</sup> I also cited A. Leo Oppenheim that “the Medes left us but scanty archaeological evidence”<sup>3</sup> and I cited others including a citation from the *Encyclopedia Britannica* that the historical and archaeological evidence for the Medes is practically non-existent. So how can Cochrane or any of the historians know, which means prove, that the homeland of the Mitanni is where they have placed this people?

Of course Cochrane can explain this problem away by calling upon “denudation” processes, which at this point would be worthless. If, however, as Heinsohn and Sweeney maintain, the historical and archaeological evidence of the Medes was given to the Mitanni, then all the missing evidence for the Medes is explained. What must be borne in mind is that the problem of finding evidence for the Medes has proved to be intractable.

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<sup>1</sup> Ev Cochrane, “Sins of the Father,” *AEON*, vol. VI, no. 6 (October 2005), p. 58

<sup>2</sup> Ginenthal, *Pillars of the Past*, *op.cit.*, p. 263

<sup>3</sup> *ibid.*



To overcome this Median Dark Age problem, a conference was held to bring together the leading researchers in the field of Median studies, at Padua in Italy on the 26th through the 28th of April 2001. The volume of these proceedings, discussed in part above, titled *Continuity of Empire (?)*, was published in 2003. Rather than this convocation coming close to unraveling this Median Dark Age, there was shock at its conclusion when it was realized that practically no headway could be made in resolving the dilemma. James D. Muhly, in the *Bryn Mawr Classical Review*, discussed the results of the Conference and the text of the proceedings:

“I believe it is fair to say that, prior to the conference, no participant had any inkling of just how difficult it would be to ‘pin down’ the Medes. The degree of exasperation this situation produced is evident in the tone of many of the published texts. Michael Roaf, for example, has concluded that ‘This survey of the evidence, both textual and archaeological for Media between 612 and 550 BC, has revealed almost nothing. Media in the first half of the sixth century [B.C.] is a Dark Age’ (p. 19). John Curtis laments that ‘It has to be admitted at the outset that there is not the slightest archaeological indication of a Median presence in Assyria after 612 BC (p. 165) and this of course is the very period for which the Medes were supposed to have provided ‘continuity’ [between the Neo Assyrians and Persians].

“What about the Median state that we have always assumed, following Herodotus and the Babylonian Chronicle, played the central role in the destruction of the [Neo-]Assyrian empire at the end of the seventh century BC? David Stronach, who certainly knows more about the Medes than any other living scholar, states early on in his essay, that ‘it may be enough to assert that there are, quite simply, no sound grounds for postulating the existence of a vigorous, separate and united Median kingdom, at any date substantially before 615 BC’ (p. 234). ...

“Some of his colleagues should pay heed. In recent years Geoffrey Summers, excavator of the fortified site of Kerkenes Dag, north of Kayseri, has argued for his site as ‘monumental’ evidence for the presence of a Median empire in eastern Anatolia. Christopher Turin, one of the main contributors to the volume under review, comes out strongly in favor of this identification (p. 354). Other contributors,

notably Robert Rollinger (pp. 321-326) and David Stronach (p. 248) take serious exception to all of Summers' arguments, including those relating to the famous eclipse of 585 BC, which Thales could not have predicted and which was in any case not visible from central Anatolia.

...<sup>4</sup>

This Dark Age regarding the evidence of the Medes echoes throughout recent historical and archaeological literature. H. Sancisi-Weerdenburg states:

“In Assyrian and Babylonian records and in the archaeological evidence no vestiges of an imperial [Median] structure can be found. The very existence of an Median empire with the emphasis on *empire*, is thus questionable.”<sup>5</sup>

The fact remains that there is no solution to this problem. As Muhly shows:

“Can this problem be approached in other ways? In 1962 Richard Barnett, then Keeper of Western Asiatic Antiquities at the British Museum, published an article on ‘Median Art’, bringing together all the objects that he felt represented the material culture of the ancient Medes. In an 1987 article, on ‘Median Art and Medizing Scholarship’, Oscar White Muscarella demonstrated that not one of the objects listed by Barnett had an excavated context. They all came from the antiquities market and were of questionable authenticity. They were declared ‘Median’ simply because they did not seem to belong to the art of Achaemenid Persia. They could not be assigned a secure cultural and historical context and must, therefore, represent the art of the elusive Medes.

“Nothing could better illustrate the ‘Black Hole’ that this conference tried so hard to fill (with a notable lack of success). It has come to be the fate of the Medes, in terms of art and iconography, to be represented by objects illicitly excavated and therefore of questionable

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<sup>4</sup> J.D. Muhly, “Review” of *Continuity of Empire (?)*... (*Bryn Mawr Classical Review* 2004), p. 4.

<sup>5</sup> H. Sancisi-Weerdenburg, “Was there ever a Median Empire?” *Achaemenid History III. Method and Theory*, A. Kuhrt, H. Sancisi-Weerdenburg eds., (Leiden, the Netherlands 1988), p. 212

authenticity. This problem has come to the fore once again, because of a flood of objects that came to the art market in the 1990s, all said to come from a cave in western Iran ... It soon became obvious that many pieces said to be from this cave were, in fact, modern forgeries. What is even more disturbing is that a number of ‘cave’ objects were inscribed with short epigraphs in cuneiform, in Neo-Assyrian and Neo-Elamite as well as alphabetic inscriptions in Aramaic ...

“Are things as bad as they seem to be? Have forgers now mastered the art of writing in cuneiform? ...

“[Thus] Scholars trying to study the period of the Iron Age in the Middle East must now confront the problem of distinguishing between genuine Iron Age historical documents and objects [that are forgeries] that only appear to be of Iron Age date. Museum curators have always claimed that they can tell the difference, but what this means is that they can pick out the obvious forgeries and even the not-so-obvious, but the very good fakes go undetected and end up on display in the museums of the world. This is true for every major museum, without exception ... This is why Median archaeology is such a mess.”<sup>6</sup>

All the Medish materials having been given to the Mitanni, naturally this “Black Hole”—“Dark Age” must exist. The fact of the matter is that neither the Greeks, Persians, Neo-Assyrians, Neo-Babylonians, nor the Medes knew of the existence of a civilization known as Mitanni. “The kingdom of the Mitanni was completely forgotten for millennia until discoveries in the nineteenth century revealed its name and existence.”<sup>7</sup>

In this respect Ev Cochrane has challenged the Medes/Mitanni connection posited by Heinsohn and Sweeney and supported by this author in volume I of *Pillars of the Past*. He argues that the Medes and Mitanni could not and did not occupy the same regions (as I following Sweeney stated), and I repeat:

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<sup>6</sup> Muhly, “Review...”, *op.cit.*, pp. 4-5

<sup>7</sup> A.K. Grayson, “Mitanni,” *The Penguin Encyclopedia of Ancient Civilizations*, A. Cotterell, ed. (London 1980), p. 109

“Ginenthal proclaims that the Medes and Mitanni occupied ‘virtually the same region.’ Yet the traditional homeland of the Medes was in the Zagros mountains of western Iran, hundreds of miles to the east of Baghdad, while Mitanni was centered on the Khabur triangle in northern Syria, hundreds of miles to the west of Baghdad (see accompanying map [which shows where the two different homelands are].) I dare say that few scholars would accept that such disparately located homelands constituted ‘virtually the same region’.”<sup>8</sup>

And dutifully Cochrane does present a map on page 59 of his criticism which indicates these separate homelands. But what is this map based on to prove these were in fact the proper locations? The fact of the matter is that Cochrane neither knows nor, as Muhly pointed out above, is there clear-cut archaeological evidence in the ground of the Medes’ homeland to prove that they actually lived there. This is presented by Cochrane in spite of the fact that I pointed out that there is really nothing to prove the Medes lived in this region. To make the point, I cited a very simple source, *The Encyclopedia Britannica*, that “Few identifiable ‘Median’ objects have been found.” If one claims one has the homeland of a great civilization which lasted for centuries, then there should exist in that homeland plenty of archaeological evidence to prove that contention, especially in view of the fact that Muhly, Kuhrt and Sancisi-Weerdenburg have shown that there is no evidence of the existence of a Median empire, and, as Muhly pointed out, the archaeological objects used as evidence for them in their homeland and empire are in all probability forgeries. How does Cochrane dismiss the published work of an authority in the field, Michael Roaf, above, who informs us that “This survey of the evidence, both textual and archaeological, for Media between 612 and 550 B.C. has revealed almost nothing.” As for the period prior to around 612 B.C., David Stronach stated “there are quite simply no sound grounds for postulating the existence of a vigorous, separate and united Median kingdom at any date substantially before 615 B.C.”

David Stronach reports:

“Studies of the archaeology of pre-Achaemenid Media are still a markedly recent phenomenon. In 1964, for example, R. Ghirshman was moved to observe that ‘we know next to nothing about Median architecture.’ A year later E. Porada found it necessary to state that it

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<sup>8</sup> Cochrane, *loc.cit.*

was ‘impossible to give a survey of Median art’ because ‘no unquestionable Median site’ had been excavated. ... In 1966 H. von Gall was able to demonstrate that the supposedly pre-Achaemenid rock-cut tombs of the central western Zagros were mostly unlikely to be anything but late Achaemenid or post-Achaemenid in date ... At one stroke ... the only supposedly secure monuments of independent Media ... were shown to possess incontrovertible [Persian] fourth century or later association ... [Thus] direct knowledge of the Medes on their home terrain was ... by the close of 1966, a subject on which little if anything could be vouchsafed.”<sup>9</sup>

Stronach goes on to discuss other excavations in the Median homeland; for example Godin Tepe and Tepe Nush-i-Jan as well as its supposed capital Hamadan, and concludes

“... that if this exceptionally modest geological triangle is all that we can reliably point to in terms of identifying a Median homeland we are ... almost back to square one. That is to say that, since virtually no relevant pottery or architecture has been identified in the current excavations at Hamadan, the work at Godin and Nush-i-Jan may not really have identified genuine archaeological markers of Median identity at all—and the whole notion of a significant, flourishing Median homeland before the Achaemenid period could be regarded, once again, as a very doubtful construct.”<sup>10</sup>

Stronach does suggest that one site, Tepe Ozbaki, seems to be Median but only insofar as it is similar to Tepe Nush-i-Jan’s pottery which is not known to be Median nor to its wooden columns which are also known not to be Median. The archaeologists are thus inferring what they cannot prove directly. In conclusion he writes “there is still no unequivocal instance of Median construction in this broad region.”<sup>11</sup> And “a finding will still fall short of an affirmation of systematically co-occurring artifacts and other forms of architectural data that would support the notion

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<sup>9</sup> David Stronach, “Independent Media: Archaeological Notes from the Homeland,” *Continuity of Empire (?)*, G.B. Lanfranchi *et al.*, eds. (Padova, Italy 2003), p. 234

<sup>10</sup> *ibid.*, p. 236

<sup>11</sup> *ibid.*, p. 248

of a city built expressly by or for the Medes.”<sup>12</sup> It is still correct to repeat “Muscarella’s clarion call to define ‘what is Media, what is Median’.”<sup>13</sup>

In essence, there is no evidence for the very existence of the Medes in the places or periods of time where and when they are said to exist. Is Cochrane going to suggest that he has any solid textual or archaeological evidence to support his contention and the map he offers? The true value of a map is that in the place named one will find indisputable evidence that the people who are supposed to have lived there, really existed there. Cochrane who, unlike me, places great emphasis on archaeology, cannot present such evidence to uphold this map with regard to the Medish homeland.

As George Robert Rapp wrote of another map of that ancient region related to tin mines:

“A now somewhat dated overview of tin in the ancient world is available in Franklin *et al.* (1978). The literature is full of misinformation or at least uncritical reporting. For example Wilson (1994:18) presents a map of ‘mines of the Middle East’. On this map tin mines are shown near Eskisehir and Ezincan, Turkey. The author has visited this area and there is no evidence of significant tin in regional geologic formations.”<sup>14</sup>

So too with respect to Cochrane’s map. The map gives us no information or misinformation and is, at the very least, uncritical because if one goes to the Medish homeland, as the archaeologists did and as the historians say, there is not the slightest solidly based archaeological evidence of Medish habitation there. Archaeologists have been digging there for many years but cannot find the evidence to prove conclusively that the Medes lived there. The map is based only on the presumption that the Medes resided in this region without proof. F. Bates in his *Principles of Evidence*, 3rd ed. (Sidney, Australia 1985), page 40, explains:

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<sup>12</sup> *ibid.*

<sup>13</sup> *ibid.*, p. 247

<sup>14</sup> George Robert Rapp, *Archaeomineralogy* (Berlin Germany 2002), p. 167

“In general terms a presumption is simply the assumption of the truth of a particular fact and hence, in the law of evidence, the effect of a presumption will be to establish a fact without proof.”

That, as a matter of fact, is all Cochrane has presented—an unproven presumption. The evidence for the supposed homeland of the Medes is simply not there, so there is no basis for Cochrane’s claim or map. As Muhly rightly states, the evidence of the Medes’ homeland and empire is a “black hole.”

But in the end Cochrane can explain this problem away, as he has the strata of the Old Babylonians and Neo-Assyrians, by suggesting that “denudation” processes simply removed the Median strata which are inconveniently missing and in contradiction to the established chronology. Is there evidence of a Mitanni homeland and empire in the stratigraphical/archaeological record? The pottery ware of the Mitanni is considered an excellent marker of the territories they occupied. This ware is known as Nuzi ware. Peter Akkermans and Glenn Schwartz discuss Mitanni “Nuzi ware from Alalakh” and at “Brak [a] Mitanni palace and temple.”<sup>15</sup> In fact I.M. Drakonoff admits the archaeological evidence for Mitanni is well known. “The rise of Mitanni coincides with [two inventions ... Then, a]bout the eighteenth or the seventeenth centuries [B.C.] ... the Hurrians [that the Mitanni ruled] devised a method of making small vessels from opaque, colored glass. ...”<sup>16</sup> As for the Mitanni capitals we are informed, “The sites of the capital of Mitanni (TELL ATCHANA and NUZI) are located at the western and eastern edges of the [Mitanni] kingdom respectively.”<sup>17</sup> Furthermore, the name of their capital “Washshiganni” [also known under many different spellings] is known but its location has not been found.<sup>18</sup> At Tell Munbaqa, Heinsohn pointed out that not only Nuzi ware is found but cylinder seals which had not changed over 700 to 800 years.

The Medish civilization, well known to the ancient world, left nothing of their homeland and empire, while the Mitanni civilization, invented by the historians and unknown to the ancient world, left a great deal of evidence of its supposed existence. Rather than realize that these civilizations were one and the

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<sup>15</sup> Peter M.M.G. Akkermans, Glenn M. Schwartz, *The Archaeology of Syria: From Complex Hunter-Gatherers to Early Urban Societies (c. 16,000-3000 BC)* (Cambridge UK 2003), p. 486

<sup>16</sup> I.M. Drakonoff and I.M. Diakonov, *Early Antiquity* (Chicago IL 1991), pp. 291-292

<sup>17</sup> *Dictionary of Archaeology*, Ian Shaw, Robert Jameson eds. (Oxford UK 1999), p. 402

<sup>18</sup> *CAH*, vol. II, pt. I, p. 433

same, occupying the correct homeland and empire, the historians have invented a people—the Mitanni—who are in reality the Medes. This was the exact case, as we saw above, for the Old Babylonians and Neo-Assyrians who were invented in place of the Persians in Babylonia and Assyria. When one takes the strata of a known people and empire and gives them to another that never existed, of course the stratigraphical evidence for the real people and their empire will disappear. The stratigraphical evidence does not fit the established chronology because the *alter egos* or twins cannot both exist. If they did, there would be clear-cut stratigraphical evidence for both. In each of the cases discussed above this is certainly not the case. There are too many unreal characters in the established chronological scenario and thus the history cannot be made to align with the stratigraphy.

This concept of having too many extraneous characters in a plot was beautifully outlined by Mark Twain in the “Preface” of his book, *Those Extraordinary Twins*:

“A man who is born with the ... writing gift has a troublesome time of it when he tries to build a novel [or historical chronology] ... He [at the beginning] has no clear idea of his story [or history]. In fact he has no story. He merely has some people in his mind and an incident or two, also a locality. He [believes he] knows these people, he [believes he] knows the selected locality ... So he goes to work ... In the beginning he is only proposing to tell a little tale [or history] ... But as it is a tale [ or history] which he is not acquainted with, and can only find out what it is by listening [to the evidence] as it goes along telling itself, it is more than apt to go on and on till it spreads itself into a [long] book.

“And I have noticed another thing, that as the short tale [or history] grows into the long [one] ... [just as the short chronology of the ancient world grew into the long one] the original intention [or motif] is apt to get abolished and find itself superseded by a quite different one. It was so in the case of a ... sketch which I once started to write ... about a prince and a pauper ... Much the same thing happened with [my book] ‘Pudd’head Wilson.’ I had a sufficiently hard time with that tale, because it changed itself from a farce to a tragedy ... a most embarrassing circumstance. BUT WHAT WAS A GREAT DEAL WORSE WAS, THAT IT WAS NOT ONE STORY,



BUT TWO STORIES TANGLED TOGETHER; AND THEY OBSTRUCTED AND INTERRUPTED EACH OTHER AT EVERY TURN AND CREATED NO END OF CONFUSION AND ANNOYANCE [as does the history of the Medes and Mitanni]. I could not offer the book for publication, for I was afraid it would unseat the reader's reason. I did not know what was the matter with it, for I had not noticed, as yet, that it was two stories in one. It took ... months to make that discovery ... and at last I saw where the difficulty lay. I had no further trouble. I pulled one of the stories out by the roots and left the other one [in]. ..."<sup>19</sup> (capitalization added)

This is in part one of the reasons that I maintain the Mitanni are, as Heinsohn and Sweeney claim, the Medes and shared the same homeland and empire. Cochrane then argues:

“Among the most preposterous statements in a book notable for its numerous preposterous statements is Ginenthal's claim that ‘the scientific and technological facts’ corroborate the identification of Mitanni and the Medes. Yet in the chapter in question, Ginenthal doesn't cite a *single* scientific or technological fact, much less one that unequivocally supports the identification of the Medes and Mitanni.”<sup>20</sup>

I ask the reader to note that Cochrane is discussing “Chapter 8” which is an introduction to the evidence for Mesopotamia, “the chapter in question.” What Cochrane failed to report to his readers, which is not only “preposterous” but “unethical”, is my statement regarding some of this evidence from the ensuing chapters. I specifically informed Cochrane in that “chapter in question”,

“If Heinsohn's and Sweeney's claims are correct and instead of eight or ten civilizations there were only four or five that each ruled for some centuries, then Mesopotamian history would be exceedingly short and its chronology would begin about 1200 to 1500 B.C. THIS IN

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<sup>19</sup> Mark Twain, “Preface” *The tragedy of Pudd'nhead Wilson and the Comedy of Those Extraordinary Twins* (Oxford UK 1996) pp. 309-315

<sup>20</sup> Cochrane, “Sins of the Father,” *op.cit.* , pp. 58-59.

LARGE MEASURE IS WHAT THE FOLLOWING CHAPTERS OF THIS BOOK WILL DEMONSTRATE.”<sup>21</sup> (Capitalization added).

Thus I made it absolutely clear to Cochrane that scientific and technological evidence touching on the Mitanni/Mede connection would be presented in the following chapters. But Cochrane had no scruple in only being willing to discuss “the chapter in question” as if there was no evidence related to that connection in the other chapters. Sigmund Freud’s remark on Havelock Ellis clearly fits Cochrane’s underhandedness. “It isn’t only the things he doesn’t know, it’s the things he does know that aren’t true.”<sup>22</sup> Knowing what I wrote, Cochrane deviously failed to deal with these other chapters in order to leave the impression that I did not deal with the scientific and technological evidence regarding the Mitanni/Medes.

Let us examine some of the scientific and technological evidence Cochrane has failed to discuss. In the very next chapter, “Mesopotamian Stratigraphy”, the stratigraphical evidence from Tell Munbaqa—the geological stratigraphical evidence, not just, the archaeological stratigraphy that dealt with the Mitanni and the Old Akkadians was presented. There I cited a geologist who proved that the geological stratigraphy contradicts the established chronology. In the following chapters I dealt with hard iron—steel—being used, well before the Mitanni period, in Mesopotamia to cut diorite. This requires a total contraction in time of these pre-Mitanni societies into the first millennium B.C., which further requires that the Mitanni/Medes also existed in the first millennium B.C. I then discussed the problem of the sources of tin for the manufacture of tin bronzes in pre-Mitanni societies. Since this requires that these societies be moved into the first millennium B.C. in order for them to have tin bronzes, it also requires that the Mitanni/Medes be moved into the first millennium B.C. and not remain in the second. I also discussed the impossibility of having irrigation agriculture in Babylonia for 3000 or more years in southern Babylonia—given the same climatic conditions as the historians claim existed then, without a pole shift. Cochrane has long maintained that no such pole shift in the 8th century B.C. occurred, yet failed to explain how without it, irrigation agriculture was employed there for millennia, when the scientific and technological evidence prove such agriculture could only have lasted for about 400 years.

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<sup>21</sup> Ginenthal, *Pillars of the Past*, *op.cit.* , p. 255.

<sup>22</sup> Sigmund Freud, quoted in *Leo Rosten’s Carnival of Wit*, *op.cit.* , p. 140

None of this scientific/technological evidence which pertains to the Mitanni/Mede connection was in the “chapter in question,” so Cochrane adroitly omitted these facts.

The crucial point that must be stressed is that the scientific and technological evidence demands that the so-called Mitanni did not exist in the second millennium B.C., as Cochrane maintains, but existed in the first millennium B.C., as Heinsohn and Sweeney claim. There is a great deal of this kind of evidence related to the Mitanni/Mede connection which will now be addressed. Cochrane in this regard cites Sweeney from my book as follows:

“From the personal names of the Mitanni kings we know that they were a race who spoke a language closely related to Persian...virtually identical in fact to the Indo-Iranian language of the Medes. *The text of a treaty between Mitanni and the Hittite land* shows that Mitra, Varuna, and Indra, deities of Indo-Iranian origin, comprised the Mitanni pantheon. Indo-Iranian technical terms appear with great frequency in the Mitanni vocabulary.”<sup>23</sup> (emphasis added)

On the basis of this I claimed:

“Is it reasonable to accept that a nation—the Mitanni—that ruled eight hundred years before the Medes ... should worship the same gods, have the same technical terms, have kings with the same names all by coincidence?”<sup>24</sup>

Cochrane then argues”

“It would be difficult to find a statement more at odds with the facts. Virtually every claim advanced by Ginenthal is blatantly false. ... Nor ... is it true that they ‘worshipped the same gods.’ Certainly Ginenthal fails to present any evidence in *Pillars of the Past* that would lead us to believe that this was the case. ...

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<sup>23</sup> Cochrane, *op.cit.* , pp. 56-57

<sup>24</sup> Ginenthal, *op.cit.*, p. 265

“What Ginenthal has in mind, needless to say, is that Mithra, Varuna and Indra *were invoked in a Mitanni treaty with the Hittites* (conventionally dated to 1350 BCE), a point emphasized by Velikovsky as well as Sweeney. Insofar as this [Mitanni/Hittite] treaty constitutes the earliest evidence for the prominent Indo-European gods, it is an important bit of religious history. Yet there is no evidence that the Medes worshipped any of these gods.”<sup>25</sup> (emphasis added)

In this respect Cochrane has omitted facts from *Pillars of the Past* that are in scientific and technological terms at odds with these facts. Virtually *every claim* advanced by Cochrane is blatantly false in this regard.

Notice that Cochrane has not and will not raise evidence related to the scientific evidence I did raise about the Mitanni. While he does make his point, he ignored an important fact that negates the chronology of the Mitanni and undermines the entire thrust of this criticism. Proving who the Medes and Mitanni worshipped only comes after the science and technology allow for such a judgment. That treaty between the so-called Mitanni and Hittites must take into account the time in which the Hittites lived in Anatolia. What Cochrane failed to discuss is that I presented an entire chapter on the chronological placement of the Hittites. In Chapter 16, “Hittites–Lydians” I gave various scientific and technological proofs that the Hittites as the *alter ego* of the Lydians lived in the first millennium B.C. and not the second! Among these forms of evidence I pointed out that J.G. Macqueen showed the Lydians and Hittites spoke the same language.<sup>26</sup> That not only Macqueen and Sweeney made this claim but that Velikovsky, citing Macqueen, showed, “THE LYDIAN AND THE HITTITE KINGDOMS WERE CONTEMPORARY AND USED THE SAME LANGUAGE.”<sup>27</sup> (capitalization added). If the Mitanni of the second millennium B.C. used a language it would have changed dramatically over 800 years when the Lydians used it. Thus the Hittites who had made a treaty with the so-called Mitanni, invoking Mitanni gods, had to live in the first millennium B.C. How could the Mitanni of the second millennium B.C. make a treaty with a people, the Hittite/Lydians, who lived 800 years after them? This Cochrane did not address or let his readers know of.

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<sup>25</sup> Cochrane, *op.cit.*, p. 58

<sup>26</sup> Ginenthal, *op.cit.*, p. 506

<sup>27</sup> *ibid*, p. 507

Cochrane, well aware of this evidence in *Pillars of the Past* that contradicted the chronology he was advocating, never addressed this evidence. It contradicted all that he was offering.

I also presented evidence related to the fact that the Hittites were trading and making tin and tin bronze hundreds of years before tin from known sources entered the Middle East.<sup>28</sup> Again, how could the Hittites have lived in the second millennium, and have tin bronzes before tin came to that part of the world? This aspect of the evidence Cochrane also chose to ignore. Rather, he has simply accepted the gospel of historiography over the facts of science and technology.

I further pointed out the evidence of iron as it relates to the chronology of the Hittites.<sup>29</sup> That is, the Hittites were using iron and writing about iron hundreds of years before iron was invented. Once again, how could the Hittites have lived in the second millennium B.C. and have iron and write about iron which was only invented much later? This aspect of these scientific and technological forms of evidence Cochrane blithely skipped over without so much as a murmur that these facts flew in the face of his criticisms.

In addition to linguistics, tin bronzes, and iron, I showed how the historical and archaeological forms of evidence correlated with and corroborated these forms of science and technology and concluded:

“Again as with tin bronzes we have anachronistic mention and discussion of iron by the Hittites long before this metal is properly developed ... when taken together with art, sculpture, language, stratigraphy, etc. ... [t]he scientific and technological facts as well as the various forms of historical evidence all come together to indicate that the Hittites are the Lydians, that this civilization existed in the first millennium B.C. and not the second, [and] that the two cultures are indeed really one.

“To then argue [as does Cochrane] that other non-scientific, non-technological facets of historical analysis can be invoked to dismiss these concordances is to deny the validity of scientific and

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<sup>28</sup> *ibid.*, p. 512-516

<sup>29</sup> *ibid.*, p. 516-518

technological evidence. Unfortunately, it seems that this is the final refuge of those [like Cochrane] who simply refuse to understand the weight of all this physical evidence.”<sup>30</sup>

If Heinsohn and Sweeney are correct we must expect to find similar forms of scientific and technological evidence for the Mitanni that indisputably place them in the first millennium B.C. and not the second. In this regard Cochrane turns to archaeological stratigraphy in order to dispute the geological stratigraphy at Tell Munbaqa. He claims that:

“At Tell Brak, for example, the Old Babylonian strata (levels 8 through 10) lay well beneath the strata associated with the Mitanni period (levels 2 through 6) much as anyone would expect in conventional history.”<sup>31</sup>

The archaeological evidence Cochrane cites for this is D. Oates, *et. al.*, *Excavations at Tell Brak: The Mitanni and Old Babylonian Periods*, vol. I. (Cambridge UK 1997). It is here that Cochrane’s devious approach to evidence once again comes to the fore, because in that book are chapters which deal with facets of scientific and technological evidence which Cochrane completely failed to mention. These clearly show that the Mitanni could not have lived in the mid-second millennium B.C. and it is evident that Cochrane, having read this material, decided to keep it buried.

Let us remember the scientific, technological facts discussed above which show that neither tin nor iron was available in the supposed times of the Mitanni. Tin and iron, being primarily first millennium B.C. materials, could not be utilized by the Mitanni who are assumed to have lived hundreds of years before these materials were produced *en masse*. That being the case, if the Mitanni lived in the mid-second millennium B.C., one would not expect to find tin bronzes or iron artifacts or, most importantly, the production facilities and abilities for the manufacture of iron.

Nevertheless, iron objects were uncovered at Tell Brak in Mitanni/Mede level strata. Graham Philip cites Colin Shell that various iron objects

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<sup>30</sup> *ibid.*, p. 518

<sup>31</sup> Cochrane, *op.cit.*, p. 63

were found there. A Mitanni layer contained “a small rod of completely corroded iron.”<sup>32</sup> In addition there were also copper objects containing iron.

“The majority of these pieces studied are copper as cast from the smelting furnace, where the reducing conditions and high temperatures reduced both copper and iron from the roasted matte charge ... the iron content of these raw coppers is often higher than 25 per cent, which indicates temperatures close to or in excess of 1400 degrees C achieved in the smelting.”<sup>33</sup>

As was pointed out with respect to the Old Babylonians at Mari, cited above, Bronze Age furnaces cannot generate temperatures high enough to smelt iron. Because of this, Philip reached this astonishing conclusion about the iron mixed into copper at Tell Brak:

“The range of material analyzed provides clear evidence for the co-smelting of copper and iron at Tell Brak and at least as early as the fifteenth century [B.C.]. The material itself was not found directly associated with a metal workshop, but from the range of metallurgical activities, the working area was probably situated close by the find locations.

“The smelting conditions necessary to produce the high-iron content present in Tell Brak raw coppers are unequivocal evidence that the smelting technology necessary for producing raw iron was achieved at Tell Brak at a time prior to the large scale production of iron in the region.”<sup>34</sup>

That is to say, iron was smelted/melted to produce raw iron and then mixed with copper and heated until both alloyed together. All this was supposedly done in the pre Iron Age before any of this technology had come into being. It is a fundamental contradiction to the established chronology. However, when we move the Mitanni into the first millennium the entire problem vanishes.

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<sup>32</sup> Graham Philip, “The Metal Objects,” *Excavations at Tell Brak*, vol. I, David Oates, Joan Oates, Helen McDonald eds. (London 1997), p. 120

<sup>33</sup> *ibid.*

<sup>34</sup> Philip, *loc.cit.*

Further proof also comes from Tell Brak which Cochrane also ignored. That is, the Mitanni were carving and incising hard stone. Donald Matthews shows that at Tell Brak there were found “two major groups of Mitanni seals cut in hard stone.”<sup>35</sup> All this, of course, we are expected to accept was done hundreds of years before carbonized iron appears.

As we discussed glass objects at Old Babylonian Alalakh being chronologically out of line for all the reasons given above, the very same contradiction exists at Tell Brak because glass objects were found there, too. E.B. French in *Excavations at Tell Brak*, vol. I, which Cochrane cited, presents evidence. We read:

“Over 160 individual glass objects were found ... including glass beads ... the majority of the glass objects from Brak come from Level 2, ... many repeat types [of glass items] known from elsewhere, for example at Nuzi and Tell Rimah ... Among the most interesting objects are a number of glass ingots and many pieces of cullet, strongly suggesting that glass objects were being manufactured at Brak. The earliest securely stratified second-millennium glass found at Brak comes from Level 6, contemporary with the construction of the Mitanni Palace, and includes polychrome beads. Also possibly from Level 6 are two small fragments of core-moulded bottles ... Core-moulded glass bottles are unequivocally attested in Level 5, which can be dated to the fifteenth century [B.C.]. A core-moulded glass bottle was recovered from an early Mitanni context at Rimah ... The presence of cullet at Brak Level 5 suggests the manufacture of glass objects at the site in the fifteenth century [B.C.]”<sup>36</sup>

Julian Henderson in part G of the chapter on glass claims:

“Overall ... it can be stated that the glass industry was sophisticated and of a high standard by the fourteenth century BC, the likely date of most of the Tell Brak glass. That the developmental stage

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<sup>35</sup> Donald Matthews, “Mitanni Seals from Tell Brak,” *Excavations at Tell Brak*, *op.cit.*, p. 52

<sup>36</sup> E.B. French, “Glass Frit and Faience,” *Excavations at Tell Brak*, *op.cit.*, p. 81



had already occurred is attested by earlier core-moulded vessels at Alalakh, Nuzi, Tell al Rimah and Brak itself.”<sup>37</sup>

The stunning facts regarding glass production existing before fully developed iron technology are somehow never addressed. Since glass technology comes out of that for iron production we have the same contradiction we had for iron.

The reason for this is explained by Kurinsky. “Even more sophisticated furnaces were required for the production of true glass than the smelting of iron.”<sup>38</sup> Hundreds of years before the Hittites supposedly smelted iron the Mitanni had even more sophisticated furnaces than those necessary for iron, and could smelt glass, and then for some obscure reason this immensely important advancement in furnace technology was lost in a Dark Age until revived by the Hittites.

We were able to show in volume I of this series that the 18th Egyptian Dynasty existed in the first millennium B.C. (pages 477-482). Regarding glass production we are told by Diana L. Stein:

“Glass and glazed wares are two characteristic luxury products which for the first time were mass-produced [*sic*] and widely distributed in the Mitanni period. The oldest glass factory yet excavated in the Near East is located at Tell-el-Amarna in Egypt, the capital of Amenophis IV/Akhenaton [of the 18th Dynasty] who maintained political relations with Mitanni.”<sup>39</sup>

Therefore this evidence ties the Mitanni/Medes to the 18th Dynasty of Egypt to the first millennium B.C. Stein further informs us:

“The fully preserved goblet from Tell a Rimah represents a common type of glass vessel, so often found in broken fragments, which clearly belong to the Mitannian tradition of Nuzi ware, both in

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<sup>37</sup> *ibid.*, p. 95

<sup>38</sup> Kurinsky, *op.cit.*, p. 115

<sup>39</sup> Diana L. Stein, “Art and Architecture,” *The Hurrians*, Gernot Wilhelm ed. (Jennifer Barnes transl.) (Warminster UK 1989), p. 89

shape and in polychrome decorations. The core-formed vessel was made by winding viscous glass around a core of clay and dung.”<sup>40</sup>

In fact it is even suggested that glass manufacture was a Mitanni development:

“It has been argued that the expansion of glass production in the fifteenth or fourteenth century BC should be attributed to Mitanni and the best early glass comes from Nuzi, but the correlation of glass innovation with the Hurrians has not been proved.”<sup>41</sup>

Since the Hurrians came somewhat earlier than the Mitanni and the Mitanni took over the Hurrian lands, how is it that they exhibit no glass innovation?

Now it may be argued that a glass production facility was found at Qantir–Piramesses, Egypt in the late Bronze Age as presented by Thilo Rehren and Edgar B. Pusch in *Science*,<sup>42</sup> but this places this facility in the 19th Egyptian dynasty. We have shown in volume I of this series related to glass, that the late Bronze Age 18th, 19th, and 20th dynasties belong in the first millennium B.C. and not the mid second.<sup>43</sup>

That is there is a 500-year glass Dark Age when Egypt no longer has glass. Yet as was pointed out during this period, according to Lionel Casson, the Phoenicians had been able to have “shipped out fine Asia Minor wines ...[and] expensive glassware.”<sup>44</sup> As I wrote:

“Since the Phoenicians were able to supply Egypt with glass during the 15th century B.C., what caused them to stop supplying their former trading partners ...? Trade was the very core of Phoenician merchants’ life and a product that was so valuable surely would have continued to be produced for other trading partners as well as Egypt ...

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<sup>40</sup> *ibid.*, p. 90

<sup>41</sup> *The Dictionary of the Ancient Near East*, P. Bienkowski, A. Millard eds. (Philadelphia 2000), p. 200

<sup>42</sup> Thilo Rehren and Edgar B. Pusch, “Late Bronze Age Glass Production at Qantir-Piramesses, Egypt,” *Science*, vol. 308 (June 17, 2005), pp. 1756-1758

<sup>43</sup> Ginethal, *op.cit.*, pp. 473-476

<sup>44</sup> Lionel Casson, *The Ancient Mariners* (NY 1967), p. 182

But moving the 18th, 19th, and 20th dynasties into the first millennium makes the dark glass age into a clear, translucent age.”<sup>45</sup>

In that volume, I gave many other reasons and correlations—scientific, technological etc.—that indicate the same. Thus, the fact is that glass manufacture is a first millennium B.C. occurrence, not a second.

In a certain sense, glass production and glazing of pottery, tiles etc., are largely dependent on the same technologies. Rather than glazing techniques being continued after the Mitanni employed them, they simply die out, as glass manufacture dies out in Egypt for a period of 500 years or more. As John Dayton shows:

“In Mesopotamia, glazing appears to die out with the fall of Mitanni. The objects found in Assyria are either left-overs or imported from the Levant ... In Elam, glazing enjoys a brief floruit ... Elam is conquered by Babylon in the twelfth century B.C. and glazing comes to an end. It is significant that, during the long, prosperous Kassite rule of Babylonia, glazing is not in evidence, no doubt owing to the fact that good silica sand was unobtainable. There is therefore a gap [Dark Age] in glazing techniques in Mesopotamia until the neo-Assyrian conquests of the ninth century B.C. Glazing [then] reappears in a very crude manner on clay and develops, no doubt with the help of deported Phoenician craftsmen.”<sup>46</sup>

That is, there is a Dark Age of glass that followed this efflorescence of its development which runs from about 1200 to 850 B.C. but shows no real progress until well into the 6th century B.C. This has well been noted by M. Spaer in his work *Ancient Glass in the Israel Museum: Beads and Other Small Objects* (Jerusalem 2001), p. 27.

According to Oppenheim, in the library of Assurbanipal (a Persian king in the short chronology) the texts related to glassmaking are conventionally dated from the 7th century B.C., though taken to be copies of some earlier date, assumed to be 1500-1000 B.C. In these texts there are directions for glassmaking. However,

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<sup>45</sup> Ginenthal, *op.cit.*, pp. 475-476

<sup>46</sup> John E. Dayton, *Minerals Metals Glazing and Man* (London 1978), p. 395

Oppenheim points out there were rituals associated with this production, that preceded the operation. Oppenheim concluded that the craftsmen still did not understand why and how glass was formed and called upon the gods to work their magic. He suggests that even at this time glass making was still in its infancy.<sup>47</sup>

How could glass making in the 7th century B.C. be in its infancy while the Mitanni were making highly advanced glass objects almost 1000 years before this time? The concept is ludicrous.

Again why didn't the Phoenicians trade with the Kassites and sell these prosperous customers glazed pottery? Did the Kassites unlike nearly all ancient peoples fail to want luxury items? Did the Phoenicians lose their capitalist itch to trade these goods to the Kassites and others to get rich? None of this is explained or explicable. But moving the Mitanni as Medes into the first millennium removes the entire problem related to glazing and glass making.

Lastly with regard to glass there is the technical problem related to the production of glass with cobalt blue materials to give it a beautiful blue to black color. We are told that at Tell Brak:

“There are other specific technological features of second-millennium BC glasses which are worthy of mention here. The use of a cobalt source high in aluminium (a cobalt alum) probably located in Egypt has been inferred from the chemical analysis of cobalt blue Egyptian 18th Dynasty glasses; this cobalt source apparently introduced a relatively high level of aluminium oxide which has only been found in cobalt blue high magnesia glasses ... This does not mean that all cobalt blue second-millennium BC glass was either made in Egypt or coloured using the cobalt-bearing source; Middle Eastern cobalt was also used.”<sup>48</sup>

However, A. Lucas and J. Harris point out:

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<sup>47</sup> A. Leo Oppenheim, “The Cuneiform Texts,” in *Glass and Glass Making in Ancient Mesopotamia* (Corning NY 1988), p. 71

<sup>48</sup> Julian Henderson, “Part G” of “Glass, Frit and Faience,” *Excavations at Tell Brak, op.cit*, p. 94

“Cobalt ores, so far as is known, do not occur in Egypt, the only cobalt compounds yet found being traces in the alum minerals of both Kharga and Dakha oases and in the nickel ore of St. John’s Island in the Red Sea, these occurrences being certainly unknown anciently and the compounds presenting almost insuperable difficulties of extraction. Any cobalt compound used, therefore, must have been imported possibly from Persia or the Caucasus region ...”<sup>49</sup>

With regard to the source or sources of cobalt blue colored glass in Mesopotamia Dayton states that:

“... traces of the scarce metal cobalt [have] been confirmed by the results of the excavation of the Ulu Burun shipwreck off the coast of Turkey near Kaş by George Bass (1986). The cargo [was] dated very accurately to around 1400 B.C. by the Mycenaean, Cypriot, and Syrian pottery on board ... the most important finds were some 20 or more ‘round glass ingots’ which were colored by cobalt. Brill (1974) has confirmed that the glass is similar chemically to that of Mycenae and Egypt, whereas the glasses of Mesopotamia and Iran, or perhaps western Asia in general, are of a different chemical type. (Bass 1986: 282)”<sup>50</sup>

The greatest concern for chronology is to point out that the cobalt blue glasses found in Tell Brak and elsewhere in Mesopotamia are not derived from the same sources as those in Egypt and Mesopotamia. Nevertheless it is clear that Egypt was not the source of Mitanni cobalt blue colored glass. The problem is that:

“Cobalt blue glass ... found with the bronze and silver artifacts in the Middle East ... is even scarcer than tin ...”<sup>51</sup>

Hence we have a product—cobalt—far scarcer than tin in the Middle East, yet in Mesopotamia we have cobalt blue colored glass in abundance which

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<sup>49</sup> A. Lucas, *Ancient Egyptian Materials and Industries* (London 1962), Dover edition (Mineola NY 1999) p. 260

<sup>50</sup> John E. Dayton, *The Discovery of Glass. Experiments in the Smelting of Rich Dry Silver Ores and the Reproduction of Bronze Age-type Cobalt Blue Glass as a Slag* (Cambridge MA 1993) p. 27

<sup>51</sup> *ibid.*, p. 1

requires a source for this mineral. But since cobalt is exceedingly scarce in Mesopotamia, it, like tin, had to come from sources outside that region. This suggests that cobalt is a first millennium item.

However, there is a factor often overlooked by all researchers into cobalt blue colored glass as explained by Dayton:

“After 1300 B.C. the supply of cobalt seems to have been disrupted, and there is a curious hiatus until glass appears again in large amounts with the Phoenicians, perhaps as late as 600 B.C. The body of this glass is once again colored by cobalt, often almost black in color.”<sup>52</sup>

That is, we have a cobalt blue Dark Age of about 500 to 600 years. Needless to say, the fall of so-called Mitanni comes at around this time. If we accept for a moment that Mitanni fell as stated in the literature.

“Historical evidence in the form of cuneiform annals provides the information that two Middle Assyrian kings Adadnerari I (1305-1274 BC) and Shalmaneser I (1273-1244 [B.C.]) claim to have destroyed the cities of Mitanni, including Taidu which if [it] is not Tell Brak is certainly in the immediate vicinity. Both kings claim to have captured the Mitanni cities ‘from Taidu to Irridu ... the fortress of Harran to Carchemish which is on the Euphrates,’ and Shalmaneser concludes ‘I ruled over their lands but I set fire to the remainder of their cities.’”<sup>53</sup>

That is, Mitanni supposedly fell about 1240 B.C. and when we remove the cobalt blue Dark Age, Mitanni ruled at around the same time as the Medes. By removing the cobalt blue Dark Age the production of cobalt blue colored glass continues without interruption. It begins in the first millennium in the time of the Medes and develops forward as a well-known technology. The reader is requested to recall that there was no way the scientific, mathematical developments of ancient times were supposed to be passed along to the Persians by the Medes. In this case by placing Mitanni in the time and place of the Medes there is no “curious hiatus” in the manufacture of cobalt blue glass in Mesopotamia.

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<sup>52</sup> *ibid.*, p. 2

<sup>53</sup> D. Oates *et al.*, eds., *Excavations at Tell Brak, op.cit.*, p. 14

Let us turn again to tin bronzes in Mitanni times. Among the metal objects found at Tell Brak were those of bronze. As Philip states:

“[Among the objects found at Tell Brak at the Mitanni level was a] copper/bronze drinking tube”<sup>54</sup> as well as “sheet bronze”<sup>55</sup> and “copper/bronze furniture fittings from [the Mitanni] Palace Room 11.”<sup>56</sup>

In terms of their horses Richard A. Gabriel remarks:

“It was another Mitanni practice to armor the[ir] chariot horses. Often this horse armor consisted of a textile coat ... Equally common was the covering of the textile coat with a leather, copper or a bronze scale overcoat.”<sup>57</sup>

This brings us back to the source of tin at the supposed time of the Mitanni. As pointed out earlier, a 2003 book *Das Zinn der Bronzezeit in Mittelasien*, reviewed in the *Journal of Near Eastern Studies*, made the claim that archaeological evidence was found in the regions of Uzbekistan and Tajikistan and that a flourishing tin mining industry encompassing the first half of the second to early part of the first millennium B.C. existed to supply the Mitanni with tin for tin bronzes. Uzbekistan and Tajikistan are situated east of the Caspian Sea and north of the Iranian Plateau. Any tin from these mines would have to be carried through the Elburz mountain chain in the northern part of the Iranian Plateau or along the south shore of the Caspian Sea to the southern Caucasus mountain chain to reach so-called Mitanni. Another route would be across the Caspian Sea or around its northern shore and then south across the Caucasus. However, Philip L. Kohl, writing about the “Trans-Caucasian ‘Periphery’ in the Bronze Age,” fully admits that such trade links simply do not exist:

“With the possible exception of relating the advent of tin-bronzes and weaponry best known from Asia Minor with the Old Assyrian trade, such linkages have not been suggested by our review of the

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<sup>54</sup> Philip, “The Metal Objects,” *op.cit.*, p. 115

<sup>55</sup> *ibid.*, p. 117

<sup>56</sup> *ibid.*

<sup>57</sup> Richard A. Gabriel, *The Great Armies of Antiquity* (Westport CT 2002), p. 90

Transcaucasian prehistoric sequence. Part of the problem certainly relates to the archaeological record: in completeness, chronological difficulties and the like ...

“Perhaps further work will reveal a shift in international exchange and a redirection of Caucasian metals and source materials to the south during the second half of the second millennium B.C. when Caucasian metallurgy ‘takes off,’ truly becoming one of the richest areas of metallurgical production in the Old World ... Unfortunately the details of such a picture cannot yet be drawn. Paradoxically the rich Late Bronze and Early Iron cultures of Transcaucasia ... appear to us to be somewhat withdrawn or turned in upon themselves (culturally involuted), not active participants in a larger world system.”<sup>58</sup>

The literature discusses the concept that tin came from the east into Anatolia.

“[James] D. Muhly reminds us of the fact that a very well-known trade route, documented in numerous Old Assyrian cuneiform texts dated to the 2nd millennium B.C. recorded the passing through the Taurus Mountains of hundreds of donkey caravans bringing tin from a distant eastern source into central Anatolia.”<sup>59</sup>

But there is no evidence to support tin trade in these regions from the east (this is to be discussed below). However, I also pointed out with regard to these tin sources that camels were found in the stratum where these mines existed, and showed that, based on the established chronology, camels do not come into the Near East until around 1100 B.C. So there is no reason to suggest these mines operated in the second millennium B.C. but rather in the first.

Large amounts of tin bronzes were used in Mitanni.

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<sup>58</sup> Philip L. Kohl, “Transcaucasian ‘Periphery’ in the Bronze Age,” *Resources, Power, and Interregional Interaction*, Edward M. Schortman, Patricia A. Urban, eds. (NY 1992), pp. 133-134

<sup>59</sup> Michael Yu. Treister, *The Role of Metals in the Ancient Greek History* (Leiden, the Netherlands 1996), p. 28



“A particularly interesting aspect of the Mitanni chariot was that some of them appear to have been armored with metal scales called *sariam* [made of tin bronze] ... a suit of Mitanni body armor consisting of 500 scales weighed approximately 35 pounds [17.5 kilograms] ... horse armor consisted of a textile coat of felt or hair covering ... the textile with a leather, copper or bronze scale overcoat. ...

“... a coat of bronze horse armor would easily have weighted 100 pounds [45 kilograms].”<sup>60</sup>

It is evident that tin bronzes, iron, cobalt blue, or any of the other scientific/technological materials fall neatly into place by having the Mitanni of the mid second millennium B.C. placed in the first. The fact of the matter is that this entire period is overburdened with a lack of scientific archaeological evidence and Dark Ages with neither archaeological nor documentary evidence. And one wonders why the Phoenicians simply stopped trading all these materials for hundreds of years. Of course they did not stop trading. The only thing that is stopping them from trading all through this period is historians claiming there was a Dark Age. As we will show in Chapter 4 of this volume, it was during the first millennium B.C. that there was a far reaching, ongoing international trade connecting nearly all these civilizations. Just as we in the recent Western tradition had our great “Industrial Revolution,” we will see the ancient Near East experienced its own running from around 700 to 300 BC.

## **DENDROCHRONOLOGY AND RADIOCARBON DATING THE SECOND MILLENNIUM B.C.**

According to Heinsohn and Sweeney, civilization in Mesopotamia begins about 1200 B.C. To a great extent much if not most of the second millennium B.C.’s history is shrouded in Dark Ages, has technologies that could simply not have existed during this period, and thus should be considered a fiction. What then is the scientific and/or technological evidence for the reality of the chronology which

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<sup>60</sup> Gabriel, *op.cit.*, p. 90

places several ancient empires in this time slot? Earlier we were informed by Cryer and others that the astronomical evidence for this period—the Venus Tablets of Ammisaduqa—has no standing and thus must be disqualified as a chronological marker for this period. What then are historians left with, either scientific or technological in nature, to prove that their chronology for nearly all of the second millennium B.C. has any substance?

What they have tentatively turned to is dendrochronological evidence from Anatolia as developed by Peter Kuniholm and his team from Cornell University. Let us examine whether or not one can trust Kuniholm's evidence. If indeed this form of chronological dating is shown to be valid it could be a very serious blow to the short chronology. Nevertheless, we will find that this dendrochronological edifice is a fabrication of very little worth, if any at all. Peter James states:

“Every archaeologist's dream is a dating technique which can date finds to a precise year, and of all available methods [except astronomical dating] dendrochronology comes nearest. ...

“[T]he fact that dendrochronology deals in real, exact calendar years makes it more ‘tangible’ than radiocarbon, which can only express dates as a broad range in terms of statistical probability. To know that the last tree-ring in an artifact dates to 899 BC is far more rewarding than learning that it grew sometime between 1020 BC and 830 BC (at 95.4 % probability). One reason for the vagueness in radiocarbon dating is that the amount of carbon 14 produced in the atmosphere (and hence absorbed by living organisms) has not remained constant. It is dendrochronology that has provided the key to this puzzle: by measuring amounts of C14 in well-dated tree-rings we can calibrate radiocarbon results. Hence the double importance of a sound dendrochronology.”<sup>61</sup>

Colin Renfrew succinctly sums up the work of Kuniholm *et al.* as it relates to proving that the chronology of the ancient Near East is as long as historians maintain it to be:

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<sup>61</sup> Peter James, “The Dendrochronology Debate,” *Minerva*, vol. 13, issue 4 (July/Aug 2002), p.

“The goal of constructing a precise reliable chronology for the archaeology of the eastern Mediterranean based on tree-ring dating and supported by radiocarbon determinations is now clearly in sight. It is Anatolia that has yielded the necessary samples of ancient wood. So when the Anatolian tree-ring[s] ... confidently ... give absolute dates for the time range 3000 BC, the entire prehistory and early historic chronology of Egypt and the Near East will have to be revised using the more secure chronological framework that dendrochronology can offer.

“... a team from Cornell, Heidelberg and Reading [have] presented the latest work towards anchoring the 1,503 [year long] floating dendrochronology produced for Anatolian wood samples over a time span covering the whole of the second millennium BC, and several centuries both before and after. Kuniholm *et al.* use radiocarbon based ‘wobble-matching’ to try to fix the absolute dates for this floating sequence more precisely. (Wobble-matching involves matching specific irregularities on the master tree-ring calibration-curve with irregularities in a series of known interval radiocarbon dates from a given wood sample). A second step in their argument is to equate an exceptional growth event recorded in trees at Porsuk in south-central Anatolia with a widely recognized special marker event seen at 1628 BC [the supposed eruption of the volcano on the Aegean island of Santorini, ancient Thera] in the already well-established tree-ring chronologies for the Northern hemisphere. If [this marker is] correct, this would indeed anchor their Anatolian chronology to within a single year, with wide-ranging archaeological consequences. ...

“There is nothing more tantalizing than a ‘floating’ tree-ring chronology ... But to ‘anchor’ a floating chronology securely a continuous sequence of samples spanning the entire period ranging back from the present to the time in question is ideally required.”<sup>62</sup>

Six years later Sturt W. Manning *et al.* redid this work and claimed:

“This chronology is central to the dating of some 22 Bronze and Iron Age sites ... and forms a pivotal reference point for the

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<sup>62</sup> Colin Renfrew, “Kings, tree rings and the Old World,” *Nature*, vol. 381 (June 27, 1996), p. 1

archaeology and history of the eastern Mediterranean. The core of the chronology consists of 1026 years of cross-dated and well replicated tree-rings preserved at the Phrygian capital of Gordion.”<sup>63</sup>

If this dendrochronology is valid and there are 22 sites in Anatolia dated and/or related to it going back well into the second millennium B.C. this would be a staggering contradiction to Heinsohn’s and Sweeney’s chronologies because one would not expect to find highly civilized sites in Anatolia dated well into this period. If there were such sites for this time surely one cannot contend that there were none in Mesopotamia proper that are taken by the historians to be coeval with those in Anatolia.

To begin with, Renfrew claims that there is a break or empty period in the Anatolian dendrochronology between the period of that chronology and the present. This means that, until or unless that empty period can be spanned by a series of tree rings to connect the ancient past with the present, we really don’t have a truly reliable chronological marker. It is a kind of tree-ring Dark Age which indicates that the entire structure is dubious at best and should not be employed as a precise chronological marker upon which one can construct the chronology of the ancient world.

Before beginning it is important to point out that tree-ring dating is based on matching rings of various sets of different wooden materials with one another and to some extent this is a kind of jig-saw puzzle wherein the total picture presented after the pieces have been fitted together may represent very different pictures and not necessarily the correct one. Michael Baillie points out:

“As with conventional jig-saws, some people are better at pattern recognition than others and if the analogy is not too brutal, there are those who might force the pieces together. It has to be remembered that there is only one correct pattern: each tree has grown only once and ultimately its ring pattern can only fit at one place in time. Simply because two pieces look alike does not necessarily mean they fit together.”<sup>64</sup>

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<sup>63</sup> Sturt W. Manning *et al.*, “Anatolian Tree Rings and a New Chronology of the Eastern Mediterranean Bronze Iron Ages,” *Science*, vol. 294 (Dec. 21, 2001), p. 2532

<sup>64</sup> M.G.L. Baillie, *Tree-Ring Dating and Archaeology* (Chicago IL 1982), p. 23

A second, but more significant point that must also be taken into consideration is that tree-ring dating must not be in contradiction to astronomical dating, which is by far the most precise form of chronological analysis. We have already shown that the Old Babylonian kings Hammurabi and Ammisaduqa have almost the same 30-day months as the Persian kings Darius the Great and Artaxerxes III Ochus. The number of such months in the correct order is far too great to be expected to have occurred randomly. Thus the Old Babylonians belong in the middle to latter part of the first millennium B.C. and cannot be placed at the early part of the second. If the dendrochronology goes back that far it must not show that the Old Babylonians belong where the historians have placed them. When we add to the mix all the technological forms of evidence, the dendrochronology cannot be valid when it is contradicted by so much additional evidence. But as we will see the dendrochronology presented by Kuniholm *et al.* fails on its own because it is riddled with errors and uses invalid processes. Peter James explains how this tree-ring *master dendrochronology* was organized:

“The hopes of East Mediterranean archaeologists presently rest on the work of Professor Peter Kuniholm and his Cornell University team, who, for many years, have been working on a dendrochronology for the ancient Aegean and Anatolia. The Aegean Dendrochronology Project has had some major successes. In the late 1950s American archaeologists working at Gordion excavated a spectacular royal burial believed to be the tomb of the famous Phrygian king Midas ... Midas was a real king mentioned in [Neo-] Assyrian records of the late 8th century BC, and the tomb could well be his. ...”<sup>65</sup>

The tomb was found under a mound of earth about 190 feet or 60 meters high. Inside was a small cabin made of large logs cut lengthwise and shaved smooth on the inside wall of the cabin. Outside the cabin and built over it was a stone cabin with the space between the wooden and stone walls filled in with stones. The stone cabin was sealed with waterproof clay. The tomb contained finely inlaid wooden furniture, copper and bronze vessels, etc. On a bed a skeleton of a man about 60 years old was found. There were no weapons, no gold nor silver objects in the tomb. Yet royal burials of this era did contain gold and silver in the form of electrum, so this could hardly have been the tomb of Midas. Surely if, as the myth claims, he

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<sup>65</sup> James, *loc.cit.*

would have nothing to do with gold, he certainly would not have failed to surround himself with silver.

The most important point to stress is that no one really knows who the person buried at Gordion is, nor when he was buried, neither scientifically nor technologically. Therefore his belonging in the late 8th century is only an assumption. The date can be off by hundreds of years. In reality the size and girth of these logs indicates the trees in it cannot have come from this area.<sup>66</sup> If the climate was the same as that at present, as historians maintain, then the logs had to have been transported from some distant region. This introduces the problem of from how far away they came and even possibly that the logs came from different regions. Therefore, in spite of the dating of the Midas tomb by interpretative, archaeological methods, the dendrochronology of this tomb as an anchor may indeed be off by hundreds of years. James further tells us:

“It [the tomb] proved to be a gift to dendrochronology. Underneath a huge earth mound, the small building forming the tomb consists of large juniper logs with the bark still present. One log contained as many as 918 rings and the group, taken together, allowed Kuniholm to establish a sequence going back 1026 years. Next, the pattern of the earliest rings was found to overlap with the last ring of another long sequence from Porsuk in southern-central Turkey [separated by hundreds of miles]. Together they span 1503 years – from the Middle Bronze Age to the assumed 8th century date for the tomb.”<sup>67</sup>

But just how well does the Gordion tomb tree-ring sequence match with the one at Porsuk? Douglas J. Keenan explains the problem.

“Wood from a gateway [at Tille Höyük] was matched with the [Gordion] Anatolian dendrochronology (Kuniholm 1993). Two statistical tests were employed [to accomplish the fit between the two dendrochronologies]: correlation (of ring widths from the gateway with the ring widths from the master dendrochronology [from Gordion etc.]) and trend[ing] (number of years in which [the] gateway [tree-rings] and

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<sup>66</sup> King Midas, see: <http://mmtaylor.net/Holiday2000/Legends/Midas.html>

<sup>67</sup> *ibid.*

the master ring widths increased and decreased together). The end date for the gateway wood was found to match [not one but] three calendar dates (i.e. dates from the master dendrochronology) with very high confidence on both [correlation and trending] tests: [Excellent fits existed for the years] 1285 BC, 1140 BC and 981 BC. For 981 BC, the significance of the correlation [test] was 99.99995% and the trend [test] 95.5%. The investigators [then] matched the wood [dated] to 1140 BC (correlation [test] significance 99.9995%: trend [test significance 99.9999%]). The high significance of the other two dates are attributed to randomness.

“The exclusion [by the investigators] of the 981 BC date [in favor of the 1140 B.C. date] is not credible. Additionally, the existence of three widely-separated dates [for matching the Gordion and Porsuk tree-rings together] with such high statistical confidence implies that there are serious problems with the statistical analyses that were being employed. Hence almost any [dating] conclusion based on those analyses should be regarded as being at least questionable.”<sup>68</sup>

Not only do we not know when in time the Gordion dendrochronology fits, because we cannot precisely, scientifically date the grave, but it could have been joined with the dendrochronology of the gateway at Tille Höyük, which itself cannot be precisely scientifically dated, at three different places with extremely high statistical levels. The investigators chose one of the three dates based on their belief that that date was correct, not that they could prove it to be correct. Keenan calls such a procedure “at least questionable,” when at best it is highly dubious.

However, that is not the full extent of the problems that exist for matching the Gordion tomb and other wood materials that went into creating the master dendrochronology with the gateway wood at Tille Höyük. Keenan points to a major flaw:

“In order to ensure that a tree-ring match is reliable, it is typically necessary for the tree being matched to have at least 100 rings of overlap with the master dendrochronology ... Severe problems can

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<sup>68</sup> Douglas J. Keenan, “Why Early-Historical Radiocarbon Dates Downwind From the Mediterranean Are too Early,” *Radiocarbon*, vol. 44, no. 1 (2002), pp. 232-233

arise when there are fewer. An example from Ireland will illustrate this (described by Baillie [1995: ch. 3]). Several planks from a boat were securely dated against an Irish master [dendrochronology]. Attempts were also made to date one plank that had only 35 rings. As the investigator noted ‘Normal practice at ... most tree-ring laboratories ... would have been to ignore this small piece ... as intrinsically undatable’. As an exercise, however, an attempt was made. [There were] two positions where the 35-year ring pattern displayed visual agreement with an Irish master [dendrochronology] and with a generalized master for the British Isles ...

“As the investigators noted [as did the investigators for the Tille Höyük gateway find], these matches ... are extremely good. The archaeological context, however, makes the dates implied by [the generalized British Isles dendrochronological master] match untenable, and [the Irish master] very unlikely. The investigators concluded as follows (emphasis as in original) [Baillie, 1995: pp. 54-55]:

““The truth is that no one can put their hand on their heart and swear to a unique dating for such a short section of ring pattern. *It doesn't matter how good the match is ...* anyone trying to tell you that they have dated such a short sample is kidding both themselves and you.””

“He adds that there is nothing unique about this example. Pilcher [1990] says similarly:

““There are ... examples in the literature of [tree-ring matching] on timbers of less than 50 years ... Most of these must be treated with considerable caution ... the dating is not true dendrochronology but is tree-ring-assisted dating or even tree-ring-assisted guess-work.’

“Furthermore, Pilcher and Baillie [1987] tested sequences of rings from living (Irish oak) trees against a nearby master dendrochronology. For ring sequences of less than 80 years, half of the sequences gave no statistical indication at all for the correct date. In



other words, trees with less than 80 rings would, in general, not be reliably datable.”<sup>69</sup>

And that is precisely the problem with matching the Tille Höyük gateway wood to the Anatolian master dendrochronology to connect it with the Gordion tomb dendrochronology. In this respect Keenan shows:

“The wood matched from the [Tille Höyük] gateway had 218 annual rings. Those rings, though, were not from a single tree, but from [a] mini-dendrochronology that was constructed from fragments of 26 different trees, which been used to build the gateway (Kuniholm *et al.*, 1993). Almost all of those 26 trees had fewer than 80 rings recovered; most had *fewer* than 60. With fewer than 80 rings, dendrochronological dating can easily fail ... Indeed the Anatolian investigators themselves claim that at least 100 rings are typically needed to be certain of a match. The problem is compounded because in the Near East, during ancient items, wood was often reused. In fact, this still occurs in modern times: for example, the investigators have also concluded that the joist in a modern Turkish house is over 6000 years old (Kuniholm 2001). Thus the match claimed for some of the 26 trees could easily be false. Hence the construction of the mini-dendrochronology is likely erroneous, at least in part.”<sup>70</sup>

But even that is not the end to the problems associated with matching the Tille Höyük gateway wood to the Anatolian master dendrochronology. Keenan adds:

“Moreover, every major dendrochronology known to us has been built using a single species of tree with samples grown in similar climatic regimes. Yet Anatolian dendrochronological work (such as the gateway matching) has been done using a mixture of deciduous [like oak trees] and coniferous [fir] trees, which sometimes have been taken

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<sup>69</sup> Douglas J. Keenan, “Anatolian tree-ring studies are untrustworthy” (Internet), pp. 7-8 [www.informath.org/ATSU04a.pdf](http://www.informath.org/ATSU04a.pdf)

<sup>70</sup> Keenan, *Radiocarbon*, *loc.cit.*

from more than one of Anatolia's different climatic regimes. This plainly makes the above comparisons or ring widths fragile."<sup>71</sup>

Deciduous trees grow rings in the spring, summer and part of the autumn. The growth is due largely to rainfall. Coniferous trees grow a ring each year largely due to temperature. As R.W. Fairbridge points out!

“Certain pitfalls have been discovered in tree-ring analysis. In certain latitudes the tree growth correlates with moisture but in others it may correlate with temperature. From the climatic viewpoint, these two parameters are often inversely correlated.”<sup>72</sup>

Baillie further informs us that to produce a proper valid tree ring sequence we must have the correct species of tree, not many different varieties and even if of the same type there are still significant problems:

“... timbers must be of the correct species, have long [tree-ring] patterns, should be indigenous [to the area where the tree-ring sequence is being constructed] etc. Even if all these conditions are satisfied there is still no guarantee that a date will be forthcoming for a particular piece of wood ... Customers [who present wood samples to be matched to a master dendrochronology] tend to exert pressure for the dating of more marginal—particularly shortlived—samples.”<sup>73</sup>

In essence, as with radiocarbon dating, when customers/investigators submit wood samples to be dated against a master dendrochronology, they exert pressure to try to fit samples—even of less than 100 years—into the sequence to fit and/or strengthen their chronology. What then of wood samples that come from the same type of tree? Baillie goes on to show:

“We now know that the oak panels used in the construction of the ‘English’ art-historical chronologies had originated in [and were brought to England from] the east Baltic. ...

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<sup>71</sup> *ibid.*

<sup>72</sup> R.W. Fairbridge, “Holocene,” *Encyclopedia Britannica Macropedia* vol. 8. (Chicago 1982), p. 999

<sup>73</sup> M.G.L. Baillie, *A Slice Through Time* (Bath UK 1995), p. 49

“1. We know for certain that wood has been moved around in the past—where it is now may bear no relationship to where it grew.

“2. Oak looks like oak no matter where it came from (at least the sessile and pedunculate varieties) and so it is inherently impossible to source by appearance or physical characteristics. It is now accepted that the two sub-species cannot be separated on the grounds of wood anatomy. ...”<sup>74</sup>

In 1970 Paula V. Krebs showed that trees from the same species but from four forests separated from each other by a few hundred miles should not be used to create or fit each other’s dendrochronology. That approach to tree-ring dating is considered to be highly inappropriate. She employed bristlecone pine trees from four stands in Colorado. The tree ages from oldest to youngest spanned 1,568 years, 843 years, 666 years and 457 years. That is, from oldest to youngest, the trees dated from 403 A.D., 1138 A.D., 1305 A.D., and 1514 A.D. According to Krebs:

“There are a few similarities in the four chronologies, but the dissimilarities are more striking. For the segment common to all four chronologies, AD 1514 to 1970, there are six common diagnostic points [rings which match each other on the calibration curves]. These are [for the years] 1645, 1748, 1851, 1880 and 1902.”<sup>75</sup>

Hence, according to Krebs, this extrapolates so that the period from A.D. 1514 to 1970 averages only 1.3 common diagnostic features per century. Yet Kuniholm *et al.* would have us accept that the Gordion tomb and Tille Höyük timbers showed clear matching sequences even though these regions are several hundred miles apart!

What should be obvious at this point is that the dendrochronologies of four living stands of bristlecone pine trees clearly contradict the notion of Kuniholm *et al.*, that Gordion and Tille Höyük, separated by many hundreds of miles, can be integrated or matched to create a unified dendrochronology for Gordion and the Tille

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<sup>74</sup> *ibid.*, p. 50

<sup>75</sup> Paula V. Krebs, “Dendrochronology of Bristlecone Pine (*Pinus aristata* Engelm.) in Colorado,” *Arctic and Alpine Research*, vol. 5, no. 2 (Spring 1973), p. 149

Höyük gateway. Different climatic zones do not produce tree-ring sequences that can be integrated with each other. As Keenan remarks:

“... the wood from Tille Höyük might be undatable even in principle. In order to match trees against the Gordion master dendrochronology, there must be substantial correlation between the climate of the site where the trees grew and the climate of Gordion. Of the various aspects of climate that affect tree growth, (growing-season) precipitation is arguably the most important (with temperature and perhaps cloud cover also being highly consequential). Yet only about 12 % of the variation in precipitation at Tille Höyük is shared with the variation in precipitation at Gordion, at least in modern times. (Ancient times would seem unlikely to have been greatly different although that cannot be ruled out.) This seems likely too small [a climate match] to be confident of a reliable match for Tille Höyük (given the number of years for which rings are available).”<sup>76</sup>

Keenan further shows:

“Even within a very small area, local effects can sometimes lead to large differences in tree-ring growth. For example La Marche [1974] measured living trees at two sites in Nevada that were on the same side of a mountain, with one site just 130 m[eters or 400 feet] higher than the other, near the tree line: the ring widths of trees from the two sites showed no general correlation. As another example, Hillam [1980] compared master dendrochronologies from two sites at York, England, with a master dendrochronology from Exeter, also in England: the first York site compared with Exeter gave a [very low statistical matching] *t*-score of only 0.5, and yet the second York site compared with Exeter gave a [moderately low statistical matching] *t*-score of 3.5. (In modern times York and Exeter share about 50% of their [climatic] variation in precipitation. It should be noted though that these English master dendrochronologies included few trees, which weakens matching.) Local effect might be particularly relevant in Anatolia, because it has a highly variable topography with [high] mountains [unlike England].”<sup>77</sup>

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<sup>76</sup> Keenan, “Anatolian tree-ring studies are untrustworthy,” *op.cit.*, p. 8

<sup>77</sup> *ibid.*, p. 12

While the above living trees in England fairly approximate to each other were few in number, this is not at all the case for numerous stands of maple trees in Canada where dendrochronologists did an analysis to determine the effect of acid rain on them. D.L. McLaughlin *et al.*, explain:

“Growth is the cumulative result of all factors acting on trees over time. Some factors such as tree species, tree genetics and site quality, are fixed and, therefore, exert a relatively constant influence on the resultant tree growth. Stress factors, such as climate, insects and disease, [as well as] tree competition ... can change dramatically with time and contribute to radical growth fluctuation.”<sup>78</sup>

The problem, though never discussed, is explained below.

“One day while working on ... [a] project, we went outside to watch a neighboring farmer cut down a large old hemlock that had become diseased. As his saw cut deeper into the tree, we joked that it had now bit into history as far back as the Depression. ‘*Depression?*’ grunted our friend. ‘I thought you fellas were historians. I’m deep enough now so’s Hoover wasn’t even a gleam in his father’s eye.’

“With the tree down, the three of us examined the stump. Our woodcutter surprised us with what he saw.

“‘Here’s when my folks moved into this place,’ he said pointing to a ring. ‘1922.’

“‘How do you know without counting rings?’ We asked.

“‘Oh, *well*,’ he said, as if the answer were obvious. ‘Look at the core, here. The rings are all bunched up tight. I bet there’s sixty or seventy—and all within a couple of inches. They came when the place was still forest. Then, you notice, the rings start getting fatter all of a sudden. That’s when my dad cleared behind the house—in ’22—and

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<sup>78</sup> D.L. McLaughlin, D.E. Corrigan, W.D. McIlveen, “Etiology of Sugar Maple Decline at Selected Sites in Ontario (1984-1990),” report prepared by the Phytotoxicology Section Air Resources Branch, Ontario Ministry of the Environment (Ontario, Canada 1992), p. 94

the tree started getting a lot more light. And look further out here—see how the rings set together again for a couple [of] years? That’s from loopers.’

“Loopers?’ We asked cautiously.

“Sure—*loopers*. You know. The ones with only front legs and back.’ His hand imitating a looping, hopping [insect] crawl across the log. ‘Inch worms. They damn near killed the tree. That was sometime after the war—’49 or ’50.’ As his fingers traced back and forth among the concentric circles, he spoke of other events [that affected tree ring size] from years gone by. Before we returned home, we had learned a good deal about past doings in the area.

“Now it occurs to us that our neighbor had a pretty good knack for putting together [tree ring] history [from past events].”<sup>79</sup>

This woodcutter could tell why the tree rings were narrower or wider because he had lived in that area and seen all that had happened to the tree that affected its tree ring sizes. But dendrochronologists who examine only the tree rings without having lived on the spot to know these events that affected the tree ring sizes must assume that the sizes of the tree-rings were only affected by climatic conditions.

Certain insects cause defoliation periodically which can greatly inhibit photosynthesis during the growing season. During such periodic infestations trees produce less food and deplete the food stored as starch in their roots. When this stored food is depleted trees produce significantly narrower tree-rings. Periodic or long-term plant disease can also result in narrower tree rings, particularly those diseases affecting the roots which absorb water and minerals for food production. Even in ideal climates, trees can be affected by these problems. Further tree branch damage during strong periodic wind, snow or ice storms can also cause growth problems. If a tree is significantly damaged by such an event it must use whatever resources it possesses to repair the damage so that these resources cannot go into the production of larger tree-rings. How can one know if these periodic damages

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<sup>79</sup> James West Davidson, Mark Hamilton Lytle, *After the Fact. The Art of Historical Detection* (NY 2004), pp. XIII-XIV

occurred when examining dead ancient logs? This, it is strongly suggested, cannot ever be known. Trees, like all living organisms, are subject to all the vicissitudes of life, nor can these problems connected with ring production be distinguished from stresses caused by the climate. A dendrochronologist can only determine this if he or she knows the climate and these other conditions year by year for the life of any ancient tree examined. Without such knowledge, all such analysis is simply guess work! I have still not found a paper by a dendrochronologist that clearly addresses these problems to explain how, without knowing the ancient climate and other factors that affect tree ring size in the first place, year by year, before analyzing an ancient log, one can determine if narrow rings are related to climate (precipitation etc) or result from insect defoliation or disease, etc. I believe dendrochronologists do not address this question because they simply do not have an answer for it.

In Ontario, Canada, the work carried out by the provincial government's Ministry of the Environment between 1984 and 1990 was done with the dendrochronologists knowing the climate year by year, and importantly it was conducted on numerous stands of trees approximate to each other. There were literally dozens upon dozens of trees examined. The study investigated acid rain effects on sugar maple trees from 11 sites. Each stand of trees studied covered a few acres at each site. In order to qualify for the tests the trees had to be on level ground and in full sunlight, so as to be subject to exactly the same climatic conditions. Two increment cores were taken, one from the north side and the other from the south side of each tree at the same height above the ground. These cores were mounted on plywood boards and sanded to make the tree-rings stand out more clearly. Fluoroglucinol was used to bring out particularly faint rings. About 10% of the cores had sections so difficult to measure with any degree of confidence that they were not considered. "The cores with the clearest rings were measured first and used as quality control bench marks against which the less clear tree ring series could be compared."<sup>80</sup>

The trees were segregated into two groups: one was composed of healthy trees; the other of sick trees in decline. Graphs were made of the ring growth of both groups. Similarities were noted in the patterns between the two groups, yet great discrepancies were also noted. During some years, while one group of trees had rings that were increasing in size, the other group had rings that were decreasing in size. While, for example, the healthy trees began to produce narrow tree-rings,

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<sup>80</sup> McLaughlin *et al.*, *op.cit.* p. 95

the sick trees began, in the same years, to produce wide ones. There was clearly anticorrelation growth between the tree-rings of both groups during the same years. Furthermore, anticorrelations were averaged; and these anticorrelations happened within each group at the 11 sites.

The Ontario dendrochronologists made the following admission after examining the evidence: “Even though the tree rings can be measured with considerable accuracy and the data [can be] detrended (statistically corrected) to minimize the geometry associated with tree age and stand competition, **the effect of climate may be significant and remains largely unquantifiable**”<sup>81</sup> (bold emphasis added). This means that the dendrochronologists with a complete set of tree-rings from numerous living trees in the same or approximate area, were unable to organize the climate conditions that would explain their sizes. But climate is supposed to be the most significant factor that determines tree-ring size. Surely the healthy trees by themselves should have shown what the climate conditions were. The Ontario dendrochronologists finally did show correlations, but only by *choosing* comparable ring sizes from each group of trees. This, of course, proves nothing since they already knew the years these rings grew. But from ancient trees, it is the matching that determines where the correlations are and the dendrochronologists have reversed the process to get their results. A further point must also be stressed, namely that one cannot know, nor can it ever be known, whether the dead trees chosen for correlation of the ancient past all grew on level ground and in full sunlight. On steep slopes, such trees would most probably have very different growth patterns; however, if their ring sequences mimicked the pattern of trees in the same or other localities that were hundreds of years older or younger, they would be included in the group which the tree-rings most resembled. The same applies to trees growing in areas that were highly shaded, say by a nearby mountain or cliff. Once these trees are harvested and brought together at a site, any correlations would be spurious.

Lastly, as we know from volume I of this series, wood was a precious commodity in the Near East and logs may have been reused again and again over long periods of time before arriving at the final site, such as Gordion or Tille Höyük where they were then found and employed to create a dendrochronology. How can anyone know if these logs or wooden pieces were used at other places before they

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<sup>81</sup> *ibid.*, p. 96



arrived at these sites? The obvious answer is that this cannot be known, only assumed to be the case, not proven to be.

The work presented below is taken directly from Douglas J. Keenan's "Anatolian tree-ring studies are untrustworthy" and adapted by shortening it.

The ability to match different sets of tree rings is fundamental to dendrochronology and strongly depends on the use of statistics. Usually the dendrochronologist will attempt to find out if the statistical match is also a good visual one. This is quite difficult and more often impossible because the wood remains found are the burnt remains of trees of different species. Therefore the tree ring matches are usually made almost exclusively on tree ring width measurements. Thus statistics are quite significant for this work. Keenan explains *t*-scores thus:

"Ideally, a statistical method should give the (statistical) confidence level of a potential match. For example, comparison of two trees might conclude that we can be 99.7% confident that their rings match (such a match would then be accepted as valid). Unfortunately, no method of calculating confidence levels of tree-ring matches is known. The[re are] three commonly-used statistical methods. ...

"The most commonly-used method for statistically matching tree rings relies on what are called "*t*-scores". (The *t*-score is detailed in most introductory statistical texts; it is closely related to the coefficient of correlation.) In principle, a *t*-score is just a way of giving a confidence level *assuming* the following:

- the ring width in one year is independent of ring widths in other years, and
- ring widths have the same normal (i.e. bell-shaped) probability distribution.

"The first assumption is well known to be false (because the environment in one growing season affects the tree not only in that growing season but also in the next). Experience with the *t*-score method indicates that it can nonetheless work well, provided that it is

used in a manner appropriate for tree rings. Broadly speaking, a *t*-score above 3.5 is considered to indicate a tree-ring match. A *t*-score above 5.0 would be considered as implying a certain match by most tree-ring specialists (These levels for *t*-scores are conventional...)<sup>82</sup>

Below there will be presented a “critical discussion” of just how valid this statistical convention is. With respect to *g*-scores, Keenan adds:

“Another statistical method used in tree-ring matching relies on what I will call ‘*g*-scores’. (The *g*-score is commonly called ‘*Gleichläufigkeit*’ [Schweingruber, 1984] or ‘trend’.) The *g*-score is the proportion (or percentage) of years in which two trees’ ring widths increased or decreased together (i.e. increased or decreased from the prior year). This method thus ignores the size of the increase or decrease. Because it ignores so much information, the *g*-score method might be expected to be less reliable than the *t*-score method. Experience at Hohenheim, Germany, where *g*-scores were previously used, seemed to support this: [but the] matches were thrice found to be in error, each time after [the investigators made] strong assertions of reliability. [Baillie, 1995: ch.2; Spurk et al., 1998]. Early trials in Ireland also indicated problems and the method was abandoned there [Baillie 1982: p. 81-82, 95]. Other testing found very high *g*-scores for matches known to be incorrect [Schweingruber 1989: p. 77]. In the pre-computer age, though, *g*-scores had one advantage: being easy to calculate. They are still sometimes used, perhaps out of habit.”<sup>83</sup>

As for the third method Keenan further explains that this “... method used in tree-ring matching is the ‘linear time series’ method. Very briefly, this method is similar to the *t*-score method, except that the first assumption of *t*-scores (ring widths are independent of each other) is replaced by this:

- the ring width in one year is linearly-dependent on ring widths of prior years.

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<sup>82</sup> Keenan, *op.cit.*, pp. 3-4

<sup>83</sup> *ibid.*, p. 4

“This assumption is much more realistic than the first *t*-score assumption, though still not fully accurate (because the growth mechanism of the tree rings is more complicated).

“Among the three methods, then, the linear time series method is the best. That method, however, is not widely used in tree-ring studies. The reason is unclear (perhaps it is convention). ... [T]he method is nowadays taught ... at what is often considered to be the world’s leading institution for tree-ring studies, the University of Arizona laboratory of Tree-Ring Research.”<sup>84</sup>

Each of these statistical methods were available to the dendrochronologists who conducted their studies of tree rings from Anatolia. However, though they used these at first, they turned to a fourth method that is based on a form of statistical analysis almost completely unrelated to reality—statistical or otherwise. Keenan discloses:

“The approach that was adopted for Anatolia, however, was to rely largely on what is called a ‘*D*-score’.

“The *D*-score does not exist in statistics. It has been used [in Anatolia] solely with tree rings. *D*-scores do not have a mathematical derivation—unlike *t*-scores, *g*-scores, and times [linear] series. In fact *D*-scores were more or less made up (in an unpublished 1987 thesis) [by B. Schmidt] ... [The author of the thesis has acknowledged that it has no mathematical derivation (B. Schmidt private communication, November, 2003)] and using them to evaluate a tree-ring match turns out to be little better than rolling dice. ...”<sup>85</sup>

The lack of a proper mathematical or statistical derivation is explained by any mathematical formula that derives a particular quantity. For example, any side of a right angled triangle can be derived by the Pythagorean formula: sides *a* and *b* and the hypotenuse *c* of a right angled triangle can be represented by the formula:

$$a^2 + b^2 = c^2 \text{ or}$$

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<sup>84</sup> *ibid.*

<sup>85</sup> *ibid.*, pp. 4-5, including footnote

$$3^2 + 4^2 = 5^2 \text{ or}$$

$$9 + 16 = 25$$

which is true and real. This formula is accurate and always works with any right angled triangle; it is related to reality, it always derives [derivation] the values and therefore has meaning, in this case mathematical truth regarding any right angled triangle. Keenan however, tells us that *D*-scores are based on the following formula: “ $gt-t/2$ .”<sup>86</sup> In other words, one multiplies the *g*-score by the *t*-score to get a particular number. This number then has half the value of the *t*-score subtracted from it to give a final value that is used to determine how good the tree rings from separate logs match. As Keenan points out:

“The problem ... is that the ... formula has no apparent meaning [derivation]. ...

“The choice of  $gt-t/2$  is an arbitrary one among numerous formulae that could have been chosen to combine a *t*-score and *g*-score. For example, this formula might have been chosen instead:  $gt^2$ .

“There is no reason given for choosing one formula over the other. ... if the second formula [ $gt^2$ ] had been chosen [by the Anatolian dendrochronologists], then the wood from the [Tille Höyük] gateway ... would have been dated to 981 BC, rather than 1140 BC [using the  $gt-t/2$  formula]. This illustrates that the choice of the date for the wood (among dates with high *g*-scores and *t*-scores) is baseless—i.e. the date might almost just as well be chosen at random.”<sup>87</sup>

It is also probable that a combination of *g*-scores and *t*-scores could be found that, when put into a formula, would require that all trees from Anatolia be placed in the first millennium B.C. If, for example we suggest that any number less than +10 requires a tree be placed in the first millennium B.C. and invent the formula  $(g+t) - gt$ , no number derived by this formula would be greater than +10 and therefore Heinsohn’ and Sweeney’s short chronology would be proven to be correct because dendrochronology proved it. This would be called pure nonsense, and properly so. When we apply an arbitrary formula to the dendrochronology of

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<sup>86</sup> *ibid.*, p. 5

<sup>87</sup> *ibid.*

Anatolia and then say it spans over 1500 years from the third millennium into the first millennium B.C., this too should be called pure nonsense, and properly so. Interestingly, one wonders how it is possible that some of the most prestigious scientific journals in the world, like *Nature* and *Science*, permitted their pages to be used to promote a dendrochronology based on a formula that has absolutely no proper derivation of reality.

Are *t*-scores a truly accurate method for matching different sets of tree rings? We have been told that “specialists will tend to consider a *t*-score greater than 3.5 ... a valid match and ... above 5.0 will almost always be considered as being a certainly-valid match.” To test the validity of tree-ring matches with *t*-score matches above 5.0 to see if such high scores truly give an unquestionable match, Keenan first analyzed tree rings from a place in southwest Turkey (36.7°N [Lat.], 29.9°E [Long.], 1800 meters or about 5000 feet above sea level.<sup>88</sup> As he reports:

“The master dendrochronology for the site spans AD 1360-1988. The trees are junipers (the same species as the master dendrochronology for Gordion from ancient times). Consider the century-long portion spanning 1533-1632. If trees (from the site master) spanning 1533-1632 are compared with the site master at 1651-1750 [A.D.], the *t*-score is very high: 5.9. That is, if the tree rings from 1533-1632 had been found without any context [or way to determine what time and place they belong], and if the master [dendrochronology] had only spanned, say, AD 1600-1998, then the tree rings would almost certainly have been claimed to date from [between] 1651-1750. Indeed, the *t*-score of 5.9 is so high that almost all tree-ring specialists would accept the match, yet the date would be incorrect.”<sup>89</sup>

Why would this superb match with a *t*-score of 5.9 not be correctly placed between the years 1533-1632 AD? What Keenan did was show with a diagram that the highest *t*-scores were still from an incorrect match for each century-long portion of the master dendrochronology.<sup>90</sup> He then did the same for another site in Turkey about 85 km [50 miles] northwest of Gordion for the years 1306-1980 AD. Keenan comments on these supposed fits which are in reality misfits:

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<sup>88</sup> *ibid.*, p. 9

<sup>89</sup> *ibid.*, pp. 9-10

<sup>90</sup> *ibid.*, pp. 10-11

“These two examples actually underestimate the [high *t*-score] problem, because each compares a century-long portion of the master against the master, rather than a single tree against the master! Comparison of a single tree against the master would naturally tend to be less reliable ... Additionally, the master dendrochronologies are less than a millennium long: the longer the master, the greater the chance of an incorrect match. (The two sites examined here have the longest masters among those Anatolian sites for which (i) there is data in the International Tree-Ring Data bank and (ii) P.I. Kuniholm [the head of the Anatolian, that is Aegean, Dendrochronology Project] is a (co-)contributor).

“In both ... examples, the trees being matched against the master [dendrochronology] were of the same species and grown at the same site as the master. This is what is done when constructing a master dendrochronology for a site. The [two] examples used century-long portions of ring widths; a century was used because the Anatolian investigators have claimed that they can generally date a tree with 100 rings. That claim is contradicted by the above [analysis] ... Plainly, then, the master dendrochronologies for ancient Anatolia are not reliable. (The master dendrochronologies of the modern sites [unlike those of the ancient sites] probably are reliable, or nearly so, because many of the trees that were used to construct them were living so the dates of those trees were known with certainty)”<sup>91</sup>

In summary we can say the following:

1. We don't know who was buried in the Gordion mound or when. Therefore, the point in time that the chronology begins and ends is uncertain.

2. We don't know where the trees used at Gordion came from, whether they were from a temple that had been standing for over 100 years, or the trees died and were buried in sand and then found after centuries. Therefore the true connection of the trees with that site is in question and thus uncertain.

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<sup>91</sup> *ibid.*, p. 10

3. The Anatolian dendrochronology is a floating one unattached to the present and, because of this, is uncertain.

4. The Anatolian dendrochronology from sites dating to the time of the Old Babylonians (Hammurabi's dynasty) is contradicted by the astronomical and stratigraphical evidence that shows this period of time never existed in history for these people.

5. The matching and correlation of the master dendrochronology at Gordion cannot be properly made to fit with the Tille Höyük gateway wood because the number of rings of the gateway wood is in each log less than 100 years old.

6. The matching and correlation of the master dendrochronology at Gordion can not be properly made to fit with the Tille Höyük gateway because some of the logs employed to connect these sites were of different species of trees.

7. Two high *t*-scores that conflicted with the investigators' assumptions of where and when the Tille Höyük gateway was to be integrated with the Gordion master were dismissed based on the arbitrary bias of the investigators so they chose a third high *t*-score which did not conflict. This is not only arbitrary but a form of selectiveness that should have no place in such work.

8. The distance between Gordion and Tille Höyük is several hundred miles, and the climatic conditions of these two sites are not known for ancient times. To assume that the same climatic conditions existed at both sites is highly improbable and therefore the matching of tree rings from both sites is highly dubious.

9. Even in a small area tree rings from the same living tree species can fail to match one another in a general way and can even give anticorrelations for the same years, which is a fundamental contradiction to tree-ring dating. No one so far as I have read has explained how to determine if narrow tree rings were caused by other conditions than climate such as insect defoliation, disease, tree

competition for light and water, damage from storms, etc. Until this is done, the entire field of dendrochronology is suspect.

10. The Anatolian dendrochronology was not based on *t*-scores, or even *g*-scores. It was based on a formula whose derivation has absolutely nothing to do with reality. This in itself places the Anatolian dendrochronology outside the bonds of scientific research.

11. High *t*-scores can be found for tree-ring matches as long as 100 years, and yet these matches can be found to be incorrect.

Thomas Lynch, while discussing another concept, sums up the quagmire of having so many forms of weak evidence stand in the way of scientific understanding:

“A number of archaeologists [and] historians seem to believe that the marshaling of an increasing number of low-probability cases makes their argument ... stronger. The opposite is true, both intuitively and statistically. The longer we look without finding any certainty, the more certain and more hopeless ... [that] proposition appears—much as the multiplication of low probabilities ... [They] need only one incontrovertible case ... to demonstrate the proposition.”<sup>92</sup>

And that is what we find in great measure for the dendrochronology of Anatolia. When the date of the burial site—Gordion—is unknown, there is a floating dendrochronology, with the provenance of the trees found there unknown and unknowable, while the tree rings matched to it from Tille Höyük, some come from different tree species with less than 100 rings, and Tille Höyük is hundreds of miles distant from Gordion with quite probably a very different climate, and very high *t*-scores are ignored that make different dendrochronological connects, and one employs a dendrochronological formula based on a derivation of nothing, etc., it is worse than having a proposition of minute probability, one has created a kind of scientific Frankenstein. The Anatolian dendrochronologists have created a Pinocchio puppet of wood and attempted by pulling strings to breathe life into it and then pass it off as good chronological/dendrochronological science. Keenan’s own conclusions are worth noting:

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<sup>92</sup> Thomas Lynch in Tom Koppel, *Lost Worlds* (NY 2003), p. 149



“The central conclusion is clear: Anatolian tree-ring studies are very untrustworthy and the problems with the work should be plain to anyone who has familiarity with the field. This is a serious matter. Consider that the work has been published in respected research journals and been ongoing for many years. How could this have happened?

“In almost all branches of science there is a check on [replication of] the validity of published work: other researchers can, and often will, independently, seek to replicate the research. For example, if a scientist does an experiment in a laboratory, [and] comes to some interesting conclusion and publishes this, then another scientist will replicate the experiment, in another laboratory, and if the conclusion is not the same there will be some investigation. [The replication] check helps to insure the integrity of the system. Tree-ring studies, though, do not have this check, because the wood that forms the basis of a tree-ring study is irreplaceable: no other researcher can gather that [particular piece of] wood.

“Additionally, tree-ring investigators typically publish little more than conclusions (occasionally with average ring widths for a master dendrochronology). This is true everywhere, not just for Anatolia. Moreover, there is little competition among tree-ring investigators, in part because investigators for one region typically do not have access to data from other regions. The result is *a system in which investigators can claim any plausible results and are accountable to no one*. [Keenan’s emphasis].

“Archaeologists should not submit to this system. There might be temptation to accept a tree-ring date without supporting measurements, particularly when the date agrees with the archaeologists’ hypothesis. To accept such a date, however, implies acquiescence to a system that does not have sufficient checks to insure its integrity. Moreover, a solution for the problem is clear: ring measurements for each tree should be published to make them available for independent scrutiny. A data depository is already established: The International Tree-Ring Data Bank. And as Kuniholm [2002, p. 67] has

stated regarding tree-ring data, 'keep in mind that unpublished information is next to worthless'."<sup>93</sup>

## THE SHIPWRECK OFF ULUBURUN AND THE ANATOLIAN DENDROCHRONOLOGY

Supposedly during the Late Bronze Age, ca. 1300 B.C., a ship sank off the south west coast of Turkey near Uluburun (also spelled Ulu Burun). It contained a great number of trade items. Its ship timbers were dendro-chronologically dated by P. Kuniholm *et al.*, as presented in *Nature*:

“Wood found as part of the cargo on the Kaş/Uluburun shipwreck has a last preserved ring at 1316 BC; other finds include Mycenaean pottery from Greece (the most recent material present is early Late Helladic III B: J.B. Rutter, personal communication), and a gold scarab of Nefertiti, wife of Akhenaten, pharaoh of Egypt. These provide links to the chronologies and histories of the Aegean and Egypt, and confirm conventional 14th-12th century BC chronology against recent radical critiques [of the length of history, especially Peter James *et al.*, *Centuries of Darkness*, 1991]. Tree-ring dating now offers the route to a new absolute chronology of the Old World that is independent of existing assumptions, gaps in evidence and debates.”<sup>94</sup>

We have already looked at the many very serious problems connected with Anatolian dendrochronology, and we know this ship carried tin and glass of a cobalt blue color which could not possibly have been made around 1316 B.C. No one has explained how these products could be traded at this time. If indeed this ship was plying the waters of the Mediterranean Sea for trade in ca. 1300 B.C., then again Heinsohn and Sweeney's short chronology cannot be correct since they only have civilization beginning at this period. Peter James, whose chronology also

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<sup>93</sup> *ibid.*, pp. 15-16

<sup>94</sup> P.I. Kuniholm *et al.*, “Anatolian Tree Rings and the Absolute Chronology of the Eastern Mediterranean,” *Nature*, vol. 381 (1996), p. 782

shortens history by some 200 years, felt the evidence presented was compelling enough to write a response which he posted on the Internet and which was archived at <http://oi.uchicago.edu/research/library/ane/digest/1999/v1999.n314>

The letter was corrected and presented on the Internet site, [www.centuries.co.uk/uluburun.pdf](http://www.centuries.co.uk/uluburun.pdf) under the title “The Uluburun Shipwreck—a Dendrochronological Scandal.”

The following closely follows these reports:

When a wooden ship sinks such as the one at Ulu Burun there can be no dendrochronological date for the time it went down. The timbers of the boat may have been cut over 200 years earlier and the ship built 10 years later, and seen service, repairs in dry dock for, say, 190 years (though these estimated numbers may be somewhat shorter than I have suggested). Furthermore timbers from other ships may have been added to repair various damages. If we don't have the bark on these wooden boards, then we may have no idea at all how long ago the tree was harvested and its inner ring—now on the outside of a board—may be hundreds of years older than the final outside ring. But Kuniholm, without giving any details or explanation, claimed the wood found as “part of the cargo ... has a last preserved tree ring date of 1315 BC.”<sup>95</sup> In a 1996 newsletter he reported the

“... last ring at Ulu Burun is 1315 B.C. I do not think that we are missing any rings on the exterior. Since the shipwreck is a time-capsule, the date for the wood dunnage on board helps date all the rather more glamorous cargo items from half a dozen civilizations or more.”<sup>96</sup>

Nevertheless Kuniholm was in error. After three years, the wood having dried out, an additional nine rings were seen beyond what Kuniholm assumed to be the last ring on the wood. That means one does not and cannot know how many others were actually cut away—perhaps 50, perhaps 200. No bark was ever reported to be seen. But the wood itself was dunnage. Dunnage may be mats, brush, wood, gratings, or other wooden materials that were stowed under or between stores of cargo in order to prevent them from moving and scraping, or, if pottery, from

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<sup>95</sup> *ibid.*

<sup>96</sup> P. Kuniholm, “Aegean Dendrochronology Project December 1996 Progress Report” (1996), Department of the History of Art, Cornell University, Ithaca NY circular

breaking. Dunnage was used over and over again. It usually was wood that had no other intrinsic value and therefore had been discarded. How many times this piece of dunnage had been used before it ended up on the shipwreck one does not and cannot know. Yet Kuniholm suggested it gave a reliable dendrochronological date. But why Kuniholm did not attempt to date one of the timbers from the vessel remains a mystery.

The best report on the sample dating was a paper by Malcolm H. Weiner who supports Kuniholm's laboratory and thus could discuss the facts Kuniholm reported. Weiner nevertheless states that the specimen was of a "badly-twisted piece of cedar about six inches in diameter and over four feet long."<sup>97</sup> Weiner further points out citing Kuniholm (1997):

"Any dates from a sampling of only two timbers must be treated with caution, especially when the wood is cedar which can often have eccentric growth characteristics."<sup>98</sup>

What the other timber was we are not told nor what it indicated. But we now have a piece of dunnage claimed to fit the Anatolian dendrochronology even though cedar "can often have eccentric growth characteristics."

How was the cedar dunnage specimen fit into that dendrochronology? Was it by *t*-scores or *g*-scores or even *D*-scores? The answer to this question is that it was by none of these statistical methods. As Weiner admits:

"The timber in question is particularly twisted and gnarled, and thus is unable to provide a conclusive computer-generated statistical match with the Anatolian master Bronze-Iron Age chronology, although the match (by Student's *t*-score, Trend co-efficient and *D*-score) which results in 1305 BC as the year of the observable ring is superior to that for any other relevant year. The microscopic **VISUAL FIT** is convincing, however (Kuniholm and Steele, personal

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<sup>97</sup> Malcolm H. Weiner, "The Absolute Chronology of Late Helladic IIIA2," *Sardinian and Aegean Chronology: Proceedings of the International Colloquium 'Sardinian Stratigraphy and Mediterranean Chronology,' Tufts University, March 17-19, 1995* pp. 309-319

<sup>98</sup> *ibid.*

communication). Thus there is a high likelihood that the last observable ring represents the year 1305 BC.”<sup>99</sup> [capitalization and bold added]

The cedar dunnage was matched to the Anatolian dendrochronology by a visual inspection of the timber. It is not a statistical dendrochronological date. The sample was so twisted as to be unusable for that purpose. First, let us understand that the Anatolian dendrochronology is based largely on juniper and not cedar wood. Where this cedar sample grew is completely unknown. It could have come from Italy as far as anyone can guess. Not knowing where it grew, how many times it was used before ending up as dunnage, and for how many years it was employed for this purpose, the investigators claim that a piece of cedar wood has a “high likelihood that the last observable ring represents the year 1305 BC.”

But how accurate is that visual match? Keenan compared the match, stating:

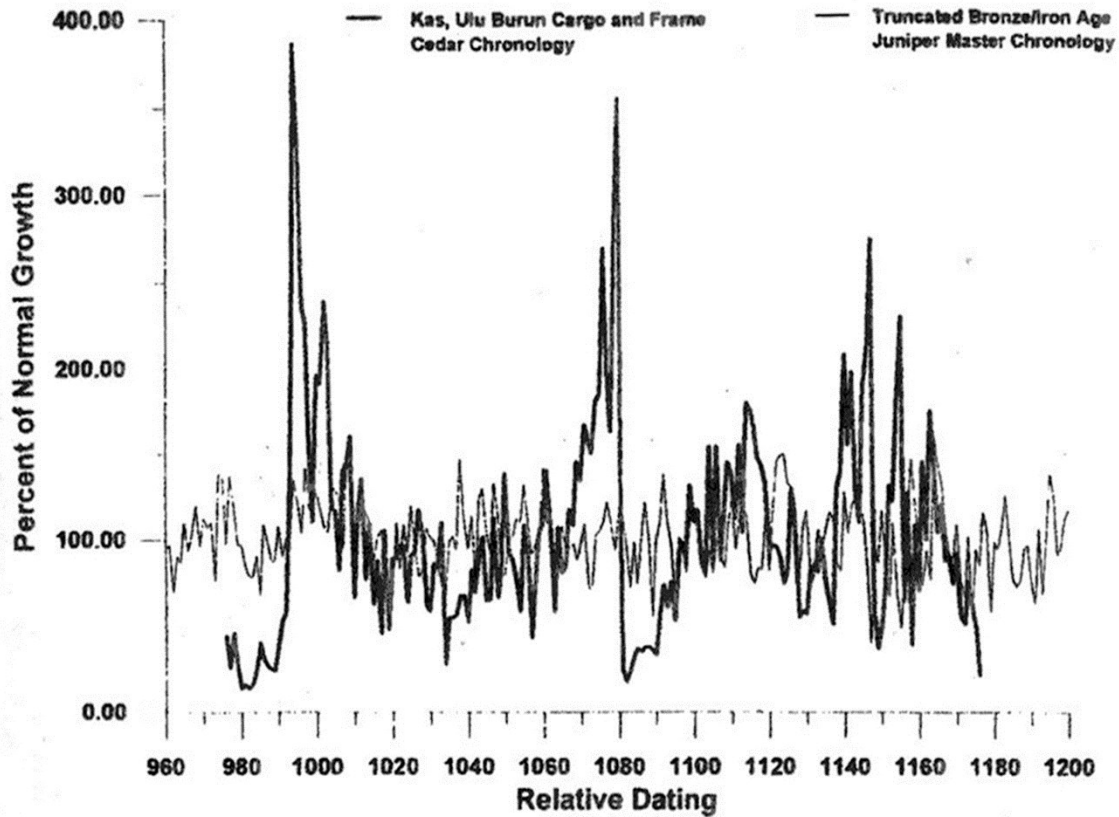
“The wood has been dated against the Gordion master solely on the basis of visual matching—a dubious practice. The visual match is shown ... The light line represents the Gordion master, it is high [on the graph] for years that had wide rings and low for years that had narrow rings. The heavy line represents the shipwreck [dunnage] wood. ... It is clear that there is not a visual match ... at all. The claim that the shipwreck wood has been dated was spurious.”<sup>100</sup>

The graph is presented below:

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<sup>99</sup> *ibid.*

<sup>100</sup> Keenan, *op.cit.*, pp. 5-6



The shipwreck wood matched against the Gordion master dendrochronology. (This figure is given by both Kuniholm [1997: fig.7] and Manning [1999: fig.63].)

Peter James posted a note to various E-mail lists claiming that the visual matching had many problems. Sturt W. Manning and Peter I. Kuniholm *et al.* may have known of this material when they wrote:

“Caution should be exercised concerning a previously stated date derived from just two poorly preserved pieces of cargo/dunnage wood from the famous Ulu Burun shipwreck ... The quality and security of

the dendrochronological placement of these samples versus the Bronze-Iron master chronology are not especially strong.”<sup>101</sup>

They go on to say:

“If the fit is confirmed, the last preserved ring would now lie *ca.* 1327+4/-7 B.C. This would confirm the conventional chronology because the presence of a gold scarab of Nefertiti on the ship requires her [to live in the] mid -14th century B.C. data range.”<sup>102</sup>

In other words the visual match does not seem to have been made by statistical dendrochronological procedures. It seems to have been made by seeing where in the Anatolian master dendrochronology—based on the established chronology—Queen Nefertiti of Egypt would have lived. Then, using that mid-14th century B.C. date, the investigators knew where to look for a possible match. Weiner spoke of this fit being ‘superior to that for any other relevant year.’ Thus it seems that any other period which did not come close to Nefertiti’s time was not relevant and was excluded.

This is indirectly admitted by Weiner:

“The placement of the Uluburun branch within the Gordion floating sequence was based on close examination after comparison by computer proved inconclusive; the visual examination was reported to indicate a better match here [1305 B.C.] than at any point fifty years in either direction.”<sup>103</sup>

The entire process was a grand piece of circular reasoning: Since Nefertiti is conventionally dated to the mid-14th century B.C., that was the only place the investigators deemed worthy of consideration for their dating scheme and that was how they found “their best date,” not “the best possible of all dates.”

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<sup>101</sup> S.W. Manning *et al.*, “Anatolian Tree rings . . .,” *op.cit.*, p. 2535, n. 38

<sup>102</sup> *ibid.*

<sup>103</sup> Malcolm H. Weiner, “The Absolute Chronology of Late Helladic III A2 revisited” in *Sardinian and Aegean Chronology towards the Resolution of Relative and Absolute Dating in the Mediterranean* M. S. Balmuth, R. H. Tykot, eds., *Annual of the British School at Athens*, vol. 98 (Oxford UK 1998) p. 244

In 2003 Weiner in the face of what the evidence indicated was finally forced to make the following admission:

“Director Peter Kuniholm has recently informed me that ... he and his colleagues are no longer confident as to the visual fit of the Anatolian floating sequence, and would prefer to suspend judgement until additional dendrochronological material from the Uluburun shipwreck is received and examined and current work comparing climate patterns reflected in wood from Anatolia with wood from Syria and the Levant can be completed. In addition, while dunnage or fire wood would not have been deliberately aged, as may sometimes be the case with logs used in construction, nevertheless the possibility exists that dunnage (which may have been collected to cushion oxhide ingots in transit, for example) could have been reused over a period of time.”<sup>104</sup>

Ultimately Manfred Bietak in his review of Sturt Manning’s *A Test of Time* presented these sharp comments regarding the date of the Uluburun dunnage:

“It is also by no means certain, nor even likely, that the cedar wood from the Ulun [*sic*] Burun shipwreck comes from inner Anatolia. It could be Lebanese, Cypriot or Amanus cedar. Secondly there is no possibility to verify the claim made for fitting the Ulun Burun ship with the Gordion tree-rings, either by data nor in the graphs published. If S. Manning and P.I. Kuniholm want us to believe in those results, they will have to come up with far more detailed data for an independent date of this shipwreck. *We are still forced to use artifacts from the ship as a way of dating.*”<sup>105</sup> (emphasis added).

Peter James well summarizes the entire question of Anatolian dendrochronology and the dating of the Uluburun shipwreck as follows:

“Dating a tree-ring sequence is one thing; using it to provide actual dates for archaeology is another. Unfortunately, Kuniholm’s team have made some premature announcements which subsequent

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<sup>104</sup> *ibid.*, p. 245

<sup>105</sup> M. Bietak “Review of Manning 1999,” *Bibliotheca Orientalis*, vol. LXI no. 1-2, pp. 221-222



work has shown to be invalid. A date of 1305 for the Late Bronze Age shipwreck of Uluburun ... was trumpeted as confirmation of the Late Bronze Age. In the recent *Science* paper it was virtually retracted ... Another date of 1621 BC for a wooden bowl from the shaft graves at Mycenae has been categorically withdrawn. (The rings were never properly measured.) While the fact has not been advertised by Kuniholm and his colleagues, these are the only two results so far declared [as of August 2002] for the Aegean Late Bronze Age, and both have proved to be faulty. For the same period, dates for a number of Anatolian sites have been announced in newsletters, but the results from only one have been fully published and these are clearly anomalous for the standard chronology. The salutary experience of Uluburun and Mycenae means that large question marks will remain over the other Anatolian sites until they are formally published for scrutiny.

“About chronology-building Sir Mortimer Wheeler once remarked ‘we have ... been preparing time-tables: let us now have some trains.’ With respect to the time tables, the work of the Aegean Dendrochronology Project has been steady ... As for the trains, some have already been derailed. Precision in archaeological dating is a desideratum, but it is not achieved by having a ... time table alone. It need[s] ... precision in the selection of impeccable and fully published credentials.”<sup>106</sup>

While James does suggest the work of Kuniholm and his team has been “meticulous” and may in time be “fully realized,”<sup>107</sup> it appears to be so overburdened with problems of every kind that it may eventually be shown for what I believe it is, an edifice of wood that has been rotted away.

Let us briefly remember that Bietak admitted: “We are still forced to use artifacts from the ship [wreck] as a way of dating [it].” Bietak, I believe, meant that the artifacts to do this were those of pottery, etc., that have traditionally been utilized to date sites. This approach as a first case procedure for dating is categorically rejected by this author. Scientific and technological artifacts have precedence over archaeology. Dating the Uluburun shipwreck with respect to this

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<sup>106</sup> James, “The Dendrochronology Debate,” *loc.cit.*

<sup>107</sup> *ibid.*

evidence, namely tin—and, as we will see, other forms of cargo—would be rejecting technological evidence and superimposing archaeological evidence in its place. Since neither the historians nor the archaeologists can specify where that tin was mined or when, they must float the wreck on a cushion of archaeological air in order to date it well inside the second millennium B.C. Until they provide this evidence their mid-second-millennium date will remain a chronological shipwreck.

To wit, there is no scientific, technological or even dendrochronological evidence from Anatolian tree ring research, nor from the Uluburun shipwreck, that provides us with solid evidence linking these materials of the second millennium B.C. Neither Heinsohn's nor Sweeney's theses have been invalidated by such evidence. The fact is that no such evidence exists regarding this material, and no evidence means exactly that—no evidence. With regard to dating the second millennium B.C. *via* dendrochronology, the evidence above shows it to be a failure. Thus, any attempt to employ such evidence to refute Heinsohn's or Sweeney's chronologies would also be a failure. In a certain sense this failure is an indication that their chronology is correct. As the 19th century English philosopher William Whewell explained:

“Every failure is a step to success; every detection of what is false directs us toward what is true; every trial exhausts some tempting form of error.”<sup>108</sup>

As we proceed we will see that the various other forms of evidence that could be raised to suggest that civilization existed early in the second millennium B.C. are also failures.

Furthermore, there is evidence from this shipwreck that is of a technological nature that requires it be placed in the first millennium B.C. and not the second. According to Brian Fagan the ship was constructed using technology that comes about in that region 1000 years later.

“A thick [ship's] plank 11 inches (27.5 cm) wide served as its rudimentary keel, the adjoining garboard strakes edge-joined to it with mortise-and-tenon joints secured with hardwood pegs. The second plank is secured to the garboard in the same way, a mode of

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<sup>108</sup> William Whewell, quoted in *The New Dictionary of Thought*, *op.cit.*, p. 197

construction that is very similar to that of a merchantman of the fourth century B.C. ...”<sup>109</sup>

A writing desk was also in the shipwreck that is only known to have been used about 600 years later, that is the first millennium B.C. According to Fagan:

“... tiny wood fragment[s were found]. When Cemal Pulak pieced them together he discovered [they were] a wooden writing table, a diptych once covered with a wax writing surface. Two wooden leaves with recessed inner surfaces were once joined with an ivory hinge, forming a tiny ‘book.’ Crossed lines scored the inner surfaces: [This was] a remarkable discovery [because it was] 600 years older than the oldest known such tablet found ashore at the Assyrian city of Nimrud in northern Iraq. At Nimrud the scribes had used walnut wood tablets and a wax mixed with 25 percent orpiment (arsenic trisulfide) to give it the right consistency and color. The Uluburun ship [also] yielded a Canaanite amphora [jar] with orpiment, presumably destined for exactly the same use.”<sup>110</sup>

It is that the construction of this ship as well as the writing desk and type of orpiment wax mixture are only known from the first millennium B.C. and not the second. It is unreasonable to assume that this type of writing table remained unchanged for 600 years. It is significant that the mortise-and-tenon jointing as well as the writing desk are only known to exist in the first millennium B.C. and not the second. It is not possible that this type of writing table remained unchanged for 600 years.

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<sup>109</sup> Brian Fagan, *Time Detectives* (NY 1995), pp. 190-191

<sup>110</sup> *ibid.*, pp. 184-190

## DATING THE VOLCANIC ERUPTION OF THERA VIA RADIOCARBON

The eruption of ancient Thera, modern Santorini, in the island chain of the Cyclades, has been placed conventionally around 1500-1645 B.C. by various investigators. Egyptologists maintain the eruption occurred closer to the 1500 B.C. date, while others put it closer to the older one. This second group, lead by Sturt Manning of Cornell University, have relied on radiocarbon to uphold that older date. Nevertheless, if either of these dates were to prove to be correct then the chronological views of Heinsohn and Sweeney, that civilization only began around 1200 B.C., would obviously have been proven false. The dating of the Thera eruption is therefore not only important for the proper valuation of the established chronology, whether supported by the Egyptologists or by those who with Manning would push that date back in time by over 100 years, but also for Heinsohn's and Sweeney's chronologies which require the eruption to have occurred sometime after 1200 B.C.

That is the question to be addressed and resolved in the following pages. However, before dealing with the date of the Thera eruption, we wish to address a scientific problem related to the magnitude of that explosive event. The literature is replete with statements, books, papers, articles, etc., that the eruption of Thera brought down the Minoan civilization but was also responsible for other civilizations collapsing, changes in climate, and even explaining the plagues of the Hebrew Exodus from Egypt. But was this eruption as stupendous and devastating as has been suggested, or was it only a mild eruption that caused none of these results?

Those who either make the claim for the Thera eruption's destructive effects or deal with these are far too numerous to list, so a very abbreviated list is herein presented. They are Angelos G. Galanopoulos and Edward Bacon, *Atlantis The Truth Behind The Legend* (London 1969); Spyridon Marinatos, "The volcanic destruction of Minoan Crete," *Antiquity*, vol. 13 (1939): pp. 425-439, J.V. Luce, *Lost Atlantis New Light on an Old Legend* (NY 1969); James W. Mavor Jr., *Voyage to Atlantis* (NY 1969); D. Ninkovich and B.C. Heezen, "Santorini Tephra," *Submarine Geology and Geophysics*, vol. 17 (1965); M.S.F. Hoad, *The Home of the Heroes* (London 1967), pp. 106-108; Richard Ellis, *Imagining Atlantis* (NY 1998); Dorothy B. Vitaliano, *Legends of the Earth* (Bloomington IN/London 1973), chapters 8, 9, 10.

Some of these researchers have attempted to show that Immanuel Velikovsky's thesis presented in *Worlds in Collision* (New York 1950) are fully explained away by this supposedly cataclysmic event. For example Carl Sagan wrote:

“In a certain sense, the Galanopoulos explanation of the events in *Exodus* is even more provocative than the Velikovsky explanation, because Galanopoulos has presented moderately convincing evidence that Thera corresponds in almost all essential details to the legendary civilization of Atlantis. If he is right, it is the destruction of Atlantis [being Thera] rather than the apparition of a comet that permitted the Israelites to leave Egypt.”<sup>111</sup>

As to the might of the eruption J.V. Luce compared the Thera eruption to that of the great 1883 eruption of Krakatoa, as do others. One need not go into detail regarding the force of the Krakatoa event except to point out that its “blast waves broke windows and cracked walls [of buildings] up to 160 km [about 100 miles] off ... Aerial vibrations from the ... explosion were detected all over the globe.”<sup>112</sup>

If that was truly the case for Thera, any building or town standing nearby, directly in the path of the blast wave, would have been demolished. All one has to do is consider the buildings in the path of the blast wave at Hiroshima, or Nagasaki, Japan or at the Atom bomb test site at Alamogordo, New Mexico to understand how blast waves from these small atomic devices obliterated almost every building for miles. Any building or town on Thera facing the force of that eruption, which is estimated to have been many times greater than these early atomic bomb blasts, would not survive intact or largely intact.

Nevertheless the site of Akrotiri is that of a town that was standing on Thera right up until the final moment of the volcano's eruption. It is located about one mile (less than 2 km) from the edge of the crater. One would therefore expect that the blast wave, and the gigantic earthquake that accompanied the eruption, would have turned Akrotiri into a heap of rubble. The blast wave, that is purported to be capable of cracking walls in buildings 100 miles/160 km distant and powerful

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<sup>111</sup> Carl Sagan, *Broca's Brain* (NY 1979), pp. 124-125

<sup>112</sup> J.V. Luce, *Lost Atlantis. New Light on an Old Legend* (NY 1969), p. 74

enough to supposedly destroy the civilization on Crete about 60 miles away, didn't obliterate Akrotiri at the very site of the eruption. J. Lesley Fitton describes the town as a visitor will find it today, where the pumice ash has been removed:

“At Akrotiri ... the visitor can ... stroll through the streets of a late Minoan town. The preservation of the remains under the ash of the great volcanic explosion has created a sort of Aegean Pompeii. Excavators have so far revealed houses [still standing] around a main street and square, and although affected by earthquake damage, their state of preservation is quite remarkable. It is possible to walk into the square and be surrounded by Minoan-style houses with facades two stories high.”<sup>113</sup>

To see the remarkable state of preservation of the stone block houses see J. Lesley Fitton's *The Discovery of the Greek Bronze Age* (Cambridge MA 1996), p. 187. Not only that, but inside these buildings, though the furniture and other wood has rotted away, they have left molds in the ash, which means they remained after the eruption along with the houses. According to Luce there were two periods of eruption, the first

“... ejected the coarse pumice which [is] found [in] the lowest layer in the tephra deposits. This layer is about four metres [13 feet] thick in the Phira [Thera] quarry [nearby]. The effects of this phase of the eruption were probably confined to Thera itself. It did not result in the formation of the [volcanic] caldera, but almost certainly all settlements on the island were obliterated.”<sup>114</sup>

That being the case, Akrotiri had to be rebuilt after that first “obliteration,” and was standing when the final eruption occurred. The buildings in the town, as we will see below, were not only two stories high, but some were three and even four stories high. Thus, they would still stand above the first coarser pumice-ash level even after the first eruption. Much of the upper parts of the two story buildings would have also stood above that ash if they had somehow survived the first smaller eruption. But the second final blast would have ripped apart any obstacle in its way including nearly all of Akrotiri.

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<sup>113</sup> J. Lesley Fitton, *Minoans* (Cambridge MA 2002), p. 165

<sup>114</sup> Luce, *op.cit.*, p. 69

If, as the many proponents of the force of this eruption suggest, Thera was comparable in power to Krakatoa, Akrotiri should not, and could not, still be largely standing today. Does anyone truly contend that such an immense blast wave would leave behind almost an entire town one mile away from the edge of the caldera—the source of the blast wave—in so remarkable a state of preservation? So far as I have read, not a single proponent of the theory that the Thera eruption was a colossal cataclysm with world-wide effects has ever acknowledged or even recognized that this blast wave has demolished their thesis.

Generations of researchers have directly and indirectly denied the validity of Velikovsky's thesis because they say it could be explained by the eruption of Thera. But as we can plainly see, the shock wave of that eruption was entirely too weak to do what it should have done if it was as powerful as it is presented—namely topple nearly every building at Akrotiri.

Although we have spent some time on the blast wave of the Thera eruption we must also recognize that when a volcano erupts the land surrounding it experiences enormous earthquakes. Earthquakes produce two types of ground waves; without becoming technical, one of these waves like an ocean wave moves up and down. The other wave type shakes the ground from side to side. The closer one is to the crater of a volcanic eruption the greater these waves are. Given that the town of Akrotiri was only one mile from the edge of the erupting caldera, and given the assumed violence of this eruption, comparable to that of Krakatoa, it is not possible, nor even probable, that two, three or even four story stone block buildings would have been left standing.

Together the blast wave and the volcanic earthquake from that volcano would have obliterated most, if not all the buildings of Akrotiri and hardly a single one of these buildings should have survived the destruction. Yet to our amazement many of the buildings in this ancient town are standing as testimony and evidence that the volcanic event was not in any way as great as the historians would have us believe.

There is further evidence of buildings found on the very edge of the large bay formed by the Thera eruption. This would be impossible if the eruption with its accompanying blast wave and volcanic earthquakes was anywhere as great as that of Krakatoa. The following situation was presented by Ferdinand Fouqué:

“The bay of Santorini [the great caldera] was formed by a frightful cataclysm ... Today a layer of pumice, which in places reaches thicknesses of up to 30 meters, covers what remains of the former island. Houses and the relics of an advanced culture have recently been found beneath this shroud ...

“The circumstances that led to these discoveries are interesting enough to warrant brief mention.

“A few years ago, the Suez Canal Company, having undertaken the great masonry structures for the port and buildings of Port Said, had the idea of using pozzolana to make high-quality cement. The pumice of Santorini, mixed with half or even a third as much [pozzolana] lime, quickly turns exceptionally hard while at the same time becoming perfectly resistant to seawater. The available amounts of this material and the ease with which it can be mined and transported at low cost to Egypt induced the company to procure it in great quantities [from Santorini]. From that time on, the pumice has been exploited on a large scale from the coasts of Thera and Therasia. Arriving ships are stationed at certain places close to the shore, where they load the pozzolana as it is broken away from the [crater] escarpments above and allowed to fall or slide down the tortuous slopes of the cliff. Substantial parts of the pumice layer of Thera and Therasia have been removed in this way ... In all these excavations the pumice was removed down to its basal layer, which contains numerous lithic blocks ... Some of these blocks were found to follow regular lines and turned out to be the tops of walls. The workers and owners realized this perfectly well and knew they were dealing with ancient buildings ...

“The buildings were clearly built on what was then the ground surface and later filled with [volcanic] tuff that has not since been reworked. They [the buildings] are older than Santorini’s great eruption of pumice.<sup>115</sup>

The buildings were “45 to 50 cm [17 to 20 inches] thick.”<sup>116</sup>

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<sup>115</sup> Ferdinand Fouqué, *Santorini and its Eruption* (Baltimore 1998), pp. 94-96

<sup>116</sup> *ibid.* p. 120



Fouqué goes on to say these buildings:

“... were built prior to the event that produced the [great Santorini] bay [caldera]. In the case of the houses discovered on the ravine [these] had earlier [been] thought [to have had] their foundations above the pumice layer ... The houses [however] uncovered in the ravine of Akrotiri ... were beneath the pumice.”<sup>117</sup>

Long ago Lewis M. Greenberg presciently foretold what the evidence would ultimately prove:

“While it is true that volcanoes may not necessarily require a celestial trigger (an area in which modern geology is highly reluctant to consider [in 1975]), there is, nonetheless, an outside possibility that an extra-terrestrial agent (Mars? [in the 8th century B.C.]) helped to precipitate the Theran eruption WHICH DOES NOT APPEAR TO HAVE BEEN AS TREMENDOUS NOR AS DESTRUCTIVE AS GEOLOGISTS ONCE THOUGHT.”<sup>118</sup> (capitalization added)

Here Greenberg suggests that not only was the eruption a mild one, but that the date for it was not around 1500-1650 B.C., but in the 8th century B.C. The dating of the eruption by Manning and his team has engendered much debate with the Egyptologists led by Manfred Bietak and Kenneth K. Kitchen who cannot conceive that Egyptian history may be off its hinges by 100 to 150 years. And thus they will not accept any of Manning’s radiocarbon evidence which it is suggested makes the case for lengthening that history.

Esmenia Simmoes Osborne encapsulates the debate thus:

“In the present state of knowledge, neither ice-core nor tree rings can be regarded as providing a satisfactory date for the destruction of [Akrotiri on] Santorini. Renfrew (1996) reviewing the Anatolian tree-ring evidence of Kuniholm *et al.*, (1996) remains similarly skeptical. The close link to the Egyptian chronology provided by the explicitly

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<sup>117</sup> *ibid.* p. 124

<sup>118</sup> Lewis M. Greenberg, “Reply to Isaacson,” *Kronos*, vol. I, no. 2 (2nd printing) (June 1975), p. 99

[similar] ‘Minoan’ wall paintings from the Hyksos capital, Avaris in the Nile Delta, further complicates the issue. As Bietak (1996) comments, to accept the 1628 BC date for the eruption would require an additional 130 years of Egyptian history, something no Egyptologist is likely to approve.”<sup>119</sup>

The debate has been going on for years and if Egyptologists are adamant in their refusal to move Egyptian history back by 130 years one can imagine their reaction to the short chronology which moves all of Egyptian history closer to the present by up to 1500 years. Bietak well explains the great importance of dating the Thera eruption which buried the town of Akrotiri under some 30 feet of ash:

“The problem of dating the Minoan eruption of the volcano of Thera ... is still a matter of keen debate and year on year, the research into resolving its chronological

“The problem of dating the Minoan eruption of the volcano of Thera ... is still a matter of keen debate and year on year, the research into resolving its chronological status reaps dramatic new aspects and results. Be that as it may, the issue has not been resolved, despite frequent premature claims to the contrary. It will still take some or even many years before the long ongoing discussion can reach a conclusion. The date of this event is of critical importance for the synchronisation of the civilisations of the Eastern Mediterranean. A solution of this matter is the key to most of our present synchronisation problems. Any scholar who is able to present cogent evidence of this eruption date—evidence that will stand the *test of time*—will deserve an archaeological Nobel Prize.”<sup>120</sup>

The problem Manning faces regarding 14C dating is that, as was pointed out in volume I of this series, historians (particularly Egyptologists) and archaeologists steadfastly refuse to accept such datings especially when they contradict their long-held, deeply-believed, established chronology. There are

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<sup>119</sup> Esmenia Simmoes Osborne, *Climate Change: Critical concepts in the Environment and Physical Geography* (London/NY 2002), p. 261

<sup>120</sup> Bietak, “Review of Manning 1999,” *op.cit.*, p. 200

explicit statements to this effect in the peer-reviewed literature. Keenan here mentions some:

“Some Ancient Near Eastern archaeologists have gone on record stating that they do not accept dates based on 14C. For example, a reviewer of 14C dates from Sardinia concluded ‘I will only accept ... dates provided by [14C] as indicative when they do not blatantly contradict information already obtained by using [archaeological] methods. (Tine 1998) And a respected Egyptologist says similarly, ‘I am mistrustful ... [14C dating] does not often match with historical dating’ (Weiner *et al.* 1995). Too, some archaeologists have said that if they attempt to publish 14C dates contradicting archaeo-historical chronologies, their papers are rejected (Nelson *et al.* 1990). One archaeologist reviewing the situation for the eastern Mediterranean concluded blandly ‘... radiocarbon dates are invoked if they support a particular hypothesis ... and dismissed if they do not’ (Merriless 1992).”<sup>121</sup>

For anyone to then suggest that radiocarbon dating of the Ancient Near East has any validity at all requires one to give up all reason and ethics. As I have repeatedly claimed, radiocarbon dating in its present application is the very worst kind of science.<sup>122</sup> When only radiocarbon dates that support the established chronology are printed or accepted by a community of scholars and when radiocarbon dates that contradict the established chronology are suppressed, and if published, unacceptable to that same community of scholars, what we have is criminal behavior; it is no different than a dictatorship. As the American journalist Walter Lippmann stated, “Where all think alike, no one thinks very much.”<sup>123</sup> Or, as John Foster Dulles wrote:

“Dictatorships usually present a formidable exterior. They seem on the outside to be hard, glittering, and irresistible. Within they are full of rotteness.”<sup>124</sup>

<sup>121</sup> Keenan, “Why Early-Historical Radiocarbon Dates ...”, *op.cit.*, p. 225

<sup>122</sup> see, for example, Charles Ginenthal, “Scientific Dating Methods in Ruins,” *The Velikovskian*, vol. II, no. 1 (1994), pp. 52-60

<sup>123</sup> Walter Lippmann quoted in *Leo Rosten’s Carnival of Wit*, *op.cit.*, p. 145

<sup>124</sup> John Foster Dulles, quoted in *The Dictionary of Thoughts*, *op.cit.*, p. 143

To paraphrase Gandhi, the historical establishment is very much like a dictator that claims to seek the truth and apparently believes it can reach it across a sea of suppression and misrepresentation.

Bietak in his own discussion of radiocarbon dating claims:

“It is a great illusion to believe that sciences are more reliable [than archaeological and historical evidence], at least just now, in obtaining absolute dates. Manning ... deals extensively with the problems of radiocarbon chronology, although not with all the problems. It is astonishing that he has so much confidence in proposing a  $^{14}\text{C}$  chronology for the Aegean, when the data available are so limited, and limited even more by the selective process of the authors’.”<sup>125</sup>

First of all, the number of specimens that were employed to date the eruption at the time—2004—were only four. Several others were discarded:

“[Manning] does not balk at mentioning most of these [radiocarbon] dating problems. He also tells the sad story of the numerous samples collected from Akrotiri that are gauged by a variety of laboratories with highly inconsistent results ... he also asks the question whether the burial of the town of Akrotiri under metres of pumice [light volcanic rock] did not create a soil chemistry [different] from normal contexts. P. 237 [he states] ‘*The samples could easily have exchanged carbon with permeated water.*’ Anyway, all the previous samples were discarded. Only four samples that, according to the Copenhagen Laboratory were up to standard as they were fully carbonized, ended up being considered ... [The] calibration programme ... shows three peaks, one in the late 18th [century B.C.], one filling the whole 17th and the final peak filling the period between 1590-1520 BC. The other Seattle 1993 calibration dataset produced five peaks, the last one in the 16th century BC being the strongest. Under such circumstances and premised on such a tenuous statistical basis, Manning says: ‘*We may therefore conclude that the good quality radiocarbon data presently available from Thera ... cover both the*

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<sup>125</sup> Bietak, “Review of Manning 1999,” *op.cit.*, p. 216

*Aegean “early” and “compromise early” (by this he means the moderate low) chronology. A date after [closer to the present] c. 1500 BC is ruled out—and the recently proposed date for the eruption between 1515 and 1460 BC is simply not possible.’*

“Such a strong statement based on four handpicked samples and dismissing samples which do not fit the high chronology ..., seems to be propped up on weak and biased foundations, especially when considering the many problems encompassing the radiocarbon chronology in a place like Thera. Far more caution and patience are needed, especially as the drop of the [tree ring] calibration curve between 1525 and 1515 BC can be considered the result of an artificial construction.”<sup>126</sup>

There is much more but one can now see how selective and biased the process used to date the eruption of Thera which buried the town of Akrotiri is. Reading Bietak’s full discussion is of salutary value before considering how well radiocarbon dating at Akrotiri fits the Thera eruption. We will return to the newest evidence regarding this process below.

In Velikovskian and other literature various critics of Velikovsky’s chronology respecting the Thera eruption claimed its date precluded Velikovsky’s timetable.<sup>127</sup>

The argument and evidence for the dating of Thera is that when it erupted it cast considerable material into the sky which in time blocked sufficient sunlight so as to cool down the Earth and to leave extremely narrow tree rings from 1628 B.C. in Bristlecone Pine trees in the White Mountains of California, and frost-damaged tree rings in Irish oaks for the same year, as well as an acid signal in the Camp Century, Dye 3, and Summit ice-cores in Greenland. Sean Mewhinney writes:

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<sup>126</sup> *ibid.*, pp. 216-217

<sup>127</sup> Sean Mewhinney, “Ice Cores and Common Sense, Part I”, *Catastrophism and Ancient History* (Jan. 1990), pp. 5-33; and “Ice Cores and Common Sense, Part II”, *Catastrophism and Ancient History* (July 1990), pp. 117-146; C. Leroy Ellenberger, “Still Facing Many Problems, Part I,” *Kronos*, vol. X, no. 1 (Fall 1989), pp. 97-102

“Quite a lot has been written lately by Velikovskians on the great explosive eruption of Thera. The question is, first, whether the eruption has been identified in the ice-core acidity record, ...

“The first attempt to provide a more precise dating was made by the ice-core team at the University of Copenhagen, led by Claus Hammer. ... A prominent peak stood out at  $1390 \pm 50$  B.C. ... This was in 1980.

“Then it was the turn of the dendrochronologists in the United States. ...

“Variations in [climatic] conditions leave distinctive patterns in the widths of the annual growth rings, which are used to match and cross-date different trees. The trees are susceptible to damage when the temperature falls well below freezing during their growing season. ...

“Valmore La Marche and Katherine Hirschboeck compiled a list of 19 known major volcanic eruptions (estimated to have injected at least as much dust into the stratosphere as Krakatoa), beginning in the year 1500 AD. They compared it with a list of 17 ‘notable frost-ring events’ during the same period at multiple bristlecone pine sites hundreds of miles apart. In ten cases a major eruption was followed a year or two later by a notable frost-ring event. In other words, there is a better than even chance that a major eruption will cause notable frost damage, and a better than even chance that a notable frost event was caused by an eruption. ...

“One especially severe frost event of earlier times was found in the year 1627 B.C. In a paper in *Nature*. La Marche and Hirschboeck suggested a date of 1629 to 1627 B.C. for the Thera eruption.

“Meanwhile, the glaciologists had been busy. During 1980 and 1981 a new deep ice core was recovered from Dye 3 in southern Greenland. ...

“They place the actual eruption ... in 1645 B.C., ‘with an estimated standard derivation of  $\pm 7$  yr, and an estimated error limit of  $\pm 20$  yr.’

“Then another party was heard from, once more in *Nature*. Dendrochronologists in Belfast [Northern Ireland] ... compiled a 7000-year Irish oak tree chronology. Much of the wood used in this work came from subfossil trees from a big bog in Northern Ireland. Michael Baillie and M. Munro have made a study of especially narrow growth rings in these oaks. ...

“Baillie and Munro found a cluster of very narrow growth rings beginning in 1627 B.C., providing additional support for La Marche and Hirschboeck’s dating of the Thera eruption at 1629–1627 B.C.

“These developments do not seem to have been properly digested as yet by those whose comments have appeared in Velikovskian organs.”<sup>128</sup>

Mewhinney summarizes the evidence for the dating of the eruption of Thera:

“Chemical analysis confirms that the Dye 3 acidity peak is due to sulfates which could only have come from a volcanic eruption. Bristlecone pines all across the southwest were damaged by frost in the same year that Irish bog oaks began to put on extremely narrow growth rings. The same weather anomaly must have been responsible for both. The close correspondence in dating between tree rings and [ice core] acidity peaks makes it very likely that the cause of the weather anomaly was dust from the same eruption that left sulfates in the ice. The approximate coincidence of all three records with the Thera radiocarbon dates, and the rarity of events of such great magnitude, render it likely that the eruption was that of Thera.”<sup>129</sup>

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<sup>128</sup> Mewhinney, “Ice Cores and Common Sense, Part II”, *op.cit.*, pp. 141-142

<sup>129</sup> *ibid.*, p. 143

According to Gregory A. Zielinski and Mark S. Germani, the ice core evidence in no way at all supports Mewhinney or anyone else's dating of the Thera eruption:

“Determining a reliable calendrical age of the Santorini (Minoan) eruption is necessary to place the impact of the eruption into its proper context within Bronze Age society in the Aegean region. The high-resolution record of the deposition of volcanically produced acids on polar ice sheets, as available in the  $\text{SO}_4^{2-}$ -time series from ice cores (a direct signal), and the high-resolution record of the climatic impact of past volcanism inferred in tree rings (a secondary signal) have been widely used to assign a 1628/1627 BC age to the eruption. The layer of ice in the GISP2 (Greenland) ice core corresponding to 1623±36 Image, which is probably correlative to the 1628/1627 BC event, not only contains a large volcanic- $\text{SO}_4^{2-}$ -spike, but it contains volcanic glass. Composition of this glass does not match the composition of glass from the Santorini eruption, thus severely challenging the 1620s BC age for the eruption. Similarly, the GISP2 glass does not match the composition of glass from other eruptions (Aniakchak, Mt. St. Helens, Vesuvius) thought to have occurred in the 17th century BC nor does it match potential Icelandic sources. These findings suggest that an eruption not documented in the geological record is responsible for the many climate-proxy signals in the late 1620s Image. Although these findings do not unequivocally discount the 1620s BC age, we recommend that 1628/1627 BC no longer be held as the “definitive” age for the Santorini eruption.”<sup>130</sup>

Mewhinney's ice core evidence that allegedly contradicts Velikovsky has therefore been shown to be in serious error.

Paul C. Buckland *et al.* explain the problem related to constructing a chronology on such disparate forms of evidence:

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<sup>130</sup> Gregory A. Zielinski and Mark S. Germani, “New Ice-Core Evidence Challenges the 1620s BC age for the Santorini (Minoan) Eruption”, *Journal of Archaeological Science*, vol. 25, issue 3 (March 1998), p. 279



“The first rule of statistics is that the existence of a correlation does not itself prove a causal connection ... This paper examines some of the available evidence for ... two Bronze Age ‘catastrophes,’ the one real and in need of a calendar date, the other hypothesized on archaeological ground and dated by a tenuous link through tree rings to an Icelandic volcano. ... Despite several cautionary comments from both archaeologists ... geologists ..., the 1628 B.C. date or one close to it continues to be accepted ... without questioning why the effects of the Santorini eruption should be especially recognizable in the ice-core and tree-ring sequence. Large-scale explosive volcanic activity is common on a global scale ..., and so before accepting the possibility that the Santorini eruption can be recognized by unusual perturbations in the regional records of ice-cores or tree-rings, the case for its distinctive character must be proved.”<sup>131</sup>

Let us therefore examine the tree ring evidence that Mewhinney presented. He stated that the bristlecone pines from the White Mountains of California properly show frost ring damage for the years between 1629 and 1627. What he failed to deal with is that bristlecone pine trees during times of great stress, such as in extremely cold years or during drought, will stop growing tree rings. R.W. Fairbridge states, “Certain pitfalls have been discovered in tree-ring analysis. Sometimes, as in a very severe season, a growth ring may not form.”<sup>132</sup> E. Schulman, one of the major figures in dendrochronological research, explicitly stated in 1958:

“... such trees [bristlecone pines] cautiously add no more than an inch to their girth in a century. With so little tissue to nourish they can afford to shut up shop almost entirely in lean years.”<sup>133</sup>

He further remarks:

“There is something a little fantastic in the persistent ability of a 4000 year old tree to shut up shop almost everywhere throughout its

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<sup>131</sup> Paul C. Buckland *et al.*, “Bronze Age Myths? Volcanic activity and human response in the Mediterranean and North Atlantic regions,” *Antiquity*, vol. 7 (1997), pp. 581-583

<sup>132</sup> R.W. Fairbridge, “Holocene,” *Encyclopedia Britannica Macropedia*, vol. 8 (London/Chicago IL 1982), p. 999

<sup>133</sup> E. Schulman, “Bristlecone Pine, Oldest Known Living Thing,” *National Geographic Magazine* (March 1958), p. 356

stem in a very dry year and faithfully to reawaken to add new cells in a favourable year.”<sup>134</sup>

With this problem, it is not reasonable to expect to have an accurate dendrochronology that reflects reality over 3500 years back into history. Of course there had to be long as well as short periods of drought throughout such a lengthy period, when these trees simply stopped making tree rings, and Schulman said this was a “persistent ability” with these trees. There must have been dozens of short- and long-term droughts over such an extremely long period of time. Further, there is no way to know how many such droughts occurred and for how long they lasted.

What the dendrochronologists who constructed the bristlecone pine chronology require for their tree ring count to accurately reflect every year back to 1628/1627 B.C. is that there were no long-term or short-term droughts across three and a half millennia. By simply counting tree rings back in time to 1628/1627 B.C., they say that every year has a ring and there never was a long- or short-term drought that caused these trees to shut up shop. That is simply not credible. Before anyone accepts this outrageous dating conclusion the dendrochronologists must prove that for some 3500 years the American Southwest *which is highly susceptible to droughts* for some amazing reason had no droughts! I am quite sure that they cannot prove this nor will they explain this immense problem for their chronology away.

Therefore one cannot say with any assurance that the date (1629-1627) of the frost ring damage on these trees has anything to do with reality or historical chronology, or the Thera eruption. As with everything else discussed above regarding dendrochronology, the date of the frost ring damage is dubious and quite probably erroneous. No one, so far as I know, has scientifically explained how to determine when and for how long droughts occurred in the American Southwest to cause these trees to shut up shop, nor do I believe it is possible for this to be done.

A related problem to dating bristlecone pines was pointed out by John Gribbin in 1982:

“First ... the dendrochronologists [in dating bristlecone pine tree rings] have to establish that in living trees the (oxygen-16 and oxygen-18) isotopes do not migrate across the tree-rings, garbling the

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<sup>134</sup> *ibid.*, p. 368

temperature record. This can be done by calibrating the outer rings of living trees against the historical record of temperature variations, before using inner rings to find out temperature changes in the more distant past. *And among other things, that means throwing out the data from the famous bristlecone pines ... of California, where there are no decent records of modern temperatures to establish the calibration.*<sup>135</sup> (emphasis added)

Nevertheless, Mewhinney presents as valid that the bristlecone pine frost ring damage is properly dated in spite of the fact that without decent records of modern temperatures to establish the calibration, one must throw out this dendrochronology and in spite of the well-known fact that droughts—common in the American Southwest—would cause these trees to shut up shop and stop producing tree rings. Evidence like this seems not to have been properly digested as yet by Mewhinney.

What then of the Irish oak dendrochronology? *In a certain sense* that chronology is floating and not fixed. Thus it is less reliable as a record. The trees that compose it were found buried in bogs in different locations of Ireland. The germane point is that there are major gaps in that record. According to Michael Baillie,

“[o]ne point should be clarified. No one set out to look for gaps ... The gaps are there, however, and show strongly in the fact that they appear in the results of separate workers. So we can suggest that there are periods from which relatively large numbers of long-lived timbers survive, *separated by intervals when such timbers virtually do not exist.*”<sup>136</sup> (emphasis added)

According to the investigators these gaps, of 100 years or more, are found in both the B.C. time period and in the A.D. era. Baillie, unable to explain this anomaly, admits:

“In theory, oaks should have existed continuously on the bog in order to explain their presence after the gap. Extensive sampling was

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<sup>135</sup> John Gribbin, *Future Weather and the Greenhouse Effect* (NY 1982), p. 124

<sup>136</sup> M.G.L. Baillie, *Tree Ring Dating ...*, *op.cit.*, p. 222

undertaken and the whole length of each of the chronologies was duplicated with fresh material. No trees were found which would bridge the gap although numerous specimens fell on either side.”<sup>137</sup>

He further adds: “Now obviously material of this age must exist: *oaks did not die out at 900 BC.*”<sup>138</sup> (emphasis added)

It is clearly impossible for trees to not have fallen in the bogs that were investigated or to have failed to produce a single specimen for long, identical time periods, and then, as if by magic, for the trees at these bogs to begin in unison to fall again. The trees could not have all completely vanished into thin air during these identical time periods. This evidence indicates that there is something fundamentally wrong with the methodology used to complete the Irish oak dendrochronology. What appears obvious to me is that *the trees from the gaps must have been incorporated into chronological periods where they do not belong.* What would follow if this is in fact the case is explained by Keenan:

“... it should be noted what happens if trees are erroneously matched against a master dendrochronology and then included as part of the master. This error will lead to corruption of the average ring widths of the master. (And that could then lead to the incorrect inclusion of other trees in the master ... If the master is unreliable, then plainly the match of other trees against it would tend to be less reliable.”<sup>139</sup>

Once the Irish oak master dendrochronology was organized incorporating the trees from the gaps, then naturally any other investigator using the same methodology and the same materials would also incorporate the very same tree rings from other trees from the gaps into the very same sites of the master dendrochronology. This, I believe, well explains the gaps in the chronology being identical for all investigators.

The most egregious problem related to the tree ring and radiocarbon interlocking method is explained by the fact that tree rings are dated not only by cross-matching but are correlated; according to James L. Dyson:

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<sup>137</sup> *ibid.*, p. 219

<sup>138</sup> *ibid.*, p. 221

<sup>139</sup> Keenan, *op.cit.*, p. 14

“dendrochronologists derive corroboration for their methodology also by employment of radiocarbon dating which supplements the procedure.”<sup>140</sup> On the other hand, radiocarbon dating derives its calibration (or correction) curves for this methodology by the employment of dendrochronology! Dendrochronological dates into the deep past are determined to be corrected by the use of radiocarbon dating while radiocarbon dates are determined to be correct only by the use of dendrochronology.

At this point I smell a circular reasoning rat. A beautiful example of circular reasoning was performed by the comedians George Burns and Gracie Allen:

“Gracie: Gentlemen prefer blondes.

George: How do you know that?

Gracie: A gentleman told me so.

George: How do you know he was a gentleman?

Gracie: Because he preferred blondes.”<sup>141</sup>

By the same token, if we question a scientist who believes in the validity of dendrochronology and radiocarbon dating methods we find:

Scientist: Dendrochronology is an exact science.

Questioner: How do you know that?

Scientist: Radiocarbon proves it is an exact science.

Questioner: How do you know radiocarbon is an exact science?

Scientist: Because dendrochronology proves it.

Neither radiocarbon nor dendrochronology can stand independent of the other as a methodology. In both cases both methodologies are only deemed valid after they correct one another. What the dendrochronologist/radiocarbon investigator does not understand is that one cannot correct an incorrect methodology

<sup>140</sup> James L. Dyson, *The World of Ice* (NY 1972), p. 208

<sup>141</sup> Joanne A. Epps *et al.*, *The Winning Argument* (Chicago IL 2001), p. 58

by one that is also incorrect. Two incorrect methodologies do not make each other right. Warren Weaver succinctly put his finger on the error in the logic of dendrochronology correcting radiocarbon dating because radiocarbon dating corrects dendrochronology:

“One often hears the complimentary phrase ‘flawless logic;’ and it is hard to realize that this discipline, so long considered the one kind of thinking or reasoning beyond criticism should have this mysterious inner flaw which consists of the fact that you can never discover whether or not it really has a flaw.”<sup>142</sup>

This applies, too, with the complementary sciences of dendrochronology and radiocarbon dating. It is hard to realize that these disciplines, so long considered as valid scientific methodologies and beyond criticism, should have this obvious inner flaw which consists of the fact that each discipline has to call on the other for validation. Therefore you can never discover whether or not either is scientifically valid. Dendrochronology proves radiocarbon dating valid while radiocarbon dating proves dendrochronology valid. When A proves B valid and B proves A valid, nothing is proved. It is all a piece of monumental circular supports rationale. This is one of the major *logical reasons* this author does not subscribe to either of these disciplines. On the other hand, William H. Stiebing, Jr., a noted Egyptologist, claimed:

“... rejection of radiocarbon dating also means rejection of dendrochronology ..., one of the most accurate [scientific] methods yet developed. A tree ring sequence 7,000 years long has been developed from the long-lived bristlecone pine trees of the southwestern United States [which is assumed to never have short or long-term droughts that might cause the tree to stop growing tree rings]. ... *More recently, a 6,000 year tree-ring sequence developed from Irish Oak Trees has been radiocarbon tested [to confirm it]*, and its calibration curve generally agrees with that established from the bristlecone pine sequence [even though it has major gaps in the sequence when the Irish oaks stopped existing to fill these gaps]. Both sets of calibrated dates generally support the conventional chronology for ancient Egypt and, thus,

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<sup>142</sup> Warren Weaver, quoted in *Leo Rosten's Carnival of Wit, op.cit.*, p. 285

cannot be reconciled with revisions like those proposed by Heinsohn.”<sup>143</sup> (emphasis added)

It is staggering to read this statement loaded with such fundamental ignorance. First of all, Stiebing fully admits that the Irish oak tree sequence had to be tested by “radiocarbon” to confirm it, which points unambiguously to the fact that each discipline is validated by the other. Second, Stiebing was ignorant of what Keenan reported about his Egyptologist colleagues, especially Nelson *et al.*, 1990, a year prior to this statement; namely that “some archaeologists have said that if they attempted to publish 14C dates, contradicting archaeo-historical chronologies [meaning the established chronology], their papers are rejected.” It is not at all difficult to realize that when other investigators learned about the unscholarly and unethical procedure, they would not even submit contradictory radiocarbon evidence to the established chronology, knowing it, too, would be rejected and perhaps their careers endangered.

What Stiebing wishes is to have it both ways: to attack Heinsohn’s chronology based on his ignorant belief that radiocarbon and tree ring dating are valid, but then failing to address the fact that his colleagues suppress radiocarbon dates that contradict that chronology. Radiocarbon/tree ring evidence that upholds the established chronology is valid where it suits him, but it is invalid if it fails to uphold that chronology and is then suppressed. With such an approach to evidence one can prove whatever one wishes simply by only publishing what fits and suppressing what fails to fit. When used in the way, radiocarbon and dendrochronology is bad science and bad historical evidence.

The oak trees of Ireland are found in bogs, and it is well known that this wood is easily contaminated by radiocarbon materials of other small leafy plants, mosses and leaves that fall into the bog to eventually form peat. Peat contains a great deal of carbon—up to 70 percent—in a matrix of soil, plant material, and water.<sup>144</sup> This is an ideal environment for radiocarbon contamination to occur.

The contamination process operates this way: a tree falls into a bog, and its leaves decay along with other vegetation, releasing old or dead carbon into the water. The next that falls does the same, and this process is repeated throughout the

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<sup>143</sup> W.H. Stiebing, Jr., “Heinsohn’s Revised Chronology,” *op.cit.*, p. 49

<sup>144</sup> Ivan Sanderson, *Investigating the Unexplained* (Englewood Cliffs NJ 1972), pp. 89-91

lifetime of the bog. Therefore, all the trees in a bog can become contaminated with carbon from younger trees and dead bog vegetation. As E.C. Pielou states, “A buried specimen can absorb young carbon if the water in overlying fresh humus drains downward into it ...”<sup>145</sup> In a bog, the circulating water allows the specimen to absorb young carbon. There is clearly no way a bog can develop over a long period of time without tremendous amounts of contamination from young and old carbon happening. Young trees also absorb carbon from older ones.

Colin Renfrew, one of the world’s foremost archaeologists and anthropologists of European pre-history admits that when oak wood specimens are radiocarbon dated which were not recovered from a bog but which give a date that does not agree with its assumed dendrochronological age, the “favourite explanation of archaeologists” is that the wood was originally “bog oak.”<sup>146</sup> The archaeologists, unable to match the dendrochronological and radiocarbon ages, fall back on the excuse of “contamination.” In essence, radiocarbon is corrected by dendrochronology, and *vice versa*, to establish the Irish oak dendrochronology. But when the two methodologies fail to fit the expectations of the investigators, they claim the tree rings must be correct because the wood must have come from bog oak and was contaminated, so it gave an incorrect date. That is, the methodology allows the investigators to make the evidence say what they want it to say. When the radiocarbon data, via calibration, does not fit the tree ring date, there had to be contamination. When a methodology can be manipulated in this way the results obtained naturally have to fit the established chronology; it is a foregone conclusion. The methodology is doomed to support that chronology. In the end this abuse of these methods leads to what G. Bailey said long ago: “The first and worst of all frauds is to cheat oneself.”<sup>147</sup>

With these problems, it is evident that the Irish oak chronology may well be misdating the narrow rings, attributed to the eruption of Thera, and cannot be relied upon to be all it claims for itself. In order to salvage the Thera eruption date, Hammer *et al.* examined a portion of the Greenland ice-core and found another acid signal as well as glass shards which they claimed came from Thera, dated to 1645 B.C., again proving the dating of Akrotiri. But are that signal and the glass shards that accompanied it actually from Thera, as Hammer *et al.* claim? What they

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<sup>145</sup> E.C. Pielou, *After the Ice Age* (Chicago 1992), p. 58

<sup>146</sup> Colin Renfrew, *Before Civilization* (NY 1973), p. 265

<sup>147</sup> G. Bailey, quoted in *The New Dictionary of Thoughts, op.cit.*, p. 220



claimed is that the glass shards in the ice core were chemically so similar to the tephra and glass shards taken from that Aegean volcano that there could be no problem with it. Yet though this ice layer was at first dated to around 1633 B.C., it was thereafter corrected to date to about 1644 B.C.

The Dye 3 core is assumed to be very well dated and hence the material was also assumed to be from Thera. In an “Extended abstract for the Haindorf Castle SCIEM Conference, April [sic] 2001,” Hammer and his team presented a paper “Recent ice core analysis strengthens the argument for a mid 17th century BC eruption of Thera.” These investigators claimed that the tephra was

“... of a very similar composition to the Thera pumice and glass. Not only has the tephra the same bulk mineral composition as Thera, but also the REE [Rare Earth Element] composition closely resembles the abundance of rare earth elements in the Thera ash.” [The paper was not published in a peer-review journal.]

To strengthen their evidence, Hammer *et al.* suggested that the volcanic acid signal in the ice core, dated to 1645 B.C., would be in line with a volcanic eruption in the northern hemisphere which would naturally fit Thera. While other investigators suggested the signal was from other volcanoes such as Aniakchak, Alaska, or Avellino, Hammer *et al.* maintained that the chemical makeup did not allow for such an identification. However, in 2003 they finally published their “Thera eruption data 1645 B.C. confirmed by new ice-core data?” in *The Synchronisation of Civilizations in the Eastern Mediterranean in the Second Millennium B.C. II* [M. Bietak ed. (Vienna 2003), pp. 87-94] where all the evidence was presented. It seemed that they had finally dated the Thera eruption and Egyptian history had to be lengthened by some 130 years.

On the face of it, it seemed the case was closed. Heinsohn’s and Sweeney’s chronologies had been dealt a fatal blow. But that was before other investigators took a long careful look at the evidence of Hammer *et al.* and showed that it was not at all what had been claimed for it. Nicholas J.G. Pearce *et al.* wrote as follows:

“Minute shards of volcanic glass recovered from the  $1645 \pm 4$  BC layer in the Greenland GRIP ice core have recently been claimed to originate from the Minoan eruption of Santorini [*Hammer et al.*, 2003].

This is a significant claim because a precise age for the Minoan eruption provides an important time constraint on the evolution of civilizations in the Eastern Mediterranean. There are however significant differences between the concentrations of SiO<sub>2</sub>, TiO<sub>2</sub>, MgO, Ba, Sr, Nb and LREE between the ice core glass and the Minoan eruption, such that they cannot be correlatives. New chemical analyses of tephra from the Late Holocene eruption of the Aniakchak Volcano in Alaska, however, show a remarkable similarity to the ice core glass for all elements, and this eruption is proposed as the most likely source of the glass in the GRIP ice core. ... The age of the Minoan eruption of Santorini [ancient Thera], however, remains unresolved.”<sup>148</sup>

The work of Pearce and his team makes it quite clear that the chemical composition of the glass shards is not the same as those on Thera, and thus the eruption must be dated by other means. Eight months later, Douglas J. Keenan analyzed Hammer *et al.*'s 2003 claims and concluded “the Greenlandic tephra is not from the Minoan eruption of Thera.”<sup>149</sup> Keenan showed that most of the chemical makeup of the shards could not be matched even at low statistical levels with Thera. Although the rare earth elements were shown to be not as strongly different, there was much other material that was chemically incompatible with Thera so that these rare earths were deemed to be of little merit.

Keenan used 174 items of tephra and glass from the GRIP ice core, and compared them to 38 items of tephra and glass from Thera; he showed that the matches presented by Hammer *et al.* simply had no standing. Although there were disagreements between Keenan and Pearce *et al.*'s, it was clear that the particle analysis by both parties show that the materials found in the Greenland ice core could not be from Thera. Both agreed that the comparisons they made of the chemical makeup of the tephra and glass from the Dye 3 core showed that there could not be the same as the tephra and glass from the Minoan eruption. There is apparently no

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<sup>148</sup> Nicholas J.G. Pearce *et al.*, “Identification of Aniakchak (Alaska) tephra in Greenland ice core challenges the 1645 BC date for Minoan eruption of Santorini,” *Geochemistry, Geophysics, Geosystems*, vol. 5, no. 3 (Mar. 20, 2004), p. 1 (Abstract) of Internet online publication (<http://www.agu.org/pubs/crossref/2004/2003GC000672.shtml>)

<sup>149</sup> Douglas J. Keenan, “Volcanic ash retrieved from the GRIP ice core is not from Thera,” *Geochemistry, Geophysics, Geosystems*, vol. 4, no. 11 (Nov. 15, 2003), p. 7, Internet online publication [[www.informath.org/pubs/G%5E303a.pdf](http://www.informath.org/pubs/G%5E303a.pdf)]

evidence in the Greenland ice core that solidly connects it with Thera and thus one cannot claim that this connection is there.

As a sidebar to this discussion, in 2004 a group of investigators suggested that the signal in the Greenland ice core is not even that of a volcanic eruption.<sup>150</sup> This, if shown to be correct, should be devastating to protagonists of the thesis that such acid signals in the ice must always reflect volcanic eruptions.

However, when we look at the connections made for the frost rings in the bristlecone pines of the American southwest with those of the Irish oak and Anatolian dendrochronology with the Greenland ice core, another fundamental problem exists. How could Thera have produced all these forms of evidence when it was incapable of leaving its mark in the Greenland ice cap for this date?

The point is no-one truly knows which volcano, if any, was responsible for the acid signal, along with the tephra and the glass shards left in the ice core.

This brings us at last to the final piece of evidence that Manning, Kuniholm and the other proponents of the 1645 B.C. eruption date for Thera have, namely an olive branch. In April 2006, *Science* magazine published two papers, one by Walter L. Friedrich *et al.* which contained the following information:

“Precise and direct dating of the Minoan eruption of Santorini (Thera) in Greece, a global Bronze Age [chronology] marker, has been made possible by the unique find of an olive branch buried alive in life position [with its bark covered] by the tephra (pumice and ashes) on Santorini. We applied so-called radiocarbon wiggle-matching to a carbon-14 sequence of tree-ring segments to constrain the eruption date to the range 1627-1600 B.C. with 95.4% probability. Our result is in the range of previous, less precise, and less direct results of several

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<sup>150</sup> Peter M. Fischer and Martin J. Whitehouse, “Quantitative SIMS (IMS 1272) of Particles from the GRIP Greenland ice core and Thera”, paper presented to the workshop “Ashes + Ice” VERA Laboratory University of Vienna (8-10 July 2004) (<http://www.nhm-wien.ac.at/sciem2000/>)

scientific dating methods, but it is a century earlier than the date derived from traditional Egyptian chronologies.”<sup>151</sup>

The other paper by Sturt W. Manning *et al.* came to a similar conclusion:

“Radiocarbon ... data from the Aegean Bronze Age 1700-1400 B.C. show that the Santorini (Thera) eruption must have occurred in the late 17th century B.C. By using carbon-14 dates from the surrounding region, cultural phases and Bayesian statistical analysis, we established a chronology for the initial Aegean Late Bronze Age cultural phases ... This chronology contrasts with conventional archaeological dates and cultural synthesis ... by ~100 years ...”<sup>152</sup>

The olive tree branch was found at a site where the olive tree grew, exhibiting roots as well as leaves, and thus there could be no doubt that it was buried by the Thera eruption. The radiocarbon analysis was carried out, as I gather, by three laboratories which all reached the same conclusion about its date being 1613 B.C. Thus it would seem that the eruption of Thera and destruction and burial of Akrotiri occurred long before Heinsohn’s and Sweeney’s theses would allow. Nevertheless, there is a serious problem with this 1613 date even given all the evidence presented by the investigators. Sturt Manning in *A Test of Time* explains:

“Volcanoes emit carbon-dioxide and [in doing so] can affect normal radiocarbon levels in the atmosphere close to the vents. This effect has been observed, Thus it has been suggested that the radiocarbon determinations from Thera [prior to the finding of the olive branch] have been contaminated, and are not correct. In practice such effects are strictly limited to plants growing very close to the volcanic source (a vent) and are virtually non-existent at [greater than] 100 m[eters, 110 yards] (unless the local environment traps air and does not permit rapid atmospheric mixing) ... high levels of atmospheric mixing on this windy island, render a significant volcanic effect negligible, it

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<sup>151</sup> Walter L. Friedrich *et al.*, “Santorini Eruption Radiocarbon Dated to 1627-1600 B.C.,” *Science*, vol. 312, no. 5773 (April 28, 2006), p. 548

<sup>152</sup> Sturt W. Manning *et al.*, “Chronology for the Aegean Late Bronze Age 1700-1400 B.C.,” *Science*, vol. 294 (Dec. 21, 2001), p. 565

should, moreover, be evident in anomalous  $\delta^{13}\text{C}$  values [carbon 13 values much different than they are presently found in samples], this does not appear to be the case for the Thera [seed] samples [found at Akrotiri] with known  $\delta$  (delta)  $^{13}\text{C}$  values. Nonetheless some doubt remains in some minds.”<sup>153</sup>

Manning’s assumption that the olive tree had to be very close to a vent in order to be greatly contaminated by dead carbon 12 is not truly correct. As Bernard Pipkin *et al.* explain:

“Mammoth Mountain, a large rhyolitic volcano is still hot. Tree killing emissions of  $\text{CO}_2$  were discovered at the base of the mountain in 1990. This indicated magma activity at shallow depth ... By 1995 about 30 hectares (75 acres) [12 million square feet] of pine and fir trees had suffocated and  $\text{CO}_2$  concentrations of up to 90 percent of total gas were found in the trees’ soil and root systems. Where  $\text{CO}_2$  concentrations exceeded 30 percent most of the trees were dead. One large campground had to be closed because park rangers found high levels of  $\text{CO}_2$  in restrooms and cabins after campers exhibited symptoms of asphyxia. It appears that carbon dioxide degasses from the magma and migrates into a deep gas reservoir. When [volcanic] earthquake swarms cause fractures to develop in the reservoir rock, the gas finds its way [through the soil] to the surface. It is estimated that between 200 and 3000 tons of carbon dioxide flow into the soil and atmosphere per day.”<sup>154</sup>

One will note that the carbon dioxide was not only venting from wide or narrow vents in the sides of the volcano as Manning claims, but that it was welling up into the soil and root systems of acres of trees. That means it was constantly rising from the soil around the trees and taken into their leaf-pore openings, over time not only contaminating them with additional carbon 12 compared to radiocarbon 14, but eventually killing these trees. This carbon-dioxide was not being blown away rapidly, so as not to contaminate these trees. Surely, the winds around Santorini/Thera didn't constantly blow away all the  $\text{CO}_2$  gas. And this process does not necessarily kill trees but it would make them radiocarbon date much older than

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<sup>153</sup> Sturt W. Manning, *A Test of Time* (Oxford UK 1999), pp. 234-236

<sup>154</sup> Bernard Pipkin *et al.*, *Geology and the Environment* (Independence KY 2005), p. 141

they are in reality. To suggest, as does Manning, that this olive branch found at Thera could not possibly have been contaminated cannot at the present time be known. It is only an assumption on Manning's part that it wasn't. In such a high volcanic carbon dioxide environment this assumption is contrary to good science.

Comparing seeds found with the olive tree branch has no value. In the paper published, carbon 13 values were not discussed. Furthermore, it is not necessarily so that high delta 13 values are always associated with volcanic gases. That is why there has always remained a doubt regarding any radiocarbon date from this volcanic island. We do not know if the island prior to its eruption had numerous volcanic vents spewing carbon dioxide into the atmosphere to create a Suess Effect which makes samples date much older than they are in reality. We do not know the wind patterns around the island thousands of years ago. At that time gases vented may have shrouded the entire island before dissipating.

Four arguments, however, have been raised to deny the possibility that the olive branch and other items from Santorini and elsewhere were affected by volcano-vented carbon dioxide:

1. The C14 data from Santorini matches with data from other areas (not affected by vented CO<sub>2</sub> gas).
2. Because the C14 data from Santorini exhibit none of the characteristics one would expect, such as the seeds, they are therefore consistent with and very similar to the C14 data from Santorini.
3. Because all the data from regions outside Santorini matches with the C14 dating of Santorini the placement must be correct.
4. Because the C14 data at Santorini has consistently during seven decades shown the 1645 B.C. data to be correct, if the olive branch was so out of line, it would not exhibit this closeness with the other Santorini data. [All this was presented on the Internet by those who support the 1645 B.C. date.]

What is obvious is that the proponents of the 1613 B.C. date for the Thera eruption suggest that all other materials at Santorini and far from Santorini give radiocarbon dates that interlock to some extent, therefore the olive branch has

to be properly dated and unaffected by vented dead carbon dioxide. But before items of this nature can be called upon as support for the 1613 C14 date for the olive branch, it must be able to stand by itself as evidence. What is being done to give it support is to cite other radiocarbon dates from other areas to support the validity of the date of that olive branch.

Bietak succinctly points out that “Manning and others have recently abandoned the potential eruption date of 1628/27 B.C. ... together with P.I. Kuniholm ...”<sup>155</sup> That is, they are now admitting that the Irish oak as well as the Anatolian dendrochronologies dating the eruption to 1628-27 B.C. are of no value. And they are admitting that the Dye 3 ice core dating the eruption to 1628-27 B.C. is of no value. Instead of using dendrochronology and ice core evidence as solid support, which it never was, they now suggest all this surrounding support evidence be jettisoned and that only radiocarbon data (that they selected) should be used. Why? Because their case is extremely weak due to their relying only on the radiocarbon evidence. If the eruption was as tremendous as is claimed, why did it fail to leave a signal in the ice-core or produce frost damage in bristlecone pines and narrow tree rings in Irish bog oaks? The evidence suggests the eruption was not, as Greenberg suggested and as the standing buildings in the town of Akrotiri show, as large as the claims made for it.

Eberhard Zangger outlines the evidence which shows the eruption at Thera was not as huge and destructive as the popular literature presents. He first looked for evidence of the great size of the tidal wave supposedly created by the eruption. Dale Dominey-Howes was able to drill a bore hole on Crete 90 miles from Santorini “with an overall length of 9.3 metres [42 feet].”<sup>156</sup> From this were taken three samples every five centimeters [2 inches]. One set of samples went to Dominey-Howes, the other to the University of Groningen.<sup>157</sup>

“Just a few weeks after the field studies had finished, we had a letter from Dale, who had studied all the samples in the meantime. In it he stated that by far the majority of the sediments had originated under conditions that corresponded to those found today: in a fresh-

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<sup>155</sup> Bietak, “Review of Manning 1999,” *op.cit.*, p. 218

<sup>156</sup> Eberhard Zangger, *The Future of the Past: Archaeology in the 21st Century*, transl. Storm Dunlop (London 2001), p. 43

<sup>157</sup> *ibid.*

water marsh. Only at a depth of 4.6 metres [13 feet] was there a thin layer of marine deposits that could be attributed to a tsunami. The position could correspond to a Bronze Age layer, but to be able to actually ascribe the thin layer to the eruption of Thera further detailed studies are required. But at least this much was already certain: If waves were actually produced during the eruption of Thera, their maximum effect was restricted to depositing a thin layer of sand, of just about one centimeter [about a quarter of an inch] in thickness, at a depth of 3.2 metres [10 feet] below the present-day sea levels.

“Crete is a geologically active region, in that elevation and depression of land—and thus also shifts in the coastline—occur repeatedly. As far as the area around Malia [on Crete] is concerned, archaeologists assume that the sea level in Minoan times was about two to three metres [8 to 12 feet] lower than at the present day. This means that relative to modern conditions, settlement stretched farther out to sea. Our small tsunami deposit would thus have been produced at about the sea level that prevailed at the time [and not by a tidal wave from Thera].”<sup>158</sup>

Zangger goes on to show how old the Thera caldera was as well as how very weak was the eruption:

“Studies, like those that we performed on Crete, have naturally been tackled to a far greater extent on Thera. These have shown that the claim, originally made by Fouqué, that the caldera was formed immediately after the Minoan eruption, is not correct. Marinatos, Galanopoulos and the other advocates of the catastrophe theory always assumed that, in the Bronze Age, the island consisted of a volcanic cone more than 1600 metres [5,250 ft] high, which was initially blasted away in the Minoan eruption, creating the ring of islands with the water-filled basin in the centre. Since then, however, geologists have found petrified blue-green algae, which must have lived inside the caldera, below sea level, more than ten thousand years ago. So there was no mountain at all in the Minoan period, but a caldera instead, just like the one today. This is also shown by erosion gullies in the Minoan surface layers,

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<sup>158</sup> *ibid.*, pp. 43-44



which slope in the direction of the current caldera. In addition, layers of pumice stone from the Minoan eruption may be found on the inner, steep coast of the island, and the deposits at these sites imply that the caldera already existed. Archaeologists have discovered both Minoan buildings and graves right on the edge of the caldera, and not one of these remains shows any signs of movement.

”Thera did not, therefore, possess a large volcanic cone, at least not immediately before the eruption in the Minoan period. The caldera was formed much earlier, possibly during the last great eruption, 23,000 years ago. As far as may be determined nowadays, during the Bronze Age the group of islands appeared very similar to the way it does today. Certainly no great tectonic movements have occurred, and definitely not of the size of a caldera collapse—otherwise the Minoan buildings and the pumice layers would have been far more strongly disturbed.

“The whole tsunami theory rests on the assumption, however, that the caldera collapsed in the Minoan period, and thus inevitably created giant waves.”<sup>159</sup>

In toto, there was no immense volcanic eruption of Thera. Had there been one in Minoan times, as presented by the historians, the immensity of the ash cloud would have left tiny glass shards in the Greenland ice at the proper level, but no such material exists to prove the connection between the Thera eruption and the ice core. The frost rings of bristlecone pines in the White Mountains cannot be correlated with the Thera eruption unless one is willing to accept the conclusion that there were never any droughts in this region to shut down growth in these trees. The damage to Irish oak tree-rings of comparable age suggested for Thera requires that one assume that these oak trees completely stopped growing for periods of time and then suddenly began to grow again after these time gaps found by all researchers. The great Thera tsunami that supposedly struck Crete was not great. It was tiny and supposedly left less than an inch of sediment. The great Thera caldera was created many thousands of years before the historical one is said to have been blasted and then collapsed. The Minoan buildings and grave sites at the edge of the caldera would not be there if the eruption was as stupendous as claimed. Akrotiri’s many standing buildings with murals, etc., would have been destroyed by the blast wave

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<sup>159</sup> *ibid.*, pp. 44-45

and earthquake at the time of the eruption. All the evidence and correlations of these to prove that the Thera eruption was a colossal catastrophic event—to put it bluntly—are wrong!

Therefore, how is one to determine whether the Thera eruption occurred in the second millennium or the first (as Heinsohn's and Sweeney's chronologies require)? Peter James stated above, "Precision in archaeological dating is a desideratum ... It needs ... precision in the selection of high-quality samples from contexts with impeccable and fully published credentials." Bietak, also above, with respect to the Uluburun shipwreck, also claimed "We are still forced to use artifacts from the [site] ... as a way of dating." Rather than turn to scientific or technological evidence such as tin and its sources along with other materials, the historians and archaeologists wedded to the established chronology are suggesting that pottery dating and other similar forms of archaeological evidence should be turned to, to settle the dispute. Instead of employing these highly interpreted forms of evidence, we suggest that forensic historical forms of evidence be employed to suggest a date, not necessarily the precise date, but the general time frame in which the eruption had to have occurred.

As seen above, all the methodologies that were formerly employed to put the date of the Thera eruption in the mid-second millennium B.C. have failed. As with all the evidence presented in volume I of this series, and all the evidence cited above, scientific and technological evidence must now be presented and applied to the timing of that eruption.

That eruption is regarded as belonging to roughly 1500-1645 B.C., the Egyptologists staunchly standing by the 1500 B.C. date, and Manning, Kuniholm, and their associates holding with the *ca.* 1613 B.C. date. What no one has discussed is that this date falls into Minoan chronology which is the Bronze Age. But the source of tin for making bronze does not (according to the established chronology) become available for 400 to 545 years. We reported in volume I of *Pillars of the Past*, p. 488, that John Chadwick in *The Mycenaean World* (London 1976), p. 130 ff, pointed out:

"It is the source of tin which is the puzzle ... there are objects of bronze at Knossos ..." At Pylos, "there is a long series of documents listing the smiths ... and the amounts of bronze issued to them ..."

Tin trade was supposed to be in full bloom in the Aegean world between 1645 and 1500 B.C., but its source is unknown and comes 400 to 545 years in the future, based on the established chronology and even hundreds of years closer to the present based on Heinsohn's and Sweeney's chronologies. Bietak and Manning spent much effort on relating the various artifacts and wall paintings found at Akrotiri and at Tell el-Daba in Egypt. Tell el-Daba is the Hyksos site of Avaris<sup>160</sup>. But again neither of these investigators discusses the fact that the Hyksos used tin bronze and introduced its use into Egypt. Here is a brief list of citations regarding Hyksos use of bronze. Kurinsky states that during the Hyksos period, *ca.* 1640-1532, "Tin was bourned ... into Egypt ... The Egyptians were taught [by the Hyksos] to alloy copper with this magic metal to transfer it into a new, harder, and more durable material, bronze."<sup>161</sup>

Richard A. Gabriel claims that the Hyksos had "Bronze scale body armor [which] protected their infantry from ... Egyptian infantry weapons (which at that time were) often primitive and poorly manufactured."<sup>162</sup> Peter Jay tells us the Hyksos introduced "Bronze-working" into Egypt.<sup>163</sup>

Finally, in Mesopotamia, the 1600s were the period of the Mitanni, and we have shown above that they, too, possessed tin bronze. But as is also well known, there were no sources of tin for bronze anywhere in the ancient world during this period which requires that all these societies associated with the Aegean Minoan civilization of Akrotiri be moved into the first millennium B.C. What of Akrotiri itself? Kristian Kristiansen and Thomas B. Larsson remark that among the ruins of the town "Minoan bronze cups from Akrotiri [were uncovered]."<sup>164</sup> Sinclair Hood informs us "Short bronze dress-pins and a group of silver rings, either earrings or hair-rings, have been recovered from Akrotiri."<sup>165</sup>

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<sup>160</sup> Bietak, "Review of Manning 1999", *op.cit.*, pp. 204-208

<sup>161</sup> Kurinsky, *op.cit.*, pp. 114-115

<sup>162</sup> Richard A. Gabriel, *op.cit.*, p. 30

<sup>163</sup> Peter Jay, *The Wealth of Man* (NY 2000), p. 27

<sup>164</sup> Kristian Kristiansen and Thomas B. Larsson, *The Rise of Bronze Age Society: Travels, Transmissions and Transformations* (Cambridge UK 2005), p. VII

<sup>165</sup> Sinclair Hood, *The Arts of Prehistoric Greece*, The Yale University Press's *Pelican History of Art* (New Haven CT 1978), p. 197

Ernest Zebrowski states that at Akrotiri “Bronze tools have been found, even a piece of glass that appears to be a lens.”<sup>166</sup> All this evidence of tin bronze at Akrotiri indicates that it must be placed in the first millennium B.C. and therefore the eruption of Thera occurred some time in that millennium. The question is: How far into the first millennium B.C. must Akrotiri be situated?

According to historians and archaeologists, the Mycenaean or mainland Greek civilization and the Minoan or Aegean civilization collapsed around 1200 B.C. Then for about 500 years, there was a Dark Age, as discussed in volume I of this series and in this volume in Mesopotamia. Therefore, the Mycenaean and the Minoan civilizations must be moved at least 500 years or more forward in time to fill this gap. Much historical documentation for this period comes either from Homeric literature, especially from classic authors who supposedly lived about a thousand years after these civilizations fell and that part of the world was engulfed in a Dark Age. Philip de Souza nonetheless claims “legendary exploits of the ancestors of the Greeks reported in the authors of the Classical period cannot be taken as evidence for the history of the Bronze Age.”<sup>167</sup>

Almost nothing of the history of these Aegean Minoans is actually known. Steven Roger Fischer informs us:

“No one truly knows what race of people these marvelous Minoans of ancient Crete [and Santorini/Thera] belonged to. What they called themselves. Where they came from. What really prompted them about the middle of the second millennium B.C., gradually to yield their sovereignty ... —apparently in no violent fashion— to the northern mainlanders, the Mycenaean Greeks.

“As Europe’s greatest mystery, the Minoans are the historians’ greatest challenge ...

“[What we do have] to tell us anything about these brash pioneers of Europe’s frontier days is ... their rare architecture, art and artifacts.

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<sup>166</sup> Ernest Zebrowski, *Perils of A Restless Planet. Scientific Perspectives on Bronze Age Natural Disasters* (Cambridge UK 1997), p. 10

<sup>167</sup> Philip de Souza, *Piracy in the Graeco-Roman World* (Cambridge UK 2000), p. 16

“And that of their rarer scripts ...”<sup>168</sup>

J. Lesley Fitton discusses what is assumed to be known regarding the early Minoans of Crete:

“Thucydides, the great historian of the fifth century BC ... gives us our first written account of the early history of Crete ... All he could do was to take the traditional stories, eschew the supernatural and rationalize the rest into as coherent a picture as possible. In fact, he does not find much to say about early Crete—a less methodical and rigorous mind would certainly have found [taken] more [from these legends].”<sup>169</sup>

She goes on to show that Crete is actually mentioned in the *Iliad* in the “Catalogue of Ships” that carried the Greek army to Troy. Yet Fitton then explains that the “Catalogue”

“... is widely thought to be an interpolation into the *Iliad*, and of a later date than much of the poem ...

“In the *Odyssey* (XIX, 174ff) ... Odysseus pretends ... that he is Idomeneus’ young brother and claims that Odysseus visited him in Crete: ‘Out of the wine-dark sea there lies a land called Crete, a rich and lovely land washed by waves on every side, densely peopled and boasting ninety cities ... one [of which] ... is the great city called Knossos, and there for nine years King Minos ruled.’”<sup>170</sup>

One is led to ask, why did Thucydides know so little of Knossos if trade relations between Crete and Greece were ongoing for centuries? Why did the Homeric legends contain so little information of a land that was Greece’s trading partner for all that time? It seems highly probable that the mainland and Aegean peoples had very little and only sporadic contact to explain this.

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<sup>168</sup> Steven Roger Fischer, *Glyphbreaker* (NY 1997), p. 10

<sup>169</sup> Fitton, *op.cit.*, p. 199

<sup>170</sup> *ibid.*, pp. 200-201

The artifacts we will turn to now to add to the tin bronzes for the Minoan and Mycenaean sites discussed are technological. With regard to metallurgy, there is a unique item found at Akrotiri that points directly to the first millennium as outlined by Rodney Castleden:

“Although no metal working site has been proved at Akrotiri, there is evidence silversmiths operated in Building 4: Traces of crystalline lead monoxide have been found there. The isotope composition shows that the lead comes from Laurion on the Greek mainland which later became famous for its silver mines. Theans were already importing from Laurion in the Bronze Age.”<sup>171</sup>

Also in 1970, one of the fragments at Mycenae with a glaze known as Taylour’s plaque was analyzed regarding its lead isotope composition. R.H. Brill of the Corning Museum of Glass in Corning, New York, was shocked to admit:

“The lead in the glaze is definitely of type L. Most type L lead in the ancient world came from the Laurion region [of Greece]. The lead in the glaze differs markedly from leads found in numerous examples of 18th Dynasty yellow glasses, glazes and kohls. Improbable as it may seem, the most straightforward interpretation would be that the object was glazed (and presumably fabricated) near Mycenae, and not in Egypt.”<sup>172</sup>

But the fact is that the mainland Greeks of around 1600 B.C. were not exploiting the Laurion mines. This occurred about 1000 years later. Milton Meltzer shows that it became a mining district when “the discovery of silver at nearby Laurion about 550 B.C. [was exploited].”<sup>173</sup> V.R. D’A. Desborough further explains:

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<sup>171</sup> Rodney Castleden, *Atlantis Destroyed* (London/NY 2001), p. 60

<sup>172</sup> Eric Cline, “An Unpublished Amenhotep III Faience Plaque From Mycenae,” *Journal of the American Oriental Society*, vol. 110 (1990), p. 209

<sup>173</sup> Milton Meltzer, *Slavery: A World History* (NY 1993)

“... there were ... the mines of Laurion in Attica. In the Mycenaean period, however, there is no evidence that any of these deposits were known [or exploited].”<sup>174</sup>

The silver/lead mines of Laurion may well have been known 150 years earlier but were not fully exploited until the ability—the knowledge—of how to separate silver from lead/galena ore had been acquired. The mines were clearly used for lead production before they were exploited for the silver contained in the lead.

In addition, the Minoans had glass artifacts. Sinclair Hood points out that “Beads and other ornaments of solid glass including seals were being made in Crete before ... 1450 B.C.”<sup>175</sup> However, he corrects this, stating they were “Egyptian imports.”<sup>176</sup> Yet at this early date, the advanced kilns necessary to melt glass from its original ingredients did not exist, as discussed.

This evidence indicates that Thera, Crete, and mainland Greece should be placed around 700 B.C., or that the Minoans and Mycenaeans come directly before 700 B.C., removing the Greek Dark Age. That being the case, one would expect the very structures of Akrotiri to resemble towns from this latter period.

It is well known that the town had paved streets, terra-cotta piped plumbing, drainage and sewer systems, two-, three-, and even four-storey buildings, fountains, etc. Regarding piped plumbing, Zebrowski writes: “It [Akrotiri] had running water a thousand years before any other city we know of [in Europe].”<sup>177</sup>

Charles Pellegrino shows:

“Under the direction of Christos Doumas, the excavators at [Akrotiri on] Thera are revealing multistoried exquisitely decorated buildings complete with bathrooms and running water ... The walls and streets are literally honeycombed with pipes.”<sup>178</sup>

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<sup>174</sup> Vincent Robin D’Arba Desborough, *The Greek Dark Age* (NY 1972), p. 314

<sup>175</sup> Sinclair Hood, *The Minoans* (NY 1971), p. 106

<sup>176</sup> *ibid.*

<sup>177</sup> Zebrowski, *op.cit.*, p. 320

<sup>178</sup> Charles Pellegrino, *Unearthing Atlantis* (NY 1991), p. 17

With respect to the drainage or sewer system, Vincent Burke reports at Akrotiri, “There was a central drainage system.”<sup>179</sup> Julie Zeilinge de Boer and Donald Theodore Sanders write that “Like the ruins of Pompeii, the ruins of ancient Akrotiri consist of paved streets and floors and walls of houses, some of them four stories high ...”<sup>180</sup> Dorothy B. Vitaliano reports the same conditions for Knossos on Crete:

“Even the smaller houses were two or three stories high, with wide windows, courts, and often several kitchens. Such refinements as toilets with arrangements for flushing, with drainage and sewer systems were known in some of the palaces.”<sup>181</sup>

The question, of course, is: When do drainage and sewer systems become commonplace in ancient Greece? Although precisely when this happened is difficult to say, Diodorus Siculus in his *Library*, Book II, chapter 25, section 3, describes a Greek city founded in Sicily *ca.* 582 B.C.:

“Most of the [captive] slaves were handed over to the state, and it was these men who quarried the stone of which not only the largest temples of the gods were constructed but also the sewers were built to lead the waters from the city.”

From this passage it seems evident that the construction of urban sewer systems was commonplace by 525 B.C. or somewhat earlier as a basic health necessity for the infrastructure of a Greek city. For an excellent general discussion of Greek drainage and sewer systems, A.N. Angelakis *et al.*'s paper “Urban wastewater and stormwater technologies in ancient Greece”<sup>182</sup> is worth reading. The above citation is found there, and much more.

What is surprising: the mainland Mycenaeans had little of such systems, as Oliver Dickinson shows:

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<sup>179</sup> Vincent Burke, *Discovering the Ancient Sites of Western Europe* (Watson Bay NSW, Australia 2005), p. 104

<sup>180</sup> Julie Zeilinge de Boer, Donald Theodore Sanders, *Volcanoes in Human History. The Far-Reaching Effects of Major Volcanoes* (Princeton NJ 2002), p. 60

<sup>181</sup> Dorothy B. Vitaliano, *Legends of the Earth* (Bloomington IN/London 1973), p. 197

<sup>182</sup> A.N. Angelakis *et al.*, “Urban wastewater and stormwater technologies in ancient Greece,” *Water Research*, 39 (1), (2005), pp. 210-220



“... tunnels cut through the rock [brought] water [to] Mycenae, Tiryns and Athens. These demonstrate a striking degree of practical knowledge about water sources and of engineering skill ... [There were also] drains at Mycenae and Tiryns [which] unlike the Cretan drainage systems, made little or no use of clay piping.”<sup>183</sup>

This evidence also places both Minoan and Mycenaean societies squarely in the first millennium B.C.

Many of the buildings at Akrotiri were constructed with ashlar stone architecture. Ashlar architecture is made with smooth-cut stone masonry as opposed to rough split stones.<sup>184</sup> Ferdinand A. Fouqué states that “Ashlar masonry at corners is familiar in Thera [at Akrotiri] and Minoan architecture.”<sup>185</sup> Castleden, describing Akrotiri, speaks of “another fine ashlar building fronting ... west.”<sup>186</sup> But according to John McK Camp II, ashlar architecture was developed by the Greeks in Lydian, first millennium times. He cites Crawford Greenwalt’s important work at Sardis which indicates that the ashlar form of stone architecture “should be sought in Greek contacts with the Lydians.”<sup>187</sup> According to Ilan Sharon, ashlar architecture in Israel also only “dates to the late Iron Age.”<sup>188</sup> Steven Roger Fischer neatly sums up the advanced nature of Minoan civilization:

“Knossos and other Minoan centers boasted sophisticated indoor ... pipe plumbing and riotous frescoes and red columned loggias and jetting fountains ... The Minoans’ famed ‘hundred cities’ of myth maintained networks of paved [roads].”<sup>189</sup>

Instead of having only one form of evidence—radio carbon for the dating of Akrotiri and the Thera eruption which as we know was selected—we have

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<sup>183</sup> Oliver Dickinson, *The Aegean Bronze Age* (Cambridge UK 1994), p. 163

<sup>184</sup> Nicholas T. Dines and Kyle D. Brown, *Landscape Architect’s Portable Handbook* (NY 2001), p. 275

<sup>185</sup> Ferdinand A. Fouqué, *Santorini and Its Eruption* (Baltimore 1998), p. 468

<sup>186</sup> Castleden, *op.cit.*, p. 99

<sup>187</sup> John McK Camp II, “Walls and the Polis,” in *Polis & Politics: Studies in Ancient Greek History* (Copenhagen, Denmark 2000), p. 44

<sup>188</sup> Ilan Sharon, “Phoenician and Greek Ashlar Construction Techniques at Tel Dor,” *Bulletin of the American School of Oriental Research*, no. 267 (Aug. 1987), p. 21

<sup>189</sup> Fischer, *Glyphbreaker*, *op.cit.*, p. 9

several forms of technical and even scientific evidence: a Laurion isotope of lead, to uphold a first millennium placement of Minoan and Mycenaean civilizations. Suppose that 3000 years in the future a group of archaeologists were to unearth a town completely buried under volcanic ash. In the town square are the complete remains of a geodesic dome, under the streets and in the houses are found electric lines leading to electric appliances. Some of the buildings are ten stories high, made with steel girders and iron-rod reinforced concrete. In the houses are found plastic utensils and other objects. Could any archaeologist honestly place this town in the mid-first millennium AD as opposed to the 20th century AD? I suggest that the same sort of condition pertains to the sophisticated advanced findings at Akrotiri which place the town and its burial by the eruption of Thera also in the first millennium B.C. To paraphrase Thomas Lynch, cited above:

“I believe that the marshalling of an increasing number of technological high probability cases for the dating of the eruption of Thera makes the short chronology’s case even stronger. This is true both intuitively and statistically. These technological facts presented above require Akrotiri be placed in the mid to earlier part of the first millennium B.C. with more certainty than other dating propositions appear. Rather than having one doubtful form of proof we have several excellent forms of evidence all correlating, corroborating one another and clearly converging on Heinsohn and Sweeney’s short chronologies that demonstrate the proposition.”

At this juncture, let us examine the artistic nature of Minoan and to some extent Mycenaean cultures. Although this evidence is not thoroughly technical in nature it does follow from the technical evidence discussed above. The artistic evidence should thus also indicate that these civilizations belong to the first millennium B.C.

Among the items discovered at Akrotiri were spiral designs of a unique nature for that era. These were analyzed by Constantin Papaodysseus and his colleagues of the National Technical University of Athens, Greece. What they discovered was that the spiral designs at Akrotiri were nearly perfect Archimedes’ spirals. While spirals such as those found on snail shells are common in nature, while others can be formed by coiling a rope on a flat surface, Archimedes’ spirals

are different. They are mathematically constructed and appear not to be found in nature. Papaodysseus *et al.* report:

“This paper studies a set of wall-paintings of the Late Bronze Age (c. 1650 BC) ... excavated at Akrotiri, Thera ... It deals with the methods used for the drawing of the geometrical figures appearing in these wall-paintings. It is demonstrated that most of the depicted configurations correspond with accuracy to geometrical prototypes such as linear spirals and canonical polygons. It is pointed out that the steady lines of the figures, their remarkable repeatability, the precision of the geometrical shapes and their even distribution in the wall-paintings indicate a very distinctive use [of mathematics] which is now investigated.”<sup>190</sup>

What Papaodysseus and his colleagues show is that it would be immensely improbable that the ancient artisans at Akrotiri could have produced these spirals without some advanced understanding of mathematics because to do so by hand repeatedly defies credulity. Philip Ball writes:

“‘This is the earliest time that such advanced geometric figures have been spotted,’ says Papaodysseus. ‘The next such figures appear only 1,300 years later’...

“‘Experiments with geometry must lie behind the construction of these paintings.’ Papaodysseus and his co-workers ... admit that they cannot know how much the Theran artists actually understood about the geometric principles they used for the paintings because no written documents from this period are known to exist.

“But he says that, at the very least, ‘experimentation with geometric tools must lie behind the construction of these wall paintings, as well as an impressive feeling for geometry.’

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<sup>190</sup> C. Papaodysseus *et al.*, “Distinct, Late Bronze Age (c. 1650 BC) Wall-Paintings from Akrotiri, Thera, Comprising Advanced Geometrical Patterns,” *Archaeometry*, vol. 48, no. 1 (Feb. 2006), p. 97

“Spiral designs [there] were first noticed years ago by archaeologists working at the [Akrotiri] site. But Papaodysseus says that most people previously assumed that the shapes were painted freehand.

“His studies suggest that the curves are just too accurate for that: the edges deviate from their strict mathematical form by typically less than a third of a millimetre. Papaodysseus thinks that this precision was probably achieved by the use of stencils, which appear to have been broken up into six parts to make them easier to transport and the paintings easier to fit a given space.

“The key question is how the stencil itself was made. ...

“The researchers point out one relatively simple way of constructing such a spiral, without knowing the precise mathematical formula for it. One could divide up a circle using a large number of radial lines with equal angles between them, and a large number of concentric circles. A series of dots moving out one radial line and one concentric circle at a time could be joined together into an Archimedes’ spiral. But dividing a circle into more than a dozen equal sections is not a trivial task; try it yourself.

“Papaodysseus and his colleagues find that the dots decorating the spirals seem to be positioned almost exactly on the radial lines of circles that are divided into 48 sections.

“The wall paintings don’t in themselves prove that the Therans knew enough geometry to bisect angles. But it certainly looks that way, says Papaodysseus.”<sup>191</sup>

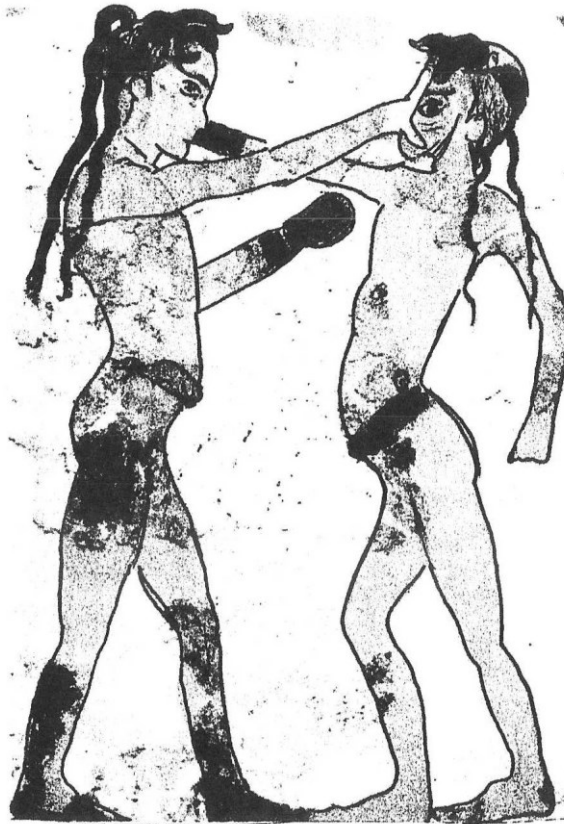
By moving the eruption of Thera well down into the first millennium this great artistic/mathematical anomaly disappears and we do not have a 1300 year spiral Dark Age separating Akrotiri from the period when this knowledge began to be known.

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<sup>191</sup> Philip Ball, “Were ancient Minoans centuries ahead of their time?”, *BioEdOnline* (Feb. 28, 2006), p. 2, Internet ([www.bioedonline.org/news/news.cfm?art=2371](http://www.bioedonline.org/news/news.cfm?art=2371))

In addition there is further artistic evidence of a technical nature that indicates the same compression of time for Akrotiri: On one of the walls there is a painting of two boys engaged in a boxing match. In this case there is another unique piece of evidence that shows that the painting was executed well in the first and not the mid-second millennium B.C. Each of the boys who are having the boxing match has a boxing glove on his right hand. The glove goes well up the arm beyond the wrist and appears to be tied above or around the wrist with a rope. According to R.F. Willetts, this wall painting at Akrotiri is in fact the “first artistic representation of boxing gloves.”<sup>192</sup> (see Fig. 6 below)

Figure 6



Boys boxing with boxing gloves

Boys boxing with boxing gloves

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<sup>192</sup> R.F. Willetts, *The Civilization of Ancient Crete* (London/NY 1976), p. 80

The problem is that boxing gloves were not invented until well into the first millennium B.C. Even when the Olympic Games began, around 776 B.C., boxers did not use boxing gloves. According to Mark Golden, the boxers wore:

“Himas ... ‘thongs,’ hand coverings used by Greek boxers in competition ... The earliest [were] soft thongs ... light strips of leather about three to four metres (ten to twelve feet) in length wrapped around the wrists and/or hands; they did not soften the force of his blows. In the fourth century [B.C.] Greek boxers took to wearing heavier and more damaging coverings, the sharp thongs ... These were gloves ...”<sup>193</sup>

It is worth noting that the Archimedes’ spirals at Akrotiri as well as the use of boxing gloves should both be inventions of around the fourth century B.C. or somewhat earlier, but there is more. Not only are these paintings of the Archimedes’ spirals and boxing gloves a thousand years ahead of their time, but the wall painting of these boxing boys may represent another first. Susan Ferrence explains:

“The late Bronze Age wall painting, the boxing boys ... This article considers a medical interpretation for the spinal-pelvic anomaly in the anatomy of one of the boys. The artist has depicted a combination of structural anatomical adjustments diagnostic of spondylolisthesis, forward slippage of one of the lumbar [hip] vertebrae. The accurate portrayal of the surface appearance of this condition suggests the artist painted directly from a live subject. Thus [the] Boxing Boys mural may be the earliest visual record of a sports-induced injury. Although the meaning of the wall painting is unclear, the [naturalistic depiction of the] wild goats [antelope] on the adjoining walls simulate swayback as a reflection of the boys’ torso deformity and shape features with the boys adding to the unifying characteristics of the room. The [naturalistic] abnormal morphology appears to be the earliest achievement of transforming disease into aesthetic charm on a monumental scale.”<sup>194</sup>

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<sup>193</sup> Mark Golden, *Sport in the Ancient World from A to Z* (London/NY 2004), p. 82; see also Judith Swaddings, *The Ancient Olympic Games*, 2nd ed. (Austin TX 1999), p. 79

<sup>194</sup> Susan Ferrence, “Deformity in the ‘Boxing Boy’,” *Perspectives in Biology and Medicine*, vol. 40, no. 1 (Winter 2005), p. 105

When did the ancient Greeks begin to paint more and more naturalistically? David Konstan shows:

“In ancient Greece ... a development in the interpretation of the emotions may have been accompanied by a like transition in conventional artistic representation ... The watershed comes with the Hellenistic era. After Alexander’s campaigns, ... the painting and plastic arts produced in this period exhibit a tendency towards increased realist or naturalism, abandoning the classical preference for idealized figures in favour of [the] marginal and even grotesque ...”<sup>195</sup>

The deformities of the boxing boy and the swayback animals show the same departure from the idealized to the more realistic or naturalistic art of the Hellenistic period of the 4th century B.C. This is also found to be the case on Crete about 60 miles away from Akrotiri on Thera. Both areas were contemporary and exhibit the same form of naturalistic mural painting. Ellis reports, “Frescoes in Thera have a white background, while those of Knossos are polychrome [multicolored], but the subject matter and style are largely similar.”<sup>196</sup>

Jacquetta Hawkes cites the surprise the early excavators felt when they first laid eyes on these works of art:

“Sir Leonard Woolley has described its art as ‘the most inspired of the ancient world.’ So unenthusiastic a writer as Arnold Hauser has been driven to the use of exclamation marks. Quite against his own convictions he has admitted that the Cretans shared in the ‘artistic social order’ of the Bronze Age Orient — ‘and yet what a difference in the whole concept of art! What freedom in artistic life in the rest of the Ancient–Oriental World!’ Mrs. Groenewegen Frankfort [wife of historian Henri Frankfort], normally a minutely analytical art historian, has written of Minoan Crete: ‘Here and here alone the human bid for timelessness was disregarded in the most complete acceptance of the grace of life the world has ever known’.”<sup>197</sup>

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<sup>195</sup> David Konstan, *The Emotions of the Ancient Greeks* (Toronto 2006), p. 29

<sup>196</sup> Ellis, *op.cit.*, p. 83

<sup>197</sup> Jacquetta Hawkes, *Dawn of the Gods: Minoan and Mycenaean Origins of Greece* (NY 1968), p. 73

Hawkes goes on:

“All civilizations have, of course, been unique; each has grown into its distinctive ‘form’, and its products are unlike all others. Yet as Arnold Hauser insisted, Minoan culture is exceptional in the essential difference of its spirit from that of its contemporaries. The few thousand gifted men and women of Crete who inspired it seem to have had a way of experiencing life and judging its values quite unlike that of any other Bronze Age people—including those from whom they learnt and those whom they taught.”<sup>198</sup>

Hawkes goes on to describe how the earlier empires “liked to show heavy, static figures of kings and governors praying to or confronting the gods ...”<sup>199</sup> and adds, “The Cretans [and the people of Akrotiri] had absolutely no counterpart to the grandiose image of the royal and the divine or of the monumental temples that went with them.”<sup>200</sup> Of the Egyptian arts she reports, “In spite of their more peaceful life beside the Nile exactly the same themes [warfare, hunting, and adoration of the king] were glorified.”<sup>201</sup> She concludes:

“Yet in Crete [and Akrotiri], where hallowed rulers commanded wealth and power and lived in splendid palaces, there was hardly a trace of these manifestations of masculine pride and unthinking cruelty [of war]. There are no great statues or reliefs of those who sat on the thrones of Knossos or of any of the palaces. Indeed, so far as can be seen, there are no royal portrayals of any kind until the latest phase—and then the sole possible exception, the painted relief sometimes identified as the young Prince, shows a long-haired youth, unarmed, naked to the waist, crowned with peacock plumes and walking among flowers and butterflies. Nor are there in Minoan Crete [or Akrotiri] any grandiose scenes of battle or of hunting.”<sup>202</sup>

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<sup>198</sup> *ibid.*

<sup>199</sup> *ibid.*, p. 74

<sup>200</sup> *ibid.*

<sup>201</sup> *ibid.*, p. 75

<sup>202</sup> *ibid.*, pp. 75-76



Dorothy B. Vitaliano, speaking of the mural painting of Knossos, reports “Fresco and vase painting and gem carving ... reached a perfection not again achieved for almost a thousand years.”<sup>203</sup>

The art belongs about a thousand years closer to the present. The lead Laurion isotope at Akrotiri belongs about a thousand years closer to the present. The ashlar architecture belongs about a thousand years closer to the present. The water and drainage systems belong about a thousand years closer to the present. The Archimedes’ spirals and the boxing gloves on the mural belong about a thousand years closer to the present, *etc.* The evidence is conclusive. Altogether these forms of evidence are in monumental contradiction to the 1500-1650 B.C. date for the eruption of Thera.

## **LINEAR A AND LINEAR B SCRIPTS, MYCENAEAN AND MINOAN CHRONOLOGY**

Placing the Minoans and Mycenaeans just prior to the 8th century B.C. allows us to explain the nature of the written scripts that they used. Linear A script as well as Linear B are both hieroglyphic, or pictorial forms of writing. Linear A was found by Sir Arthur Evans during his excavations at Knossos, and has been found at other Aegean sites. The language it represents is taken to be Cretan but translations of it by various researchers have so far failed to be accepted by the historians. Fitton explains:

“The use of Hieroglyphic died out early in the neopalatial period, and Linear A was essentially the script of the Cretan Second Palaces. The language represented remains unknown, in spite of various suggestions from a large specialist literature. A connection with Luvian [Luwian], a branch of Hittite from south-west Anatolia would seem an attractive possibility as this appears to have been the region from which some early settlers came to Crete, but other scholars see similarities with Semitic languages, while some maintain that the language of

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<sup>203</sup> Vitaliano, *loc.cit.*

Linear A is not only Indo-European but also has connections with Greek.”<sup>204</sup>

Linear B has been translated by Michael Ventris. It is a syllabic form of writing and is found on mainland Greece almost exclusively, except for one find by Carl Blegen at Knossos. Like Linear A, it too, supposedly, disappeared with the Greek Dark Age around 1200 B.C. That is, both forms of writing ended about this time and then, when writing did recommence in Greece and on Crete, it was in the form of a Phoenician alphabetic script which the Greeks changed and added vowels to.

According to Saggs:

“Two clay cups bear Linear A inscriptions written in ink made from the secretion of the cuttlefish which was later the source of the pigment sepia ...”<sup>205</sup>

But the use of cuttlefish sepia as an ink only came about in Roman times<sup>206</sup>. The earliest mentions of this ink are found in Cicero: “The ink of the cuttlefish was, as Cicero says, used as ink in his day.”<sup>207</sup> In *Plutarch’s Lives*<sup>208</sup>, there is mention of an “ink fish.” So far as I have read, there are no earlier mentions of cuttlefish ink going back to 1200 B.C. and prior times in the ancient literature. It is extremely improbable nor historically acceptable to expect that the ancient Minoans invented or obtained this ink, which is made from dried sacs of cuttlefish or squid or other cephalopods, around 1400 to 1200 B.C. and that this ink was then forgotten not used for 800 or more years. What appears to be much more probable is that this ink was first used in the first millennium, possibly by the Minoans or Phoenicians, who brought it to the Romans.

In terms of the short chronology, there was no Greek Dark Age for Mycenaean and Minoan civilizations and this writing was continuous across this period, first using Linear A at Crete and in the Aegean and Linear B on the mainland.

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<sup>204</sup> Fitton, *Minoans*, *op.cit.*, p. 161

<sup>205</sup> Saggs, *Civilization Before Greece and Rome*, *op.cit.*, p. 93

<sup>206</sup> *Introduction to Archival Materials Writing Links* (Richmond, Surrey UK 1998), p. 18

<sup>207</sup> William John Thomas *et al.*, *Notes and Queries* (London 1864), p. 322

<sup>208</sup> *Harvard Classics no. 12* (NY 1909–14), p. 16

When the Phoenician alphabet was introduced about 700 B.C., both societies converted to this less complex and therefore more useful script. Alan Wace disagreed with the idea that the Dark Age was one of illiteracy. In the Introduction to Michael Ventris and John Chadwick's *Documents in Mycenaean Greek* (NY 1956), Wace argued:

“From the beginning of Schliemann’s discoveries at Mycenae, the conservatism of classical archaeologists has obstructed progress in the study of Greek civilization as a whole ... This spirit ... has impeded progress in our studies of pre-Classical Greece ... Greek Art is one and indivisible, and has a continuous history from the first arrival of the Greeks.”<sup>209</sup>

Although Fitton disagrees with Wace, she suggests:

“Wace’s picture of an unbroken continuum seems overstated. His claim that writing cannot have been lost has not been substantiated. The fact that deities familiar from the Classical pantheon appear on Linear B tablets, while at first seeming to indicate continuity [from Mycenaean times to the 8th century B.C.] needs assessment.”<sup>210</sup>

When a language does not have a written form it has nothing to stop it from changing and over about 500 years it does so greatly. It is assumed and repeatedly reported that the *Iliad* and *Odyssey* were passed along by oral tradition from generation to generation by trained bards from the Mycenaean Age to the Classical Age and then written down in the new Phoenician/Greek alphabetic script. Hawkes suggests:

“There are a number of ways in which the language of the *Iliad* and *Odyssey* reveals how the poetry went from lip to lip through the generations.”<sup>211</sup>

She further tells us:

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<sup>209</sup> Fitton, *op.cit.*, pp. 201-202

<sup>210</sup> *ibid.*, p. 202

<sup>211</sup> Hawkes, *op.cit.*, p. 256

“It is difficult now to judge the relative importance of memorizing and improvising in the handing on of oral literature ... at any rate in later centuries, there were hack reciters, true bards improvising each performance, hardly knowing as they took up their lyres what words would come. Yet learning by heart was an important part of their preparation ...

“On the lips of the bards the stories were altered and re-combined.”<sup>212</sup>

The fact is that what Hawkes and many of her colleagues suggest about memorization by generations of bards is a figment of scholarly imagination. Adam Parry *et al.* state “that the [Homeric] poems themselves, as we have them, were in fact composed in the process of improvising recitation has not been proven, and probably cannot be proved.”<sup>213</sup> The authors write:

“The problem is that we ‘see’ or ‘hear’ this oral Homer only in the mind’s eye of scholarly hypothesis and imagination.”<sup>214</sup>

We are expected to believe that over 500 or more years the language of the Homeric bards remained quite similar to Mycenaean forms of expression found in Linear B inscriptions, passed on by illiterates! Hawkes speaks of how the language of the *Iliad* and *Odyssey* reflects Linear B:

“Some [forms] are grammatical — there are for instance details of an archaic syntax linking the epic with forms found in Linear B texts ...

“The most telling of all the linguistic arguments concern the Arcadian elements that can be recognized underlying the later Ionian dialect of Homer. The [Linear B] tablets have now confirmed what was already suspected — that the Arcadian [Homeric Greek] came very

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<sup>212</sup> *ibid.*, p. 254

<sup>213</sup> Adam Parry in Frederick Ahl, Hanna M. Roisman, *The Odyssey Re-Formed* (Cornell NY 1996), p. 7

<sup>214</sup> *ibid.*

close to the Mycenaean [Linear B] dialect. The same ancient speech ... survived ... on the tongues of the [Homeric] bards.”<sup>215</sup>

Yet if the people of mainland Greece became illiterate for 500 or more years, the Homeric language of 700-600 B.C. could not “come very close to the Mycenaean [Linear B] dialect”. In terms of military equipment used by the heroes of the *Iliad* and *Odyssey* we run into the same conundrum:

“In the equipment of the heroes, both Achaeans [Greeks] and Trojans, there is a very great deal that belongs only to the Mycenaean Age. Homer has deliberately set them in a Bronze-using world — iron is ... mentioned. Indeed some of their weapons go back to centuries well before the Trojan War, and could not have conceivably been known to Homer by any way except through the tradition of [memorized] poetry. The boars’ tusk helmet ... was obsolete by the twelfth century. The shield of Ajax ... must be one of the semi-cylinder kind carried by the lion hunters ... from the Upper Grave Circle at Mycenae, Hector’s shield [like that of Ajax recalls] forms of body shield [that] were going out of use by 1400 B.C. By the time of the Trojan War they had been displaced by small circular shields ...

“War chariots were another item of equipment which Homer allowed his heroes but which had gone out of use by his own day.”<sup>216</sup>

Moving all of Mycenaean history and chronology forward along with that of the surrounding world does not require Homeric bards to have to reach far back beyond their own times to know of these forms of armor and equipment of war. They knew of these elements because they had seen them with their own eyes. By connecting 1200 to 700 B.C. without a Dark Age, the linguistic close similarities of Mycenaean Linear B language are tied to those of the Greek of Homer and the equipment of war used by the Homeric heroes does not require memorization across this invisible gulf.

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<sup>215</sup> Hawkes, *op.cit.*, p. 256

<sup>216</sup> *ibid.*, p. 255-256

Going from Linear B Greek in this regard follows the natural development of language scripts along certain paths. This is clearly outlined by Bruce G. Trigger:

“Nevertheless it is recognized [by linguists] that given the nature of language it is impossible to represent speech without employing some purely phonetic elements ... Therefore it is believed that scripts evolved from largely logographic [ideograms] beginnings in the direction of increasing reliance on writing sounds, phonography [which] moves through a syllabic phase to an alphabetic one.”<sup>217</sup>

This is largely what we have when we remove the Greek Dark Age. The Greeks employ hieroglyphs to represent syllables and then use an alphabet that is more advanced which they take from the Phoenicians to convert their language into a purely alphabetic/phonetic script. Therefore there would be many elements of Mycenaean Linear B which, when transliterated in alphabetic Greek, contain highly similar elements of grammar as well as elements of syntax. In terms of chronology, both Linear A and Linear B should therefore disappear around the same time.

According to historians, Mycenaean mainland civilization is assumed to have fallen around 1200 B.C. while supposedly the Minoans fell about 1450 B.C. With these falls the Linear A and B scripts are said to have ceased to exist during a Dark Age. It is also claimed that the Mycenaeans at some time took over the Minoan civilization which was in a state of decay.

Here then is the quandary: Minoan civilization, having fallen around 1450 B.C., could not present tablets that contain the Linear B script from the Mycenaeans on the mainland that is clearly dated to 1200 B.C. That would be like finding in the ruins of a city destroyed 250 years ago writing in a script that is only used today. 250 years ago the scripts were very different from those written today. Nevertheless, that is just what was found at Knossos on Crete that supposedly had fallen ca. 1450 B.C. Among its ruins were uncovered Linear B tablets that could only have been written in that form around 1200 B.C. on mainland Greece. That is the great historical/archaeological/chronological problem. Hawkes points out:

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<sup>217</sup> Bruce C. Trigger, “Writing Systems: a case study in cultural evolution,” *The First Writing Script Invention as History and Process*, Stephen D. Houston, ed. (Cambridge UK 2004), p. 41

“But there is one great objection to it. The Linear B tablets from Pylos [on the mainland] are very nearly identical with those from Knossos [on Crete] in writing and similar in content, yet are known to have been inscribed as late as 1200 B.C.”<sup>218</sup>

To explain away this contradiction to the established chronology, Hawkes suggests:

“An entirely new interpretation has therefore been proposed, and has led to one of the hottest academic disputes of the [20th] century. It is being said [proposed] that the Linear B tablets at Knossos were not written before a destruction of [it in] 1400 [B.C.] or a little later, but after a destruction in about 1300; that the Minoan capital [Knossos] had (*as was originally thought*) remained under its own royal house until that date — when it fell before an Achaean [Mycenaean Greek] attack. Then instead of being abandoned *as has been said*, the Kephala [Palace] was occupied by a Mycenaean palace and by Achaean [Mycenaean Greek] rulers who successfully controlled the whole island. Their scribes, naturally enough, wrote in ... Greek — using the Linear B script.”<sup>219</sup> (emphasis added)

Hawkes calls this an

“... economical theory fitting in very well with what is known of the rise and fall of Mycenaean power, and with the Homeric tradition ... *But those who know most about Minoan culture say that there are formidable archaeological difficulties* in so late a date for the last palace of Knossos.”<sup>220</sup> (emphasis added)

In order to salvage the established chronology of Minoan/Mycenaean civilizations, historians are forced to propose assumptions. When Hawkes writes “It is being said that the Linear B tablets at Knossos were not written before a destruction of 1400 [B.C.] or a little later, but after a destruction in about 1300”, we are being given nothing as evidence except “It is being said”. Not even

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<sup>218</sup> Hawkes, *op.cit.*, p. 69

<sup>219</sup> *ibid.*

<sup>220</sup> *ibid.*

archaeological evidence is presented by Hawkes. Where she suggests the Mycenaeans/Achaean took over and occupied the palace “instead of [it] being abandoned as has been said,” again we are being given nothing as evidence except “as has been said.” The archaeological evidence which historians use to resolve such issues, according to “those who know most about Minoan culture say that there are formidable archaeological difficulties in so late a date for the last palace of Knossos,” does not exist. The economical/hypothetical theory that Hawkes proposes is only economical if one who accepts archaeological evidence ignores it. H.E.L. Mellersh explains:

“Another surprising fact emerged. Evans has dated his Linear B finds at Knossos at about 1450 [B.C.] Blegen dated his Linear B finds at Pylos at about 1200 [B.C.]. Could both those dates be right? Was it reasonable to suppose that a method of writing should remain to all intents and purposes entirely unchanged for something like a quarter of a millennium?

“Leonard Palmer ... thought not. The Pylos dating seemed indisputable. Evans must therefore be wrong. The accusations were unfortunately taken to the Press: Evans was accused of old man’s forgetfulness or carelessness at best, or of deliberate falsification in order to back up his preconceived theories at the worst. The date of the Knossos tablets, said Palmer, should obviously be put much nearer those of the Pylos tablets; this would mean another two hundred years or so of Minoan hegemony before the appearance in Crete of the Mycenaeans.”<sup>221</sup>

He adds, “There is no certainty [regarding these dates]. Not one of the solutions offered lacks some sense and probability behind it; the only answer, in the evidence to date, is to choose the most probable.”<sup>222</sup> As we pointed out in volume I of this series, citing Trevor Bryce,

“Over a period of several centuries it will undergo a number of changes in its mode of expression, its grammar, its orthography, and in

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<sup>221</sup> H.E.L. Mellersh, *The Destruction of Knossos: The Rise and Fall of Minoan Crete* (NY 1970), p. 122

<sup>222</sup> *ibid.*, p. 123



the characteristics of the script used to write it. ... We would have little difficulty today in determining whether a piece of English prose was written in Elizabethan, Victorian, or modern times, on the basis of [these elements].”<sup>223</sup>

In spite of this well-understood aspect of script change over even short periods of time, Fitton offers the following explanation for Linear B scripts at Knossos and Pylos being so very alike:

“The Theban [mainland Greece] finds [of Linear B tablets] however were of interest because of their date ... to about 1300 BC ... Since the Theban Linear B was very like that from Pylos this could indicate that the writing system ... had scarcely changed over the course of a hundred years. This might, then, support the notion that the Knossos tablets could easily be even two centuries older [and be almost identical to those of Pylos].”<sup>224</sup>

John Boardman and Leonard R. Palmer went back to examine Sir Arthur Evans’s excavation reports to see what could be done to resolve the issue. Boardman claimed the archaeological evidence placed the destruction of Knossos at about 1400 B.C. Palmer, on the other hand, looking at the very same evidence as Boardman, continued to hold that Evans was wrong and that the date of the destruction of Knossos should be pushed closer to the destruction of Pylos.<sup>225</sup>

As one can see, the very same archaeological evidence can be INTERPRETED to prove completely antithetical chronological time schemes. Fitton admits that “some three decades later [1993], a consensus has still not been reached.”<sup>226</sup> What we wish to show now is that we must not manipulate linguistic and scriptural evidence to explain away contradictions to the established chronology. People supposedly separated by long periods of time who write in the very same or almost identical scripts and the same language belong to the same time, not to different times. In this regard, the Linear B script of Knossos and Pylos being so nearly identical prove these people were contemporary with one another.

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<sup>223</sup> Trevor Bryce, *The Kingdom of the Hittites* (paperback ed., Oxford UK 1999), p. 414

<sup>224</sup> Fitton, *op.cit.*, p. 176

<sup>225</sup> *ibid.*, p. 177

<sup>226</sup> *ibid.*

Carl Blegen, when he saw that the Linear B tablets he had unearthed at Pylos were nearly identical to those found at Knossos, correctly noted “that one can hardly avoid reaching the conclusion that the documents are more or less contemporary ...”<sup>227</sup>

There is also the question of when these scripts actually disappeared. If, as is assumed, Minoan and Mycenaean civilizations ended before the end of the second millennium B.C., then the scripts they wrote in should have died with them as well. But this did not happen. Linear A and B contain many of the same hieroglyphs, though also many others that are dissimilar. It is also believed that the Linear A script came somewhat prior to Linear B. According to Paul Hollander,

“Writing came to Cyprus quite early in prehistoric times. The pre-Greek Cypriots of the Late Bronze Age in the second half of the 2nd millennium BC used the so-called Cypro-Minoan script. This was evidently derived from the Linear A script of Minoan Crete and is presumed [like Linear A] to be syllabic ...

“The Cypro-Minoan script which has not been deciphered consists of about 60 symbols and occurred on clay tablets in Cyprus and at Ras Shamra on the Syrian coast. It has also been encountered on a range of other artifacts from Cyprus. The Cypro-syllabic script [because of its close resemblance to the Linear A script] almost certainly evolved from the Cypro-Minoan script, but has so far been attested in Cyprus BETWEEN THE 8TH AND 3RD CENTURIES B.C.”<sup>228</sup> (capitalization added)

According to Cyrus H. Gordon, the Linear A script not only did not die out at all, but was also used at the same time as an alphabetic script down into Hellenic times:

“The system was so strongly entrenched in East Mediterranean culture that it survived in Crete, and especially in Cyprus until the

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<sup>227</sup> Carl Blegen, “A Chronological Problem,” *Minoica. Festschrift zum 80. Geburtstag von Johannes Sundwall*, E. Grumach ed. (Berlin 1958), pp. 61-62

<sup>228</sup> Paul Hollander, *Lonely Planet Cyprus* (Footscray, Victoria Australia 2003), p. 12

Hellenistic Age, when it was used in addition to the alphabet[ic script of Phoenician/Greek writing].”<sup>229</sup>

Not only did the Cypro-Minoan script use some signs that were identical, but it also expressed the identical sounds. Gordon explains:

“There is a continuity of Minoan writing, starting with the Cretan Hieroglyphs, continuing through the various forms of so-called Linear A and Linear B, and the Cypro-Minoan, down to the Cypriote texts that continue until late in the third century B.C. Both Linear B and the late Cypriote texts are in Greek. Some of the signs are identical (in both form and sound) in both sets of these Greek texts despite the lapse of time [supposedly 1000 years] and difference in locale.”<sup>230</sup>

It is extremely difficult to believe that the form of some of these signs and their sounds stayed identical after about 1000 years of usage. That would be comparable to the people of England using letters today that were used 1000 years ago that had not changed in all that time in the form that they were written or the sound that they expressed. The concept defies linguistic reality.

That is, Linear A did not die out; it continued to be used for language on Cyprus in a slightly different evolved form alongside the alphabet. How did the people of Cyprus get the script if it died out and why were they using it from the 8th through the 3rd centuries B.C.? This indicates that the scripts on Crete and mainland Greece converted from Linear A and B to alphabetic Phoenician/Greek at about 700 B.C. while on Cyprus the people still retained a form of it into the 3rd century B.C.

The language, though undeciphered, is called eteo-Cypriot, from the ancient Greek *eteos*, meaning “true,” and refers to the people of this island as true Cypriots. Furthermore, on Crete itself there were peoples who lived beyond Minoan times, called eteo-Cretans, “true Cretans” who also used the Greek alphabetic script to write a non-Greek language still undeciphered. Stelios Jackson’s book review of J. Lesley Fitton’s *Minoans* reports:

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<sup>229</sup> Cyrus H. Gordon, *Forgotten Scripts; how they were deciphered and their impact on contemporary culture* (NY 1968), p. 116

<sup>230</sup> Cyrus H. Gordon, *The Common Background of Greek and Hebrew Civilizations* (NY 1965), pp. 206-207

“... the indigenous [Minoan] peoples of Crete didn't just disappear ... The *Odyssey* (Book 19) makes mention of the eteo-Cretans (true Cretans) ... [who] are one of five peoples comprising Crete's population. This date [12th century B.C.] of course makes the eteo-Cretans 'Minoans,' as we are still in the Bronze Age. However, Herodotus uses the same term in his 'Histories' (Book 7), and Strabo alludes to eteo-Cretans frequently in his 'Geographies.' The 'eteo-Cretans' lived on, with a language of their own (inscriptions found at Praisos [Crete] in the east of the island are written in Greek script but are not in the Greek language) to at least Hellenistic times (4th C BC) and possibly later.”<sup>231</sup>

What we have is various people using Linear A and Mycenaean using Linear B converting to Greek alphabetic script (except on Cyprus) some time around 700 B.C.

Let us ask the question: Were the Greeks really later comers to the art of writing or were they in step with all the rest of the civilizations in the ancient Near East? G.S. Kirk in pursuit of this question writes:

“The truth is that ancient Greece acquired a fully practicable writing system (which the Linear B syllabic script never was) unusually late in its general cultural development, in comparison with the transition from non-literacy to literacy in other observable societies. Both Mesopotamia and Egypt [were] already quite advanced, when they developed the art of writing ages before, back in the third millennium B.C.; but some of the Achaean [Mycenaean Greek] kingdoms, if they lagged behind in many respects, had already in the second half of the second millennium [B.C.] reached a stage of sophistication in art and administration at least comparable with that of their Near Eastern neighbours. Yet they could do no better in the way of writing than imitate the most cumbersome features of the hieroglyphic and cuneiform systems in order to develop a syllabary

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<sup>231</sup> Stelios Jackson, *The Hellenic Book Service*, Book Review of *Minoans* by J. Lesley Fitton, Internet ([www.west-crete.com/book-review-minoans.htm](http://www.west-crete.com/book-review-minoans.htm))

[which] could never have coped with anything beyond basic documentary [tax and commercial] uses.”<sup>232</sup>

Mellersh echoes this problem:

“When the Minoan-Mycenaean civilization died it is therefore not very surprising to find that this way of writing died too, the Greeks and Cretans presumably remaining illiterate until Phoenician signs were borrowed [after 500 years or so] and improved to form the classical Greek alphabet ...

“What may seem surprising is that such an intelligent people as the Minoans [as well as the Mycenaeans] never evolved a less stiff and anchylosed sort of writing medium.”<sup>233</sup>

Nevertheless, because Heinsohn and Sweeney begin historical times around 1200 B.C., neither the Egyptians nor the peoples of Mesopotamia were very far advanced in their writing scripts over the Greeks. When the Mycenaeans and Minoans borrowed hieroglyphs to write, they did so only a few hundred years after Egypt and Mesopotamia had discovered this mechanism to communicate with each other. Thus, these ancient Aegean and mainland Greeks were not imitating a considerably far older form of writing and using the cumbersome features of the hieroglyphic and cuneiform systems, but borrowed and copied as best they could the developed forms of hieroglyphic script that existed around that time. Rather than being retarded in the development of script writing, they were really close at about the level of their Near Eastern neighbors.

Charles Norton in 1878 wrote of how problematic it was to find

“... by what test we may discriminate between that which is merely plausible fiction and that residuum of true history which can be detected under a mythic guise in ... Greek legends’ ... This was in his optimistic hope that further archaeological excavation will elucidate such matters. He would have been disappointed to know how entirely

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<sup>232</sup> G.S. Kirk, *The Iliad: A Commentary* (Cambridge UK 2001), p. 14

<sup>233</sup> Mellersh, *op.cit.*, pp. 126-127

inappropriate his formulation of the [Mycenaean-Minoan] problem still seems today.”<sup>234</sup>

To overcome these seemingly intractable problems that surround the Minoan/Mycenaean civilizations, Fitton turns to the old standby, “complexity.” She claims our knowledge of the early Greeks and Aegeans

“... is a picture of [the] inexplicable, because unrecoverable complexity.

“Such reflections may lead to nihilistic despair about how much we can ever know. It is perhaps scant comfort to reflect that the very recognition of complexity is an advance ... The historian and archaeologist Franz Maier has appositely drawn attention to the term ‘factoids,’ a term used to refer to hypotheses so often repeated in the literature that they acquire a spurious quasi-factual status.”<sup>235</sup>

That is all we have for evidence that civilization existed well into the second and third millennium, ‘factoids,’ – hypotheses so often repeated that they have acquired as spurious quasi-factual status, while they are, in fact, “plausible fiction” and not “the residuum of true history.” The script evidence for placing the Minoans and Mycenaeans well into the mid-second millennium B.C., like all the rest, has no standing in the face of this and all the rest of the forensic historical evidence noted above. The simple resolution to this distressing convoluted chronology is not complexity that must be explained via *deus ex machina* solutions, but science and technology which so ably support Heinsohn’s and Sweeney’s short chronologies. Mellersh asks and answers the question that Fitton posed:

“What sense or certainty can one extract from this long series of complications [regarding the Minoans and Mycenaeans]?”

[He answers with respect to the established chronology:] “There is no certainty.”<sup>236</sup>

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<sup>234</sup> Fitton, *op.cit.*, p. 203

<sup>235</sup> *ibid.*, p. 205

<sup>236</sup> Mellersh, *op.cit.*, p. 123

I would add that given the established chronology, there will never be even a hint of certainty.

The final piece of evidence relates to the art of the Mycenaeans. The Mycenaeans are assumed to have lived up to 1200 B.C. and then vanished into a Dark Age lasting 500 years. But their art is astonishingly like that of the Scythians who lived only in the first millennium B.C.

Herodotus cites the Scythians on the age of their history as being “the youngest of all nations.” Nevertheless, the burial mounds of the Scythian chieftains of the region of the Black and Caspian Seas contained in them structures very much like those found in Mycenaean civilization.<sup>237</sup> As Tamara Talbot Rice states, these burial mounds incorporated “a dressed stone construction, ascribed by Rostovzeff to Greek influence ...”<sup>238</sup> In M. Rostovzeff’s own words, this burial structure was built of “enormous blocks of dressed stone set to overlap each other so as to meet in the center in an impressive [corbelled] vault.”<sup>239</sup> These corbelled vaulted structures were not only found in the Crimea but also in Scythian burial mounds in Bulgaria and in Asia Minor at Pontus, in Caria and in Lycia.<sup>240</sup> Rostovzeff, however, recognized them as being “Mycenaean type” corbelled vaults.<sup>241</sup>

There was, according to the established chronology, a 500-year Dark Age intervening between the end of Mycenaean civilization and the Scythians. But during this 500-year Dark Age, there exists not a single corbelled vaulted Greek structure in Greece. To get around this chasm, Rostovzeff suggested, “I have no doubt, [that] although we possess no examples in Thrace, and in Greece and in Asia Minor as well, from the Mycenaean period onwards [the Scythians learned to build these].”<sup>242</sup>

But this was not the only aspect of art that the Scythians received from the Mycenaeans. The motifs of their animal art are strikingly reflective of the Mycenaeans. Gregory Borovka states that there is

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<sup>237</sup> E.H. Minns, *Scythians and Greeks* (Cambridge UK 1913), p. 194

<sup>238</sup> Tamara Talbot Rice, *Scythians* (NY 1957), p. 96

<sup>239</sup> M. Rostovzeff, *Iranians and Greeks in Southern Russia* (Oxford UK 1922), p. 78

<sup>240</sup> *ibid.*, p. 77

<sup>241</sup> *ibid.*

<sup>242</sup> *ibid.*, p. 78

“... the striking circumstance that the Scytho-Siberian animal style exhibits [have] an inexplicable but far-reaching affinity with the Minoan-Mycenaean. Nearly all [motifs] recur in Minoan-Mycenaean art.”<sup>243</sup>

So does Solomon Reinach who points to the highly unusual resemblances between Minoan-Mycenaean art and that of the Scythians, such as the organization of the animal's body in a flying gallop.<sup>244</sup> He writes:

“Lion au galop sur une rondelle en bois mycénienne.”<sup>245</sup> “Il a déjà été question d'une rondelle de bois mycénienne découverte en Egypte sur laquelle est figuré un lion bondissant, l'arrière-train soulevé avec une telle violence que les pattes de derrière viennent toucher le front (fig. 58). Nous reproduisons ici cette figure (fig. 144 bis) pour la rapprocher d'une plaque d'or sibérienne représentant un cheval attaqué par un tigre. Cheval et tigre offrent également ce singulier motif des membres postérieurs vers le dos et l'enclosure (fig. 114).”<sup>246</sup>

What is highly unusual is that one of the reproductions of Mycenaean art contains a figure of a “Siberian tiger attacking a horse”. The Mycenaeans could not be in contact with the Scythians between 1400-1200 B.C. because the Scythians did not live in and around the Black and Caspian seas at that time. Therefore they could only have done so in the mid-first millennium B.C.

Another common aspect of these figures is that Scythian art shows animal combats with a lion killing an animal that lies on its back with its leg in the air. These depictions were presented by this author not only for Scythian plaques but also for a lid of a powder box found in the Royal Cemetery at Ur.<sup>247</sup> This type of artistic evidence, a high reflection of the similarity between the art of the Scytho-Siberians and the Minoan-Mycenaean styles some 700 to 800 years earlier, is also discussed by Borovka:

<sup>243</sup> Gregory Borovka, *Scythian Art* (London 1928), p. 96

<sup>244</sup> S. Reinach, “La représentation du galop dans l'art ancien et moderne,” *Revue archéologique*, vol. XXXVIII (1901), fig. 144

<sup>245</sup> *ibid.*, p. 38

<sup>246</sup> *ibid.*

<sup>247</sup> Charles Ginenthal, “Arguments of Straw: Dwardu Cardona and *Pillars of the Past*”, *The Velikovskian*, vol. VII, no. 1, Supplement (2007), pp. 28 and 29



“[On t]he Siberian gold and bronze plaques depicting scenes of fighting animals ... [h]ow often are the animals depicted with the body so twisted that the forequarters are turned downwards, while the hindquarters are turned upwards? Can the agonized writhings of a wounded beast or the fury of his assailant be more simply rendered?”<sup>248</sup>

He goes on to show:

“Other [Scythian motifs] of the animal style, too, reappear in Minoan and Mycenaean art. We may cite the animals with hanging legs and those which are curled almost into a circle. Conversely, the standard motif of the Minoan-Mycenaean lion, often represented in the Aegean with reverted head, reappears again in Scythian and Siberian art.”<sup>249</sup>

Borovka cannot explain these striking similarities that are separated by great spaces of time and also by immense distances:

“How are we to explain this far-reaching kinship in aim between the two artistic schools? It remains on the face of it a riddle. Immediate relations between Minoan-Mycenaean and Scytho-Siberian civilization are unthinkable, the two are too widely separated in space and time. An interval of [at the least] some 500 years separates them. ... Still the kinship between the two provinces of art remains striking and typical of both of them.”<sup>250</sup>

The closeness between Mycenaean and Scythian art discussed by Reinach and analyzed by Borovka is, to me, quite unique. Here is the chronological paradox, namely that Mycenaean and Scythian art should exhibit such affinities while separated from each other by an enormous distance but, more significantly, by about a thousand years in time. There is no explanation for the unusual affinities in these separate artistic traditions, in the established chronology, other than coincidence. In volume I we discussed the gallop of the horse with all four legs off the ground. In this regards, Richard Lydekker reports:

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<sup>248</sup> Borovka, *op.cit.*, pp. 53-54

<sup>249</sup> *ibid.*

<sup>250</sup> *ibid.*

“Sir E. Ray Lankester in one of his articles ... has, for instance, pointed out ‘that what has been drawn by artists and called “the flying gallop,” in which the legs [of the horse] are fully extended and all the feet are off the ground, with the hind-hoofs turned upward, never occurs at all in the galloping horse. ...’

“Later on in the article it is mentioned that M. Solomon Reinach has shown ‘that in Assyrian, Egyptian, Greek, Roman, mediaeval, and modern art up to the end of the eighteenth century’, the flying gallop does not appear at all. ... Reinach has shown that in pre-Homeric art of Greece—which is called ‘Mycenaean’ ... the figures of animals, horses, deer, bull ... dogs, lions and griffins [seen] in the exact conventional pose of the ‘flying gallop’ are quite abundant. M. Reinach shows by actual specimens of art-work that the Mycenaean art tradition, and with it ‘the flying gallop’, passed slowly from Asia Minor into ancient Persia, thence to [Scythian] southern Siberia to the Chinese Empire [and thence] ... back to Europe.”<sup>251</sup>

But the Assyrians were never related to the Mycenaeans so “the flying gallop” could travel to China. It was by a Scythian route that it traveled. Yet the Scythians did not exist in Mycenaean times according to the established chronology, but only around the mid-first millennium B.C. That requires again that Mycenaean civilization was contemporary with the Scythians.

By moving the Minoans and Mycenaeans well into the first millennium B.C., these further “riddles” or, more aptly, impossible situations vanish, as do all the other anomalies.

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<sup>251</sup> Richard Lydekker, *The Horse and its Relatives* (NY 1912), pp. 63-65

## CHAPTER 4: OLD ASSYRIANS AND AKKADIANS

It was shown in volume II, chapter 1 of *Pillars of the Past* that there is no scientific or technical evidence that the civilizations held by historians to have existed in the second millennium could have existed that early. Now it will be shown that both the Old Assyrians and Akkadians could not have existed as assumed where they are conventionally placed. According to Heinsohn and Sweeney, the Old Assyrians and the Akkadians are one and the same people, that is, *alter egos* of each other. Heinsohn bases this on the stratigraphy, namely that the Akkadian stratum lies directly beneath the Median/Mitanni stratum. According to the established chronology, the Akkadians reigned from around the end of the fourth millennium B.C. into the early third, while the Old Assyrians reigned from about 2000 to 1700 B.C.

This dating scheme runs up against the contradiction of irrigation salinization in southern Mesopotamia outlined in volume I, chapter 14, of this series. Given the present climate that, according to the historians, has existed in Mesopotamia for the past 5000 years, it is evident that irrigation agriculture could only be carried on there for about 400 years, and would not permit history to run back to such early times. This is merely the first contradiction to placing the Akkadians/Old Assyrians in these distant times.

There is also the contradiction between this long chronology and the fact that Akkadians/Old Assyrians and others of that period were carving and engraving diorite statues long before steel was created and manufactured in bulk—also detailed in volume I, chapter 10, of this series. In addition there is the contradiction of the geological stratigraphic evidence from Tell Munbaqa that proves there is no stratigraphical break between the Akkadians/Old Assyrians and the Mitanni/Medes. This places the Akkadians/Old Assyrians directly below them and shows they came just prior to the Medes/Mitanni of the first millennium. This being the case, they could never have lived at any other time than in the first millennium B.C. where the Medes/Mitanni are now placed. This stratigraphical evidence is the type of proof that Heinsohn has mainly relied upon to organize this short chronology.

Furthermore, in our discussion of Assyrian and Babylonian linguistics above, we were able to show how little the Akkadian/Old Assyrian and Babylonian dialects of the language they both spoke over an assumed period of 1000 years or more had changed (in fact, it hardly changed at all) over those 1000 or more years. We cited Georges Contenau's *Every Day Life in Babylonia* to the effect that "the two forms of the language are practically identical in grammar and vocabulary, and probably differed most in their method of pronunciation." Over such a long period of time, postulated by the historians, the languages spoken by the Assyrians in northern Mesopotamia and the Babylonians, separated by great distance, for those times, should have evolved into greatly different languages, completely unintelligible to one another as modern English is from Anglo-Saxon.

Having established these contradictions to the established chronology and having shown that these contradictions are clear confirmation of Heinsohn and Sweeney's short history, because they correlate with, and corroborate, one another and converge on the short chronology, one may wonder what else need be added.

Historians and archaeologists believe that somehow, prior to 1100 B.C. (based on the established chronology), tin was employed to make bronze. They suggest that since tin bronze artifacts are dug up in supposedly pre-1100 B.C. strata, the chronology must reach back to 2000 B.C., even without tin mine sources to support that contention. But because documents weigh so heavily in the analyses of and deliberations about these empires—especially that of the Old Assyrians—which stipulate that they were trading "tin" to Anatolia, the historians and archaeologists were ultimately led to conclude that since these documents use the word "tin," that is precisely what is meant. Since they traded tin, they had to have tin, and from it make tin bronzes. Let us therefore examine the evidence pertaining to this question.

## ALL THAT GLITTERS IS NOT GOLD (OR SILVER)

Crow's Law: "Do not believe what you want to believe until you know what you ought to know."

In M.G.L. Baillie,  
*A Slice Through Time*  
(Bath UK 1995), p. 49

Historians and archaeologists believe that not only was tin known well back in the early second and even in the third millennium B.C., because the word "tin," "annaku" or "annakum," was found in Old Assyrian/Akkadian documents, but that these early ancient peoples were able to mine and separate gold and silver from electrum—a natural alloy in which gold and silver are found together. They also believe that these early ancient peoples could separate silver from lead ore (galena ore) back at this early period because the documents of this ancient time likewise have unambiguous words that mean "gold" and "silver."

What will be argued in this unit is that the ancients' knowledge of what was "tin," what was "gold," and what was "silver" was very different from what we understand today. The historians believe that tin, gold, and silver were the same in the past and were viewed in exactly the same way they are in the present. But this, as we will see, is far from being the case. In order to unravel these mistaken concepts, we must first examine the trade in these metals and the processes by which they are refined and/or separated from the materials or ores in which they are found. Much of this relates to the trade carried out by the Old Assyrians/Akkadians.

Surprisingly, what we know of the political conditions of Old Assyria came not from its homeland but from its major trading partner in Anatolia. According to Saggs:

"The dearth of political information about early Assyria gives our gleanings about its trade added importance. In fact, we know more about the trade of Ashur between the time of [a later king dated to around 1800 B.C.] than we do about its politics. Most of this information comes, however, not from Ashur itself but from a source hundreds of miles to the north. The greater part of the evidence in

question consists of clay tablets found at a site named Kültepe (ancient name Kanesh) ... in the region often called Cappadocia [in Turkey].<sup>1</sup>

The very same condition pertains to the Akkadians. Kurinsky reports:

“The empirical Akkadian interest in protecting [their] traders is articulated in a number of legends and is particularly pointed in an epic poem, ‘The King of Battle.’ In this poem [the great Akkadian king] Sargon I, disturbed by reports that the rights of the merchants of a [trading] colony in Cappadocia [Anatolia] are being abused by the local rulers, boasts in florid language of how he took action by heroically leading his army through strange, uncharted mountain passes (the Taurus mountains) and deep into Anatolia to Purushkanda, south of the Great Salt Lake in Central Anatolia, to reinstate and preserve the abrogated rights of this colony of Akkadian merchants, and how the punishment inflicted taught the local rulers to restore and honor those rights ...

“Sargon I championed the cause of the Akkadian merchants precisely because his empire and its affluence depended on a flow of goods from all corners of the known world.”<sup>2</sup>

Thus we have both the Old Assyrians and Akkadians maintaining trading colonies in Anatolia. How this trade began is yet unknown:

“From Ashur [in Assyria] to Kanesh [in Anatolia] ... is no easy journey even today, with all the advantages of motor transport. The distance as the crow flies is about five hundred miles, but any practicable land route involves over seven hundred miles at least. Even today some of the roads may be no more than gravel tracks; the Taurus, a mighty mountain chain, has to be crossed, and in parts security is bad. In 2000 [B.C.] all these adverse factors applied with much greater force. One may accept that once the tradition of trading caravans crossing this area had developed it could continue by its own inertia, facilitated by a

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<sup>1</sup> Saggs, *The Might that Was Assyria*, *op.cit.*, p. 28

<sup>2</sup> Kurinsky, *op.cit.*, p. 40

system of posting stages which grew up around it. However, it still needs to be explained why such a system even began in the first place.”<sup>3</sup>

However that trade began, it seems obvious that the Akkadian/Old Assyrian king Sargon I, when he marched there with his army pacifying the tribes along the route, probably set up relay stations to facilitate that trade.

Karum (Kanesh, modern Kültepe) literally means harbor or quay. It has been surmised that the early trade may also have been via ship to Anatolia and then overland by caravan.<sup>4</sup> Turkish archaeologist Ekrem Akurgal writes:

“At the beginning of the 2nd millennium B.C., Anatolia enjoyed one of the most prosperous periods in its history. The Assyrians who inhabited Northern Mesopotamia, established great trading outposts in Anatolia which they called Karums. Among these, the Karum of Kanesh was the controlling centre to which all the other Karums were subordinate; in its turn, it was directly connected to Assur. Using routes through Assur – Diyarbakır– Malatya – Kayseri, or Assur – Urfa – Adana – the pass of Külek, Assyrian merchants imported tin, garments, and cloth by means of caravans of 200-250 donkeys, and sold their goods to the native people, in exchange for gold and silver. ...”<sup>5</sup>

Richard A. Gabriel points out, citing Sargon I, “I had my men carry mighty bronze pick axes in my equipment and they shattered the side of high mountains as one does in breaking blocks of building stone making a good road [to Anatolia].”<sup>6</sup> I suspect that this route, not a proper road *per se*, may have been the forerunner of the Persian Royal Road. Kurinsky suggests just this:

“The city [of Akkad/Agade] lay along the main overland route which penetrated Anatolia, crossed the formidable Taurus mountains through Aram-Naharaim, and descended down the Euphrates through Akkadia into southwestern Iran, a route which probably had been traveled as early as the seventh millennium B.C.E. and continued in use

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<sup>3</sup> Saggs, *op.cit.*, p. 29

<sup>4</sup> Drakonoff and Diakonoff, *op.cit.*, p. 268

<sup>5</sup> Ekrem Akurgal, *Ancient Civilizations and Ruins of Turkey* (Istanbul 1970), p. 319

<sup>6</sup> Richard A. Gabriel, *Great Captains of Antiquity* (Westport CT 2001), p. 73

past the time of Alexander the Great, when it became the ‘Achaemenid [Persian] Royal Road’ from Sardis [in Anatolia] to Susa [in Iran].”<sup>7</sup>

If this is the case, then the Old Assyrians/Akkadians were the early builders of this artery of trade. It is generally held that such a road and roads in general hardly existed at this early period. However, Morris Silver, to the contrary, reports:

“J. Lewy ... commenting on the [Old] Assyrian trade in Anatolia notes that ‘between some towns ... copper was shipped in wagons, a fact which attests the existence in Anatolia of real highways.’ Indeed, the earliest known three-dimensional evidence for spoked wheels comes from eighteenth century [B.C.] Anatolia ... Similarly a letter of about the same time mentions a caravan of donkeys with their harnesses and wagons carrying juniper wood from northern Syria into southern Babylonia. Much later in the sixth century [B.C.], a text calls for wagons to carry 360 beams (ranging in length from 12 to 18 feet) from Uruk to Dybat in Assyria. The Neo-Assyrian version of the myth of Gilgamesh and the Cedar Forest credits that region with a ‘path kept in good order’ and a ‘smooth main road.’ [Old] Assyria possessed roads of sufficient permanence and importance to be mentioned as boundaries of fields in legal documents. Texts of about the same time (the mid-second millennium [B.C.] from Alalakh refer to tolls collected at bridges that were probably maintained by the government.”<sup>8</sup>

What makes this evidence of trade across Mesopotamia important is that such roads are taken to be a first millennium B.C. feature of trade and not of the late third or early second. The concept that will be discussed in the final unit of this chapter has to do with the development of international trade, and with it the advancement of various forms of technology from around 700 to 300 B.C. which will be shown to be the period of the ancient world’s “Industrial Revolution.” In this respect let us see what was being traded between Assyria and Anatolia which indicates that this trade was of merchandise from the first millennium.

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<sup>7</sup> Kurinsky, *op.cit.*, p. 36

<sup>8</sup> Morris Silver, *Economic Structures of Antiquity* (Westport CT 1995), p. 83



The documents found at Kanesh make it clear what was being traded. It is these materials that will illustrate that the trade was clearly being carried out in the first millennium. The items being traded by Old Assyria/Akkadia to the colonies in Anatolia were “tin,” or so it is claimed, from Assyria, along with “textiles,” generally accepted as coming from Babylonia. From Anatolia the materials being traded to Assyria were “tin bronze,” along with “silver” and “gold.” Each of these items of trade have a first millennium story to tell. The documents of what was being sent from Assyria are unambiguous:

“A conservative estimate (the figure accounts only for shipments listed on recovered records) of shipments over a period of fifty years in the eighteenth century B.C.E. to the Karum of Kanesh in Anatolia totals quantities of 100,000 textiles and 13,000 kilograms [30,000 pounds] of Tin.”<sup>9</sup>

We will get to the nature of just what the Assyrians meant by their usage of the word that historians concluded was “tin.” But as we will see, this was a very different metal. For now, let us concentrate on the nature of the textiles sent from Old Assyrian Assur to Anatolia by donkey caravan as this relates to chronology. The question is: Were the textiles and garments sent to Anatolia products of early second millennium B.C. looms or of looms from a much later time? Could these traded textiles have been woven on such early looms? Saggs outlines the problem:

“The textiles, done up as rolls, were ... sealed in some way, though how is uncertain. They were mainly bales of woolen cloth, although there were also made-up garments; as to [the] size, one text specifies a complete textile as measuring about thirteen and a half feet by twelve. There can hardly have been looms weaving to such a width at this period, and so [it is assumed] the finished textile must have been made by joining narrower strips, as was still the practice until at least the 1950s with the handmade cloth of Kurdistan.”<sup>10</sup>

K.R. Veenhof has examined this question of textile size in Anatolian trade and states:

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<sup>9</sup> Kurinsky, *op.cit.*, p. 39

<sup>10</sup> Saggs, *op.cit.*, p. 31

“I consider the great majority of textile products imported by the [Old] Assyrians in Anatolia not finished garments, but pieces of woven fabric, sheets of textiles which could be used by the buyer for several purposes.”<sup>11</sup>

As for whether the textiles were pieced together, Veenhof shows that the size of the textile sheets discussed in the trade texts

“... should be nine cubits long and eight cubits wide, this means a textile of some 4 ½ by 4 metres [15 by 13 feet] ... A textile, whose name is not preserved, measures 8 by 7 cubits. From Nuzi we have information about textiles measuring 5 by 15 and once 10 by 15 cubits.

“These large sizes pose some problems. The widths mentioned surpass by far the widths of the widest looms attested for the Ancient Near East around 2000 B.C., as reconstructed from (almost exclusively) Egyptian remains and pictures. They seem to have been never wider than ca. 2 metres [6 ⅛ feet]. D. Cross, when dealing with the data from Nuzi, sensed the problem speaking of ‘primitive elongated [oversized] looms.’

”Could one consider these large pieces as composed of smaller strips, woven separately? The piece in Nuzi of twice the normal width ... might be an example [of two strips sewn together]; and the use of the [Akkadian] expression *subātum gamrum* ... could suggest it, though the words mean rather ‘finished textile,’ than ‘complete[d] textile’ [one completed from strips]. I know of no further data [that suggest strips were ever pieced together].”<sup>12</sup>

Veenhof asks: “Should one assume for Mesopotamia much wider looms than are attested for contemporary Egypt ... ?” It is known that much larger looms existed in the late second and first millennia B.C., and the size of the textiles sent to Kanesh from Old Assyria being woven on these later looms makes perfect sense in terms of the short chronology. One begins to see how even the size of

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<sup>11</sup> K.R. Veenhof, *Aspects of Old Assyrian Trade and its Terminology* (Leiden, the Netherlands 1972), p. 89

<sup>12</sup> *ibid.*, p. 92

textiles, supposedly manufactured in the early second millennium B.C., clearly correlates with the other scientific and technical forms of evidence to support Heinsohn and Sweeney's short chronology.

And now, at last, we come to the meaning of words as these relate to making bronze. What is the meaning of the word "tin"? Surely, it must mean "tin" as we understand the word today, or say "*annaku*" or "*annakum*" in the Akkadian language of ancient Mesopotamia. Also, what is the meaning of the words "silver" and "gold"? Is it really obvious they mean pure silver and pure gold? Here we come to one of the crucial aspects of comparing the meanings of these words in the ancient documents with the realities or facts of technological metallurgy. The historians have taken these words to mean exactly what they believe them to mean. But these words do not mean what our present knowledge over the past two centuries suggests. Forensic historical analysis will show them to have meanings that are clearly different.

The deeper question has to do with metallurgy. We were told, above, that tin was also sent by the Old Assyrians to Anatolia, and in return the Anatolians paid them with gold and silver. Philip D. Curtin explains that the Old Assyrians at "Ashur wanted metals, especially the gold and silver of the Anatolian highlands, and had access to tin from [some unknown region] and textiles [from Babylonia]."<sup>13</sup> Trevor Bryce also writes: "Tin and textiles were traded by Assyrian merchants in exchange for Anatolian gold and silver."<sup>14</sup> It is assumed that this trade in gold and silver goes back to the third millennium B.C., as presented by Ainslie T. Embree and Carol Gluck:

"The use for trading purposes of metal objects (coils, rings, blocks) usually made of silver, but also of gold and lead, was an ancient Near Eastern practice attested in Babylonian texts as far back as the third millennium [B.C.]."<sup>15</sup>

Although statements like these are constant refrains in the literature of historians, those who present them have failed to show where the tin that was

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<sup>13</sup> Philip D. Curtin, *Cross-Cultural Trade in World History*, reprint (Cambridge UK 1998), p. 68

<sup>14</sup> Trevor Bryce, *Letters of the Great Kings of the Ancient Near East* (London 2003), p. 12

<sup>15</sup> Ainslie T. Embree and Carol Gluck, *Asia in Western and World History* (NY 1997), p. 37

supposedly traded came from and how the silver and gold from Anatolia were processed. This is the crux of the matter.

K. Aslihan Yener deals with the problem of silver:

“In comparison with what is known of the chronology and style of artifacts made of ancient silver, knowledge of the production, utilization and exchange of silver is very limited indeed. That is regrettable as silver actually has considerable significance in ancient Near Eastern archaeology and in the history of trade and technology...

“The research into silver initially began as an attempt to understand three major problems that have puzzled archaeologists for years: [1.] the source(s) of silver and gold in antiquity, [2.] the development of ancient metallurgy [of gold and silver] and its related technologies, such as smelting, casting, annealing and alloying, [3.] and the emergence of the international trade in metals. ...”<sup>16</sup>

Silver can be found as nuggets but this occurs very rarely. It is known as native silver. Vincent Rich shows “The first metals utilized by man ... are likely to have been those that were found in their native or metallic state—gold, copper and to a lesser degree silver.”<sup>17</sup> In various places on the Earth, huge isolated silver blocks (as it were) have been found. But these are unique and are not considered or known to be a source or sources of ancient silver.

A.R. Burns writes: “Silver is rarely found [in its] native [state], and its isolation [from lead or from gold] called for considerable metallurgical skill, in consequence it was a much later exchange medium than gold.”<sup>18</sup>

Alfred Lucas reports:

“What then was the source of the ancient silver [in Egypt]? It could not have been obtained from the local gold [and the mixture of silver with gold known as] electrum, though these contain considerable

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<sup>16</sup> K. Aslihan Yener, “The Archaeometry of Silver in Anatolia: The Bolkardag Mining District,” *American Journal of Archaeology*, vol. 90, no. 4 (Oct. 1986), p. 469

<sup>17</sup> Vincent Rich, *The International Lead Trade* (Cambridge UK 1994), p. 3

<sup>18</sup> A.R. Burns, *Money and Monetary Policy in Early Times* (NY 1965), p. 12

proportions [of silver], ... the necessary knowledge of how to separate silver from gold was lacking as late as [early] Greek times ...”<sup>19</sup>

The last metallurgical method by which silver could be extracted from an ore is smelting it from galena [lead ore]. Erich Ebeling and Bruno Meissner claim “that virtually all the silver used in antiquity must have been derived from argentiferous galena ... a sulphide lead ore.”<sup>20</sup> But did ancient man, prior to the first millennium B.C., actually have the knowledge to smelt silver from argentiferous galena? According to William Henry Pulsifer the answer is categorically “no”:

“The ancient miners probably smelted only the [lead] ore found near the surface, and their methods of refining were considered crude as early as Strabo’s time, who informs us that they were unable to properly separate the silver from the lead and that those who came after them reworked the refuse with profit.”<sup>21</sup>

The galena mines at Laurion in Greece were their source of silver, but the cupellation process used to extract silver from this ore goes back only to around 500-600 B.C. as Dayton shows:

“The mines of Laurion in Greece are first mentioned in the Athenian records of 500 B.C. ... They produced about 84,000 ounces of silver in 484 B.C. The earlier *Wappenmünzen* coins of Athens have been shown, by the writer’s lead isotope analyses ... not to have come from Laurion ore, while the turtle coins [of silver] of nearby Aegina were of Spanish silver.”<sup>22</sup>

Dayton suggests that silver was derived from slags of metal ore produced in central Europe but can only show that this process was possible, not, in fact, that this is where silver came from in the second millennium B.C.

What then of the source or sources of gold in ancient Mesopotamia? Gold can be found in streams and this probably provided some gold for the ancients,

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<sup>19</sup> Lucas, *op.cit.*, p. 248

<sup>20</sup> Erich Ebeling and Bruno Meissner, “Metalle B”, *Reallexikon der Assyriologie und vorderasiatischen Archäologie*, Band 8 (1997), p. 130

<sup>21</sup> William Henry Pulsifer, *Notes for a History of Lead* (NY 1888), p. 18-19

<sup>22</sup> John E. Dayton, *The Discovery of Glass...*, *op.cit.*, p. 14

but to remove gold from electrum—a mixture of silver and gold—also requires a somewhat similar metallurgical cupellation technique, called cementation, as that employed to remove silver from lead. Georgius Agricola, in his classic *De Re Metallica*, admits:

“Although the earlier classics contain innumerable references to refining gold and silver, there is little that is tangible in them upon which to hinge the metallurgy of parting [separating] the precious metals. It appears to us, however, that some ability to part the metals is implied in the use of touchstones [which when rubbed on the metal will give an indication of its purity], for we fail to see what use a knowledge of the ratio of gold and silver in bullion could have been with the power to separate them. The touchstone was known to the Greeks at least as early as the 5th century B.C.”<sup>23</sup>

This is reinforced by P.R.S. Moorey:

“The use of a touchstone (...black chert) to test the purity of gold (it approximates carat value in modern terms) is attested by classical authors from at least the sixth century B.C.”<sup>24</sup>

With respect to silver, Moorey shows that “It was not until the Neo-Babylonian period that common terms for assaying and refining [silver] regularly appear in texts.”<sup>25</sup>

In essence, we have no evidence that the ancient civilizations of Mesopotamia had knowledge of the technologies necessary to separate gold and silver from one another in electrum, or smelt silver from galena before, say, 700 B.C. Furthermore, no one truly suggests that all the silver in the early history of Mesopotamia and Egypt was silver found in its native state as nuggets. This being the case, the question remains: What were the metals called “silver” and “gold”? In spite of the knowledge that the early ancient people could not manufacture silver or gold, the literature is replete with statements that the process of cementation and

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<sup>23</sup> Georgius Agricola, *De Re Metallica*, transl. by Lou Henry Hoover, Herbert Clark Hoover (NY 1950), p. 458

<sup>24</sup> P.R.S. Moorey, *Ancient Mesopotamian Materials and Industries* (Oxford UK 1994), p. 219

<sup>25</sup> *ibid.*, p. 237

cupellation, which can be used to separate gold from electrum and then from the slag left behind, smelt the silver, or even remove the silver from galena, was known and actually carried out. R.J. Forbes proposed that

“Natural [native] silver was collected from the earliest times but its role in metallurgy remained insignificant until men learned to produce silver from lead ore [galena] ... It would seem that the refining of silver and lead was worked out in Pontus in about the early third millennium B.C. and that this knowledge spread gradually over the ancient Near East until both metals [silver and lead] were in common use in the fifteenth century B.C. By that time producing centers in Armenia ... and possibly Elam supplied the rapidly growing demand for silver ... At the beginning of the first millennium [B.C.] silver and lead were common metals in the Near East except in Egypt where they did not become common until the Persian period.”<sup>26</sup>

In this case one is expected to believe that it took about 1500 to 2000 years for the Egyptians to somehow learn how to derive silver from lead ore. D.T. Potts claims:

“In antiquity it was not silver ore which was principally exploited, but rather argentiferous lead ores from which silver was extracted by a two-step method of smelting and cupellation. While a variety of ancient lands are attested in literary texts as sources of silver ... all of which are to the east and south of Mesopotamia, Sargon of Akkad referred to a locale in Anatolia as the ‘Silver Mountain’.”<sup>27</sup>

It can clearly be said that these authors and others who propose these metallurgical processes to separate gold from silver in electrum or silver from galena do not cite any physical evidence (meaning furnaces and physical remains of production facilities) to support their statements. These are nothing but historical assumptions put forward as facts.

With regard to the location of these assumed silver—really argentiferous galena—mines, Lucas states that in

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<sup>26</sup> R.J. Forbes, “Silver and Lead in Antiquity,” *American Journal of Antiquity*, vol. xlv (1940), p. 361

<sup>27</sup> Potts, *op.cit.*, p. 174

“... Armenia and Anatolia there are many ancient silver mines which unfortunately cannot be dated, the ores being chiefly argentiferous galena associated with sulphide of zinc. In Georgia and Caucasia there are similar ores though whether they were worked anciently or not is uncertain. In Persia, too, lead ore containing silver are [*sic*] widely distributed, but again it is not known whether they were exploited anciently or not.”<sup>28</sup>

What then were the metals that the ancients of Mesopotamia named “gold” and “silver”? Alfred Lucas gives us a clear explanation regarding these matters from Egypt and Mesopotamia:

“In my opinion, there is no doubt that, both in Egypt and western Asia [Mesopotamia] there were alloys of gold and silver, of the nature of electrum, so rich in silver that they were silver-white, and that it was these alloys that constitute the first ancient silver, that is to say ‘white gold,’ which is what the Egyptians called silver. This seems to be proved by the fact that all the early silver from Egypt is in fact such an alloy.

“None of the Egyptian silver is of the purity of that smelted from ore ...

“... and it is not unreasonable to suppose, therefore, that the silver was also a natural product, through the fact that an alloy of gold and silver, containing so large a proportion of the latter as to have a white colour, is still to be found and is not usually recognized. Nowadays, however, such an alloy would generally be classed as a poor-quality gold and its true character might be masked by the manner in which it would be considered and reported. Anciently the case was different: silver was scarce and in consequence was several times the value of gold, and hence it would have been the object of diligent search and even the smallest deposits found would have been highly prized and would have been worked until exhausted. That it still occurs, however, is proved by the results of the assay [by A.C. Claudet] of twenty-six specimens of modern Egyptian gold from quartz [veins]

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<sup>28</sup> Lucas, *op.cit.*, (NY 1999)p. 249



quoted by Alford. When the ratio of silver to gold in these specimens is calculated it is seen that in fifteen instances this is one part or more of silver to one part of gold, the highest ratio being 3.3 parts of silver to one part of gold. All these specimens, therefore, would be silver white, since the silver-gold alloy containing 50 percent or more of silver has a white colour.”<sup>29</sup>

As for gold, it is evident that electrum with a small percentage of silver will be golden in color and thus will be taken for gold. Carol Andrews reports:

“Since most ancient Egyptian gold is impure, containing by nature various proportions of silver (up to as much as 20 percent), it is often uncertain whether the metal in question is to be considered electrum or low-grade gold. ...”<sup>30</sup>

This is explained by the fact that the peoples of the ancient world did not have our modern understanding of elements. There was no knowledge in those early times that gold and silver were different elements on the Periodic Table. They could only evaluate a metal by its color, weight, etc. Therefore, any heavy metal with a silvery color that had certain qualities they accepted as being silver; the same applies to gold. This will be gone into again below in slightly more depth.

In fact, the ancients believed that metals, like other living things such as trees, actually grew in the earth. Forbes tells us:

“Certain passages in classical authors ... suggest a belief (which goes back to pre-classical times) that metals grow in the earth and thus supplement [grow back] deposits which would have been exhausted long ago by men’s depredations. Servius’ commentary on a passage by Vergil says this happened [at a mine] on the Isle of Elba, and Strabo says that ‘the digging [of metals] which are in time filled up again [by regrowth].”<sup>31</sup>

Jean-Pierre Sánchez shows:

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<sup>29</sup> *ibid.*, pp. 248-249

<sup>30</sup> Carol Andrews, *Amulets in Ancient Egypt* (Austin TX 1994), p. 105

<sup>31</sup> R.J. Forbes, *Studies in Ancient Technology* (Leiden, the Netherlands 1966), p. 76

“From very ancient times gold [like other metals] has been regarded as a living being that ‘grows’ in the earth like some kind of plant, and is capable of both of increasing itself [by growth] in perfect fashion and of regenerating itself by ripening in the depths of the earth.  
 ...”<sup>32</sup>

These understandings regarding metals by ancient man were to deeply influence what he would think and write about the various metals he mined and the names that he used to designate one metal from another.

Electrum that was silver in color was therefore taken by the ancients to be silver, and likewise electrum which had a golden color was gold. Yet, as the documents plainly state, the Old Assyrians were supposedly trading tin (which was not mined at that early time). In this case we have yet to explain what the Old Assyrians meant by the word “tin” and how that tin could have been used in the manufacture of gold- and silver-colored metals that were considered by the ancients to be gold and silver.

## ARSENIC AND OLD ASSYRIA

It must first be understood that there was another metal employed by smiths in Mesopotamia that could be used to alloy with copper to produce bronze with the same distinct color and hardness as tin bronze. According to Robert Raymond:

“The first metal to appear in smelted copper in proportions significant enough to indicate [it was added by] intention was arsenic. Early objects from many parts of the ancient world contain up to seven percent arsenic. This is more than is usually found in copper ores and suggests the addition of enriched arsenic ores during the smelting.”<sup>33</sup>

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<sup>32</sup> Jean-Pierre Sánchez, *The Classical Tradition and the Americas* (Berlin Germany/Washington DC 1994) p. 34

<sup>33</sup> Robert Raymond, *Out of the Fiery Furnace: The Impact of Metals on the History of Mankind* (Pennsylvania State 1986), p. 25

One of the most astonishing things about this metal, used extremely early by civilized man according to conventional chronology, is that not only was it used by itself but, most importantly, with regard to our thesis, there was no ancient name for arsenic:

“There is no evidence that arsenic was used as a separate metal in ancient Mesopotamia, and there was no word for arsenic in any text of Bronze Age date.”<sup>34</sup>

Is it truly possible that ancient man used a metal over centuries but had no name for it? This I consider so improbable as to be impossible. Arsenic when added to copper in different proportions can have profound influences on the alloy, giving it very different colors and characteristics, such as hardness or brittleness. A.J. Wilson reports:

“Much of the early bronze produced in the Near and Middle East derived from copper-arsenic ores which were in good local supply. ... The amount of arsenic in the ores varied enormously and there was no way of telling from their appearance just how much they contained.”<sup>35</sup>

This is simply a belief, because different amounts of arsenic alloyed to copper give very distinctive indications of what these amounts did.

This question regarding the use of arsenic as an alloy with copper has, *as far as I have learned*, not been adequately or properly understood. Rather than attempting to understand how it was used, the entire subject has been relegated to the status of a non-question or non-problem. It is this question and problem that will now be engaged to show the profound influence arsenic has had on the ancient world's early metallurgy and how its use as an alloy with copper explains away the problems related to tin as a word, tin trade, and the ways in which gold and silver were created from these alloys that were neither gold nor silver. These alloys gave copper either a silver or a golden color, but early man did not understand this. Our understanding of the history of metallurgy in the ancient world has been confused because of the failure to deal with arsenic/copper alloys. James D. Muhly, in

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<sup>34</sup> Ebeling and Meissner, *op.cit.*, p. 120

<sup>35</sup> A.J. Wilson, *The Living Rock: The Story of Metals Since Earliest Times and Their Impact on Civilization* (Cambridge UK 1996), p. 26

discussing the modern understanding of the metallurgy of the ancient Near East, clearly and directly admits to this confusion:

“Many basic problems remain and in certain areas [i.e. metallurgical development], we have yet to see a major breakthrough of significant change in [the] traditional confusion.”<sup>36</sup>

The reason for this is the problem of determining what metal the word “tin” referred to. Benno Landsberger in a landmark paper regarding the meaning of the words *anaku* and *abāru* as either “tin” or “lead” states “How can we rationalize the fact that the journey toward the meaning of our two vocables, *anaku* and *abāru*, has taken so many years and that even today no single solution is accepted by all Assyriologists.”<sup>37</sup> Landsberger points out:

“Thompson, as late as 1935, in his handbook, *A Dictionary of Assyrian Chemistry*, p. 66, quotes several metallurgical authorities and presents an analysis of bronze, but still declares “there is no proper word for tin; *anāku* (his transcription) is ‘tin, sometimes lead.’ The reasons are: (a) ‘etymology,’ ... the role of *anaku* [taken as tin by modern historians] in Old Assyrian commerce, [is] not correctly understood ... the report of [king] Assur-nasir-pal II (883-859 [B.C.]) in *AKA* pp. 238-343 [which states] ‘one hundred talents of AN.NA.MES’ ... prohibits tin entirely, according to Thompson. ...”<sup>38</sup>

One of the problems attendant to the question of tin, Landsberger explains, citing George F. Bass, is that if tin was a major metal of that era as opposed to, say, lead, there would be a great deal of tin artifacts found among the most ancient strata. The fact of the matter is that exceedingly few tin objects are found. Thus one must explain what happened to these tin artifacts, and Bass has suggested:

“If much pure tin was made in antiquity, it has perished, perhaps for two reasons: First, it has been transformed by direct intercrystalline

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<sup>36</sup> James D. Muhly, “Sources of Tin and the Beginnings of Bronze Age Metallurgy,” *American Journal of Archaeology*, vol. 89, no. 2 (April 1985), p. 275

<sup>37</sup> Benno Landsberger, “Tin and Lead: The Adventure of Two Vocables,” *Journal of Near Eastern Studies*, vol. 24, no. IV (1965), p. 295

<sup>38</sup> *ibid.*, p. 288

[action in]to mixed stannous and stannic [different chemical forms of tin] oxides ... Secondly, it is sometimes transformed by allotropic modification to powdery gray tin.”<sup>39</sup>

Most historians have opted for *annakum* to mean tin, rather than lead. Stephanie Dalley puts the case thus:

“There are several unresolved problems connected with tin and the manufacture of bronze at this period [the Early to Middle Bronze Age]. Although there is no doubt that, in some contexts the word *annûkum* does mean tin, in other contexts it may mean lead. We certainly cannot assume that a single word in Akkadian had a single equivalent in English. *Analysis of pieces of bronze from good strata on [in] excavations seems to show that arsenic-bronze, not tin bronze was still much in use at this period which does not seem to tally with the interpretation of annûkum as tin being imported in large amounts ...* Lead occurs naturally and plentifully in Anatolia, so that it could not have had to be imported into Anatolia by [the Old] Assyrians; it makes nonsense to suppose that lead was the mainstay of the Old Assyrian trading colony at Kanish—it was so common locally that very often figurines were made of solid lead.”<sup>40</sup> (emphasis added)

What has failed to register are the words emphasized in the above citation, namely that arsenic bronzes were exceedingly common during the Old Assyrian period so that the interpretation of *annakum* does not tally with its meaning of tin. If tin was the metal or ore employed to make bronze at this period, tin-bronze artifacts, not arsenic-bronze artifacts would have been found in great numbers. Therefore, arsenic was the metal, or arsenic copper ore, that was being traded by the Old Assyrians to Anatolia. And, in fact, there is one unambiguous statement in the historical literature that requires just this interpretation:

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<sup>39</sup> *ibid.*, pp. 292-293

<sup>40</sup> Stephanie Dalley, *Mari and Karana* (Piscataway NJ 2002), pp. 63-64

“The difficult word ‘anak’ in .... (Akk[adian] anaku) often translated ‘tin’ could refer to an alloy of copper and arsenic, characterized by unusual hardness.”<sup>41</sup>

A one percent amount of arsenic alloyed to copper makes a copper bronze that is much stronger than pure copper. Therefore, *annakum* can also refer to either arsenic or tin ores or perhaps the metals themselves.

Saggs tells us:

“Even the word *annaku*, which we translate as ‘tin,’ offers a problem. Controversy long raged over whether the word meant tin or lead with opinion now settling on the former. Even so, it is by no means assumed that the term denoted pure metallic tin. It may have been some form of tin ore, the most recent suggestion at the time of writing is that *annaku* was a mixed ore containing variable amounts of tin and arsenic.”<sup>42</sup>

In essence, we are being told that historians now directly take the word *annakum* to be tin ore with arsenic in it. But it makes greater historical, linguistic and, above all, geological sense that the ore is one of arsenic with here and there some tin. The first reason for this interpretation is that nearly all early bronzes were alloyed with arsenic, as Heather Lechtman explains:

“During the Middle Bronze Age (ca. 2200-1600 B.C.), arsenic bronze remained the more common of the two bronze alloys throughout ... western Asia. Only in the Late Bronze Age did tin bronze displace the arsenic variety in most of this area.”<sup>43</sup>

Lechtman further shows that the interpretation of *annakum* had two meanings even down into Persian times: “In some contexts and at some periods there

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<sup>41</sup> *Theological Dictionary of the Old Testament*, G. Johannes Botterweck *et al.*, eds. (Grand Rapids MI 1988), p. 374

<sup>42</sup> Saggs, *op.cit.*, p. 30

<sup>43</sup> Heather Lechtman, “Arsenic Bronze: Dirty Copper or Chosen Alloy? A view from the Americas,” *Journal of Field Archaeology*, vol. 23, no. 4 (Winter 1996), p. 477

is the possibility that *annakum* may mean lead as well as tin. The later Persian word *anuk* may mean tin and lead.”<sup>44</sup>

What we have is a major problem for the established chronology. According to that chronology the Old Assyrians (ca. 2000-1700 B.C.) were trading tin to Anatolia and receiving gold, silver, and bronze in return. But if that was the case, then tin bronze artifacts should be the dominant ones in Old Assyria, not ones of arsenic bronze. Lechtman concisely showed us that between 2200-1600 B.C., “arsenic bronze remained the more common of the two bronze alloys throughout ... western Asia.” The Old Assyrians, supposedly dating from 2000-1700 B.C., had mostly arsenic bronze artifacts which makes no sense or logic at all. Trading tin to Anatolia they should have gotten tin bronze in return, not arsenic bronze.

The second reason for accepting that *annakum* meant both arsenic and tin is that mining copper ore alloys *geologically* follows a certain sequence as pointed out by M. Martini:

“Although not followed in detail in all regions, the sequence of copper alloys used in antiquity in the Near East and Europe is [first, the use of] native copper nuggets [followed by] smelted copper [from its ore, followed by] arsenic copper, [followed by] bronze (copper plus tin), [followed by] leaded copper and finally brass (copper plus zinc). To some extent this sequence of alloys reflects the sequence of ores in an idealized copper deposit. Thus, the thin, upper oxidized zone provides the native copper vein or nuggets [from it] and the ore for producing smelted unalloyed copper; the next secondary enrichment zone [below] provides for the arsenic-bearing sulphide ores for producing arsenical copper, and the final [lower] primary deposit of sulphide ores is used to produce the copper which is alloyed with tin to produce bronze.”<sup>45</sup>

The argument used to dispute that tin came from the west is that the documents say it came from the east on donkey caravans. In this regard Potts reports:

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<sup>44</sup> *ibid.*, p. 27

<sup>45</sup> Marco Martini, *Physics Methods in Archaeometry* (Amsterdam, the Netherlands 2004), p. 362

“Throughout Western Asia arsenical copper was one of the most important types of copper exploited in antiquity. Of these arsenic-rich copper sources closest to southern Mesopotamia, Anarak in central western Iran ... where the important [arsenical copper] Talmessi mine is located (Muhly 1973 [p.] 232) is the most likely to have been the source of arsenical copper.”<sup>46</sup>

Assyria lies west of this region, therefore the *annakum* would have been arsenical copper ore coming from the EAST, and therefore the question of the source of this *annakum* is resolved. I also hazard a guess that, if the name of this town or region “Anarak” is its ancient name, then maybe the Akkadian word *annaku* is derived from “Anarak.”

The thesis being presented here is that the Old Assyrians were shipping *annakum* from the zone that contained arsenic ores to Anatolia which was then smelted to produce an arsenic bronze which was harder than copper but most significantly also provided two other arsenic-copper alloys which gave totally different appearances. The arsenic in arsenic-copper, which represents about 5 percent of the alloy, creates a silver color that has been taken by historians to indeed be silver. Ebeling and Meissner explain the metallurgical process that produces this effect:

“Through inverse segregation the arsenic in the copper would tend to move [during the smelting operation] towards the surface of the [copper-arsenic] metal. This phenomenon, also known as exudation or ‘sweating’ often produced a silvery arsenic-rich coating on the object that could have as much as 21% arsenic.”<sup>47</sup>

Aslıhan Yener also shows:

“Polymetallism and polychromatic effects on artifacts became widespread in the 3rd millennium [B.C.] and were achieved by altering alloying materials ... or mixing a variety of metals together. Two of the effects are the shimmering silver quality of high-arsenical coppers

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<sup>46</sup> Potts, *op.cit.*, p. 165

<sup>47</sup> Ebeling and Meissner, *op.cit.*, p. 120



... [and] the establishment of ... silver [as a trading] standard in Mesopotamia. ...”<sup>48</sup>

How good was the appearance of silver arsenic-copper bronzes? Yener goes on to show:

“High arsenical bronze ... also gives the object a silvery color. High arsenical bronze alloys were a deliberate choice especially in the later period, the Early Bronze Age. Evidence of this technique was discovered when a bull figurine from Turkey in the Boston Museum, thought to be silver plated, was found to have a rich surface of arsenic.”<sup>49</sup>

If the experts in one of the great modern museums of antiquities could not tell a silver coating from that of arsenic, we should not expect ancient man to have known this either. Therefore, what the early ancient peoples of Mesopotamia and Egypt took to be silver was either high-silver-content electrum or high-arsenic-copper bronze; *both looked like silver and were clearly taken to be silver*. This high-arsenic-copper metal has a characteristic that gives away its identity. While about 1 percent arsenic in copper makes the alloy hard, when more than 5 percent arsenic is added, the alloy becomes extremely brittle. Frederick Overman reports directly: “*The presence of [more than 5 percent] arsenic [in copper] renders the alloy extremely brittle and unfit for any use.*”<sup>50</sup> (emphasis added) Daniel C. Snell explains the nature of the way silver was “broken” to pieces in early trading:

“The precious metal that eventually came to the fore as money was silver and the etymology of the word for silver in Akkadian *haspum* has led scholars to guess that it was ‘broken’ and then weighed on scales ... silver ... from later periods ... was derived from cut off [pieces].”<sup>51</sup>

It seems to be obvious that the earliest or earlier silver medium of exchange was high-arsenic-copper bronze which was so brittle that it could be

<sup>48</sup> K. Aslihan Yener, *The Domestication of Metals: The Rise of Complex Metal Industries in Anatolia* (Leiden, the Netherlands, *et al.*, 2000), p. 68

<sup>49</sup> *ibid.*, p. 69

<sup>50</sup> Frederick Overman, *A Treatise on Metallurgy: Comprising Mining and General and Particular Metallurgical Operations*, 6th ed. (NY 1865), p. 626

<sup>51</sup> Daniel C. Snell, *Life in the Ancient Near East* (New Haven CT 1997), p. 24

“broken” into pieces to then be weighed on scales while perhaps electrum with a silver color was not brittle and had to be cut before being weighed.

Overman has informed us that high-arsenic-copper bronze is so brittle as to be “unfit for any use,” and in this respect Samuel Noah Kramer, who translated an early so-called Sumerian text, explicitly shows this was the case with ancient silver:

“Silver, only in the palace do you find a station, that’s the place to which you are assigned. If there were no palace, you would have no station; gone would be your dwelling place. ... In the [ordinary] home you are buried away in its darkest spots, its graves, its ‘places of escape.’ When irrigation time comes you don’t supply man with the stubble-loosening copper-mattock; that’s why nobody pays any attention to you! When planting time comes you don’t supply man with the plough-fashioning copper adze; that’s why nobody pays any attention to you! When winter comes you don’t supply man with the fire wood cutting copper ax; that’s why nobody pays any attention to you! When the harvest time comes you don’t supply man with the grain-cutting copper-sickle; that’s why nobody pays any attention to you!”<sup>52</sup>

High-arsenic-copper bronze looking like silver would be so brittle that it was unfit for any of these uses.

Yet silver with only one percent copper would serve all these purposes, as reported by W. Stanley Jevons: “When suitably alloyed, silver is sufficiently hard to stand much wear. ... A coin or other object made of silver may be known by [its] considerable hardness.”<sup>53</sup>

What is being specifically suggested is that the early ancient peoples of Mesopotamia who traded with Anatolia received high-arsenic-copper bronze which they took to be silver and later substituted high-silver-content electrum which also passed for silver and high gold-content electrum which passed for gold.

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<sup>52</sup> Samuel Noah Kramer, quoted in Potts, *op.cit.*, p. 174

<sup>53</sup> W. Stanley Jevons, *Money and the Mechanisms of Trade* (NY 1984), p. 46

The significant point is that the people of that time did not understand the difference between arsenic and tin. Both, I maintain, were termed *annakum*. They did not change that word for arsenic into one meaning tin because both came from similar zones as shown by Martini above and both, used in the appropriate amounts, would harden copper. Thus we are informed:

“The first copper alloy ... arsenic bronze ... that melted at lower temperatures than pure copper and was more fluid and easier to cast ... but before the Middle Bronze [Age] ... in the Near East a tin-copper alloy had largely replaced the earlier arsenic-copper alloy. Tin copper ... was ... absorbed by the Hebrew and Greek word [for arsenic copper bronze]. NO NEW WORD WAS CREATED TO DISTINGUISH THIS NEW COPPER ALLOY [from the arsenic alloy].”<sup>54</sup>  
(capitalization added)

That is, in the earliest periods arsenic-copper bronze and tin-copper bronze had the same name. The people were not yet knowledgeable that these alloys were actually made from different metals. Sharing the same characteristics and looking much alike they could not be distinguished one from the other. This was probably due to the fact that metallurgists at this period could not always make arsenic or tin bronze with the same ideal qualities and sometimes these were inferior to copper.

Not only was there no new name for arsenic as opposed to tin bronzes but the same also applies to zinc-copper bronze or, as we call it, “brass.”

“Brass [is an] alloy of copper and zinc, of historical and enduring importance because of its hardness and workability. The earliest brass dating to Neolithic times was probably made by accidental mixing of zinc ore with copper ores. In addition in documents, such as the Bible, the term brass is often used to denote bronze, the alloy of copper with tin.”<sup>55</sup>

Along these same lines, Mary Frank Gaston explains:

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<sup>54</sup> FARMS Journal of the Book of Mormon Studies, vol. 19. issue 2 (2000); Internet from Brigham Young University Foundation of Ancient Research and Mormon Studies

<sup>55</sup> *Encyclopaedia Britannica Macropaedia*, vol. 2 (Chicago IL 1974), p. 238

“Brass is not as old as bronze, although the actual discovery of brass (who, when, and where) is not very clear. The reason for this is because [tin] bronze and brass often were confused in ancient times as being one and the same. The two alloys have the same basic ingredient: copper. But tin, rather than zinc, is combined to make bronze. In most instances, the color of the two alloys is different. Bronze is usually reddish-brown while brass is gold. But when less zinc is added to [make] brass, the color is reddish and thus similar to bronze in color.”<sup>56</sup>

The people of early antiquity confused arsenic, tin, and zinc bronze as being one and the same. The alloys all have the same basic ingredient: copper, and when alloyed in the proper amounts all these alloys were harder than copper and also exhibited similar color.

W.S. Humphrey *et al.* report:

“Further experimentation with different metals and ores led to the development of alloys and other processes such as casting, but this was done with little understanding of the true chemical nature of ores and metals. Greeks and Romans understood little about the metals except in terms of their external properties, and treatment in our sources is often confused, a problem that is aggravated by the use of the same term to describe several different ores and metals.”<sup>57</sup>

Let us now turn to electrum which later in time passed for either silver or gold based on whether silver predominated in this metal, giving it a silvery color, or gold predominated, giving it a golden color.

Neither silver nor gold could be separated from electrum at these early times nor could silver be extracted from galena. It is also quite probable that 2 percent arsenic-copper bronze was produced by Anatolian smiths which could also pass for gold. Joseph Needham in his monumental work, *Science and Civilization in China*, well outlines this process and concept:

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<sup>56</sup> Mary Frank Gaston, *Copper Identification + Value: Identification and Value Guide* (Paducah KY 1992), p. 8

<sup>57</sup> J.W. Humphrey, John P. Olssen, Andrew N. Sherwood, *Greek and Roman Technology: A Source Book* (London 1998), p. 205

“There is no better way of this transition than by considering copper and bronzes containing arsenic, for the remarkable fact is that 2 % of arsenic can confer upon copper a beautiful golden color, while 4.6 % makes it shine and glow like silver, proportions higher than 8.0 to 9.5 % will not combine homogeneously [with the copper] at all, and above 6 % the metal is a dull white. In view of the relatively small additions of arsenic (as sulphides or oxides) necessary to bring about these effects and presuming that the alchemists [metallurgists] used appropriate means to prevent loss by volatilization, one can see at once, yet another process by which both aurifaction and argentifaction by projection could have been carried on, indeed as far back in Chinese [or Mesopotamian] history ... as the 4th century for example.

“This is one of those rare cases where we need not have undue fear of to [*sic*] precocious [early] datings, for archaeologists in recent years have discovered objects of copper and bronze high in arsenic from many ancient cultures. The Aegean Early Bronze Age and Middle Bronze Age periods for instance (c. -2500 to -1500) have yielded hundreds of tools and weapons containing 8.8 % tin and 9.5 % arsenic [together] ... Random ... selection could never give as much as 8 % arsenic in copper, and it is much more reasonable to visualise the ancient craftsmen adding well-chosen amounts of blackish enargite or yellow orpiment or realgar [ores].

“What they did it for, apart from the scarcity of tin ... appears by the properties of the resulting alloy; for arsenic is a strong deoxidising agent, and by minimising the amount of copper formed in the metal makes it much more ductile and workable, whether cold or hot. Besides being more easily forged the eventual alloy is harder than ordinary copper or bronze yet not brittle. Charles concludes that the ‘arsenical phase’ of Bronze Age metallurgy came to an end whenever tin became available in plenty because of the extreme risk of poisoning by arsenic, especially under primitive working conditions. All this shows that we have to do here with a very ancient technique, common probably to the whole of the Old World, and one which is an inescapable part of the background of later aurifaction [gold production] and argentifaction [silver production] both in the [ancient] East and West. ...

“The Chinese incorporated arsenic in copper and bronze to get artificial gold and silver.”<sup>58</sup>

We must not be surprised if critics of the short chronology raise the argument that tin bronze comes into general use some time during Old Assyrian times and thus the Sumerians/Chaldeans should not have this alloy. But we must separate in time the first Sumerians/Chaldeans from the second Sumerians/Chaldeans and, as we pointed out in volume I of this series, tin bronzes were found in the Royal Cemetery at Ur.<sup>59</sup> That is, it was in late Old Assyrian times that the Scythians came to Ur, bringing tin bronzes.

The important questions are: When did the ancient Mesopotamians come into the knowledge of how to separate gold from silver in electrum? Also, when did they come into the knowledge of how to separate silver from lead ore known as galena? These are crucial questions to this study since this knowledge and the application of it marks a metallurgical milestone in ancient history.

As was pointed out in volume I of *Pillars of the Past* and above, tin bronzes could only have been produced after 1100 B.C. based on the established chronology, but much later based on Heinsohn's and Sweeney's work. That chronological marker meant that where tin bronzes were found plentifully or in reasonable numbers, the people who had them or brought them to the sites must, at the very least, have come after 1100 B.C. What then is the chronological marker for refined gold and silver?

In 1994 an excavation carried out at Sardis in Turkey (Anatolia) finally discovered a gold refining center. The researchers worked over a period of six years before publishing their results in a book titled *King Croesus' Gold*. P.T. Craddock, one of the principal investigators, reports:

“The evidence from remote antiquity for the deliberate surface treatment of gold artefacts which removed both silver and copper, coupled with the comparative ease with which silver could have been totally removed from gold using [in the process] either common salt or

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<sup>58</sup> Joseph Needham, *Science and Civilization in China*, reprint (Cambridge UK 1997), pp. 223-224

<sup>59</sup> Ginenthal, *op.cit.*, p. 387

a salt/corrosive iron sulfate mixture, suggests that gold refining was well within the technical capabilities of the ancients long before the Sardis [gold] refinery. Thus the possibility must be considered [though it is not proven] that true gold refining has a much longer history.

“First, it must be understood that there was no *a priori* reason for the ancients to refine gold or to consider what came out of the ground as impure. Our idea of pure gold as a single and precisely defined element is based on the relatively modern scientific concepts of the nature of elements, and in particular on the Law of Constant Composition, by which each element has precise, invariant and unchangeable properties. To us, this seems no more than stating the obvious, but the ancients did not have such a concept of an ultimate, pure elemental material. Thus metals such as gold, coming from various sources, could have widely different properties, but still be gold. Given the widely held belief that metals ‘grew’ in the ground, it would seem only logical to expect the properties of the metals to depend on their environment. For example, a light-coloured [electrum] natural alloy from a given locality would be regarded as gold of that place, rather than necessarily a natural alloy of rather high silver content. Refining [by a series of firings in a furnace] would be a long and expensive process to [only] improve the colour but reduce the weight of the gold, whereas surface treatment [of the metal] would be much easier and enhance the colour whilst preserving the weight [but not actually refining the gold by removing silver or other impurities].

“The discovery of the application of depletion gilding to the Ur chisels shows that even in the third millennium B.C., the technology existed to remove at least some of the silver from the surface of gold-silver alloys. Clearly, the whole object of the treatment was to make the metal look golden at the surface whilst preserving as much weight [of the gold-like artifact] as possible.”<sup>60</sup>

Because metals were not always of the same quality in terms of weight, color, hardness, etc., in ancient times there developed an entire grading system to

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<sup>60</sup> P.T. Craddock, “Historical Survey of Gold Refining,” *King Croesus’ Gold*, Andrew Ramage, Paul T. Craddock, eds. (Cambridge MA 2000), p. 37

evaluate the worth of a metal used as a medium of exchange in Neo-Assyrian times, a period which we have equated with the Persian era. Forbes reports:

“In late [Neo-]Assyrian and Neo-Babylonian times such terms as *saginnu* ... standardized [as] pure quality [silver] ... *kalu* roasted purified ... *pisu* (white) or *nuchchutu* (inferior) ... are used to describe [different grades of the metal]. Special terms for pure silver (*damqu*) and specially refined silver (*watru*), precious [etc.] are used.”<sup>61</sup>

This, in essence, is an additional form of evidence that the Neo-Assyrians and Neo-Babylonians were Persians or lived after Persian times because before about 650 B.C. the ability to separate gold and silver from electrum was unknown in the Near East. Both the Neo-Assyrians and Neo-Babylonians, based on the established chronology, came prior to 650 B.C. and therefore would not have terms for “pure” silver since the ability to make pure silver did not exist.

Craddock goes on to show that only with the introduction of coinage around 650 B.C. were the cementation of gold and the cupellation of silver employed to separate gold from electrum and refine the silver from the left-over slag:

“Perhaps the most direct evidence for the refining of gold in antiquity is the purity of the surviving metal. In general ancient goldwork contains appreciable silver compatible with unrefined natural gold [electrum], but Nicolini, for example, has claimed that the high purity of some ancient gold artifacts indicated they had been made of refined metal. However, these high-purity pieces are not part of a consistent series but rather isolated examples amongst otherwise impure metal. There is the additional complication that the analyses themselves may not be representative of the true composition of the metal.

“From such analyses of gold antiquities as have been performed, there is no evidence prior to the introduction of coinage for the use of a true cementation process that would have totally removed silver and any other metals from throughout the gold.”<sup>62</sup>

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<sup>61</sup> Forbes, *Studies in Ancient Technology*, *op.cit.*, p. 248

<sup>62</sup> Craddock, *loc.cit.*



Craddock then shows that there is no physical evidence for separating gold and silver prior to *ca.* 650 B.C.:

“The material excavated at Sardis is the earliest surviving physical evidence for the parting of gold and silver. Other more indirect archaeological and literary evidence has been put forward to suggest that gold parting was practiced earlier. ...

“There are many ancient texts from both Mesopotamia and Egypt referring to the refining of impure gold. Unfortunately they are accounting records of quantities rather than technologies, thus usually they state no more than the weight of the metal before refining and the weight loss after refining. Without exception, no details are given of either the metal with which the gold was debased [naturally mixed together with] or of the refining process, beyond that it was often performed by fire in the furnace.”<sup>63</sup>

He adds: “Unequivocal literary references to, or descriptions of, the parting process all post-date the Sardis refinery ... usually where gold coinage was also in production. ...”<sup>64</sup> What then of silver refining or separating gold from electrum? In fact, the cementation and cupellation processes used to refine gold from electrum are very similar to the processes that remove the slag impurities to leave silver behind. With respect to removing silver from galena, salt was used, or a salt/corrosive iron sulfate mixture that causes the impurities to be absorbed in the cupel or caldron which refines silver. In discussing *King Croesus’ Gold*, in a review for the *Bryn Mawr Classical Review*, Cindy L. Nimchuk states:

“*King Croesus’ Gold* begins with a prologue ([by] Ramage and Craddock) introducing the reader to the Sardis excavations of the refinery, with a brief explanation of the processes involved in refining gold and silver, most notably cementation (a parting process that separates the silver from the gold in a gold-silver alloy [electrum]) and

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<sup>63</sup> *ibid.*, p. 32

<sup>64</sup> *ibid.*

cupellation (a process that separates the silver from the smelting remains). ...”<sup>65</sup>

The ability to part and/or refine gold and silver only develops around 650 B.C., and that is the bench mark for dating these materials. Pure gold or silver artifacts when found in large amounts or those so highly refined as to indicate this cementation and cupellation process was employed, prove these metals were produced around 650 B.C. or later.

But as we will see it took time for these processes to spread across the ancient world so that, even though highly refined gold and silver was produced at Sardis in Anatolia, in other regions gold and silver were still passed along as “white gold” (electrum with a silver color) or “yellow gold” (electrum with a golden color).

All this evidence explains why archaeologists have been unable to locate tin mines in early antiquity. There weren’t any.

## COINS AND CHRONOLOGY

Related to the metallurgy of silver and gold is the creation of coinage. The overall medium of exchange (as a metal) in the early ancient world according to nearly all historians was silver. It was not as coins that this metal was employed for trade along with gold for transactions, but it was weighed to allow trade. Robert R. Stieglitz explains:

“The metals used in trade varied but the basic and most common unit of transaction [throughout Mesopotamia—beside barter] was the silver shekel.”<sup>66</sup>

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<sup>65</sup> Cindy L. Nimchuk, review of “Andrew Ramage, Paul Craddock, *King Croesus’ Gold ...*” in *Bryn Mawr Classical Review* 8:10 (2001), p. 1

<sup>66</sup> Robert R. Stieglitz, “Commodity Prices at Ugarit,” *Journal of the American Oriental Society*, vol. 99, no. 1 (Jan-Mar 1979), p. 15

The shekel was, at that time, a unit of weight and not a coin. Throughout Mesopotamia and nearby regions various hoards of *hacksilver*—broken or cut pieces of metal that looked like silver, such as white gold, etc.— have been excavated. Since silver was the fundamental exchange medium in the ancient world for carrying on trade, one would expect that when coinage was at last introduced (generally taken to be invented by the Lydians/Hittites under King Gyges in the 7th century B.C.) it would be silver that was first minted rather than some other metal. However, based on the evidence given above, neither highly refined silver nor gold was available before around 650 B.C. Thus it would be impossible to mint such coins until the cementation and cupellation methodologies to separate gold and silver were known and utilized. The well-known fact is that electrum—not silver and not gold—was the metal from which the very first coins were struck. Geoffrey W. Bromley shows “The first electrum coins were actually irregularly shaped ingots and bore the stamp of King Gyges of Lydia.”<sup>67</sup>

Bernard C. Beaudreau puts the problem this way:

“The fact that the first coins were made of electrum leads to [a] ... problem ... Electrum is a naturally-occurring alloy [of gold and silver which] ranges in color [from white to yellow or silver to gold] and color alone is not a good indicator of alloy [or of what the metal is].”<sup>68</sup>

Elizabeth Kosmetatou in the *Bryn Mawr Classical Review* puts the problem in these terms:

“Scholars are still debating the reasons why the earliest coins were struck only in electrum, and consensus has not been achieved. Theories have been proposed that link electrum to the practice of fraud upon its citizens on the part of the Lydian state, or to a policy of elevating and standardizing its value. Alternatively, its production [for coinage] may reflect the conflicts in which Lydian kings found themselves in the 7th century BCE.

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<sup>67</sup> Geoffrey W. Bromley, *International Standard Bible Encyclopedia K–P* (Grand Rapids MI 1956), p. 404

<sup>68</sup> Bernard C. Beaudreau, *Making Markets and Making Money: Strategy and Monetary Exchange* (Lincoln NE 2004), p. 15

“Robert W. Wallace ... presents a summary of the various theories that have been proposed ... [related to minting] electrum coins ... The subject has received the attention of several scholars recently, and the debate can be traced in numerous new publications.”<sup>69</sup>

The technological evidence that has eluded all the historians is that electrum coins were first coined rather than ones of silver or gold because the ability to refine silver and gold did not come into use before around 650 B.C., after coinage had begun to be used. This, as is well-known, was during the reign of Croesus and it was in his era, or slightly before that time, that the refining plant at Sardis was built and used. Stanley Lane Poole reports:

“The great reform in coinage would seem to have been introduced under the rule of Croesus; the coinage in electrum was entirely abolished [in Lydia], and in its place a double currency in pure gold and in silver was issued. In the introduction of this new coinage regard, however, seems to have been to [minting gold and silver coins to] the weight of the previously current electrum staters [Lydian coins], each of which was thenceforth represented by an equal value [of the electrum coin] though of course not by an equal weight of pure gold.  
... ”<sup>70</sup>

Percy Gardner as far back as 1883 did not understand why Croesus changed electrum currency to bimetallic—silver and gold:

“This brings us to the days of the wealthy and powerful Croesus, who introduced a complete reform of the Lydian coinage. For some reason unknown to us he abolished the issue of electrum and reintroduced a currency of gold and silver, or rather substituted gold and silver coins.”<sup>71</sup>

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<sup>69</sup> Elizabeth Kosmetatou, “*Hacksilber to Coinage...*”, *Bryn Mawr Classical Review*, 03.16 (2004), p. 4

<sup>70</sup> Stanley Lane Poole, “Metrological Notes on Ancient Electrum Coins,” *The Numismatic Chronicle and Journal of the Numismatic Society*, vol. XV, John Evans, ed. (London 1875), p. 257

<sup>71</sup> Percy Gardner, *Types of Greek Coins: An Archaeological Essay* (Cambridge UK 1883), p. 5

The reason is obviously that since metals looking like gold and silver had been the media of exchange, when Croesus could produce highly refined forms of silver and gold, he possessed far better media for trade that always had one and the same value and did not (at first) have to be weighed. Once coinage backed by the government came into circulation, it could be accepted and traded at face value, helping facilitate trade just as it does in the modern world.

We now have a clearer understanding of the history of metals in the ancient world and also know how the word for “tin” via an incorrect reading and understanding of the documents of the Old Assyrian traders in Anatolia led generations of historians to accept that tin was the metal or ore being traded with Anatolia. We also see that the cementation process to separate gold from the left-over slag developed during the ancient world’s industrial revolution which began around 750 B.C.

One point to emphasize is that Craddock has told us “The evidence from remote antiquity for the deliberate surface treatment of gold artefacts ... with the comparative ease with which silver could have been totally removed from gold using either common salt [etc.] suggests that gold [as well as silver] refining was well within the technical capabilities of the ancients long before the Sardis refinery. Thus the possibility must be considered that true gold refining has a much longer history.” Why, if the capacity to refine gold and silver was within reach of mankind around 3000 to 1200 B.C., didn’t the metallurgists of those times actually produce refined gold or silver? The reason is that these centuries filled with Dark Age after Dark Age did not exist. Once metal smiths had begun to explore the various processes at their disposal after *ca.* 1200 B.C., it took them only around 550 years to go from the Copper Age to the Arsenic Bronze Age to the Tin Bronze Age to the Lead Bronze Age to the Iron Age to the Gold and Silver Age.

Hesiod’s metaphorical description of the ages of man from the Golden Age to the Silver Age to the Bronze Age to the Iron Age has the actual development of metallurgy as confused and misunderstood as with the historians. While Hesiod was mistaken because he was dealing with a metaphorical concept of human debasement, the historians are mistaken because they have given documents—written testimony—greater weight than technology. In the balance scales of justice and historical truth, as with the balance scales upon which ancient traders weighed out their metals, the weight of technology must always have greater weight than all the documents to which the historians have turned when reading the word *annaku*.

They failed to relate it to the facts—the reality—that showed they conflated the words “tin” and “arsenic” and conflated the words “silver” and “gold” with “electrum” and also confused them with “white arsenic bronze” and “red arsenic bronze.” The evidence was clearly there before their eyes, but imbued with an overly lengthy chronology they gave the “words” greater weight than the “metallurgical technology” allowed. Having done this, they could not and still cannot extricate themselves from the convoluted history/chronology they have concocted.

When the historians divided the history of man into ages, namely the Stone Age, Copper Age (Chalcolithic Age), Bronze Age, and Iron Age to explain the development of humanity up from savagery, they established the way that historians would look at the chronology of human progress. Historians and especially archaeologists brought this nomenclature and its timing into their excavations and deliberations, blinding themselves to the science and technology that *had* to come before any metallurgical divisions of history could be determined. In that sense they created the metal ages of human development through that indoctrination rather than from the evidence. Tin had to mean tin even though they assumed the early Old Assyrians lived in the Arsenic-Copper Age 2000 to 1700 B.C. In that way they created a fiction.

Like the Greek poet/philosopher Hesiod in his *Works and Days* and *Theogony*, which portrayed the history of man from another subjective viewpoint — showing a graduated degradation of humanity from a Golden Age through one of Silver through Bronze to Iron, where in each age man descended from a god-like being down to a brute (just the opposite of his technological growth), historians failed to regard certain facts. Neither the historians’ nor Hesiod’s views give the correct organization of the history of ancient metallurgy. I suspect that rather than facing the evidence presented above, they will remain rigid as steel in resisting these facts. It may take generations, if ever, for that steel resistance to rust away.

## THE ANCIENT NEAR EAST'S GREAT INDUSTRIAL REVOLUTION *ca.* 750–300 B.C.

What will be argued here is that by placing most of Mesopotamian history in the first millennium B.C., technological, scientific, and all other forms of advancement in the ancient world begin to grow slowly in developed stages, and then it experiences an intense development from around 750 to 300 B.C. throughout the entire region. Trade relations grew out of this efflorescence, metallurgical advances follow one another over a relatively short few hundred years. Barter rapidly gives way to mediums of exchange in precious and other metals which inevitably leads to coinage. That trade is carried by animals that go from donkey to camel, from paths through the wild to roads, from short distances to moderate travel to long distance, international trade.

Language scripts went from cuneiform in Mesopotamia and Linear A and B in Greece and the Aegean to simpler alphabetic forms such as Aramaic and the Phoenician alphabet to facilitate trade, leading to sophistication in astronomy, mathematics, medicine, which all follow the same growth. Architectural development also becomes more sophisticated, advances from cutting soft stones to hard, from moving and raising small stones to huge ones. The ancient world goes from being simple to a more and more integrated growth in rapid stages, so that people begin to feel safer. Though they still hang on to their astronomical gods who had destroyed their world, in that Axial Age of change they then believe that their own behavior will determine how the gods will treat them in the after-life. As Kristian Kristiansen states:

“During the Bronze Age [placed in the first millennium B.C.] there emerged a truly international network of metal trade and exchange, making all regions dependent on each other despite their cultural traditions ... The beginning of the Iron Age [placed in the same millennium] brought about a revolution that had significant long-term implications and changed the nature and potential for political and economic control, just as it favoured the development of new regional traditions.”<sup>72</sup>

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<sup>72</sup> Kristian Kristiansen, *Europe Before History* (Cambridge UK 2000), pp. 1-2

## CAMEL, CLIMATE, AND CHRONOLOGY

Let us begin with the camel and the assumed tin trade from the east. In this respect the use of the camel in trade plays a major role especially, as we will see below, in the incense trade. According to Casson:

“One must be careful, in tracing the use of the camel, to distinguish between the two-humped or Bactrian camel and the one-humped dromedary. The first is woolly and hence fitted for cold temperatures, climbs mountains with ease, and serves exclusively as a pack animal. The second thrives in hot weather, is useless in the mountain, and serves as often for riding and fighting as for transport.  
...

“The two-humped camel originated in Central Asia, was domesticated by at least 2000 B.C., and was in use by that time as far west as Asia. From there it made its way still further into Asia Minor [Anatolia] and Mesopotamia. The [Neo-]Assyrians, always in the forefront when it came to transportation, seem to have been responsible for bringing it to Mesopotamia, perhaps about the beginning of the first millennium B.C.”<sup>73</sup>

The problem is that the documents, as Muhly stated, do not mention the camel bringing tin to Assyria or Anatolia, but say quite explicitly that this metal was carried by “hundreds of donkeys” in caravans. If the two-humped camel was extremely adapted to the cold mountainous regions north of Mesopotamia and Anatolia, and was domesticated by 2000 B.C. then *it* would have carried the tin, not donkey caravans.

It should be remembered that the Neo-Assyrians, as outlined above, are equated with the Persian rulers of Assyria. Therefore, based on this chronology, the two-humped Bactrian camel could not have been introduced into northern Mesopotamia, Iran, or Anatolia until around Persian times. What will be discussed and analyzed below is how the one-humped camel or dromedary indicates that trade across the deserts of Mesopotamia and Egypt was conducted not during the third,

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<sup>73</sup> Lionel Casson, *Travel in the Ancient World* (Baltimore MD 1994), p. 54



second, or early part of the first millennium, but all such trade was carried on around the start of the Persian era. With respect to the dromedary Casson adds:

“The history of the dromedary is more complicated and presents something of a puzzle. It was first tamed [not domesticated] and bred in Arabia. Figurines of dromedaries dating from 3000 B.C., or even before have been found in Egypt, so the animal was known there in very early times—yet Egyptian records reveal no evidence whatsoever of camels as beasts of burden until thousands of years later ... The Babylonians knew it at least as early as the fourteenth century B.C.; but their regular pack-animal remained the donkey all through their history. One solution to the puzzle is to assume that the dromedary found favour as early as 3000 B.C. among desert peoples living on the periphery of the civilized world, who presumably used it not only for transport, riding and fighting, but for its milk, wool, hide and dung [as fuel for cooking fires in the desert] even as today. This would explain how Egyptians, Israelites, Babylonians and all [the] others who were in contact with such peoples were familiar with the animal; not living amid desert conditions, however, they had no compelling reason to adopt it [for trade]. Eventually the dromedary was introduced by Assyria in the ninth century B.C., or perhaps earlier. After a number of campaigns waged against camel-riding peoples, the [Neo-]Assyrian army recognized its value and began to include it in their baggage trains.

“... It brought into being an unprecedented political [and economic trade] arrangement: for the first time in all history, all of the Near East was in the hands of a single well-organized and powerful [Neo-Assyrian/Persian] state. Hitherto transport from Mesopotamia and the Mediterranean had taken a wide arc [by following the Tigris and Euphrates rivers] to avoid crossing the no-mans-land of the Syrian Desert. Now without toll barriers and with a government strong enough to keep desert marauders in hand and rich enough to plant and maintain a series of watering points, a short-cut across the desert was suddenly feasible. The Persians ... use[d] ... the dromedary for transport ... so

it was a natural choice as pack-animal for the new [trade]. The camel caravan was launched on its long career.”<sup>74</sup>

What Casson wishes the reader to accept is that, although being familiar with the camel as a means of desert transportation as early as 2000 B.C. or earlier, the peoples of Mesopotamia and Egypt failed to use it for this very important trade purpose and waited for a thousand or more years to do so in Mesopotamia and for “thousands of year” in Egypt. Caravans with large numbers of merchants and camel drivers have plied the desert and fought off marauders without a powerful state there to protect them.

What Casson has also failed to discuss related to the camel as a means of transport is the question of *when* it could have been utilized to either carry items for transport or even riders. This is directly related to the camel saddle. According to Diana K. Davis and Denys Frappier:

“The camel was probably domesticated between 3000 and 2000 B.C. but it was not until advances were made in the camel saddle that the camel [as a mode of transport or for riding in the desert] became ... dominant.”<sup>75</sup>

But when was the camel saddle invented which permitted this transport? According to Charles Lindholm:

“Between 500 and 100 B.C. the invention of the camel saddle allowed ... nomads to ride their camels. This was a significant innovation since it enabled them [to become] mobile [in] the desert.”<sup>76</sup>

Jerry H. Bentley concurs: “Between about 500 and 200 B.C.E. the camel saddle came into use in Arabia.”<sup>77</sup>

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<sup>74</sup> *ibid.*, pp. 54-56

<sup>75</sup> Diana K. Davis and Denys Frappier, “The Social Context for Working Equines in the Urban Middle East: The Example of Fez Medina,” *The Walled Arab City in Literature, Architecture and History: The Living Medina in the Maghrib*, Susan Slyomovics, ed. (London 2001), p. 54

<sup>76</sup> Charles Lindholm, *The Islamic Middle East: Tradition and Change*, revised ed. (Oxford UK 2002), p. 197

<sup>77</sup> Jerry H. Bentley, *Old World Encounters: Cross-Cultural Contacts and Exchanges in Pre-Modern Times* (Oxford UK/NY, etc. 1993), p. 90

Nicholas Clapp discusses the camel saddle:

“Some time in the thirteenth or twelfth centuries B.C., the animal [camel] was captured and . . . domesticated as a source of milk and flesh. It was considered useless as a beast of burden for its great hump defied pack saddles and upended riders.

“What revolutionized desert trade was a unique technological achievement: a workable camel saddle, a contraption that shifted the weight of heavy cargo from the beast’s hump to its flanks. Where it was invented is unknown, but very quickly traders throughout the Middle East delighted in a creature that could carry five to seven times as much weight as an ass...”<sup>78</sup>

Importantly, the camel saddle was also an indispensable adjunct which allowed the camel to carry baggage. According to Mary Kilbourne Matossian: “With the invention of the north Arabian [camel] saddle between 500 BC and AD 200 the camel could carry between three hundred and five hundred pounds.”<sup>79</sup> She adds that without strain camels could cover twenty miles in six hours. This, of course, is an estimate and probably a fairly accurate one.

Therefore, in order to ride on a dromedary one must have a camel saddle and to load this animal with baggage also requires a camel saddle. But as we have been informed above by various researchers in this particular field of study, the invention of the camel saddle, which allowed for both riding and baggage transport, did not occur until around 600 to 500 B.C. Nevertheless, historians suggest (as we shall see) that well before this time the camel was actually ridden in warfare and was employed as a beast of burden, specifically across the Syrian, Arabian, and Egyptian deserts.

This creates a dilemma for those who advocate the established chronology. Casson has told us that the Neo-Assyrians, who lived about 1000-612 B.C., fought enemies mounted on camels, yet to be able to do so requires a camel saddle, which did not come into being and use for perhaps a hundred years after the

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<sup>78</sup> Nicholas Clapp, *Sheba* (Boston MA/NY 2001), pp. 175-176

<sup>79</sup> Mary Kilbourne Matossian, *Shaping World History: Breakthroughs in Ecology, Technology, Science and Politics* (Armonk NY 1997), p. 77

Neo-Assyrians had disappeared from history. However, if we accept that the Neo-Assyrians were indeed the Persians who ruled Assyria after about 559 B.C., then they could have fought mounted on camels, because the camel saddle existed in Persian times. This is, therefore, a further proof that the Neo-Assyrians were indeed the Persian rulers of Assyria.

There are references to the camel as a pack and riding animal in Persian times, that is, once the camel saddle was invented. Hilde Gauthier-Pilters and Anne Innis Dagg report:

“Camels were important pack animals during early historic times. Herodotus reported that the Persian king Cambyses, whose troops all perished in 525 B.C. in a sandstorm in the Libyan desert, used camels to transport water on his campaigns to Egypt, and that in the battle of Sardes in 546 B.C. soldiers of the Persian king Cyrus mounted their pack camels [which had camel saddles], which terrified and routed the horsemen of his opponent Croesus. ... Xerxes too used camels around 480 B.C. both for transport and for cavalry in protecting his borders.”<sup>80</sup>

Some may argue that this date flies in the face of historical credibility. But this is only so if one is imbued with the view that the established chronology is correct. Yet even historians have widely disagreed about the time of camel domestication:

“Opinions on when camels were first domesticated differ widely, which is to be expected where evidence must be gleaned from ancient texts of uncertain dating, on artifacts that may or may not portray camels with harnesses or saddles, and on circumstantial evidence relating to camel technology and saddle types. Authors such as Free ..., Zeuner ..., Rapinsky ..., and Bulliet ... believe that camels were domesticated before 2000 B.C. Epstein ..., taking into account the earliest Egyptian and Mesopotamian archaeological evidence, dates domestication as early as the fourth millennium B.C. Walz ...,

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<sup>80</sup> Hilde Gauthier-Pilters and Anne Innis Dagg, *The Camel: Its Evolution, Behavior and Relationship to Man* (Chicago 1983), pp. 115-116

however, believes that camels were domesticated perhaps during the thirteenth or twelfth century B.C. ...”<sup>81</sup>

Geoffrey W. Bromley points to a date for camel domestication even closer to the present:

“Some scholars have suggested that only with the 1st millennium B.C. was the camel fully domesticated, and that it was not until the 8th and 7th cents. B.C., that it became a common beast of burden.”<sup>82</sup>

R.J. Forbes informs us:

“According to Erman cross-desert trading by camel to Syria did not start until the fourth century B.C., and we have no further data to judge its development.”<sup>83</sup>

The question of the camel saddle, invented around Persian times, is rarely if ever discussed as it relates to camel domestication. J. Zarins in an article in *Antiquity* vol. LII for March 1978 suggests the criteria that must first be met before domestication of the camel can be known:

1. There should be morphological changes in the animal to show that it was bred to meet certain human requirements. This is common with most domesticated species which are quite different than their ancestors in the wild.

2. Metric analysis to show these differences.

3. Observations which show that the animal was protected by man and that most adults lived to a reasonably old age before being slaughtered for meat. Wild populations tend to lose the very young and old to predators.

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<sup>81</sup> *ibid.*

<sup>82</sup> Bromley, *op.cit.*, p. 547

<sup>83</sup> Forbes, *op.cit.*, p. 547

4. The species should occur far outside its normal habitat to show it was protected and bred by humans and thus could live in areas not associated with it in the wild.

5. There should be unmistakable cultural evidence associated with it such as saddles, bits, etc.

6. Written documents of it.

According to Zarins, with respect to the camel, only saddles, bits, etc. and written documents have been found, and these are only within the first millennium B.C.<sup>84</sup>

The strongest evidence points to a first to mid-first millennium use of the camel for hauling cargo across the desert or for riding. Therefore, any mention of camel transportation prior to that period suggests that these civilizations existed well within the first millennium B.C. This, of course, must be examined in conjunction with all the other forms of scientific and technological evidence and not in isolation as some critics may decide to do.

Another way to determine when the camel was fully domesticated and bred for transportation is by its price. Once people understood the value of the camel, they would breed it as quickly as possible to build up herds to sell for commercial use. At the beginning the cost would be much higher but the price of the camel would fall as it became more and more abundant through breeding practices. As we maintain that the Neo-Assyrians are the Persians in Assyria, this would occur in Neo-Assyrian/Persian times. According to Georges Contenau:

“The rapid growth in trade in [Neo-]Assyria was powerfully assisted by the fact that camels, which before the [Neo-] Assyrian[/Persian] period had been worth roughly one or two thirds minas of silver [20 to 40 shekels], became so common ... that eventually they cost only half a shekel.”<sup>85</sup>

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<sup>84</sup> J. Zarins, “The Camel in Ancient Arabia: A Further Note,” *Antiquity* (March 1978), pp. 44-46

<sup>85</sup> Contenau, *op.cit.*, p. 64

Each and every form of evidence points to the first millennium for the domestication of the camel, and the camel saddle used for riding and transporting cargo on camels points to the Persian era. But there is more.

## CLIMATE

There is the well-known adage that the camel is an animal that was designed by a committee. But this humorous explanation why the camel has such an odd figure, moves with an extremely awkward gait, and stands up or sits down with awkward movements, fails to take into account the unique design of the camel which superbly fits it for life in the desert. No other large mammal is so well-suited to survive in the great deserts of the Earth as the lowly camel.

As is well known, the camel has the “almost unique ability to survive for days without drinking water even in the hottest deserts.”<sup>86</sup> “In winter the camel can go for 50 days without water, in summer only 5 days.”<sup>87</sup>

As Guy Murchie explains:

“Healthy camels can go without drinking water for months, particularly in winter [when sufficient rainfall allows weeds to grow], and still refuse good water when offered (as they sometimes do) ... For many animals from goats to desert rats can get along [eating these weeds] without drinking. ...”<sup>88</sup>

The mechanisms for preserving water are as follows:

“The camel has a number of cavities in the [nasal] rostrum that moisten inspired air. A pair of blind sacs that open into the anterior part

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<sup>86</sup> Gauthier-Pilters and Dagg, *op.cit.*, p. 70

<sup>87</sup> Kurt Schmidt-Nielsen, “The Psychology of the Camel,” *Scientific American*, vol. 201, no. 6 (Dec. 1959), pp. 140-151

<sup>88</sup> Guy Murchie, *The Seven Mysteries of Life: An Exploration of Science and Philosophy* (NY 1978), p. 33

of the nasal cavity are lined with mucous membranes [which] can cool the air from the lungs so that, although the expired air is fully saturated, because of its lower temperature some water is conserved.”<sup>89</sup>

Camels can avoid excreting lots of water. “Their ... urine becomes thick and they can lose  $\frac{1}{4}$  of body weight [mostly as water] before they die.”<sup>90</sup> Furthermore, “The camel, which is exposed to dry air and sometimes blowing sand, has [long eyelashes to block sand from entering the eye and] a constant flow of lachrymal secretions that keep the conjunctive and corneal epithelium [of its eyes] from drying out.”<sup>91</sup> The authors go on to say:

“The camel has adapted in a number of ways to help it survive in the desert: (1) it eats a wide range of plants; (2) it samples plants over a wide area instead of over-grazing and thus destroying desert vegetation in a small region; (3) it can utilize the thorns, dry vegetation, and saltbush that other mammals avoid; (4) it can go for long periods without water, which frees it to graze on pasturage far from wells [or water holes]; and (5) it can drink large quantities of water in a short time, and thus spend minimal time at usually overgrazed well areas. ...

“Camels ... conserve water by producing little urine and dry feces; ... eyes are adapted to excessive light and protected against sand; nostrils that can close to keep out sand and ... have cavities where inspired air is moistened and exhaled air can be cooled, reducing water loss; [there is] localized storage of energy as fat in the hump; [there is a] diurnal rise in body temperature in hot weather to conserve water; fur that provides insulation to some extent during hot ambient temperatures, sweat glands that provide evaporative cooling when necessary; behavior that minimizes exposure to heat; the ability to endure extreme dehydration without serious effect; a low metabolic rate, which reduces the need for water; and the ability to recycle urea when food protein is limited [all supporting the camel’s ability to live in deserts].”<sup>92</sup>

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<sup>89</sup> Gauthier-Pilters and Dagg, *op.cit.*, p. 69

<sup>90</sup> Arthur G. Leopold, *The Camel* (London/NY 1894), p. 98

<sup>91</sup> Gauthier-Pilters and Dagg, *op.cit.*, p. 68

<sup>92</sup> *ibid.*, p. 59



In terms of man's ability to utilize the camel in the desert for survival, the first important feature was that female camels produce milk:

“The primary object of this initial domestication [of camels] is fairly certain. It was milk. The pattern of camel use surviving today in the [Arabian] Hadhramaut reinforces this conclusion ... the [Arabs] use ... camel milk as a staple of life and to maximize usable milk production ... they kill at birth all but a few male animals [which they eat].”<sup>93</sup>

The camel also provided meat from old or lame adults in addition to the meat of the new born males. This meat could be cooked as water was heated over fires fueled by camel dung which is very dry; their wool could be spun into fiber and woven to make cloth; the hide provided leather.<sup>94</sup>

The camel was an ideal domesticated means of survival in the desert. Thus, if the climate for the past 5000 years was the same as it is presently, the camel should have been domesticated and used by the early ancient civilizations. But that is not the case. Instead, “it was the donkeys that for thousands of years carried on their backs all kinds of burdens and costly merchandise along the great trade routes of the ancient world until [the time of] the tame camel.”<sup>95</sup>

Leonard Woolley explains:

“In Egypt and ... Mesopotamia ... the transport of goods overland relied entirely upon the pack ass. ... In Mesopotamia asses were indispensable even for inter-city trade [such as] going up-stream and for journeys abroad they were always in demand. ...”<sup>96</sup>

The camel was superior as a beast of burden to even the ox, as P.R.S. Moorey shows:

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<sup>93</sup> Richard W. Bulliet, *The Camel and the Wheel* (Cambridge MA/London 1997), p. 49

<sup>94</sup> Gauthier-Pilters and Dagg, *op.cit.*, p. 131

<sup>95</sup> Keller, *op.cit.*, pp. 172-173

<sup>96</sup> Hawkes and Woolley, *op.cit.*, p. 616

“The one humped camel has a number of marked advantages ... A laden camel moves at about 3 m.p.h. ... [and] has great advantages over ox-drawn carts. It can carry twice as much for twenty to twenty-five miles at a stretch over [desert] terrain impossible for wagons and has no trouble in fording rivers. It will make more journeys in a year and in a respective lifetime work ... four times longer on average...”<sup>97</sup>

Why, then, wasn't the camel pressed into this extremely important work if much of Mesopotamia and Syria was desert? The camel was superior in many ways to the donkey for hauling cargo. As Keller explains:

“The new form of transport called, not unjustly, ‘ships of the desert,’ consisted of camels. They were able to compass distances that were hitherto reckoned impossible ... [This] unsuspected development both in trade and transport through those vast desolate territories took place around 1000 B.C. [supposedly without the camel saddle]. ...

“Previously it was by the employment of donkeys, plodding endlessly ... each short day's journey governed by the distance from water hole to water hole along the ancient Incense Road through 1250 miles [2000 km] of desert. ... The camel could carry many times the burden than [*sic*] an ass [donkey] could carry.”<sup>98</sup>

Gauthier-Pilters and Dagg dispute even this use of the donkey. In discussing the Incense Route, out of southern Arabia along the Red Sea, they state:

“Because most of Arabia is cut off from mainland Asia by the great expanse of desert called the Empty Quarter, the indigenous culture of southern and central Arabia based on domestic camels was confined to that area for centuries. North Arabia had little knowledge of the camel, while southern Arabia had no direct contact with pack asses. ... The impetus for the northward spread of the camel was undoubtedly the overland incense trade in the second millennium B.C.”<sup>99</sup>

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<sup>97</sup> Moorey, *op.cit.*, p. 13

<sup>98</sup> Keller, *op.cit.*, pp. 211-212

<sup>99</sup> Gauthier-Pilters and Dagg, *op.cit.*, p. 116

Again we come to the question that everyone seems to have failed to address. If the camel was indeed the carrier of incense to Egypt and Palestine, then these peoples would have known of it at least from around 1500 B.C., the assumed period when incense was brought by camels to Egypt, Palestine and perhaps from there to Mesopotamia. But the people of Egypt and Palestine have no evidence that suggests that camels were well-known at this time and had been instrumental in bringing the much-desired incense to their lands.

In fact, it is fully assumed that the development of the camel as a means of transport is related to the development of the incense trade:

“The use of the spice route through Arabia presupposes the domestication of the camel ... This is placed c. 1000 BC ... The domestication of the camel and the rise of the incense trade are usually considered to go hand in hand.”<sup>100</sup>

Ivan van Sertima echoes this, pointing out that “with the domestication of the camel the southern Arabians could effectively exploit the region’s greatest natural resources—frankincense and myrrh.”<sup>101</sup>

To put the entire case bluntly, John Jason Thompson asks:

“How did the Egyptians get incense but not know of the camel? How did the Hyksos introduce camels [to Egypt around 1600 B.C.] but the Egyptians not know of them?”<sup>102</sup>

In Palestine before around 1000 B.C., there is an “absence of camelline remains in Holy Land archaeological sites ...”<sup>103</sup>

Nevertheless, Forbes reports:

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<sup>100</sup> Hildegard Temporini in Wolfgang Haase and Hildegard Temporini, eds.: *Aufstieg und Niedergang der Römischen Welt. Geschichte und Kultur Roms im Spiegel der neueren Forschung*, part II, vol. 16, Berlin, Germany/NY 1978) p. 925

<sup>101</sup> Ivan van Sertima, *The Golden Age of the Moor*, 2nd ed. (Piscataway NJ 2004), p. 67

<sup>102</sup> John Jason Thompson, *Sir Gardner Wilkinson and His Circle* (Austin TX 1992), p. 57

<sup>103</sup> Bulliet, *op.cit.*, p. 36

“Round the beginning of this century [A.D. 1900] a controversy raged when some tried to explain the absence of the camel on ancient Egyptian monuments. Many believed it was due to the ugliness of the camel ... or to a religious taboo ... because the camel was the animal of [the god] Seth ... It was soon realized that these speculations were false as new documents and figurines of camels cropped up [dating to the first millennium B.C.] ... The Egyptian reliefs never show the Arabs riding camels and fighting on dromedaries, but the [Neo-] Assyrian monuments do.”<sup>104</sup>

As we will see, this has not stopped scholars from raising various explanations to escape the problem. Gauthier-Pilters and Dagg say:

“Domesticated camels probably entered Egypt during the early incense trade, but the first historical reference to camels in Egypt is the seventh and sixth centuries B.C. in connection with the [Neo-] Assyrian and Persian invasions of Egypt ... [These invaders came] not via the heavily cultivated Nile delta, where camels die of insect-borne diseases [but across the desert].”<sup>105</sup>

We are probably all aware of the accounts that the Egyptian queen Hatshepsut, dated to about 1500 B.C., was well-acquainted with incense from Arabia and sent an expedition to bring it back. Raymond Mauny tries to explain away this problem thus:

“In the Nile Valley ... the camel makes no appearance at all in the large corpus of Egyptian paintings; it is represented only by a few rare objects [such as a vase] which is probably predynastic, i.e. earlier than about 3400 BC, ... at Byblos [in Palestine], ... camel hair cords from Dynasties III and IV [are found] ... and in some rock-drawings whose dating is uncertain. In view of the very early [camel] caravan links between the Nile Valley [and Arabia], it would be very surprising if the camel had not reached Egypt long before the first millennium BC; doubtless there were religious reasons for this lack of representation of the animal earlier than this. ...

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<sup>104</sup> Forbes, *Studies in Ancient Technology, op.cit.*, p. 201

<sup>105</sup> Gauthier-Pilters and Dagg, *op.cit.*, p. 118

“It is from the time of the Saïte Dynasty that these animals began to be depicted on Egyptian monuments.”<sup>106</sup>

But this was disproved, as Forbes told us. First the Egyptians have a religious taboo about representing camels for over 1000 years, and then they change their religion so camels can be depicted? To explain why the camel is rarely represented before 1000 B.C., in spite of the fact of an ongoing, thriving incense trade from Arabia, Edward Hyams writes:

“Camels are uncommonly stupid and therefore very difficult and troublesome to train; they are also bad tempered, the male being notoriously aggressive and often dangerous. They have a very unpleasant smell, men may be prepared to tolerate this ... But animals are not so tolerant; unless trained to it, horses and even domestic animals are deeply disturbed and may even be panicked by the stench of camels. ...

“While it is true that camels can thrive on poor [desert] pasturage, they need an enormous range of it in order to do so; which was exactly what the ancient intensive agriculture civilization [of Mesopotamia] did not have. ...

“In short, from the point of view of the peoples of early Mesopotamian civilizations there was everything to be said against, nothing for the camel; and it was not until progressive desiccation had radically altered the environment, that camels could come into their own. ...”<sup>107</sup>

But according to the historians, Mesopotamian civilization grew in the very same climate that exists today. There were, according to these researchers, deserts around Mesopotamia from the beginning. This is made explicit by Karen Rhea Nemet-Nejal:

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<sup>106</sup> Raymond Mauny, “Trans-Saharan contacts and the Iron Age in West Africa,” *The Cambridge History of Africa*, J.D. Fage ed. (Cambridge UK 2002), pp. 287-288. More about Old Kingdom Chronology will be presented in vol. III of this series.

<sup>107</sup> Edward Hyams, *Animals in the Service of Man* (Philadelphia PA/NY 1972), p. 125

“The climate and vegetation of Mesopotamia have probably not changed over the past ten thousand years. The desert zones had mild, dry winters, and hot, dry summers, but there was no vegetation [in these deserts].”<sup>108</sup>

Marc van de Mieroop adds:

“More formidable as a border was the great [Syrian] desert stretching between Mesopotamia and the Levant [the coast of the Mediterranean Sea]. For millennia people from Mesopotamia only made their way along the Tigris or Euphrates river valleys and across the northern Syrian [grassy] steppe to reach the Mediterranean. With the domestication of the camel around the year 1000 [B.C.] direct passage became possible although infrequent.”<sup>109</sup>

R. Raikes concurs:

“My conclusions reached after critically reviewing the available evidence, are that there has been no ecologically significant secular climatic change [in the Near East] since about 7000 B.C.”<sup>110</sup>

Jacquetta Hawkes tries to explain this problem away on psychological and sociological grounds:

“[People of Mesopotamia] might pass a little encampment of desert people and their camels ... these men would draw their headcloths closer and look away; they were Bedouin, a people apart.

“The question of camels and their part in the trade of the Bronze Age is curiously obscure. There is no doubt that the pack-ass carried virtually the whole of the goods that went overland ... Yet there is no doubt either that already in the third millennium [B.C.] the people of all three river valleys [Nile, Tigris, Euphrates] knew of the dromedary ...

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<sup>108</sup> Nemet-Nejal, *op.cit.*, p. 12

<sup>109</sup> Marc van de Mieroop, *A History of the Ancient Near East ca. 3000-323 B.C.* (Victoria, Australia 2004), pp. 9-10

<sup>110</sup> R. Raikes, *Water, Weather and Prehistory* (London 1967), p. 74

Thereafter written references and portrayals are extremely rare until about the eleventh century [B.C.], when the Assyrian army is found using camel transport. In Egypt the situation is almost exactly the same: the camel was about but people saw it only out of the corner of their eyes, as it were. ...

“It seems that from the third millennium [B.C.] nomads were already crossing the Arabian and Syrian desert tracks, perhaps already building up a carrying trade, including the myrrh and spices that were to make the fortunes of the camel breeders of ... Arabia by the fourteenth century B.C. The city people and the Bedouin despised and mistrusted one another; the camel trains never entered the cities ... It was probably only with the coming of the Aramaeans that dromedaries began to be a familiar sight to the peoples of Mesopotamia.”<sup>111</sup>

The reasons given by this noted historian are patently absurd. If the camel was a rare animal, people would surely want to see this exotic animal as people have always done, and would have noted it and written of it and depicted it. The people, I suspect, who have been looking away and seeing this animal only out of the corner of their eyes, are the historians and archaeologists at their last wits who with straight faces present such infantile logic to get around this contradiction to their chronology.

Let us recall that Heinsohn and Sweeney equate the Old Babylonians of the second millennium B.C. with the Persians of the first. Since the camel was supposedly only employed for transport in the first millennium B.C., then the Old Babylonians had to be living in that millennium.

Of course there would have been great impetus to use the camel for transportation from the very beginning. But Hyams does say that only when the lands of Mesopotamia were hit by “progressive desiccation” which “had radically altered the environment could camels come into their own.” This is precisely the point I wish to make regarding climate; namely, it was only after 800 B.C., with the last pole shift, that Mesopotamia was hit by progressive desiccation that so radically altered the environment that the domestication of the camel and its usage for transporting goods allowed it to come into its own.

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<sup>111</sup> Jacquetta Hawkes: *The First Great Civilizations* (London 1973), pp. 139-140

This, then, makes perfect sense for the short chronology: the evidence of irrigation agriculture clearly indicates that Mesopotamia was facing progressive desiccation. It agrees perfectly with the evidence that the Neo-Assyrians were the Persian rulers of Assyria around 550 B.C., when we have great numbers of camels depicted and discussed and when the camel saddle is available so people could ride these animals and use them to carry goods. It also makes perfect sense in terms of climate evidence from Africa, Arabia, and elsewhere as these climatic changes coincide with the history of camel domestication.

The Sahara desert is at present an ideal habitat for camels but from the period prior to Egyptian civilization well into the first millennium B.C. camels are not often if ever depicted there. Why? Bulliet explains it thus:

“[The camel is] almost completely defenseless against predators with its soft feet and ungainly run, trusting for survival primarily upon its ability to live in desolate areas and frequent only rarely at the water holes where lions or other enemies could attack it. As the pressure of predation became greater, the value of adaptation to desert life steadily increased ... The earliest periods of Saharan rock art ... do not depict the camel which [in this region] presumably had become extinct because of predation coupled with an as yet incomplete adaptation to full desert life.”<sup>112</sup>

In prehistoric times, the Sahara was not a desert. Lions and other large predators tend to live in groups for protection and act together in order to hunt. They often hunt around water holes where their prey must come to drink, usually once a day. Therefore, during the period of the Holocene Hipsithermal, from about 6000 to 2000 B.C., when the Sahara was a savannah teeming with elephants, antelopes, giraffes, cattle, etc., the lion could well survive and hunt across the entire region, driving whatever camels may have existed there rapidly to extinction. But for some reason this did not happen on the Arabian peninsula. That is, in the north central region of Arabia the climate stayed roughly the same, while the southern area had moderate rainfall. The westerly winds coming to Arabia from north Africa and not from an ocean would have brought very little water from the African savannah at that time, while monsoon rains would have tended to be carried westward as well, inundating the Harappan civilization and reaching north from the Persian Gulf into

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<sup>112</sup> Bulliet, *op.cit.*, p. 30



Mesopotamia. Thus a small number of camels, historians assume, found a refuge in one of these arid areas away from predatory lions until the entire Arabian Peninsula dried out.

In a review of the 1996 book, *Third Millennium B.C. Climate Change and Old World Collapse*, Mike Baillie states:

“Whether looking at evidence from the Indus collapse, from drought in Bohemia, from pollen sequences in Turkey or the Near East or Italy or from varves in Germany, just about everyone thinks they can see evidence for [climate] change somewhere in the third millennium B.C.E.”<sup>113</sup>

This dating is, of course, based on the established chronology which is clearly in need of a drastic overhaul. This climate change may be related to the climate change of the 8th century B.C. posited here. What is surprising and quite astonishing to this Velikovskian writer/researcher is the fact that Harvey Weiss of Yale University has suggested that the climate change was related to a celestial event, though not necessarily to a pole shift. Baillie continues:

“I cannot resist drawing attention to Harvey Weiss’s concluding remarks. Given their evidence for what appears to be a ‘blast from the sky’ at Tell Leilan, Weiss is put into the uncomfortable position of being probably the first archaeologist to suggest an impact from cometary debris in recent millennia. I [Baillie] have absolutely no problem with that concept but Weiss does; he is torn between claiming a [cometary] ‘hit’ and being cautious. ...

“ ‘The abrupt climate change ... regardless of an improbable impact explanation situates social collapse [based on climate change] in a global but ultimately cosmic context’.”<sup>114</sup>

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<sup>113</sup> Mike Baillie, book review of H.N. Dalfes, G. Kukla, and H. Weiss (eds.): *Third Millennium BC Climate Change and Old World Collapse* (Berlin etc., 1996), *Journal of Archaeological Sciences*, vol. 25, no. 2 (1998), p. 185

<sup>114</sup> *ibid.*, pp. 185-186

John Dayton, on the other hand, concluded that the climate change which desiccated Arabia was only a temporary event. This is based on the fact that there are certain settlements in the southern part of Arabia where archaeologists have found the remains of dams across dried-out rivers associated with these settlements. This water was led to the crops by irrigation canals which can apparently still be made out. The silt that built up behind the dams is taken to have been used as a fertilizer for the crops. But then this entire system collapsed. Dayton argues:

“In this writer’s opinion progressive crop failure and lack of rain led to this abandonment, ... the great migration of Arabian tribes out of Arabia to the north was due to the major marginal effect of less rainfall ... One cannot blame everything [for the social collapse] on the failure of the Marib [dam] when so many other dams in the area, farther north and the important site of Qurayya were also abandoned at about the same time, i.e. in the last quarter of the 6th century B.C. Nor can one any longer deny the fact that quite serious minor changes in climate ... occur.”<sup>115</sup>

It is my view that this climate change was permanent, which explains why the camel stayed out of this region of Arabia and only migrated south when the area became a desert. This climate change happened at the same time across the entire Near East, creating the very conditions which made the camel indispensable for desert transport.

Evidence for the climate change of Arabia is well outlined by Michael Rice as it relates to the *ca.* 1500 to 800 B.C. pole shift described by Velikovsky:

“... one of the least anticipated results of recent archaeological surveys in Saudi Arabia, the discovery of the widespread and large-scale domestication of cattle. This is apparent in the western region of the peninsula and in the north. Large herds of bovines could only be supported by climatic conditions much more hospitable than is the case today. The cattle herds were evidently numerous and the people who herded them created an elaborate form of art which celebrated the animals, on what seems to be every available rock surface in northern

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<sup>115</sup> John Dayton, “The Problem of Climate Change in the Arabian Peninsula,” *Proceedings of the Seminar for Arabian Studies*, vol. 5 (1975), p. 58

and parts of western Arabia representations of cattle are pecked and engraved on rock surfaces.”<sup>116</sup>

Undoubtedly for Arabia to maintain numerous herds of cattle means that rainfall there had to be far, far greater and this will not occur unless there was a pole shift that permitted rain regularly and persistently there. This was discussed in this author’s *The Extinction of the Mammoth* as these climatic conditions pertain to the desert regions on pages 202 through 231, and for the north polar regions on pages 120 through 152. In volume III of *Pillars of the Past* we will go into this evidence at length for the Sahara desert

Dayton’s comments regarding how physical, climatic conditions determine history are worth repeating:

“In recent times it has been the fashion, and the word is used deliberately, for archaeologists and historians to deny that any natural causes had affected climate, and thus influence mankind and its affairs since the end of the last major ice age ... In other words that there has been no significant change in climate over the past 9000 years. This view, argued by Raikes ... has eagerly been seized upon by archaeologists to simplify the picture. Man can be blamed exclusively, with the odd very local disaster, as the cause of his own misery, the creator of deserts and dust bowls. Such a state of affairs also conveniently fits the present ‘static theory’ of archaeology. At one fell swoop the Sumerian Problem, the Hurrians, the Hyksos, and even the Dorians could be dismissed as non-existent.

“Unfortunately archaeology has always been subject to ‘fashionable’ as distinct from scientific views., and because of the curious English education system where at the age of twelve children either take up science with the total exclusion of history and the arts, or alternatively arts subjects with the complete abandonment of science, [history/archaeology] has become the happy hunting ground of ‘quasi-scientists’ ...

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<sup>116</sup> Michael Rice, *The Archaeology of the Arabian Gulf* (London 1994), p. 77

“The ‘static fashion’ now in vogue was a reasonable over-reaction to the ‘Great Sweep’ theory which in itself was partly a reaction against the Marxist theory of history. The truth, probably, as is usually the case, lies somewhere between the two extremes ... in most cases the economic circumstances [bringing about change in the ancient world] are created by the weather.”<sup>117</sup>

For the camel to have carried incense into Egypt, Palestine, and thence to Mesopotamia in or about 1500 B.C., but to have remained largely unknown and unrepresented in the art or literature of these societies for several hundred years, requires a Camel Dark Age. While it is taken by historians and archaeologists that camels plied the Incense Route for centuries, for some totally inexplicable reason the people, artists, scribes, business people etc., saw these creatures but failed to represent them, discuss them or, most significantly employ them as a means of transport for these same centuries.

In terms of the short chronology the camel as a means of conveyance of goods came into being as the land of Mesopotamia dried up and the camel which could haul goods in these deserts was employed around the beginnings of Persian times. As the Neo-Assyrians are shown to be the Persians, it is only natural that in Neo-Assyrian/Persian times the camel is portrayed, discussed, and employed to haul goods and have riders. As Woolley states:

“Only in the reign of Tiglath Pileser I, 1098-1068 BC, do the sculptors show that the Assyrian army had camel transport; a single limestone relief from Carchemish, figuring an archer on a riding-camel (apparently a dromedary) may possibly belong to the second millennium BC but is more generally attributed to the ninth or eighth century.”<sup>118</sup>

Commenting on climate change, Stephanie Dalley thinks it “curious ... at this time the whole region seems to have had a population of elephants.”<sup>119</sup> As we know from volume I of *Pillars of the Past*, pages 437-443, and to repeat part of this, I cite page 441 of my book:

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<sup>117</sup> *ibid.*, pp. 33-34

<sup>118</sup> Hawkes and Woolley, *op.cit.*, p. 617

<sup>119</sup> Dalley, *op.cit.*, p. 190

“The question has often been asked whether the climate of Syria has undergone any radical changes since antiquity. The presence of large numbers of *tells*, those flat-topped mounds which testify to the ruin of ancient towns many times rebuilt and at last abandoned, and the long lines of mounds which betray the course of ancient canal systems, points to the desiccation of once fertile and well-watered lands, and the great forests of cypress, cedar, and pine which once supplied timber for the shipwrights of Egypt and the architects of Assyria and Babylonia, have almost entirely vanished. That panthers, lions and a species of wild horse roamed the North Mesopotamian steppe is proven by animal remains from the excavations of Tell Barak and elsewhere, and elephants and aurochs [the early form of cattle] were hunted in Syria ...; the presence of such large animals implies plentiful water and a far richer vegetation than the sparse undergrowth and almost treeless conditions of the area today.”<sup>120</sup>

Both the climate change which created desert conditions around Mesopotamia and hunting in this case removed the elephant and decimated the population of lions, to be discussed below, thus creating good conditions for camels around 600 to 500 B.C.

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<sup>120</sup> *CAH*, vol. I, pt. 2, p. 318

## AMBER, COBALT BLUE, SALT, AND CHRONOLOGY

“... amber for part of the decoration of the iron sword hilt had most likely been brought all the way from the Baltic [Sea], since there is no known source for it anywhere in the Near East.”

Ian Wilson  
*Before the Flood*  
(NY 2004), p. 163

Though not unrelated to the domestication of the camel and its use as a beast of transport, amber trade tells the very same chronological story. Margaret Drower tells us:

“Amber was prized not only for its magical colour but probably for its magical reputation derived from its property of acquiring an electrical charge by friction. Ornaments of amber combined with gold or lapis lazuli, and a necklace of fifty-five beads of this material found in the tomb of Tutenkamun were perhaps a gift from Mitanni. The ultimate source of the amber lavishly used in the Mycenaean world during the fourteenth and thirteenth centuries (B.C.) was the Samland coast of the Baltic [Sea]; a branch of the trade route that passed thence through Central Europe to the Balkans may ultimately have reached Syria by way of Anatolia, and amber beads found at Ugarit are thought to be of Baltic origin.”<sup>121</sup>

Amber, as is well-known, is a tree resin which is yellow-orange in color, and looks like a gem stone. It is believed to be one of the first items of trade to be sent from Europe to Mycenaean Greece. It is extremely light and thus easy to transport and uniquely it was abundant, washing up ceaselessly at Samland on the south-eastern Baltic Sea. It was gathered there in baskets in great quantities. It cost the gatherers nothing and thus they were pleased that people to the south would pay them for an item of almost limitless supply. To the Mediterranean people amber was a gem to add to jewelry and other ornaments. Strikingly, because it is a tree resin, it can be burned, and the Germans call it the “stone that burns” or “Bernstein.”

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<sup>121</sup> Margaret S. Drower, “Syria c. 1550-1400 B.C.,” *CAH*, vol. II, pt. 1, *op.cit.*, pp. 515-516

The route by which it was traded across Europe to the Mediterranean is known as the Amber Route, in German, “Bernsteinstraße.”

Interestingly, it was traded through central Europe, the heart of the metallurgy centers in Saxony and Bohemia, etc. Thus it is possible, if the established chronology, at least to 1500 B.C., is correct, that tin, glass, cobalt blue, etc., could have been brought to the Mediterranean world, *especially Mesopotamia*, before the first millennium B.C. This is precisely the thesis John Dayton has presented to suggest that these metals that were missing or highly uncommon in that region of the Near East arrived there in the second millennium B.C. This he presented in a book of about 40 pages titled *The Discovery of Glass: Experiments in the Smelting of Rich, Dry Silver Ores, and the Reproduction of Bronze Age-type Cobalt Blue Glass as a Slag* (Cambridge MA 1993). In his conclusion on page 39 he suggests:

“The *only* area in Europe where cobalt glass could have been produced, certainly by accident, was in the ‘silver-cobalt-nickel-arsenide’ belt which runs north from Joachimsthal [today in Czechia] to Ehrenfriedersdorf in Saxony ... The blue slag was a valuable material—man’s first plastic—and was undoubtedly traded to the Adriatic, Mycenae and Egypt [via] the Baltic amber [route] ...

“Bass’s discovery (1986) of blue glass ingots in the cargo of the Mycenaean ship wrecked off Kaş in southern Turkey confirms the origin of cobalt glass in Europe. This shipwreck has been dated to around 1400 B.C., the peak of the Mycenaean trading empire. ... The wreck also contained amber beads ... The amber points to the Baltic and the route from the head of the Adriatic past the rich tin, copper and silver deposits of central Europe.”

Based on this evidence, in his “Summary” on page 40 Dayton argues:

“It would appear that what has been taken for granted for many years with regard to the movement of trade goods throughout the ancient world—namely, their origins, the direction in which they moved, and their routes—needs to be completely reexamined. Recent reexamination and analyses of some of the existing artifacts and data, a review of relevant literature, actual metallurgical experimentation, and lead isotope analysis, would indicate that many of the rich and

sophisticated artifacts of the ancient Near East had their origins in the mineral deposits of Europe rather [than] in the east itself as has been heretofore assumed.”

With one great conclusion, Dayton has appeared to sweep away all the evidence that has been presented by this author, namely, that tin, etc., could not have come to Mesopotamia, Anatolia, and Egypt in the second millennium B.C. Dayton's evidence and arguments suggest that during the second millennium B.C. European trade supplied amber and all the metals, glass, and cobalt blue, that are not found in Mesopotamia. However, although I in part agree with Dayton, I do maintain that his chronology is in error, and that all this trade was only carried on from around the mid-first millennium B.C. What does the evidence say in this regard?

## SALT

Salt in hot climates is a great necessity to people, and as Mesopotamia in large measure lies in a very hot region salt is essential for the health of its people who lose this mineral when they perspire. Paul E. Lovejoy explains:

“Salt satisfies a physiological need, and it may well be that salt is also man's earliest addiction. The body requires salt because of its role in regulating osmotic pressure [so that water in the cells remains at a constant level] and its part in hormonal and enzymatic processes, but recent studies suggest [not prove] that the addition of salt is usually not necessary. Most foods contain enough salt naturally to meet body requirements, and the body can adapt to salt-free diets. People who consume relatively high proportions of animal products need the smallest amount of salt since meat and milk are naturally saltier than cereals [grains] and vegetables. Diets that are dependent upon grain [as in Mesopotamia] are more apt to be supplemented with additional salt than diets based on animal products ... In a particularly hot climate, the usual physiological requirements that are largely satisfied though the salt contained naturally in animal and vegetable products have usually been supplemented in order to counteract the effects of the



tropical sun. The normal level of salt consumption in temperate zones—4.5 kg [9.9 pounds] per year—can be satisfied largely through the salt contained in these food sources, but in the tropics [and near tropics] this level can easily be doubled...

“Salt was once a rare commodity sought after by man in much the same way that petroleum is today.”<sup>122</sup>

Even in temperate Italy in Roman times, salt was exceedingly important, as Robert Raymond describes:

“The Romans considered salt so important to the efficiency of their armies that the legions were paid in it. The soldiers were given a special ration of salt or the means of buying it—the *Salarium argentium* or salt money, from which comes the modern derivative salary.”<sup>123</sup>

As the Biblical saying goes, “thou shalt earn thy bread by the sweat of thy brow.” And undoubtedly in Mesopotamia, especially the southern portions, people had diets largely of grain, and in fact were paid with grain, and did earn that grain by the sweat of their brow. The question is: From where and when came at least some of the salt for these people in that hot climate? Interestingly part of the answer is, from northern Europe. Of greatest importance is the time when this salt was being traded across Europe along with amber, metals, and colorants for glass such as cobalt blue, as discussed by Dayton. The evidence we do have is that this commerce did not exist in the second millennium B.C. but as expected well into the first. Again Raymond shows:

“From [about] 1000 B.C. the Salzkammergut [in Austria] and its immense resources of salt became a major centre of distribution and exchange. Its inhabitants mined the mountains and traded salt south east through the Balkans to the empires of the ancient world around the Black Sea, the Aegean, and the Mediterranean. The significance of the trade, however, was not fully realized until the latter part of the last

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<sup>122</sup> Paul E. Lovejoy, *Salt of the Desert Sun: A History of Salt Production and Trade in the Central Sudan* (Cambridge UK/NY 1986), p. 1 and p. 3

<sup>123</sup> Raymond, *op.cit.*, p. 64

century, when a remarkable find was made on the side of the Salzkammergut, not far from the present salt mine.

“The chance discovery of a skeleton led to the eventual excavation of a large burial ground on the hillside. No fewer than two thousand graves were found, apparently of very early date. They contained a rich variety of grave goods, weapons, tools, utensils and ornaments made with great skill. These were typical of the [European] Urnfield Culture of the late Bronze Age.

“But among the bronze artifacts was unexpectedly an assortment of iron swords, battle axes, bracelets and rings. The dating of the graves [was] from 700 to 500 B.C.

“[There was obviously] trade between Europe and the Mediterranean civilizations—with metals, salt and amber going south, in exchange for gemstones, jewellery and oil— [going north].”<sup>124</sup>

The date of these graves indicates that it was only between about 700 and 500 B.C. that trade in metals, amber, and salt was going on, and not in any way during the second millennium B.C., as historians assume. The dating fully coincides with the short chronology.

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<sup>124</sup> *ibid*,

## LAPIS LAZULI AND CARNELIAN

Another aspect of long distance trade are the semi-precious gemstones lapis lazuli and carnelian. According to Saggs,

“The most important of these materials as a marker for trade is lapis lazuli ... From the very beginning of civilization, it was much prized for ornamentation by the peoples of both south Mesopotamia and Egypt. But how did they obtain it? The nearest known source was Badakhshan, in north Afghanistan, 1400 miles away from Mesopotamia in a straight line across many mountain ranges and deserts, and twice as much by any practicable route. Theoretically, the ancient civilizations might have known some nearer source which had since been worked out, but any such working should have left evidence identifiable archaeologically, and no such evidence has been found ...

“Another mineral worked ... was the beautiful red stone carnelian, and for this the nearest source was in the Hindu Kush ... Shahri-i-Sakhta and Tepe Hissar lie between south Mesopotamia and the source of lapis lazuli in Badakhshan and of carnelian in the Hindu Kush, which makes it a reasonable conclusion that both [towns] were entrepots for international trade, on alternative routes ...

“The lapis lazuli routes did not terminate in Mesopotamia. Finds show that even before 3000 B.C. this precious stone was reaching Egypt ... There is no doubt that it came by way of Mesopotamia, since finds of lapis lazuli in Egypt include objects inscribed in cuneiform, such as cylinder seals from about 1800 B.C. [the time of the Hammurabi Old Babylonian/Persian empire].”<sup>125</sup>

To carry on this trade across “mountain ranges and deserts,” as Saggs admits, requires camels to cross the deserts. This would not have been possible in either the third or second millennium B.C. As we have shown, Hammurabi is an *alter ego* or a contemporary of the Persian king Darius I. Therefore, in terms of Heinsohn’s, Sweeney’s, and Rose’s chronologies, this long-distance trade across

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<sup>125</sup> Saggs, *Civilization Before Greece and Rome*, *op.cit.*, pp. 129-131

deserts was much more reasonable and probable in the late first millennium B.C. rather than the third or second.

Let us briefly reexamine this long-distance commerce with regard to the tin trade:

“More recently in 1970 ... ancient tin and copper ingots began turning up mysteriously in scrap metal shops in Haifa in Israel. The source was eventually traced to an Arab fisherman who had recovered them from the wreckage of a ship buried off the ancient port of Dor between Haifa and Tel Aviv. Over several years the fisherman raised some seven tonnes (15,400 lbs) of the metal, most of which he sold to scrap merchants for re-melting. However, two tin ingots found their way to the Haifa Museum and several of both copper and tin were rescued from local souvenir shops.

“The copper ingots were bun-shaped from the furnace in which they were smelted; the tin [ingots] were brick-shaped, weighed 11-22 kilos [24-48 lbs] and bore Cypro-Minoan markings which had evidently been carved into them as the metal cooled after casting. Unfortunately the inscriptions were not much help in dating the material as similar markings were in use both in the Minoan world in the 2nd millennium and in the Iberian [Spain] between the 7th and 6th century B.C.”<sup>126</sup>

No one has explained why the writing used during the Minoan period of the second millennium [1600-1200] B.C. would remain the same into the 7th and 6th centuries B.C. One must assume the language or symbols for a smelting plant did not change for about a thousand years, with all that was supposedly happening—wars, business failures, etc.—not to say the expected change in language that must have occurred. The obvious answer is that these markings on the tin ingots were not from the second millennium but from the 7th-6th century B.C. as all the other evidence discussed above and in volume I of *Pillars of the Past* clearly indicates. We thus have several other forms of technological and trade evidence that correlate with, and corroborate, one another and converge to show that it was only during the

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<sup>126</sup> Wilson, *The Living Rock*, *op.cit.*, pp. 27-28

first millennium B.C. that these massive developments occurred during the Ancient Near East's Industrial Revolution.

In addition to this evidence, Elizabeth Kosmetatou reports on

“ ‘The Silver Hoard from Tel Dor’ [which] deals with a find [of hoarded silver] that is dated on the basis of stratigraphy in the late eleventh or early tenth century BCE, when the area was controlled successively by the Phoenicians and [then] the Israelites and trade relations with the West were on the rise. The hoard was identified as Phoenician ... Even though it remains unclear what Phoenician weight standard is reflected ... there are indications that it resembled that in use in Palestine in the 8th-7th centuries B.C.E. [700s–600s B.C.] ...

“The discovery of this hoard helps us understand finds that have been dated to approximately the same period ... [The researcher] Stern ends his study by presenting the results of the metallurgical analysis, according to which the silver contained rather high percentages of gold set at a minimum of 11 %. The fact that similar results were obtained from silver extracted from the mines of Rio Tinto in Southern Spain tempts him to hypothesize that the silver of the Tell Dor hoard may have come from the same region. However, he prudently refrains from concluding that this find constitutes evidence for trade between the Phoenicians and Spain in the late 11th and early 10th centuries BCE, as this silver could equally have been the result of recycling [being melted with silver from other sources]. Indeed more evidence is needed in order to establish the history of trade relations between the East and West. ...

“In response ... William G. Dever offers a commentary on “ ‘The Silver Trail,’ reflecting on the implications of the theories introduced. Stern’s suggestion that the silver from the Tel Dor hoard may have originated in Spain is boldly associated with suggestions by a number of scholars that the Proto-Canaanite alphabet was spread to Greece and the Mediterranean by the Phoenician traders earlier than we thought, perhaps as early as ca. 1100 BCE. Even though new discoveries [such as this one] force us to rethink, revise and calibrate chronology all the time, the lack of physical evidence for trade between the Phoenicians

and the inhabitants of the Iberian Peninsula, or of a Phoenician-derived script in Greece at so early a date, should be indicative of the dangers of accepting Dever's proposed reconstruction. Nevertheless, I agree with him that the implications of the discovery and publication of the hoards are far-reaching and enhance our understanding of the so-called Dark Age [of trade]."<sup>127</sup>

But this Dark Age problem vanishes with the problems related to finding markings on metal in the ancient port of Dor that were in use in the Minoan and Iberian world also dated to the 7th and 6th centuries B.C. instead of around a thousand years earlier by removing the Dark Age and shortening the history/chronology. The same Dark Age problem vanishes by having the Tel Dor silver hoard down-dated from 1100 B.C. to the 8th-7th century or earlier so that trade between Palestine and Spain will flourish. And the Dark Age problem vanishes by having the Proto-Canaanite alphabet spread to Greece and the Mycenaean world by Phoenician traders who ply their trade in the 8th-7th and even the 6th centuries B.C. Rather than keeping the long-established chronology that is rife with problems and contradictions that cannot be solved in terms of that long chronology, these problems and contradictions evaporate by removing the Dark Age and shorting the chronology.

What we have, then, are a considerable number of forms of evidence, which point to the fact that trade in tin, cobalt blue, amber, and salt from northern and central Europe and Spain that meshes with the commerce across Mesopotamia, falls squarely in the first millennium B.C., contemporary with the introduction of the "ship of the desert," the camel.

With respect to the camel and climate, it is evident that prior to the rise of civilization the Sahara desert was a well-watered environment so that lions and other predators hunted the camel to extinction in this region. Thereafter, there had to be a great climatic change related to a pole shift which, in terms of Velikovsky's thesis, occurred around 1500 B.C. and which set in motion the desiccation of the Sahara. If there had been no such pole shift, then the climate of the Sahara should not have changed as drastically, and the same plants and animals that lived there before the onset of river valley civilizations in Egypt and Mesopotamia should still

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<sup>127</sup> Kosmetatou, *op.cit.*, pp. 2-3

be living in these regions. Such enormous permanent climatic shifts cannot be explained by other than a major pole shift.

The scientific and technological points of evidence that the camel was introduced into Mesopotamia during the mid-first millennium B.C. are as follows: The camel as a beast of burden or as a riding animal could not have been used for these purposes in the second millennium B.C. because these activities require that the camel saddle existed at that time. But the camel saddle is a mid-first millennium B.C. invention, which precludes any interpretation that the camel was used or known for these purposes until the mid-first millennium B.C.

This was taken as correct by Erman, as noted above, who held that “cross-desert trading by camel to Syria did not start until the fourth century B.C., and we have no further data to judge its development.” According to Zarins, also noted above, the only criteria that relate to the domestication of the camel indicate it was domesticated in the first millennium. According to Contenau, the cost of camels fell only in the first millennium, indicating it had become domesticated and employed to carry goods across the Syrian and other deserts.

That camels were utilized and known well into the second millennium B.C. to carry on the incense trade is disproved by the camel saddle being dated to around 600 B.C. (as noted above), and other evidence. If it was employed to carry incense it would have been depicted on monuments and noted in documents. If this was the case, the camel would have been able to spread out of its Arabian refuge quite early, but it only did so after a climate change led to the final drying out of the Near East which then provided habitats for camels to thrive and later be employed as beasts of burden. Dayton has shown that there was a climate shift in Arabia to even greater aridity, based on the archaeological evidence, “in the last quarter of the sixth century,” which we move back to 700 B.C. The animals that lived in Mesopotamia also provide evidence that there was a climate change. Elephants, wild horses, aurochs as well as other, smaller herbivores require a fairly large supply of water to promote vegetative growth for their food so that in turn lions and panthers can prey upon them (in the case of elephants, lions prey on baby animals).

All these forms of evidence along with all the various forms of scientific and technological evidence correlate, corroborate, and converge to prove that there was a major climate shift prior to the inception of the Egyptian and

Mesopotamian civilizations and another around 800 B.C. With the long chronology we have contradictions and mismatching of all this evidence.

With respect to international trade, the evidence of amber, tin, cobalt blue, salt, incense, as these relate to the chronology of the Near East show that these items of commerce were carried by camels, wagons and ships only in the first millennium B.C., correlating, corroborating and converging again with the other evidence.

There is furthermore an important aspect regarding all this evidence that seems to have eluded everyone. This is touched upon by Muhly in his discussion of the development of technology. Based on the established chronology we learn that the growth of technology in ancient Mesopotamia and the rest of the Near East does not proceed as it did, in an orderly manner, one technology leading to another as it does in recent history. The technological developments of the western world of the 19th and 20th centuries A.D. grew by a gradual sequence of interrelated steps, each advance leading to the next. The canal boat pulled by mule, horse, or ox was replaced by the steamboat and then came the steam-driven locomotive. These early forms of transport were then replaced by gasoline and oil engines to power these conveyances. In the ancient world because of the misplacing and doubling of several civilizations, this orderly technological development does not exist. In this regard, Muhly's statement is of profound importance:

“The idea of a unified sequence of steps or stages in technological progress [in the ancient Near East] seems to be gone forever.”<sup>128</sup>

This is well-illustrated with regard to tin for bronze production. Since both the tin ore and a fuel supply is necessary to produce bronze, it has long been known and accepted that southern Mesopotamia could not manufacture tin bronzes and it is taken in terms of the established chronology that this work was done in Anatolia where tin was shipped to and lumber supplies were fairly abundant. Nevertheless Kurinsky informs us that

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<sup>128</sup> James D. Muhly, “The Beginnings of Metallurgy in the Old World,” *The Beginnings of the Use of Metals and Alloys*, Robert Maddin, ed. (Cambridge MA 1988), p.2



“In the Museum of Anatolian Civilizations in Ankara [Turkey] ... a comprehensive Anatolian collection provides a [*sic*] extensive overview of the evolution of Anatolian culture and technology. One is immediately struck by the fact that the museum’s earliest Bronze Age exhibits are not of Anatolian [bronze] ware; they consist entirely of Mesopotamian artifacts of the Sumerian Dynastic Period (2600-2350 B.C.E.); these are followed in turn by the Ur III Dynastic and the Old Akkadian Periods (2340-2150 B.C.E.) in which Bronze artifacts first appear in Anatolia. ...”<sup>129</sup>

In other words, tin is mixed with copper in Anatolia to make bronze which is then shipped to southern Mesopotamia. But the people of the so-called Sumerian civilization supposedly have bronze artifacts hundreds of years before the Anatolians ever manufactured bronze. This is a basic contradiction to the technological development of bronze production in the ancient Near East. According to Kurinsky, “Anatolian metallurgy trails the Akkadian by at least six hundred years. ...”<sup>130</sup>

This is the sort of incongruity that occurred with technological development in Mesopotamia. It would be like suggesting that gasoline engine automobiles appeared in the early 1800s to disappear for 100 years, that railroad steam engine transport followed the automobile and between these forms of transport canal boats pulled by mules were used. The concept that technological development was just as disjointed in ancient Mesopotamia is a dysfunctional concept bereft of logic, evidence, and chronological reality.

If we accept the established chronology, the smelted iron objects found in what are assumed to be second millennium B.C. civilizations were manufactured before there were furnaces capable of melting iron. The diorite statues and engraved stelae were cut by wrought iron, or copper, before iron was hardened by carburization. Glass, which arose out of iron technology, grew out of no previous technology, because we have shown that in the assumed early second millennium B.C. Old Babylonian civilization, as discussed by Kurinsky, above, in the “residence of Hammurabi’s grandson ... a fully matriculated glassmaking technology is unmistakably evidenced by the presence of intricately wrought polychrome true

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<sup>129</sup> Kurinsky, *op.cit.*, p. 44

<sup>130</sup> *ibid.*

glass objects.” To produce raw glass out of its basic ingredients requires not only advanced furnaces, but immense amounts of wood or charcoal—a wood product.

First, let us examine how much wood is required to produce copper. According to Forbes,

“Mr. I.U. Selkield of the Rio Tinto Mines supplied us with a fairly accurate estimate of the fuel consumption of ancient smelting activities by their studies of slag and other remains of such smelting sites. In the Rio Tinto region [of Spain] the Romans mined and smelted a chalcopyrite with about 8 % of copper by a complicated chain of roastings, smeltings and refining ... The fuel requirement would be some 71,840 kcal per kilogram [2.2 lbs] of copper for the roasting process and a further 132,760 kcal per kilogram of copper for the smelting and refining. This would imply that for every kilogram [2.2 lbs] of copper 21.8 kilograms [48 lbs] of wood was needed for the roasting and another 68.5 kilograms [150 lbs] of wood for the smelting (one third of which in the form of wood, two thirds in the form of charcoal, 5 parts of wood having the same calorific value as one part of charcoal), hence [to manufacture 2.2 lbs or 1 kilogram of copper requires] a total of 90.2 kilograms [200 lbs] of wood.

“Tests at Rio Tinto have shown that the average rate of growth of oak trees in 40 years is some 300 kilograms [660 lbs] ... of wood for charcoal and some 75 kilograms [165 lbs] of small branches ... that might serve for roasting. The climate was wetter in Roman times and assuming 900 kilograms [2000 lbs] per tree, in 125 trees per acre some 112.5 tons of wood would be produced per acre every 40 years. For the production of a ton of copper a day would absorb the fuel of some 12,000 acres [40 square kilometers].”<sup>131</sup>

With regard to producing glass from its raw materials, Kurinsky informs us:

“The engineering of pneumatically drafted, reverberatory furnaces capable of producing [the] glass hulks [in the shipwreck found by

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<sup>131</sup> Forbes, *Studies in Ancient Technology*, *op.cit.*, p. 19

George Bass on the Turkish coast] at this early period bespoke a sophisticated pyrotechnology that shattered the [technological and chronological] assumptions of most historians. A temperature of some 1100 degrees Celsius [about 2000 degrees Fahrenheit] must not only be attained but unremittingly maintained by pumping a steady draft of air through the flaming furnace by means of one or more bellows for at least four days and nights to produce an ingot such as that found on board the ancient ship ... Once glass is manufactured, however, it can readily be remelted at a reasonable temperature ...

“Archaeologists and historians have rarely differentiated between glass making and glassware making ... The two crafts are quite distinct; whereas every glassmaker was capable of producing glassware, the reverse was by no means true ... In my association with the industry on the island of Murano, I had determined from the records of the period, in which wood was still being used on that island for firing the furnaces, that two tons or more of wood was required to produce a kilogram [2.2 lbs] of glass. Considering the cruder engineering of the furnace of three to four millennia ago, and variables such as the type of wood employed as fuel, it is clear that up to six tons of wood was required in ancient times that [would produce 2.2 lbs or one kilogram] of raw glass depending on the design of the furnace and the diligence by which the pumpers of the bellows worked through day and night. The first, and by no means the only question to be answered, therefore, was ‘where were the forests of Upper Egypt [or Babylonian Mesopotamia] that supplied the necessary fuel?’ ”<sup>132</sup>

The production of copper from the Timna Valley mines and production center in the Sinai Desert in Ancient Egyptian times surely presents the same problem and indicates there had to be a climate there that allowed whole forests to grow. Wilson points out that in the

“Sinai ... [is] a complex of crystalline rocks, veined with dykes whose reddish colour gives the name to the [Red] sea ... This today is Timna ...

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<sup>132</sup> Kurinsky, *op.cit.*, p. 139

“In this strange land ... eminent men and women ... have in recent years been studying the remains of a copper industry which flourished here ...

“Toward the middle of the valley are the remains of extensive [copper] smelting camps, many of them littered with substantive slag heaps. These [metal] plants have been identified as dating to the times of the Egyptian New Kingdom in [the] Late Bronze Age and early Iron Age, probably 14th to 12th centuries B.C. ...

“Perhaps the most exciting find made in 1965, on a hill to the east of Har Timna ... excavations revealed the remnants of a primitive copper smelter ... The site was confirmed as Chalcolithic [copper/stone age], dating probably to around 4000 B.C. ...

“Benno Rothenberg ... in the mid 1950s ... [found a] ... smelting furnace. [It was b]oat shaped and measuring only 45 centimeters [1.5 feet] across ...

“From analysis of the slag and ores, the simple operational process could be reconstructed. The furnace was fired with charcoal and fed with finely-crushed copper ore, mixed with equally finely-ground fluxes—iron and manganese oxides, limestone or sea-shells ... [The furnace though p]rimitive [contained] globules [of metal] found in the slag [that] were assayed at 97.43 to 98.88 percent copper, with 0.24 percent iron ...”<sup>133</sup>

Wilson, however, has failed to discuss the wood to make charcoal that was necessary to smelt all the copper of this region—the Sinai Desert. Given that the climate there of the 14th to 12th centuries B.C. was like that of the present time, copper production could not have been carried out without importing whole forests—an unimaginable concept. What is self-evident is that this mining operation requires that there was sufficient rainfall in the Sinai to allow forests to grow which, based on the present thinking of historians, that the climate then was very similar to today, demands a serious climate change.

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<sup>133</sup> Wilson, *The Living Rock*, *op.cit.*, pp. 13-16

But historians still obstinately maintain that copper smelting plants were in operation then, and at the same time there has been no climate change over the last 6000 years. It seems never to have occurred to them that these two theses are contradictory to one another and without any rational support. Perhaps, to be nasty, it might be assumed that they knew of some unknown method—such as burning sand—to heat the furnaces. It is astounding what lengths of unreality they require to hold fast to their theory that the climate has not changed during the past 6000 years and at the same time hold fast to their theory that sufficient fuel was available, then and there, in the Sinai desert for large-scale copper production to be going on.

Neither Egypt, which was trading with Byblos on the coast of Palestine for cedar, nor southern Mesopotamia which imported wood, had sufficient amounts of this fuel to carry on copper production or a glass-making industry. Again, as with so much else the technology of the ancients in terms of the uniformitarian long chronology is dysfunctional and disjointed from reality. These problems with technological development are recurrent in area after area. A form of technological development arises suddenly without the proper resources or tools necessary for that development and flourishes for a time as an important, prominent phase of some empire and then suddenly recedes into oblivion, only to arise again centuries later. Just as with the many Dark Ages for various other periods we have a whole series of technological Dark Ages. On the other hand, the short chronology presents none of these Dark Age recurrent phases of prominent technological periods of development with empty periods in-between. What we have instead is a natural evolution of technological growth with one advancement leading to the next and spreading across the ancient world. Heinsohn and Sweeney's chronology is not dysfunctional nor disjointed from technological growth nor is it disconnected from the realities of this development.

Joan Oates provocatively stated with respect to the nature of historians' understanding of ancient civilization:

“Any study of Babylonian civilization is and will remain an amalgam of near truths, misunderstandings and ignorance ... most historians would admit.”<sup>134</sup>

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<sup>134</sup> Oates, *op.cit.*, p. 197

By simply following the scientific and technological evidence where it leads this “amalgam of near truths, misunderstandings and ignorance” could be rectified to truths, understanding, and knowledge if only the historians would follow the forensic historical evidence.

Let us go back to the supposedly earliest precious commodities for trade in Mesopotamia in the assumed third millennium B.C. According to Saggs, as we have seen above in the chapter on Lapis Lazuli and Carnelian, to carry on this lapis lazuli trade required a journey from Mesopotamia of about 1300 miles each way. All the various explanations to have trade in lapis lazuli occur in the third millennium B.C. are mere probabilities. It seems far more reasonable to suggest that journeys covering 2600 miles—to the source and back—occurred in the very late second and, much more reasonably, well into the first millennium B.C.<sup>135</sup>

John Dayton nicely sums up the entire confusion and mess regarding metallurgical chronology:

“Although archaeological artifacts were divided into groups—the Stone Age, the Bronze Age, and the Iron Age—as long ago as 1828 by Thomson in Copenhagen, archaeologists have paid very little attention to metallurgy and even less to geology [for finding the ores of these metals] in the one hundred and fifty years since that date. It is rare for archaeologists to have a scientific training and the historical approach to the subject developed from the practice of looting sites for ‘goodies’ for museums. Then came the era of the art historian, the architect and the epigraphist. Small finds were ignored. Only in the last fifty years has scientific stratigraphical excavation taken place, but unfortunately during this period archaeology has become even more specialized into geographical areas, and even narrower in its subdivisions. Today they are studying the leaves of the tree, rather than the tree itself, and few can stand back and look at the forest as a whole.

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“The awful chronological mess into which archaeology has got itself by a complex system of synchronisms with doubtful Egyptian records has further confused the picture ... What has developed

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<sup>135</sup> Saggs, *Civilization Before Greece and Rome*, *op.cit.*, pp. 129-131

because of modern frontiers and passports and visas, unknown to early man, is a rigid frontier mentality amongst many archaeologists, who will specialize in Turkey but not in adjacent North Syria, or in Cyprus but not Palestine a few miles across the sea ... Because the peoples of the Near East left records on clay tablets which have survived, their civilization[‘s metallurgical or other development] has been deemed to be superior to that of barbarian Europe. In this manner, Gordon Childe, a great all-round archaeologist, was led into the error of proposing Sumerian prospectors traveling UP the Danube looking for metals and minerals—the existence of which they could not have been aware of, for the alluvial silty plains of Mesopotamia and the limestone hills of Syria and Palestine are singularly devoid of metallic ores. Childe was right in proposing the Danube/Anatolian axis, but his direction of the metal [and other minerals] was completely opposite to geological reality.”<sup>136</sup>

Dayton goes on to show and ask:

“We have evidence of a massive increase in the use of metals and precious stones [from central and northern Europe], including Baltic amber and the first appearance of glass. None of the materials are native to the region [of the Near East], and the artifacts are all witness to a widespread trading system but what is more, a metallurgical and mining [industrial] revolution of enormous import to the ancient world. The problem is where [and I add, when] did this revolution take place?”<sup>137</sup>

Dayton concludes his paper with these words:

“All the evidence points to a very sophisticated and widespread trading network in metals and ores throughout Europe to the Aegean and Western Asia. The advanced technologies of bronze and silver smelting and the casting of complicated bronze figurines could only

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<sup>136</sup> John E. Dayton, “Geology, Archaeology and Trade,” *Interaction and Acculturation in the Mediterranean*, vol. II, Jan G.P. Best, Nanny M.W. De Vries, eds. (Amsterdam 1982), pp. 153-154

<sup>137</sup> *ibid.*, p. 156

have been acquired in the metal-rich areas of Western and Central Europe and transmitted from there to the primitive [in metallurgical development] Near East.”<sup>138</sup>

A great industrial revolution, based on several forms of science and technology, takes place in the first millennium B.C.

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<sup>138</sup> *ibid.*, p. 161



## CHAPTER 5: HARAPPAN CIVILIZATION: CLIMATE, SUMMER CROPS, FLOODS, AND CHRONOLOGY

According to present-day historians, the Sumerian civilization is overlapped by, and contemporary with, the Harappan/Indus civilization. Both have been tied together via trade. Donald P. Hansen in discussing the Royal Cemetery at Ur remarks: “Trade and barter ... linked Sumer to such distant fringes of the known world as ... the Indus [civilization] ...”<sup>1</sup> Holly Pittman enlarges on this:

“To judge from the forms of various jewels [in the Royal cemetery at Ur], virtually all of the material was imported into southern Mesopotamia in either raw or semiprocessed state. From sites in eastern Iran and on the Iranian plateau comes evidence for the bulk production of blanks for beads, and perhaps even for cylinder seals, but the final production was certainly done in Sumerian workshops. Such a conclusion is based both on the fact that no comparable objects are found at sites where the materials were initially processed and on the fact that these forms were also found in other cities of Mesopotamia. Of [Lady] Puabi’s jewelry only the so-called etched carnelian beads, strung with other types of beads, can be positively identified as imports. These beads were made using a complex firing technique developed in the Indus Valley centers. The white lines were made on the surface of the carnelian following patterns traced in an alkali mixture and then subjected to heat. After several such steps, this process eventually caused the affected area [on the bead] to lose its color and translucency and become opaque white. The presence of imported artifacts from the Harappan civilization lends credibility to the more tenuous suggestions of intercultural contact.”<sup>2</sup>

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<sup>1</sup> Donald P. Hansen, “Art of the Royal Tombs of Ur: A Brief Interpretation,” *Treasures from the Royal Tombs of Ur*, R.L. Zettler, L. Horne eds. (Philadelphia PA 1998), p. 43

<sup>2</sup> Holly Pittman, “Jewelery,” *Treasures of the Royal Tombs of Ur*, *op.cit.* p. 89

Sir Mortimer Wheeler further links the Harappan civilization with Mesopotamia via trade. After giving several radiocarbon dates (*ca.* 2300-2000 B.C.) that are assumed to confirm the link between the two societies, which he labels “a blunt instrument,” meaning that it lacks precision, he writes:

“Of far greater precision is the witness of comparative archaeology in the relatively few instances where it is at present available. Both from archaeological and from epigraphic [script] evidence, it is known that in the time of Sargon of Agade and his successors contact between southern Mesopotamia and the Indus [civilization] was at its liveliest ... Here is near-historical precision of a kind by which new material evidence from the Indus and Mesopotamia ... may be expected in the fullness of time to lend a fresh exactness to the dating of our Indus civilization ... the occurrence of Indus seals in Sargonid association at Ur, Kish and Tell Asmar; of etched beads, as used by the Harappans, from similar levels at Tell Asmar; of gold-disk-beads with axial tube at Mohenjo-Daro, at Early Dynastic III-Sargonid dates in Mesopotamia ... about 2300 BC; and bone inlays and knobbed pottery of distinctive Indus types in Sargonid deposits at Tell Asmar; all these and their like are assured evidence that the Indus Civilization was mature by the time of the Sargon dynasty.”<sup>3</sup>

Therefore historians place the Sumerian and Harappan civilizations together at the end of the third into the early second millennium B.C. This, however, is contradicted by all the previous scientific and technological evidence which was presented in volume I of *Pillars of the Past* regarding salinization of the soil in Mesopotamia, tin bronzes there, the ability to carve and incise diorite, and Scythians buried in the Royal Cemetery at Ur. This scientific and technological as well as archaeological evidence requires that the Sumerian/Chaldean and Harappan civilizations must be placed in the first millennium B.C.

In this respect there is now additional evidence that links the Royal tombs at Ur to the Scythians. Recently new evidence was brought to light regarding a shocking direct similarity between the women’s graves in Scythia to those of the Royal Cemetery. Lyn Webster Wilde, citing Scythian authority Renate Rolle, deals with a unique aspect of many of these Scythian women’s graves:

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<sup>3</sup> Sir Mortimer Wheeler, *Civilizations of the Indus Valley and Beyond* (London 1966), pp. 69-70

“She [Rolle] ... re-examined a grave ... In this ... were two skeletons: the main burial was of a woman, but at her feet lay the body of a young man of about eighteen years old. It was a fairly rich grave ... Next to her lay a bronze mirror ... To her left at the head end [of the tomb] lay two iron spear points, and underneath them was a smooth square plate that had been used as a whetstone; further down they found the remains of a brightly painted [arrow] quiver made of leather and wood, forty-seven bronze three-flighted arrowheads, and two iron knives. Next to the head were two so-called ‘sling stones’ although no one can be sure they were used as weapons. ...

“What we seem to have here is the grave of a woman warrior of some social standing whose young male servant was killed to accompany her on her death journey. The woman had many of the classic female accoutrements ... but she also possessed a bow, knife and spears. ...

“In another [Scythian] grave from the sixth century BC Renate [Rolle] told of an ‘Amazon’ [who was] buried.”<sup>4</sup>

In this regard Jeannine Davis-Kimball with Mona Behan wrote:

“In the 1950s, Russian archaeologists began excavating sixth-century B.C. Kurgans [burial mounds] and discovered women’s graves containing weaponry, armor and riding gear ... In 1980 archaeologist Renate Rolle ... discovered forty ... graves in Scythia, and noting that roughly 20 percent of the Sauromatian [Scythian tribe] warrior graves excavated in the lower Volga region belonged to women, with bows and arrows being their most prevalent weapons ... I verified Rolle’s observations ... [Furthermore in] the [Scythian] Porovka burials their burials had also included spears. ...”<sup>5</sup>

It is undoubtedly clear that some Scythian women were warriors and as such were entitled, like male warriors, to be buried with their weapons, armor, etc., to keep in the afterlife. The Scythians evidently had their Xena warrior princesses.

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<sup>4</sup> Lyn Webster Wilde, *On the Trail of Women Warriors* (NY 2000), pp. 47-48

<sup>5</sup> Jeannine Davis-Kimball with Mona Behan, *Warrior Women* (NY 2002), pp. 54-55

And this is an important as well as extraordinary piece of evidence, because a relatively recent article in the journal *Science*, provocatively titled “Ur’s Xena: A Warrior Princess for Sumeria?” reports:

“One of the most spectacular archaeological discoveries in history was Leonard Woolley’s excavations of the royal tombs of Ur ...

“But one grave, tomb 1054, left Woolley perplexed. In the shaft ... was a cylinder seal inscribed with the word ‘lugal,’ Sumerian for ‘king’ or ‘ruler’ ... In the stone chamber itself were a host of weapons, including a dagger at the side of the principal occupant. But there was one hitch: Woolley determined that the remains were those of a woman. ...

“The puzzle has obsessed two generations of researchers, who have come up with a variety of theories to explain it.

“The principal occupant of [grave] 1054 herself reveals some curious gender anomalies, notes [Kathleen] McCaffrey. Her skeleton was found wearing a hair ribbon, two golden wreaths, and a gold pin, all typical for high-status Sumerian women [in the Royal Cemetery] of the day. But she was not adorned with the usual earrings or elaborate choker, and there were no floral combs or cosmetic containers. And a gold headpiece and a dagger and whetstone at her waist was typical for Sumerian men [in the Royal Cemetery]; a gold headdress near the skeleton has a brim, a style that Woolley believed was worn mostly by men.

“Also in the stone chamber were a bronze ax, dagger, and hatchet ... Other researchers attribute those weapons to the male attendants in the room. But McCaffrey notes that the attendants lack rings, weapons on the bodies, or any sign of elite materials, suggesting that they were [merely] servants.”<sup>6</sup>

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<sup>6</sup> Andrew Lawler, “Ur’s Xena: A Warrior Princess for Sumeria?”, *Science*, vol. 309 (Aug. 5, 2005), pp. 868-869

Some critics have argued that Woolley could not distinguish a male skeleton from that of a female. McGuire Gibson, for example, opined: “Woolley could not tell the difference between a man, a woman, or a monkey.”<sup>7</sup> Even though this woman’s bones were discarded, “McCaffrey counters that Woolley was competent enough to identify correctly the gender of dozens of skeletons [whose bones] still exist.”<sup>8</sup> The person buried did have women’s jewelry like the women warriors in Scythian graves.

Without getting into McCaffrey’s thesis, it must be pointed out that both in the Royal Cemetery at Ur and in the Scythian homelands, not only were high-status women buried with valuables and sacrificial victims, but in the Royal Cemetery at Ur and in the Scythian homelands, warrior-princesses were buried not only with women’s accoutrements, but also with weapons, sacrificial victims, etc.

This, as with all the other evidence regarding Scythians and the burials at the Royal Cemetery presented in volume I of this series, is a stunning and unique correlation between the burials at Ur and in Scythia. Not only were high-status women buried with valuables and sacrificial victims both in Scytho-Siberia and at Ur, and nowhere else in Mesopotamia, but warrior Xena burials containing weapons, valuables, and sacrificial victims were also found in Scytho-Siberia and in the Royal Cemetery at Ur, and nowhere else in Mesopotamia. This unique, striking evidence is yet another powerful corroboration that the Royal Tombs at Ur contain Scythians, and this places the Royal Cemetery in the first millennium B.C.

In fact, Stanley Casson, an anthropologist and historian, when he learned of Woolley’s excavations of the Royal Cemetery at Ur and the subsequent publication of it in 1934, wrote four years later:

“The Royal tombs in the Kuban and South Russia perfectly illustrate ... prodigious and terrifying ceremonials. The Royal tombs at Ur also form an exact parallel, for the wholesale sacrifice of tomb victims. ... Although typically Scythian, [they are taken] to be Sumerian in origin.”<sup>9</sup>

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<sup>7</sup> *ibid.*, p. 869

<sup>8</sup> *ibid.*

<sup>9</sup> Stanley Casson, *The Discovery of Man* (NY/London 1939), p. 49

These words show that it was apparent to Casson, and perhaps to others, that the sacrificial human victims at the Royal Cemetery at Ur were “an exact parallel” and that the rites were “typically Scythian.” However, because of the orthodox chronology that Casson accepted as correct, he was forced to place the Royal Tombs at Ur almost 2000 years earlier in time. Thus they could not be Scythian burials but had to be “Sumerian in origin.”

The only thing standing in the way of historians accepting the “exact parallel” between the Royal tombs at Ur and those of “the Kuban and South Russia” is an unwavering and absolute belief in a false chronology that is in contradiction to all the forensic historical evidence.

## **THE PIG, CLIMATE, AND THE CHRONOLOGY OF MESOPOTAMIA**

Related to the chronology of Mesopotamia is the utilization of the pig for food. It has also been argued by this author that there was a major climate shift around 750 B.C. from a rainy to an arid climate. This was based on the salinization of the southern Mesopotamian plain and will be reinforced by the evidence of the pig. In order for a society to raise pigs, certain climatic conditions are required. Pigs cannot live in highly arid areas unless they are cared for by man who needs to possess sufficient grain to feed them. This was demonstrated by Marvin Harris of Columbia University, who explains:

“Whenever a farming population possessing domesticated animals increased rapidly, a choice had to be made between growing more food plants or raising more animals. Ancient states and empires invariably gave priority to the raising of more food plants since the net calorie return on each calorie of human effort invested in plant production is on average about ten times greater than the net calorie return obtainable from animal production. In other words, it is energetically much more efficient for human beings themselves to eat food plants than to lengthen out the food chain by interposing animals between plants and people. Grains convert about .4 percent of each unit

of photosynthetically active sunlight into human edible matter. Feeding grain to cattle yields meat containing only 5 percent of this percentage, that is .02 percent of the original unit of sunlight. The decision to increase the acreage devoted to animal pasturage thus represents a strategy aimed at raising and feeding people rather than raising and feeding animals.

“But domesticated species are valuable for other products and services. To raise and slaughter them for their meat alone is to destroy their value as traction machines, as producers of fibers, and as providers of fertilizer. Since some of the domesticated species can also be made to yield a continuous supply of animal protein in the form of milk and milk products, one can readily understand why domesticated animals were used with steadily decreasing frequency. Therefore, meat gradually disappeared from the daily diet of common folk of the ancient states and empires, who after thousands of years ... found themselves consuming almost as little animal protein as the ... citizens of [Aztec] Tenochtitlan. Over a vast region of the Old World corresponding to the former zones of greatest meat and grain production, animal flesh soon became a luxury whose consumption was increasingly restricted to occasions involving ritual sacrifice. Eventually, the consumption of the flesh of the most expensive species came to be forbidden altogether, while in the regions suffering the greatest depletion [of arable land] meat itself became ritually unclean.”<sup>10</sup>

That is, as populations grew and the land needed to feed them became more difficult to cultivate or was being destroyed by salinization from irrigation agriculture, as it was in southern Mesopotamia, the ability of the land to feed these larger populations was stretched to meet these needs. Animals that required the greatest cost to feed in terms of acreage to give them pasture or raise grains to feed them, were therefore the first to stop being raised for food. Those animals that provided other products or services necessary to maintain civilization, such as traction animals (oxen, donkeys, etc.), producers of fibers (sheep), suppliers of milk (cattle, goats, etc.) were not to be banned but were maintained because these valuable products and services that they provided were necessary to a highly urbanized

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<sup>10</sup> Marvin Harris, *Cannibals and Kings: The Origin of Culture* (NY 1977), pp. 129-130

society. In times of great famine or drought they were eaten but only as a last resort. This understanding is especially germane to the pig, as Harris continues:

“Pigs can’t be milked or ridden, can’t herd other animals, pull a plow, or carry cargo, and don’t catch mice. Yet as a supplier of meat the pig is unrivaled: it is one of the most efficient converters of carbohydrates to proteins and fat in the entire animal kingdom. For every 100 pounds of feed consumed, a pig will produce about 20 pounds of meat, while for the same amount of feed cattle produce about 7 pounds. In terms of calories produced per calorie of food, pigs are over three times more efficient than cattle and about two times more efficient than chickens. (Pound for pound, pork has more calories than beef.)”<sup>11</sup>

In spite of the pig being an ideal source of protein for the people of Mesopotamia, it had fundamental drawbacks that ultimately led to it being banned, actually made a taboo there. Harris shows:

“Pig raising incurred costs that posed a threat to the entire subsistence system in the hot semiarid lands of the development of pristine and secondary states throughout the region after 4000 B.C. The pig is essentially a creature of forests, riverbanks, and the edges of swamps. It is physiologically maladapted to high temperature and direct sunlight because it cannot regulate its body temperature without external sources of moisture—it cannot sweat. In its natural forest habitat the pig eats tubers, roots, and fruits and nuts that have fallen to the ground. If it is fed on plants with a high cellulose content, it completely loses its advantage over ruminant species as a converter of plants to meat and fat. Unlike cattle, sheep, goats, donkeys, and horses, hogs cannot metabolize husks, stalks, or fibrous leaves; they are no better than people when it comes to living on grass.

“When the pig was first domesticated, there were extensive forests covering the ... upland zones of the Middle East. But beginning in 7000 B.C. the spread and intensification of mixed farming and herding communities converted millions of acres of Middle Eastern

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<sup>11</sup> *ibid.*, p. 131



forest to grasslands. At the same time millions of acres of grassland were converted to desert.”<sup>12</sup>

Robert J. Wenke, in discussing the pig, reports:

“Another important domesticated animal was the pig. The bones of this animal have been recovered in the thousands from sites all over Southwest Asia by 6000 B.C., and even as late as 2700 B.C. pig bones represent 20 to 30 percent of all animal remains at many large sites. Curiously, however, sometime after 2400 B.C. pork was religiously proscribed in most Mesopotamian cities, as well as in Egypt and elsewhere in Southwest Asia.”<sup>13</sup>

Historians as well as Jewish scholars have, on the other hand, raised the issue that pork was proscribed because it can carry a parasite in its muscles; if eaten by people, this can be deadly. William James Burroughs discussed all of these matters as follows:

“The emergence of food taboos in societies of southwest Asia around 5 to 4 kya [thousand years ago] ... is yet another arena of intense academic speculation and controversy ...

“Nowhere is the question of food taboos more fiercely debated than in respect of the humble pig ... In hotter drier climates, where shade is at a premium, they are less efficient and need their diet ... supplemented by grain ... Still worse they suffer from sunburn and hence need plenty of shade.

“All these factors [outlined by Wenke and Harris] suggest the pig may not be the ideal domesticate for the hot dry climates of the Middle East and northern India ... Nevertheless, many anthropologists do not accept these economic [and ecological] arguments. Instead, the traditional explanation is that the religious proscription of the pig was a matter of public health. Because the pig is often infected by trichinae

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<sup>12</sup> *ibid.*, pp. 131-132

<sup>13</sup> Robert J. Wenke, *Patterns in Prehistory: Mankind's First Three Million Years* (NY/Oxford UK 1980), p. 281

[trichinosis inducing] parasites that can kill people, this is cited as the reason why pigs are banned. ...

“An alternative explanation is that it was a climatic factor that sealed the fate of the pig in the Middle East. This is the [climate] upheaval that occurred in the late third millennium B.C. ... The fact that the timing of this change coincides with the hotter drier climate that [so dated] led to the collapse of the Akkadian Empire may explain the elimination of poor piggy. At a time of heat and drought, the failure of the pig to deliver and worse still its competition for valuable resources that fed humans, provided the last straw. So, while a purely economic argument may not be adequate [alone] to explain the major shift in agricultural practice, when combined with the disaster of prolonged drought this may have made it much easier to attach a religious reason to the demonization of the pig.”<sup>14</sup>

But the Akkadians, as we have shown above, were *alter egos* of the Assyrians who were destroyed well inside the first millennium B.C. This requires that the pig became proscribed in that millennium and not the third. Of special importance, therefore, is the explanation by David M. Sherman that shows the trichinosis public health explanation for the banning of the pig is without substance:

“For veterinarians, the story of Jewish[, Mesopotamian, and Egyptian] aversion to pork is ... of interest. It has been frequently suggested the ancient ... leaders recognized the risk of trichinosis from eating pork and therefore discouraged [its] consumption by religious proscription for public health reasons. This seems highly unlikely. The muscle cysts from [the worm] *trichinella spiralis* in swine muscle would not be readily identifiable in ancient times as a source of the illness. The early signs of infection, which occur within 1 or 2 days of ingestion of larva-infected pork are non-specific signs of [gastro] enteritis, namely diarrhea, nausea and abdominal pain, which could easily be caused by bacterial food poisoning from meat or dairy products [milk, cheese, etc.] of ruminant animals. The clinical signs of muscle involvement in trichinosis do not occur until 1 to 6 weeks post

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<sup>14</sup> William James Burroughs, *Climate Change in Prehistory: The End of the Reign of Chaos* (Cambridge UK 2005), pp. 255-256

ingestion, so it would be difficult to establish a causal relationship between pork consumption and the delayed onset of muscular disease. Finally, most cases of human trichinosis are subclinical [not needing a doctor's treatment for recovery] so clinical disease [requiring a doctor's treatment] would be infrequent enough that it would not likely rank high in people's minds as a major disease concern. In fact, a number of diseases that produce more consistent clinical illness, with shorter incubation periods and higher mortality rates, are readily associated with the religiously ruminant animals [that people ate]. These diseases include brucellosis, anthrax and food poisoning ... Crediting ancient Hebrews[, Mesopotamians, and Egyptians] with formulating public health policy on the basis of an understanding of the transmission of trichinosis is ... unwarranted."<sup>15</sup>

Harris further discusses another reason certain historians refuse to accept the economic/ecologic theory for the ancient proscription of pork:

"Some people feel that ecological cost benefit analysis of pig raising is superfluous because the pig is simply an exceptionally unappetizing creature that eats human excrement and likes to wallow in its own urine and feces. What this approach fails to cope with is that if everyone naturally felt that way the pig would never have been domesticated in the first place, nor would it be eagerly devoured. ... Actually, pigs wallow in their own feces and urine only when they are deprived of alternative sources of the external moisture necessary for cooling their hairless and sweatless bodies. Moreover, the pig is scarcely the only domesticated animal that will given the chance gobble up human excrement (cattle and chickens show little restraint in this regard) [and were not proscribed in ancient times]."<sup>16</sup>

Reading Harris's material on this taboo, which we have only touched on, would be of value to those who are interested in the reasons for the pig being seen as unclean. But the basic question related to this proscription is: When did the prohibition occur in Mesopotamia? Did it occur, as generally presented, around 2400 B.C. or, as suggested by the work in these volumes, some time after 800 to 700

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<sup>15</sup> David M. Sherman, *Tending Animals in the Global Village* (NY 2002), pp. 54-55

<sup>16</sup> Harris, *op.cit.*, p. 133

B.C.? This relates to when Mesopotamian agriculture started to become crippled by irrigation salinization.

If Mesopotamian agriculture was so close to disaster around 2400 B.C. it certainly would not be supplying grain to other cultures. But this could hardly be the case. Even in the first millennium B.C. it was the grain belt or, as we term it for the American Mid-West, the bread basket to the world. Herodotus in the first millennium B.C. presented a picture of the Babylonian plain as a highly productive grain environment. Homer W. Smith describes it thus:

“The climate of Mesopotamia has changed so much that it is difficult to believe that the country was once the paradigm of the Garden of Eden. Where as late as the time of Herodotus there stood the great cities of Ur, Sumer and Babylon, there remain now but hummocks of earth scarcely distinguishable from the surrounding desert of sterile, ever shifting sand, about the most intolerable sites for human habitation of any place on earth. In the hot season the temperature reaches 137° [F.] in the shade, while dust storms, so dense that they may completely hide the sun, envelope the country for six weeks on end ...

“Yet in the days of Abraham, Sumer was one vast granary and Ur and Babylon were competing for the domination of a great and wealthy empire. The countryside was dotted with prosperous hamlets, villages and farms, and rich with crops of vegetables, wheat, barley, sesame and fruits. Along the irrigated land the grass reached a height of fifteen feet [5 meters], while on the surrounding prairie flowers grew in such luxuriance that in the lush season one who walked among them was dusted with their multi-colored pollen. Moist hollows held date palm [trees and there were] cattle, **PIGS** and sheep ... [They had trees such as] fig, apple, almond, apricot and olive ... cypress, tamarisks, acacias and of course grape vine ... Fish and birds were abundant, and dog, ass [donkey] and ox were domesticated and the lion, elephant, panther, wild ass and boar were indigenous inhabitants.”<sup>17</sup>  
(capitalization and bold added)

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<sup>17</sup> Homer W. Smith, *Man and His Gods* (Boston 1952), p. 65

This colorful description of the fecund flora and fauna on the southern Babylonian plain makes it clear that this region was the granary of Mesopotamia. There was plenty of grain to export and many animal sacrifices were carried out at the various temples to appease the gods on a daily basis.

When did the prohibition of pork become invested with religious/legal sanction of the state? In this volume, above, we were able to show that toward the end of the Persian era there had been a rise in the cost of food and land, eventually leading to famine-like conditions. This was documented for the Persian period during its rule over Babylonia or that of Hammurabi's Old Babylonian empire. This was further documented for the Persian period of rule in Assyria or that of the Neo-Assyrian era. In fact, we were able to show that the attested 30 and 29 day lunar months of Hammurabi were identical to those of Darius the Great and that the same held for the Old Babylonian king Ammizaduqa and Artaxerxes III Ochus. We did show that the astronomical data attributed to the Neo-Assyrian kings and eponyms also correlated far, far better with the Persian and post-Persian/Neo-Babylonian kings. And we offered many other aspects that these cultures had in common.

Therefore the time when the pig was proscribed occurred when it began to become prohibitively expensive to raise pigs, when food shortages were beginning and growing prohibitively and to maintain the pigs meant starvation for the people. This did not occur prior to the first millennium B.C. Thus we would expect to find pork being eaten up until this period, although this diminution in the use of pork occurred gradually at first. According to Wolfram von Soden:

“Pork was not generally prohibited as unclean in Babylonia and Assyria, in contrast to Israel, and pork as meat was used just as widely as was pork lard, although its consumption was not as common as that of mutton. In documents from the first millennium, however, the hog is mentioned so seldom that we can conclude that by this time there must have been only minimal consumption of pork. How hogs were kept has yet to be studied.”<sup>18</sup>

In fact pigs were being raised during Hammurabi/Darius the Great's time. Carole M. Counihan and Penny Van Esterik show: “While pigs continued to

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<sup>18</sup> Wolfram von Soden, *The Ancient Orient: An Introduction to the Study of the Ancient Near East*, Donald G. Schley, transl. (Grand Rapids MI 1994), p. 94

be raised during Hammurabi's reign ... they virtually disappear from Mesopotamia's archaeological and historical record thereafter."<sup>19</sup> That is, the pig was being raised in early Persian times in Babylonia. Moving the Neo-Assyrians and Old Babylonians down into Persian times correlates with the growth of inflation of food costs, land costs, interest rates, soil salinization and the prohibition of pork. Pig was eaten in Sumerian/Chaldean and Assyrian/Akkadian times up until Persian times when the climate caused all these problems of food costs. The hiatus does not exist.

All this evidence integrates, correlates, corroborates, and converges to illustrate that the prohibition of the pig in Mesopotamia occurred well after the 8th century B.C. and in fact in the Persian era. Because of a climate shift, rain with some irrigation agriculture gave way to intensive irrigation agriculture which caused the Babylonian plain, at first, to become the great granary of the region. This irrigation agriculture led to the land becoming saline, inevitably leading to there being less and less land available to grow grain. The cost of food rose as less of it was grown. The price of land rose as less of it was available for agriculture. This led to inflation, etc., and ultimately some time during this period to the prohibition of the pig.

Since the pig only became proscribed during the first millennium B.C. and not the third, the so-called Sumerians, who are the Chaldeans, must also be moved to that time. There is neither scientific nor technological evidence for civilization before the end of the second millennium B.C. and there is none for the third. The chronology of the pig in Mesopotamia fits well with all the other scientific/technological evidence and is yet another support for Heinsohn's and Sweeney's short chronologies.

The pig, thus, is a kind of marker that can inform us about the climatic conditions of a region in the Old World. Where it was raised and used for food gives a good indication of the climate.

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<sup>19</sup> Carole M. Counihan and Penny Van Esterik, *Food and Culture: A Reader* (NY 1997), p. 77

## HARAPPAN CIVILIZATION AND CLIMATE, AND CHRONOLOGY

According to the evidence presented above and in volume I of this series, Harappan civilization did not flourish from about 2400 to 1600 B.C. Nevertheless, there are other basic forms of scientific evidence that further contradict the orthodox long chronology which must now be explored. The first form of evidence has to do with climate and the feeding of the huge population on the Indus plain. Harappan civilization was enormous in size. According to Hubert H. Lamb, “at its height the Indus civilization cultivated an area greater than the Nile valley and Mesopotamian civilization combined.”<sup>20</sup> “Rafique Mrigal noted that ‘the area covered by Indus Civilization was larger than any known civilizations of the ancient world’.”<sup>21</sup>

Yet according to the accepted climatological chronology the Harappan region had dried out and had become a desert around 2300–2200 B.C. That is, Harappan civilization supposedly thrived for about 800 or more years in a region where practically nothing grows. This is the paradox or, more aptly, the contradiction. The aridification of much of the ancient Near East is assumed to have occurred ca. 2250 B.C., but on the Indus plain a great civilization flourished in a desert. The contradiction is striking and the literature employed to defend that chronology is equally striking.

Let us begin with the evidence that the Harappans were growing summer crops in a region that is quite arid today and is assumed to have been so as far back as the Harappan civilization flourished. Summer crops are those that are planted by sowing seeds in the spring when in other areas of dry farming, or farming dependent on summer rainfall, warm weather allows the proper day-length period to trigger the seeds to germinate from their dormant state. They grow, produce stalks, leaves, then blossoms that turn to food crops for harvesting in fall.

Above all this is the need for farmers to keep a certain amount of the crop for seed for the next year. Therefore, the crop must be sown early enough, have sufficient water and warmth to thrive. D.K. Salunke and S.S. Kadem, speaking about

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<sup>20</sup> Hubert H. Lamb, *Climate, History and the Modern World* (London/NY 1995), p. 117

<sup>21</sup> *ibid.*

cabbage agriculture which applies to other summer crops, claim “It is, therefore, of the utmost importance that a particular variety [of a crop] be planted at the right time, otherwise the crop will produce seeds without forming [edible parts] ...”<sup>22</sup> The reverse can also occur. One can grow leaves that are edible or other parts of the plant but fail to produce seeds. This is outlined for us by Frank B. Salisbury:

“The real breakthrough [regarding flowering and seed production for summer and winter crops] was the work of ... Wrightman W. Garner and Henry A. Allard. These scientists were faced with a practical problem plus another interesting observation that they could not explain. The problem was a new [type] of tobacco ... This grew to great heights in the summer fields but did not flower, so it could not produce seeds for the [next year’s] planting of tobacco crops. When the plants were reproduced vegetatively from cuttings and maintained in the winter greenhouse, they flowered profusely even when they were quite small. The observation that puzzled Garner and Allard was that a [type] of soybeans ... that they were working with planted at various times from early spring to midsummer in the fields all flowered at the same time. That is, the plants planted in early spring were very large by the time they flowered, but the plants sown in early summer were very small at the time of flowering. It seemed obvious to Garner and Allard that some factor in the environment was responsible for these two phenomena. How did the winter greenhouse differ from the summer fields? Of course they [the greenhouses] differed in light intensity but also in temperature and even humidity. Soil factors might also have played a role. Various things were changing throughout the seasons including temperature and light intensity in the summer soybean fields. They set out to discover the factor that would account for the production of flowers [from which seeds could be derived]. Of course they were aware of day-length changes but this seemed to be quite an unlikely possibility. They carefully tested all the other factors that they could think of but to no avail. Finally almost in desperation as Allard related ... they tested the effect of the day length. The results

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<sup>22</sup> D.K. Salunke, S.S. Kadem, *Handbook of Vegetable Science and Technology* (NY/Basel, Switzerland 1998), p. 325



were unmistakably clear ... The quantity of the light that they received didn't seem to make much difference ...”<sup>23</sup>

Let us be sure we fully understand this. Summer crop seeds must be planted early enough in the spring to germinate, grow mature etc., to produce the part of the plant that is eaten, but of great importance as well, the plant must be able to produce flowers that generate seeds for the following growing season. If a summer crop is planted after the flowering time it will not produce seeds for the next year.

Given the present conditions on the Indus plain there is insufficient rainfall to carry on agriculture. I.M. Drakonoff and Igor Mikhailovich Diakonov state directly: “Most of the Indian territory [including southern Pakistan] receives over 500 millimeters of precipitation annually, mainly during the summer; a total that is inadequate for dry farming.”<sup>24</sup>

The question therefore is: How did the ancient Harappans grow summer crops in a desert? Throughout the summer growing season these crops would need water. But where did this necessary water come from? Nimal Tej Singh puts it simply: “Irrigation was needed for raising summer crops.”<sup>25</sup> In this case it is assumed that irrigation was employed to permit these crops to grow. The only canals found for irrigation for the most ancient period of Harappan civilization are in the far northern part of this region. According to Dilip K. Chakrabarti *et al.*, citing R.S. Bisht, in the proto-historic period, people

“... apparently dug canals for irrigating fields and storing water in large ponds. The existence of a network of abandoned canals and river beds dotted among the numerous Chalcolithic [Copper Age] and Early Iron Age sites in the Sarasvati [River] valley ... bears testimony to this.”<sup>26</sup>

Henri P. Frankfort also discussed these canals:

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<sup>23</sup> Frank B. Salisbury, “Biological Clocks,” *Discoveries in Plant Biology*, vol. I, Sham-Dow Kung, Shang-Fa Yang, eds. (Singapore/London 2005), pp. 304-305

<sup>24</sup> Drakonoff and Diakonov, *op.cit.*, p. 215

<sup>25</sup> Nimal Tej Singh, *Irrigation and Soil Salinity in the Indian Subcontinent: Past and Present* (Cranbury NJ 2005), p. 64

<sup>26</sup> Dilip K. Chakrabarti *et al.*, *India: An Archeological History* (Oxford UK 1999), p. 162

“The canals are slightly carbonated depressions about 1 m[eter or 40 inches] deep and 300-500 m[eters or 330-550 yards] wide. They mark the course of ancient natural waterways which were used and perhaps in some places rerouted by man. These traces of small river channels ... appear to have reached all the archaeological sites ... Not only did they supply ... water but at the same time they made the surrounding area more fertile than the [southern] natural [Indus] plain ... The picture we are left with is that of a small-scale system, less impressive than the great hydraulic [irrigation] works of Mesopotamia ... during the same period.”<sup>27</sup>

Importantly, Chakrabarti adds that these findings are “valid only in the context of the Indo-Gangetic divide,” but as for the Indus plain, he says, “No archaeological evidence of irrigation has been looked for in this region.”<sup>28</sup> This small-scale evidence of irrigation outside the Indus plain has nevertheless led many historians to claim that irrigation agriculture did indeed enable the Harappans to thrive in this desert. While this earlier small-scale irrigation is known, it is then posited to have been employed on the entire Indus plain. “The Indus River through a network of vanished canals must have provided a method of irrigation.”<sup>29</sup>

Laurence E.O. Smith writes:

“About 4000 years ago, the Indus river civilization that flourished for 1000 years at Mohenjodaro in what is now Sindh province came to an abrupt end. Some archaeologists have speculated that prolonged irrigation without drainage had caused the water table to rise.”<sup>30</sup>

Saggs also suggests irrigation was used. “Harappan civilization thrived by a combination of agriculture and trade ... [and fell] by making the fields saline

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<sup>27</sup> *ibid.* See also Henri P. Frankfort, “Evidence for Harappan Irrigation System in Haryana and Rajasthan,” *Eastern Anthropologist*, vol. 45 (1999), pp. 87-103

<sup>28</sup> *ibid.*

<sup>29</sup> *Rosicrucian Digest 1950* (2004), p. 125

<sup>30</sup> Laurence E.D. Smith, “Drainage Needs in the Lower Indus, Pakistan,” *Water for Agriculture: Irrigation Economics in International Perspective*, Stephen Merrett ed. (London/NY 2002), p.

by over-irrigation without adequate drainage.”<sup>31</sup> At this point I ask the reader to note the contradiction regarding irrigation of the Harappan civilization and that in Mesopotamia. While Mesopotamian civilization is assumed to have carried on irrigation for over 3000 years before its soil was poisoned by salt, Harappan civilization, that covered an area much, much larger than all of Mesopotamia—not just the southern Babylonian plain—was destroyed by salinization of its soil in about 800 years. In fact, as pointed out in volume I of *Pillars of the Past*, the estimate offered sets the time limit for irrigation on the Babylonian plain at 400 years *at best*.

The fact of the matter is that Harappan civilization did not utilize irrigation agriculture at all. John W. Bennett reports “at the time of writing [this book, 2002] I did not find evidence that this Bronze Age Indus River Valley civilization attempted to divert or control floodwater upstream ...”<sup>32</sup>

Special space satellites can map the sub-surface of the land with a form of radar that can see below the surface to discover and determine if relic irrigation canals are buried beneath that surface. Yet K. Shadanan Nair informs us “Satellite images ... show channels of water [riverbeds, not irrigation canals] in northern and western India that disappeared long ago.”<sup>33</sup> Yet the ancient Indus area contains “numerous reservoirs and wells.”<sup>34</sup> Charles Keith Maisels succinctly writes that during Harappan times “there is no evidence of artificial irrigation....”<sup>35</sup>

Abraham Eraly reinforces this:

“The challenge that the Indus people faced was of taming the [ancient] rivers [that have disappeared during flooding episodes] not of water scarcity. To deal with this problem they, we should assume, would have set up, with their usual efficiency and skill, a system of dikes and canals. We have, however, no actual evidence of any irrigation works.”<sup>36</sup>

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<sup>31</sup> Saggs, *Civilization Before Greece and Rome*, *op.cit.*, p. 13

<sup>32</sup> John W. Bennett, *Human Ecology as Human Behavior*, 2nd ed. (Brunswick NJ 2002), p. 208

<sup>33</sup> K. Shadanan Nair, “Role of Water in the Development of Civilization in India—A Review of the Ancient Literature, Traditional Practices and Beliefs,” *The Basis of Civilization. Water Science?* John C. Rodda, Lucio Ubertini, eds. (Wallingford, Oxfordshire UK 2004), p. 161

<sup>34</sup> *ibid.*

<sup>35</sup> Charles Keith Maisels, *Early Civilizations of the Old World* (London 1999), p. 381

<sup>36</sup> Abraham Eraly, *Gem in the Lotus: The Seeding of Indian Civilization* (London 2000), p. 30

Peter N. Peregrine and Melvin Ember in the *Encyclopedia of Prehistory* (New Haven CT 2002), p. 119, in discussing Harappan civilization, state explicitly, “There is no evidence for irrigation systems in the Indus Valley proper.”

How then could farmers raise summer crops in a region with insufficient rainfall to water them without irrigation? Since this is an impossibility, they not only couldn’t feed themselves but also couldn’t produce seeds for the next season when the crops would again fail.

One of the reasons it was believed that irrigation agriculture was employed was the discovery of dams or “gabarbands”, built supposedly to impound water in reservoirs to be used in the imagined canals for the crops. But this concept was shown to be false, as Hermann Kulke and Dietmar Rothemund write:

“Before venturing into the open plains of the lower Indus the precursors of the Indus civilization experimented with cultivating alluvial lands on a small scale in the valleys of Baluchistan. There they built stone walls (gabarbands) which retained the sediments of the annual inundation. Initially the archaeologists mistook these walls for dams, built for irrigation, but the holes in the walls showed that they were designed so as to retain soil but not water. ...”<sup>37</sup>

There is no evidence for irrigation agriculture at all. That being the case, another supposition has been advanced to allow agriculture to continue on the Indus plain. Colin Spencer suggests the Harappans were able to grow crops because of “the annual flooding of the Indus [or other rivers] as neither ploughing, manuring nor irrigation were [used].”<sup>38</sup> However, these inundations occur in the early summer season and flood the region into the fall season. S. Ali Awan explains that the Indus flood season runs from about “15 June to 15 October”<sup>39</sup> Thus, there could be planting of fall/winter growing crops or summer crops that could thrive in saturated soil or under water, but no seeds would form. Peregrine and Ember suggest:

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<sup>37</sup> Hermann Kulke and Dietmar Rothemund, *A History of India*, 4th ed. (NY 2004), p. 2

<sup>38</sup> Colin Spencer, *The Heretic’s Feast* (Hanover NH 1995), p. 71

<sup>39</sup> Shaukat Ali Awan, “Weather radar distributed hydrological modelling: a case study in the Indus Basin,” *Weather Radar Information and Distributed Hydrological Modelling* (IAHS Publications 2003), p. 30

“Throughout the Indus Valley ..., the predominant grain crop would have been planted in the fall and harvested in the spring or early spring. With sufficient winter rain there rabi, or spring [harvested] crops, would not have needed irrigation ... The main summer or monsoon crops ... are rice, sorghum and various millets. Cotton[, barley, peas and melons] would have been planted on higher ground during the monsoon or in drained land at the end of the monsoon flooding.”<sup>40</sup>

The reason these summer crops would have to have been planted on “higher ground” is that they, unlike certain crops—rice, sorghum, and various millets—cannot grow with floodwaters covering the land during the growing season. Nevertheless, Peter S. Bellwood makes it quite clear that even certain types of millet and sorghum are not able to grow under summer floodwaters nor others as well:

“The two main millet species of sorghum ... which appear in the late Harappan [period] around 2000 B.C. ... are both highly productive summer crops; well attuned to the drought conditions that frequently occur in monsoonal areas ...

“The domesticated food plants native to the subcontinent are all summer crops, such as legumes. ...”<sup>41</sup>

Are there any areas of higher ground on the Indus plain to grow crops that cannot survive under water during the summer months? The Indus plain is exceedingly flat. Gopal Singh writes that “These are uniformly level plains without end.”<sup>42</sup> Syad Muhammad Latif speaks of the “unbroken flatness of the [Indus plain] surface.”<sup>43</sup> Ronald Frederick Delderfield, who spent some of his life in this region, in his magnificent novel *God is an Englishman* (NY 1970), p. 69, describes the plain thus: “The country fell away to a grayish plain of uniform flatness.”

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<sup>40</sup> Peregrine and Ember, *op.cit.*, pp. 274-275

<sup>41</sup> Peter S. Bellwood, *First Farmers: The Origins of Agricultural Societies* (London 2004), p. 88

<sup>42</sup> Gopal Singh, *A Geography of India* (Atma Ram India 1976), p. 9

<sup>43</sup> Syad Muhammad Latif, *History of the Punjab from the Remotest Antiquity to the Present Time* (Eurasian Publ. House 1964), p. 12

Therefore, any major flood would cover the entire plain and make the summer crop seeds unable to grow and to germinate, except rice or some other hydrophilic crops. R.H. Meadow speaks of

“The ‘millets’ could be planted immediately after summer [flooding] in frost-free areas, [and having] winter floods throughout the Greater Indus Valley, thus enabling farmers to take advantage where wheat and barley would have only marginal chances of success. Similarly rice cultivation could be carried out in areas [during the summer] previously too wet for cereal agriculture.”<sup>44</sup>

It is being admitted here that certain “cereal agriculture” cannot grow during the summer season in a region that is “too wet.” However, there is a great contradiction that no one, so far as I have read, has dealt with, namely that there was no flooding of the Indus plain for much of Harappan history. It is pointed out by John Keay that it was only after Harappan civilization had flourished for hundreds of years, some time

“... after 2000 B.C. [in the thesis I present, 800 B.C.], according to the archaeologists, came the floods. If they did not actually overwhelm this precocious civilization, they certainly [over time] obliterated it. In time, layer after layer of Indus mud, possibly wind-blown as well as water-borne, choked the streets ... and piled high above the rooftops. The ground rose (gradually over time) by ten meters [32 feet] and the water table followed it.”<sup>45</sup>

When, then, was the time that summer crops would thrive on the Indus plain? Only after the floods began could they be planted in water-saturated or flooded soil. Maisels states, “important summer-growth crops—rice ... and finger millet ...—are not recorded [in the strata] in the Indus Valley proper until the late Harappan period.”<sup>46</sup> Prior to the onset of the flooding period—after 2000 B.C.—there were no annual floods to grow either summer crops or fall/winter crops. How, then, during this earlier period, could they grow any crops at all? The Harappans had no irrigation canals to water their crops at this time and they did not experience

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<sup>44</sup> R.H. Meadow in Maisels, *Early Civilizations of the Old World*, *op.cit.*, p. 255

<sup>45</sup> John Keay, *India: A History* (NY 2000), p. 5

<sup>46</sup> Maisels, *loc.cit.*

annual floods to allow for either water-loving summer food crops or winter crops dependent on winter rain. If there were never any winter rains because they do not, or rarely, show up in the archaeological record, then a great civilization on the Indus plain is clearly impossible before the floods and barely possible after they began.

Clearly, the Harappans were growing summer and winter crops prior to the onset of the floods and to do so demands water. The only way that the Harappans could grow summer and winter crops, without irrigation or annual summer floods, is via rainfall. That is, there had to be sufficient annual rainfall to allow the growth of these different seasonal crops. What is the evidence for this? Stuart Piggott comments:

“The abundant evidence of ancient occupation in the Baluchi hills or the Indus plain implies less exacting conditions in the past than at present, and though historical evidence implies that by the time of Alexander [the Great] conditions in Baluchistan approximated to those of today yet ... there is good evidence for heavier rainfall and extensive forests in the Indus Valley in ancient times.”<sup>47</sup>

One form of evidence relates to wooden building material used for the construction of great granaries at Harappa, Mohenjo-Daro, and probably at other major towns. According to Jacquetta Hawkes, at Mohenjo-Daro, there is a:

“... huge granary ... It was a heavy timber building 150 feet long by 75 feet wide ... [It] differs from the Harappan granary, which is [divided by wooden partitions into] twelve quite distinct sections, though the total storage area is about the same.”<sup>48</sup>

Furthermore, Mohenjo-Daro, Harappa and the other major towns were constructed of kiln-baked brick while small towns and villages employed sundried brick.<sup>49</sup> Gregory L. Possehl has claimed that the riverine forests could have supplied sufficient wood to manufacture the millions of bricks that were used to build these

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<sup>47</sup> Stuart Piggott, *Prehistoric India* (Baltimore MD 1961), quoted in Donovan A. Courville, *The Exodus Problem and Its Ramifications*, vol. I (Loma Linda CA 1971), p. 159

<sup>48</sup> Hawkes, *op.cit.*, p. 275

<sup>49</sup> Charles Gates, *Ancient Cities: The Archaeology of Urban Life in the Ancient East and Egypt, Greece and Rome* (London/NY 2003)

great cities, and in fact rebuild them several times after they were inundated by floods as well as supply timber for huge granaries which also had to be repeatedly rebuilt.<sup>50</sup> As Keay reports, “Indus [flood] mud ... choked the streets, rotted the timbers” of these cities.<sup>51</sup>

Here then is the conundrum. Supposedly only after 2000 B.C. the floods began to do their damage. Thus about 200 years after they began, the cities had to be rebuilt about ten times. That means every 20 years enough trees had to re-grow along the Indus river, but not in the many other river systems which stopped flowing after 2000 B.C. This, as we will see, is the time when the entire region fell into decay and was being abandoned. Why were these people rebuilding cities that they were abandoning? This strongly implies the cities were being rebuilt even before the onset of the floods. But let us return to the climatic conditions of the ancient Indus plain. Nigel Calder reports:

“Discoveries of fossil pollen showed that an area in north-west India at the edge of the Harappan region, which is now arid, was formerly a land of rich vegetation.

“An expert in fossil pollen, Gurdip Singh, from the Institute of Paleobotany in Lucknow, investigated salt lakes in north-west India and found they were formerly fresh-water lakes in the midst of richly vegetated land. The most interesting of the lakes in Lunkaransar is near Bikaner, deep in the Great Sand Desert of Rajasthan. Here, today, the hot, moist wind of summer monsoon delivers scarcely any rain; instead it piles up drifts of sand and dunes [which] march slowly across the countryside. The vegetation is sparse. But dig just a few feet through the salt of Lunkaransar and you come to neat layers of mud, laid down when the lake carried fresh water four thousand years ago [2000 B.C.]. And in the layers Singh found pollen of bulrushes and sedges. The lake also collected [in its muddy bottom layers] from the surrounding land, pollen of grass, jamun trees, mimosa and many other species. Jamun trees need at least 20 inches of rainfall a year ...”<sup>52</sup>

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<sup>50</sup> Gregory L. Possehl, *Ancient Cities of the Indus* (Durham NC 1979), p. 231

<sup>51</sup> Keay, *op.cit.*, p. 5

<sup>52</sup> Nigel Calder, *The Weather Machine* (NY 1974), p. 40



The amount of rainfall in the Indus valley region allowed the farmers to raise abundant crops and husband various animals:

“The Harappans grew, among other crops, wheat and barley, peas, melons, and on some of the seals there are representations of what may have been banana trees. They were also [live] stock farmers raising sheep and cattle, **PIGS** and goats. The camel appears to have been known, also the buffalo, the Indian bison, the bear and spotted deer.”<sup>53</sup> (capitalization and bold added)

Walter Ashlin Fairservis also reports:

“The majority of the seal tablets ... have a main motif of a bovid animal ... a ‘Unicorn Bull’ ... Other animals ... include zebu, ... the gaur [a large South and Southwest Asian wild bull] ..., the water buffalo ..., the tiger, ... the elephant ..., the rhinoceros, ... the goat or ibex ...”<sup>54</sup>

These bovids are grass eaters and do not live in deserts. Tigers that hunt these animals or others do not live in deserts or near deserts, nor do buffalo. These species tend to live in forests with grasslands which, given the assumed arid climatic conditions of the Indus plain, would never have created what the historians claim were the desiccated conditions during Harappan times. Interestingly, in Gabel’s *Man Before History*, cited above, we were informed that the Harappans raised and ate “pigs.” Mano Sharma, speaking of this civilization, writes “The chief domestic animals were the bull, sheep, elephant, **PIG** [etc.] ...”<sup>55</sup> (capitalization and bold added)

As was shown earlier, the pig was prohibited in Mesopotamia sometime after the 8th century B.C. because of soil salinization and climatic conditions that made it too costly to be raised for food. In Harappan civilization the pig was evidently plentiful, being one of its “chief domestic animals.” It required shade from trees, shrubs etc., and there was obviously plenty of grain to feed it. This strongly

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<sup>53</sup> Creighton Gabel, ed., *Man Before History* (Englewood Cliffs NJ 1964), p. 97

<sup>54</sup> Walter Ashlin Fairservis, *The Harappan Civilization and Its Writings: A Model for the Decipherment of the Indus Script* (Leiden, the Netherlands/NY etc. 1992), *op.cit.*, p. 5

<sup>55</sup> Mano Sharma, *History of World Civilization* (New Delhi, India 2005), p. 14

suggests the fall of Harappa also occurred after the 8th century B.C. Forbes further explains:

“It is certainly unlikely that the Aryans who [are assumed to have] invaded India about 1500 B.C. brought the art of irrigation to India. Though the excellent drainage and sewer system of the Indus cities was thoroughly studied no further data on the irrigation system that supported them are known ... hardly any traces of ancient canals were found. This apparent lack of any preparation for dry seasons together with the representation of forest-loving animals on the seals seems to point to the view that the rainfall in Sind and Punjab must have been much heavier.”<sup>56</sup>

Now, we were told that the Harappans, like the ancient Minoans, had drainage and sewer systems that had to be flushed constantly. But how could they do that without water canals leading into the cities to bring the water to flush away the human wastes of the inhabitants? Prior to the floods—before 2000 B.C.—there would be no water to cleanse these sewer drainage systems and after the floods began the sewers would be flushed for only one season and fill up with human waste during the fall, winter and spring. This would create a horrible stench, but worse, it would be a breeding ground for pestilence from insects and bacteria breeding in those wastes. But high annual rainfall would permit forests to grow as well as grasslands for all these domesticated and wild animals and allow these drainage systems to be flushed. Speaking of the “climate situation between [about] 3000 and 1000 B.C.,” climatologist H.H. Lamb shows that the climate

“... differed from today ... about which the evidence seems strong enough to speak with assurance ...

“In subtropical latitudes, especially between 30 and 40°N [the latitudes of the Mesopotamian and Harappan civilizations], there was probably somewhat more rainfall than now, and perhaps a less marked division of the year into dry and rainy seasons, though [there were] long-term rainfall fluctuations involving runs of drought years separated by long periods with more frequent rains ...”

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<sup>56</sup> Forbes, *Studies in Ancient Technology, op.cit.*, pp. 12-13

“The equatorial rains seem to have ranged rather farther north than now in their seasonal migration ...

“In consequence of the differences, there was more vegetation than now in the present arid regions, which even included forest in some areas that are now desert.”<sup>57</sup>

This brings us ultimately to the timing of this climate shift. Here, then, is the problem. The Harappan civilization and that of southern Mesopotamian plain lie at about the same latitude, as Lamb has just informed us. Harappa is located at about the latitude of Baghdad while the latitude of Mohenjo-Daro is slightly south of that of the city of Ur by some 100 miles [160 km]. They are both located north of the Indian Ocean. Therefore they would and should share similar climate conditions during the same periods of time:

“The region in which the Harappan communities were located had the same arid zone characteristics found in other parts of this vast [ancient] economic system which extends to Mesopotamia.”<sup>58</sup>

The contradiction, based on the established chronology, is that instead of both the Harappans and Mesopotamians sharing the same climate at the same time, each had two very distinct climates. While southern Mesopotamia was forced to turn to irrigation agriculture to maintain itself all through its existence, at the same time the Harappans had sufficient rainfall to cultivate great abundances of crops, had forests inhabited by forest-living animals, etc. This simply cannot be! Both civilizations shared very similar climates at the same time and therefore, when the Harappans had abundant rainfall, so too did the Mesopotamians! Given that trade in certain goods ties these civilizations together in time, and all the scientific/technological evidence indicates that the chronology of Mesopotamia is no older than 1300 B.C., Harappan civilization can be no older than Mesopotamian civilization. The great cities on the southern Babylonian plain are abandoned during the Persian era, perhaps 50 to 75 years before the coming of Alexander the Great, because of irrigation salinization of the soil. That requires that about 400 years earlier the southern Babylonian plain had a climate shift which had to have occurred

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<sup>57</sup> H.H. Lamb, “Climate Vegetation and Forest Limits,” *The Place of Astronomy in the Ancient World*, F.R. Hodson ed. (Oxford UK 1974), p. 226

<sup>58</sup> *Ecology and the World System*, Walter L. Goldfrank *et al.*, eds. (London/NY 1999), p. 94

around 800 to 750 B.C. which allowed only irrigation agriculture to continue there for about 400 years. The climate shift for the Harappan civilization had to occur at the same time as in Mesopotamia. In this way both civilizations down to about 800-750 B.C. had rainfall to grow crops and thereafter Mesopotamia developed irrigation which the Harappans never did.

There was irrigation in Mesopotamia prior to 800 B.C. because there is evidence for it in the Uruk period which we place in the mid to latter part of the second millennium. But because of the greater rainfall the Tigris and Euphrates rivers flowed much more rapidly and the Mesopotamians did not build levees. Therefore the water table as in ancient Egypt would only rise during the summer season and any canals or irrigation ditches dug from these rivers would only fill during this time. However, when the river levels fell, so too would the water table, so that irrigating the land would flush away the salts as it did in Egypt. The Egyptians have irrigated their land for thousands of years, but because the river level falls, so too does the water table with it, and thus they had no problem with salinization. Until around 800-750 B.C. a very similar process occurred in southern Mesopotamia.

Thereafter, with that climate shift, the amount of rainfall on Mesopotamia and its northern mountain ranges diminished. The rivers flowed more slowly, levees were built up and with the rivers raised above the plain, water constantly kept seeping from them down into the soil, raising the water table. Irrigation became a necessity with less rainfall, and gradually the soil began to become poisoned with salt. None of the processes just described are contrary to the rules of science, agronomy, or climatology.

The evidence in this unit precludes that the Sumerian/Chaldean or Harappan civilizations could have existed in the late third to the early second millennium B.C. There is no scientific or technological evidence to support the thesis that these civilizations existed then; but there is clear-cut scientific and technological evidence that they existed in the first millennium B.C. As with the evidence that the civilizations were placed in the early to late second millennium B.C. by the historians and archaeologists, no such forensic historical evidence is known for their existence at the earlier period, either.

## A THOUSAND YEAR HARAPPAN TO HINDU DARK AGE

As with the Mycenaean Greeks that supposedly disappeared around 1200 B.C. and were followed by about a 450 to 500 year Dark Age, Harappan civilization which supposedly crashed around 1700 B.C., or 500 years before the Greeks, had an additional 500 or so years added to their Dark Age. Hindu civilization began around 600 B.C., thus there was a thousand year Dark Age between the fall of the Harappans and the rise of their Hindu followers. Kulke and Rothmund point out that the collapse was somewhat sudden in some places and slower in others:

“Recent research has ... helped to explain the reasons for [the Indus civilization’s] sudden decline. All excavations support the conclusion that this decline occurred rather suddenly ... The excavations have revealed many striking symptoms of endogenous decay in those cities during the Late Harappan period. Some settlements seem to have been abandoned rather suddenly, which would explain why kitchen utensils have been found scattered around fireplaces. Other places were resettled for a short period in a rather rudimentary fashion, before they [too] were finally abandoned. The archaeologists call this the squatter period because there was no planning any longer, broken bricks were used for construction and no attention was paid to a proper sewerage system.”<sup>59</sup>

Nevertheless, a Dark Age ensued after the collapse of the Harappans. Maisels speaks of the “Post-Harappan ‘Dark Age’ ...”<sup>60</sup> David Frawley also states:

“Another proof of the difference between the Indus [Harappan] valley culture and later Hinduism was the lack of any urban archaeological sites in India from the time of the Indus to that of the urban age of later ancient India as found by the Greeks.”<sup>61</sup>

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<sup>59</sup> Kulke and Rothmund, *op.cit.*, p. 29

<sup>60</sup> Maisels, *op.cit.*, p. 249

<sup>61</sup> David Frawley, *Gods, Sages and Kings: Vedic Secrets of Ancient Civilization* (Twin Lakes WI 2000), p. 253

Wolpert dramatically explains the elements that were absent from the civilization that followed the Harappans and was, with all the rest of the historians and archaeologists, forced to assume the later people

“... lived in tribal villages with their herds. Their houses fashioned of bamboo have not survived the ravages of time, they baked no brick, built no elaborate baths or sewer systems, created no magnificent statues or even modest figurines, they had no seals or writing, no faience art, no splendid homes. ...”<sup>62</sup>

As we have so often shown, Dark Ages are a direct outcome necessary to salvage a dysfunctional chronology. They serve to fill in the gaps created by that chronology and have no evidence to prove they exist except the fact that there is no evidence for the periods into which the established chronology places them. We are expected to believe that for a thousand or more years, the remnants of a highly civilized people were totally incapable of relearning and rebuilding all that existed prior to their moving from their homeland.

The contradiction, as that of the Dark Age of ancient Greece and elsewhere, is that after the long Dark Age ended the people began to use items and build in a similar fashion to the period that preceded the dark period. Georg Feuerstein, Subhash Kak, and David Frawley explain:

“There is a striking cultural continuity between the archaeological artifacts of the Indus-Sarasvati [Harappan] civilization and subsequent Hindu society and culture. This continuity is evident in the religious ideas, arts, crafts, architecture, writing style, and the system of weights and measures. How can we explain this if the Sanskrit-speaking ... supposedly foreign invaders ... leveled the native civilization of the Indus Valley? The suggestion, made by some scholars, that the [invaders] adopted lock, stock and barrel the culture of the Indus people is equally preposterous because in that case the [invaders] would presumably have adopted the native language or languages as well. [This is identical to the Amorites who supposedly adopted lock, stock and barrel the entire physical, native culture of the Babylonians and their language. It is also identical to the Hittites who

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<sup>62</sup> Stanley Wolpert, *A New History of India*, 5th ed. (NY/Oxford UK 1997), p. 25

supposedly adopted lock, stock and barrel the entire physical native culture of the Anatolians and their language and languages.] This position is similar to the fanciful creationist belief that when God placed the first human beings on Earth, God also simultaneously created fossil evidence that now misleads evolutionists ...”<sup>63</sup>

The authors bring forth a further form of evidence that supports the fact that there was continuity between the Harappans and even modern India:

“In addition to the cultural continuity between ancient and modern India, there is also a striking racial continuity. The excavations at Harappa brought to light skulls belonging to members of various racial groups—all of which are still present in India today ... There is no evidence that a new race intruded into north India during Harappan times and that the Dravidian inhabitants of the region were driven to the south. Rather all the facts point to the continuity of the same people ...”<sup>64</sup>

Sir Mortimer Wheeler wondered about the connection between the Harappan civilization and that of the later Hindu who came a thousand years later:

“Did all the Harappans represented perish ... It may be that the continuity which ... seems to have characterized the Saurashtrian extension of the [Indus] civilization ... contains the explanation. Otherwise we are left with the paradox that the Indus civilization transmitted to its successors a metaphysics that endured, while it failed to utterly to transmit ... from its primary [Harappan] homeland the physical civilization which is its present monument.”<sup>65</sup>

Feuerstein, Kak, and Frawley admit “Later art shows no break with the Indus ... art.”<sup>66</sup> Not everything was highly alike but there are many striking similarities that exist between the two cultures. Although the Harappan script has

<sup>63</sup> Georg Feuerstein, Subhash Kak, David Frawley, *In Search of the Cradle of Civilization* (Wheaton IL 1995), p. 156

<sup>64</sup> *ibid.*, pp. 157-158

<sup>65</sup> Sir Mortimer Wheeler, quoted in William Kirk, “The Role of India in the Diffusion of Early Cultures,” *Geographical Journal*, vol. 141, no. 1 (March 1975), p. 19

<sup>66</sup> Feuerstein *et al*, *op.cit.*, p. 97

not been translated, there are most definitely clear similarities between that employed by the Harappans and that of the later Brahmins. Brahmi script is the earliest known script for writing Sanskrit, used from ca. 500 B.C. The authors further show:

“In Brahmi, as in later scripts, each letter represents a consonant combined with the vowel *a*. Combinations with other vowels are represented by the use of distinctive marks which modify the basic sign. Two consonants together were expressed by placing the signs for the two one on top of the other. This process of combination yields a total of 330 distinctive Brahmi signs for the 33 consonants alone, without taking into account consonant conjuncts ... it is not surprising that the Indus-Sarasvati inscriptions should turn out to have a total of about 400 signs. Moreover, many of these signs appear to be modified in exactly the same regular manner as in much later [600 B.C.] Brahmi script.

“It is logical to assume that a systematically designed script such as Brahmi would represent a simplification of an earlier script.”<sup>67</sup>

These researchers go on to show the degree of the relationship and concordances of the signs in both the Harappan script and that of the Brahmins supposedly a thousand years later:

“More recently one of the present authors (Subhash Kak) renewedly explored this important question [of the similarities of the two scripts and the concordances of their use] from a statistical point of view, using computer-created concordances.<sup>68</sup> This analysis has shown that the most frequent letters of the Indus-Sarasvati and Brahmi scripts look almost identical and, moreover, share a rather similar frequency. The chance that this is a coincidence is so small that one can safely say that Brahmi is derived from Indus-Sarasvati. Furthermore, a structural analysis of the inscriptions indicates that the texts on the [Harappan] steatite seals follow grammatical rules like those of Sanskrit.”<sup>69</sup>

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<sup>67</sup> *ibid.*, p. 136

<sup>68</sup> S. Kak, “Indus Writing,” *Mankind Quarterly*, vol. 30 (1989) pp. 113-118; and S. Kak, “A Frequency Analysis of the Indus Script,” *Cryptologia*, vol. 12, no. 3 (1988), pp. 129-143

<sup>69</sup> *ibid.*, pp. 137-138



The script evidence is not conclusive because the Harappan signs on seals and elsewhere are much too short to be considered as written materials. Nevertheless, even if the Harappan signs are not a script, why would a society that came about a thousand years later ever turn to such signs to use them as letters in their own script? Surely, these earlier signs, not used for many centuries, and buried beneath the ground, should have been lost and forgotten. But since the two civilizations followed one another after a very short time, the Brahmin peoples would therefore turn to the signs they knew and remembered and use them to write their own script. This means, they could not have been separated by as much as a thousand years.

As with the Dark Age of ancient Greece, the Dark Age between the Harappans and the Brahmins requires one to accept that a highly developed civilized people forgot how to make pottery, jewelry, artifacts of bronze and copper, how to erect large buildings of brick, how to organize an urban society. One must also accept that these people simply vanished off the face of the Earth or migrated to some unknown and undiscovered land. Then, many centuries later, they began to build cities of brick, create art, pottery, artifacts of metals, etc., as well as to use the signs from the earlier, lost and forgotten culture for their written script! Further, one is asked to believe that many of these renewed creations were highly similar to those that were lost and forgotten.

One must accept that the Harappans, tied by trade to the Mesopotamians in Akkadian times—in terms of Heinsohn and Sweeney to Assyrian times—employed dry agriculture because there was abundant summer rainfall while those on the Babylonian plain were forced to employ irrigation agriculture. In other words, that the two regions at the same latitudes, situation just north of the Indian Ocean, had two entirely different climate regimes. One must accept that while the Mesopotamians proscribed the eating of pork, the pig was one of the main domesticated animals represented at Harappan sites. None of this makes the slightest historical, scientific or technological sense.

Of course historians cannot accept this long Dark Age subsequent to the fall of the Harappans and suggest that it was a gradual fall. But they do not explain why an urban people should forget to build cities of brick, forget how to sculpt statues of their gods, forget how to make lots of pottery, jewelry, seals, etc., yet at the same time they somehow managed to remember the Harappan signs on seals and then created a language with Brahmi letters like those employed a thousand

years earlier to write Sanskrit literature. Again this gradual de-evolution of the Harappan culture is without any support.

The question that critics of the short chronology must answer—which they will not or cannot answer—is: What is the scientific and technological evidence that fully supports the established chronology and correlates with, corroborates, and converges on, that chronology? At this point there is no such evidence.

## CONCLUSION

The self-deception created by historians by their sole use of documents and archaeology as the alpha and omega for unraveling the chronology and history of the ancient world is that they have an illusion rather than an actual vision of historical truth. They are in this respect like the prisoners in the parable of the cave told by Plato. Lynn E. Rose in his unpublished historical novel, *Ptolemy the Shadow Maker*, explains how in

“... Plato’s story of the cave, ... the Shadow Makers would hold various objects aloft behind an artificial wall, and the light from a bonfire would cast the shadows of those objects over onto the far wall of the cave itself. The prisoners on the other side of the artificial wall could thus see only the shadows of the *objects*, and not the shadows of the Shadow Makers themselves ...

“... the prisoners—who were not allowed to turn their heads around ... could still not see the shadows of the Shadow Makers themselves, and could see only the shadows of the objects that the Shadow Makers carried aloft.”<sup>70</sup>

In this sense the historians, by employing documents and archaeological evidence, are dealing with shadows and not the underpinnings of reality. From these shadows of historical truth they have built a chronological illusion of what actually occurred in the ancient past. The problem is that this

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<sup>70</sup> Lynn E. Rose, *Ptolemy the Shadow Maker* (unpublished), p. 30

illusion of knowledge has become embedded in their minds as truth and that it is too painful for them to look at the realities rather than the shadows. As Rose shows:

“One of the prisoners was released from his chains, and was forced to look at the people behind the artificial wall: they were the ones carrying the objects over their heads, in such a way that the bonfire would cast those very shadows [taken to be reality] that had heretofore been the prisoner’s only concern . . .

“The sometime prisoner was at a loss, and was troubled and tormented by the scenes before him. He did not begin to understand the strange sights and the stranger activities that he was seeing [for the first time]. The bonfire, the people, the objects held aloft, the artificial wall, and especially the unaccustomed and blinding light—were all too much for one who had passed his entire life imprisoned in shadows. The shadows themselves had always looked so *different* from the wondrous and alien objects now being paraded before him; indeed, there was no way he could comprehend that those flickering shapes on the cave-wall had been nothing more than *shadows* of these very objects. Though he had been immersed in shadows his entire life, the fact is that he did not even know what shadows *were*.

“Now he was being forced to contemplate a new level of reality, with no means even to *guess* that it underlay and secretly generated the only level of reality that he had hitherto known. Never in his life had he so much as dreamed that such things existed, and now there was no way in which such unsuspected wonders could even begin to be assimilated. It pained him so much to have to look directly at the truth, in all its unwanted brilliance. How he longed to return to the lost innocence and darkness of his former life.”<sup>71</sup>

Having known only one shadow reality all their lives, historians must dismiss the scientific and technological realities that they cannot assimilate. Nevertheless, some historians have begun to move away from the traditional approach of using only documents and archaeological interpretations as the basis for

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<sup>71</sup> *ibid.*, pp. 30-31

seeing the facts of chronology and history, and are asking for a scientific, rigorous way approach to evidence:

“The claim that historical fields [of research] follow methods different from those of other sciences is still frequently repeated. Given the political abuses to which history is subject, we consider this to be a dangerous claim, and believe that the same rigor must be demanded in history as in any other scientific field.”<sup>72</sup>

Feuerstein, Kak, and Frawley explain why scientific and technological evidence is not a subject to abuses as historical evidence:

“‘Truth,’ says the heroine in Shakespeare’s *Measure for Measure*, ‘is truth. To the end of reckoning.’ In this the great bard echoes Plato who called truth ‘durable.’ Truth is enduring because it is singular, complete in itself. Yet for some ..., truth is little more than an abstract proposition that is self-consistent or that corresponds to the facts while for the ordinary individual it is ‘all relative.’ Relative to what? We may ask. Speaking from within ..., the answer has to be relative to the ego’s momentary desires. But the truth of the ego with its fitful passions is not the truth ...

“We have won through to the recognition that there is only one science—that the laws of science do not change relative to our varying opinions or beliefs, cultures or customs [or even our educations]. Although scientific paradigms change, and with them scientific explanations, there is a fundamental pool of understanding that remains intact. Thus whether fire burns is not a matter of social habit or personal judgement but a fact of life [and reality] to be recognized and understood. Scientists strive to comprehend the laws of nature through reason and experiment. The proof of a scientific theory is independent of the country, culture, custom and personal background of the scientist.”<sup>73</sup>

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<sup>72</sup> Steve Farmer, Richard Sprout, Michael Witzel, “The Collapse of the Indus-Script Thesis: The Myth of a Literate Harappan Civilization,” *Electronic Journal of Vedic Studies (EJVS)*, vol. 11, no. 2 (Dec. 13, 2004), p. 48

<sup>73</sup> Feuerstein *et al.*, *op.cit.*, pp. 277-278

D. Clark in his paper “Archaeology: the loss of innocence” admits that everything in history and chronology may be wrong because of the new methods of investigation:

*“New Observations*

The array of new and old methodologies have also combined over ... twenty years to produce a multitude of ‘surprising’ new observations and to detect previously unrecognized sources of variability.” and “Once again, epistemological adaptation to the empirical content of the new observations is of no less significance than the explanatory and conceptual adaptation now required to understand them. Even those most complete and finished accomplishments of the old edifice [of history]—the explanations of the development of modern man, domestication, metallurgy, urbanization and civilization—may in perspective emerge as semantic snares and metaphysical mirages [and shadows].”<sup>74</sup>

The view espoused in these volumes is actually not new. In the last century, and even before, the American philosopher Charles Sanders Peirce in his 1901 paper, “The Logic of Drawing History From Ancient Documents,” argued that science must be the arbiter of historical/chronological truth:

“In this monograph Peirce argues that even though [David] Hume’s method [for historical analysis of documents and testimonies] of balancing the veracity of a witness against the probability of his narrative may be defended in certain cases, it is not generally applicable and is rarely used by historians. The probabilities generally relied on by historians are subjective—‘mere expressions of their preconceived notions’—and are completely unreliable. Peirce claims that what is needed for scientific history is a method that does not turn on estimates of probability or degrees of belief. HE RECOMMENDS THE

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<sup>74</sup> D. Clarke, quoted in Noel H. Gale, “Disciplinary Fault Lines: Science and Archaeology – Another View”, *Mediterranean Archaeology and Archaeometry*, vol. 3, no. 1 (2003), p. 57

GENERAL METHOD OF EXPERIMENTAL SCIENCE.”<sup>75</sup>  
(capitalization added)

Peirce concluded his paper thus:

“Now ancient history occupies a place among the psychical sciences somewhat analogous to that of astronomy among the physical sciences. The one is a description of what is distant in the world of the mind, as the other is a description of what is distant in the world of matter, and curiously enough, or significantly enough, an analogy exists between the two sciences through chronology ... History, however, is much more worthy than astronomy of being studied scientifically as mind is more worthy of our attention than matter. The use we should desire to make of ancient history is to *learn* from the study of it, and not to carry our preconceived notions into it, as long as it remains, at best [at the present time], an application of the mathematical doctrine of chances.”<sup>76</sup>

Peirce explains what happened during the 1800s when chronology and history were built only on documentary foundations:

“Now within the last half of the nineteenth century the merits of the procedure [of using documents as] the historical critics [have done,] have been many times subjected to the test of archaeological exploration; and what has been the result? ... I gather that, on the whole, it has been shown that the critics [historians] were found to be more or less fundamentally wrong in nearly every case, and in particular that their fashion of throwing all the positive evidence overboard in favor of their notions of what was likely, stands condemned by those tests. If this be so, it is no slight modification, but a complete revolution, of their logic [and evidence] which is called for;

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<sup>75</sup> *The Essential Peirce: Charles Sanders Peirce, Selected Philosophical Writings*, Christian J.W. Kloesel, ed. (Bloomington IN 1998), p. 75

<sup>76</sup> Charles Sanders Peirce, “The Logic of Drawing History from Ancient Documents,” *Collected Papers of Charles Sanders Peirce*, vol. 7 (Cambridge MA 1966), p. 105

because considering their great learning and competence, and the absolute confidence which they attached to their conclusions, as perfectly ineluctable, there is no middle course between pronouncing those men to have been a pack of charlatans and concluding that their method [of using only documents to determine the truth of history] was wrong in principle. If it were not so, their pretensions to scientific determinacy of those conclusions would have been simply disgraceful.”<sup>77</sup>

As archaeology proved that the documents were wrong regarding the history of the ancient world, now the new tools of science and technology prove that the combination of documentary materials along with archaeological evidence, both subject to interpretation and therefore bias, are wrong regarding the chronology and thus the history of that ancient period. However, I strongly doubt that the historians or their colleagues, the archaeologists, will accept the evidence in this book or the procedural approach to that evidence given by me. They know they are right because of their “great learning” and “competence.” The shadows they see as reality are not to be denied. Yet I cite Daniel J. Boorstin’s magnificent conclusion about what stands in the way of understanding a revolutionary concept such as the one promulgated here:

“The greatest obstacle to growth is not ignorance, but the illusion of knowledge.”<sup>78</sup>

As with the prisoner in Plato’s cave, the illusion of knowledge upon which the historians have based their chronology of the ancient world is a shadow of reality.

William Dever has succinctly stated:

“While the ultimate goal of all archaeological and historical studies is an absolute chronology, fixed with such scientific precision that thus it commends itself to all scholars, that goal is rarely attainable. Thus all chronological arguments for the ancient Near East begin with

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<sup>77</sup> *ibid.*, pp. 106-107

<sup>78</sup> Daniel J. Boorstin, in Jim Canterucci, *Personal Brilliance: Mastering the Everyday Habits that Create a Lifetime of Success* (NY 2005), p. 25, in *The Washington Post*, ‘Ten o’Clock Scholar’

relative sequences based on exceedingly complex claims of evidence that are largely circumstantial. With even one piece of new data, one link may break, and the chain [of the organization of the chronology] will fall apart.”<sup>79</sup>

The scientific and technological evidence in this book and volume I of this series is not in the least “circumstantial” or made up of “exceeding complex claims.” Its many links are bonded together with scientific precision. All in all, these forms of evidence are many pieces of new data that destroy the established chronology. In a word, the new evidence is “real”, not “shadow.” Historians’ claim to having true historical knowledge is an illusion.

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<sup>79</sup> William Dever in John Charles Hugh Laughlin, *Archaeology and the Bible* (London/NY 2000), p. 58



## BIBLIOGRAPHY

- Aaboe, A.: "Observation and Theory in Babylonian Astronomy", *Centaurus* vol. 24 (1980)
- Adkins, Lesley: *Empire of the Plain: Henry Rawlinson and the Lost Language of Babylon* (NY 2003)
- Agricola, Georgius: *De Re Metallica*, transl. by Lou Henry Hoover, Herbert Clark Hoover (NY 1950)
- Ahl, Frederick, Hanna M. Roisman: *The Odyssey Re-Formed* (Cornell NY 1996)
- Akkermans, Peter M.M.G., Glenn M. Schwartz, *The Archaeology of Syria: From Complex Hunter-Gatherers to Early Urban Societies (c. 16,000-3000 BC)* (Cambridge UK 2003)
- Akurgal, Ekrem: *Ancient Civilizations and Ruins of Turkey* (Istanbul 1970)
- Andrews, Carol: *Amulets in Ancient Egypt* (Austin TX 1994)
- Angelakis A.N.: *et al.*, "Urban wastewater and stormwater technologies in ancient Greece," *Water Research*, 39 (1) (2005)
- Assmann, Jan: *The Mind of Egypt* (NY 2002)
- Auden, W.H.: *Collected Poems*, E. Mendelson ed. (London 1976)
- Awan, Shaukat Ali: "Weather radar distributed hydrological modelling: a case study in the Indus Basin," *Weather Radar Information and Distributed Hydrological Modelling* (IAHS Publications 2003)
- Bailey, G. quoted in *The New Dictionary of Thoughts* (NY 1961)
- Baillie, M.G.L.: *A Slice Through Time* (Bath UK 1995)
- Baillie, M.G.L.: *Tree-Ring Dating and Archaeology* (Chicago IL 1982)
- Baillie, Mike: book review of Dalfes, H.N., G. Kukla, and H. Weiss, eds.: *Third Millennium BC Climate Change and Old World Collapse* (Berlin etc. 1996) in *Journal of Archaeological Sciences*, vol. 25, no. 2 (1998)
- Baldwin, Gordon C.: *Inventors and Inventions of the Ancient World* (NY 1973)
- Ball, Philip: "Were ancient Minoans centuries ahead of their time?", *BioEdOnline* (Feb. 28, 2006), Internet ([www.bioedonline.org/news/news.cfm?art=2371](http://www.bioedonline.org/news/news.cfm?art=2371))
- Barrow, Isaac, in *The New Dictionary of Thoughts* (NY 1961)
- Baudel, Fernand: *Memory and the Mediterranean* (NY 2001),
- Beaudreau, Bernard C.: *Making Markets and Making Money: Strategy and Monetary Exchange* (Lincoln NE 2004)

- Beaulieu, Paul-Alain: "King Nabonidus and the Babylonian Empire", *Civilizations of the Ancient Near East*, J.M. Sassoon et al., eds. (NY 1995)
- Beaulieu, Paul-Alain: *The Reign of Nabonidus King of Babylon 556-539 B.C.* (New Haven CT/London 1989)
- Bednarik, R.G.: Review article "Only Time Will Tell...", *Archaeometry*, vol. 38, no. 1 (1996)
- Bell, Edward: *Early Architecture in Western Asia – Chaldean, Hittite, Assyrian, Persian: A Historical Outline* (London 1924)
- Bellwood, Peter S.: *First Farmers: The Origins of Agricultural Societies* (London 2004)
- Bennett, John W.: *Human Ecology as Human Behavior*, 2nd ed. (Brunswick NJ 2002)
- Bentley, Jerry H.: *Old World Encounters: Cross-Cultural Contacts and Exchanges in Pre-Modern Times* (Oxford UK/NY, etc. 1993)
- Best, Jan G.P., and Nanny M.W. De Vries, eds.: *Interaction and Acculturation in the Mediterranean* (Amsterdam, the Netherlands 1982)
- Bickerman, E.J.: *Chronology of the Ancient World*, rev. ed. (London 1980)
- Bietak, M.: "Review of Manning 1999," *Bibliotheca Orientalis*, vol. LXI no. 1-2
- Bietak, Manfred: "Problems of Middle Bronze Age Chronology: New Evidence from Egypt," *American Journal of Archaeology*, vol. 80, no. 4 (1984)
- Blegen, Carl: "A Chronological Problem," *Minoica. Festschrift zum 80. Geburtstag von Johannes Sundwall*, E. Grumach ed. (Berlin 1958)
- Boorsten, Daniel J., Interview before retiring, in Leigh Branham, *The Hidden Reasons Employees Leave* (NY 2005)
- Borovka, Gregory: *Scythian Art* (London 1928)
- Botterweck, G. Johannes et al., eds.: *Theological Dictionary of the Old Testament* (Grand Rapids MI 1988)
- Branham, Leigh: *The Hidden Reasons Employees Leave* (NY 2005)
- Brewster, David: *Memoirs of the Life, Writings and Discoveries of Sir Isaac Newton* (London 1855)
- Briant, Pierre: *From Cyrus to Alexander – A History of the Persian Empire* (Winona Lake IN 2002)
- Bromfield, Louis quoted in *The New Dictionary of Thoughts* (NY 1961)
- Bromley, Geoffrey W.: *International Standard Bible Encyclopedia K–P* (Grand Rapids MI 1956)
- Brown, David: *Mesopotamian Planetary Astronomy–Astrology* (Groningen, the Netherlands 2000)

- Bryce, Trevor: *Letters of the Great Kings of the Ancient Near East* (London 2003)
- Bryce, Trevor: *The Kingdom of the Hittites* (Oxford UK 1998)
- Bryce, Trevor: *The Kingdom of the Hittites* (paperback ed., Oxford UK 1999)
- Buccellati, Giorgio: "Amorites," *The Oxford Encyclopedia of Archaeology in the Near East*, E.M. Meyers, ed. (Oxford UK 1997)
- Buckland, Paul C. *et al.*, "Bronze Age Myths? Volcanic activity and human response in the Mediterranean and North Atlantic regions," *Antiquity*, vol. 7 (1997)
- Bulliet, Richard W.: *The Camel and the Wheel* (Cambridge MA/London 1997)
- Burke, Vincent: *Discovering the Ancient Sites of Western Europe* (Watson Bay NSW, Australia 2005)
- Burns, A.R.: *Money and Monetary Policy in Early Times* (NY 1965)
- Burroughs, William James: *Climate Change in Prehistory: The End of the Reign of Chaos* (Cambridge UK 2005)
- Butterfield, Herbert: *The Origin of History* (NY 1981)
- Calder, Nigel: *The Weather Machine* (NY 1974)
- Cambridge Ancient History (CAH)*, various volumes, different publication dates (Cambridge UK)
- Cambridge History of Africa, The*, J.D. Fage ed. (Cambridge UK 2002)
- Cambridge History of Iran, The*, I. Gershevitch ed. (London 1985)
- Cardona, Dwardu: "A Return to the Two Sargons and Their Successors," *Aeon*, vol. 5, no. 4 (July 1999)
- Cardona, Dwardu: *God Star* (Victoria B.C., Canada 2006)
- Casson, Lionel: *The Ancient Mariners* (NY 1967)
- Casson, Lionel: *Travel in the Ancient World* (Baltimore MD 1994)
- Casson, Stanley: *The Discovery of Man* (NY/London 1939)
- Castleden, Rodney: *Atlantis Destroyed* (London/NY 2001)
- Ceram, C.W.: *The Secret of the Hittites*, transl. Richard and Clara Winston (NY 1955)
- Chakrabarti, Dilip K. *et al.*, *India: An Archeological History* (Oxford UK 1999)
- Chambers, George F.: *The Story of Eclipses Simply Told for General Readers* (NY 1900)
- Charpin, Dominique: "The History of Ancient Mesopotamia: An Overview," *Civilizations of the Ancient Near East*, vol. II, J.M. Sassoon *et al.*, eds. (NY 1995)
- Childe, V. Gordon: *What Happened in History* (NY 1946)

- Claiborne, Robert: "How Has Climate Affected History", *Mysteries of the Past* (NY 1977)
- Clapp, Nicholas: *Sheba* (Boston MA/NY 2001)
- Clarke, D., quoted in Noel H. Gale: "Disciplinary Fault Lines: Science and Archaeology – Another View", *Mediterranean Archaeology and Archaeometry*, vol. 3, no. 1 (2003)
- Clay, Albert T.: *The Empire of the Amorites* (Yale Oriental Series), vol. VI (London 1919)
- Cline, Eric: "An Unpublished Amenhotep III Faience Plaque From Mycenae," *Journal of the American Oriental Society*, vol. 110 (1990)
- Cochrane, Ev: "Heinsohn's Ancient 'History'," *Aeon* vol. V, no. 4 (July 1999)
- Cochrane, Ev: "Sins of the Father," *AEON*, vol. VI, no. 6 (October 2005)
- Colander, David C.: *Economics*, 2nd ed. (Chicago IL 1995)
- Conder, Claude Reignier: *The Hittites and Their Language* (NY 1898)
- Contenau, George: *Everyday Life in Babylonia and Assyria* (NY 1966)
- Cook, J.M.: *The Persian Empire* (London/NY 1983)
- Counihan, Carole M. and Penny Van Esterik: *Food and Culture: A Reader* (NY 1997)
- Courville, Donovan A.: *The Exodus Problem and Its Ramifications* (Loma Linda CA 1971)
- Craddock, P.T.: "Historical Survey of Gold Refining," *King Croesus' Gold*, Andrew Ramage, Paul T. Craddock, eds. (Cambridge MA 2000)
- Crombie, A., in Francesca Rochberg, *The Heavenly Writing: Divination, Horoscopy, and Astronomy in Mesopotamian Cultures* (Cambridge UK/NY 2004)
- Cruz-Uribe, Eugene: "Scripts: An Overview", *Oxford Encyclopaedia of Ancient Egypt*, vol. 3, D.B. Redford, ed. (Oxford UK, publ. in NY 2001)
- Cryer, Frederick H.: "Chronology Issues and Problems," in *Civilizations of the Ancient Near East*, vol. I, J.M. Sassoon *et al.*, eds. (NY 1995)
- Culican, William: *The Medes and the Persians* (NY 1963)
- Curtin, Philip D.: *Cross-Cultural Trade in World History*, reprint (Cambridge UK 1998)
- Dalfes, H.N., G. Kukla, and H. Weiss, eds.: *Third Millennium BC Climate Change and Old World Collapse* (Berlin etc., 1996)
- Dalley, Stephanie: *Mari and Karana* (Piscataway NJ 2000)
- Davidson, James West, Mark Hamilton Lytle: *After the Fact. The Art of Historical Detection* (NY 2004)

- Davis, Diana K. and Denys Frappier: "The Social Context for Working Equines in the Urban Middle East: The Example of Fez Medina," *The Walled Arab City in Literature, Architecture and History: The Living Medina in the Maghrib*, Susan Slyomovics, ed. (London 2001)
- Davis, Whitney: "Amorite Tribes and the Nature of Second Millennium Western Asia," *Civilizations of the Ancient Near East*, vol. II, J.M. Sassoon *et al.*, eds. (NY 1995)
- Davis-Kimball, Jeannine, with Mona Behan: *Warrior Women* (NY 2002)
- Dayton, John E.: "Geology, Archaeology and Trade," *Interaction and Acculturation in the Mediterranean*, vol. II, Jan G.P. Best, Nanny M.W. De Vries, eds. (Amsterdam, the Netherlands 1982)
- Dayton, John E.: "The Problem of Climate Change in the Arabian Peninsula," *Proceedings of the Seminar for Arabian Studies*, vol. 5 (1975)
- Dayton, John E.: *Minerals Metals Glazing and Man* (London 1978)
- Dayton, John E.: *The Discovery of Glass: Experiments in the Smelting of Rich Dry Silver Ores and the Reproduction of Bronze Age-type Cobalt Blue Glass as a Slag* (Cambridge MA 1993)
- de Souza, Philip: *Piracy in the Graeco-Roman World* (Cambridge UK 2000)
- Delderfield, Ronald Frederick: *God is an Englishman* (NY 1970)
- Desborough, Vincent Robin D'Arba: *The Greek Dark Age* (NY 1972)
- Dever, William in John Charles Hugh Laughlin, *Archaeology and the Bible* (London/NY 2000)
- Dickinson, Oliver: *The Aegean Bronze Age* (Cambridge UK 1994)
- Dicks, D.R.: *Early Greek Astronomy to Aristotle* (Ithaca NY 1970)
- Dictionary of Archaeology*, Ian Shaw, Robert Jameson eds. (Oxford UK 1999)
- Dictionary of the Ancient Near East*, The, P. Bienkowski, A. Millard eds. (Philadelphia 2000)
- Dines, Nicholas T. and Kyle D. Brown, *Landscape Architect's Portable Handbook* (NY 2001)
- Dougherty, Raymond Philip: *Nabonidus and Belshazzar* (New Haven CT 1929)
- Drakonoff, I.M., and I.M. Diakonov: *Early Antiquity* (Chicago IL 1991)
- Drews, Robert: *Early Riders – The Beginnings of Mounted Warfare in Asia and Europe* (NY/London 2004)
- Drower, Margaret S.: "Syria c. 1550-1400 B.C.," *CAH*, vol. II, pt. 1
- Dubberstein, Waldo H.: "Assyrian-Babylonian Chronology (669-612 B.C.)," *Journal of Near Eastern Studies*, vol. 3, no. 1 (Jan 1944)
- Dulles, John Foster, quoted in *The Dictionary of Thoughts* (NY 1961)

- Durant, Will, quoted in *The New Dictionary of Thoughts* (NY 1961)
- Durant, Will: *Our Oriental Heritage* (NY 1954)
- Dyson, James L.: *The World of Ice* (NY 1972)
- Ebeling, Erich and Bruno Meissner: "Metalle B", *Reallexikon der Assyriologie*, Band 8 (1997) (Berlin Germany/NY 1938)
- Egyptian Chronology BCE*, Art History Search, Internet
- Ellenberger, C. Leroy: "Still Facing Many Problems, Part I," *Kronos*, vol. X, no. 1 (Fall 1989)
- Ellis, Richard: *Imagining Atlantis* (NY 1998)
- Embree, Ainslie T. and Carol Gluck: *Asia in Western and World History* (NY 1997)
- Encyclopaedia Britannica Macropaedia*, vol. 2 (Chicago IL 1974)
- Encyclopaedia Britannica Macropaedia*, vol. 21 (Chicago IL 2002)
- Encyclopaedia Britannica Macropaedia*, vol. 8. (London/Chicago IL 1982)
- Epps, Joanne A. et al.: *The Winning Argument* (Chicago IL 2001)
- Eraly, Abraham: *Gem in the Lotus: The Seeding of Indian Civilization* (London 2000)
- Fagan, Brian: *Time Detectives* (NY 1995)
- Fairbridge, F.W.: "Holocene," *Encyclopaedia Britannica Macropaedia*, vol. 8. (London/Chicago IL 1982)
- Fairservis, Walter Ashlin: *The Harappan Civilization and Its Writings: A Model for the Decipherment of the Indus Script* (Leiden, the Netherlands/NY etc. 1992)
- FARMS Journal of the Book of Mormon Studies, vol. 19. issue 2 (2000); Internet from Brigham Young University Foundation of Ancient Research and Mormon Studies
- Ferrence, Susan: "Deformity in the 'Boxing Boy'," *Perspectives in Biology and Medicine*, vol. 40, no. 1 (Winter 2005)
- Feuerstein, Georg, Subhash Kak, David Frawley: *In Search of the Cradle of Civilization* (Wheaton IL 1995)
- Fischer, David Hackett: *Historians' Fallacies* (NY 1970)
- Fischer, Peter M. and Martin J. Whitehouse: "Quantitative SIMS (IMS 1272) of Particles from the GRIP Greenland ice core and Thera," paper presented to the workshop "Ashes + Ice" VERA Laboratory University of Vienna (8-10 July 2004) (<http://www.nhm-wien.ac.at/sciem2000/>)
- Fischer, Steven Roger: *Glyphbreaker* (NY 1997)
- Fitton, J. Lesley: *Minoans* (Cambridge MA 2002)
- Fitton, J. Lesley: *The Discovery of the Greek Bronze Age* (Cambridge MA 1996)

- Fomenko, Anatoly T.: *Empirico-Statistical Analysis of Narrative Materials and its Application to Historical Dating* (Dordrecht, the Netherlands/Boston MA/London 1994)
- Fomenko, Anatoly T.: *History: Fiction or Science? Chronology*, 2nd, revised ed. (Paris, France/London/NY 2003)
- Forbes, R.J. and E.J. Dijksterhuis: *A History of Science and Technology* (Baltimore MD 1963)
- Forbes, R.J.: "Extracting, Smelting, and Alloying," *A History of Technology*, Charles Singer *et al.*, eds., vol. I (London 1954)
- Forbes, R.J.: "Silver and Lead in Antiquity," *American Journal of Antiquity*, vol. xliv (1940)
- Forbes, R.J.: *Studies in Ancient Technology* (Leiden, the Netherlands 1966)
- Fouqué, Ferdinand A.: *Santorini and Its Eruption* (Baltimore 1998)
- Frame, Grant: *Babylonia 689-627 B.C. A Political History* (Istanbul, Turkey 1992)
- Frame, Grant: *Rulers of Babylonia From the Second Dynasty of Isin to the End of the Assyrian Domination (1157-612 B.C.)* (Toronto Canada 1995)
- France, Anatole, in Fix, Wm.R.: *The Bone Peddlers* (NY 1948)
- Frankfort, Henri P.: "Evidence for Harappan Irrigation System in Haryan and Rajastan," *Eastern Anthropologist*, vol. 45 (1999)
- Frankfort, Henri: "The Making of a King" (Internet), Introduction to Mesopotamia [www.gatewaystobabylon.com](http://www.gatewaystobabylon.com)
- Frawley, David: *Gods, Sages and Kings: Vedic Secrets of Ancient Civilization* (Twin Lakes WI 2000)
- French, E.B.: "Glass Frit and Faience," *Excavations at Tell Brak*, vol. I, David Oates, Joan Oates, Helen McDonald eds. (London 1997)
- Freud, Sigmund quoted in *Leo Rosten's Carnival of Wit*
- Friedrich Walter L. *et al.*, "Santorini Eruption Radiocarbon Dated to 1627-1600 B.C.," *Science*, vol. 312, no. 5773 (April 28. 2006)
- Frye, Richard N.: *The Heritage of Persia* (Cleveland OH & NY 1963)
- Gabel, Creighton, ed.: *Man Before History* (Englewood Cliffs NJ 1964)
- Gabriel, Richard A.: *Great Captains of Antiquity* (Westport CT 2001)
- Gabriel, Richard A.: *The Great Armies of Antiquity* (Westport CT 2002)
- Gadd, C.G.: "Hammurabi and the End of his Dynasty," *The Cambridge Ancient History*, vol. 2, pt. 1 (1973)
- Galanopoulos, Angelos G. and Edward Bacon: *Atlantis. The Truth Behind The Legend* (London 1969)

- Gale, Noel H.: "Disciplinary Fault Lines: Science and Archaeology – Another View", *Mediterranean Archaeology and Archaeometry*, vol. 3, no. 1 (2003)
- Galen, quoted in Otto Neugebauer, *The Exact Sciences in Antiquity* (Providence RI 1957)
- Gardner, Percy: *The Types of Greek Coins: An Archaeological Essay* (Cambridge UK 1883)
- Gasche, H. et al.: *Dating the Fall of Babylon* (Ghent, Belgium/Chicago IL 1998)
- Gaston, Mary Frank: *Copper Identification + Value: Identification and Value Guide* (Paducah KY 1992)
- Gates, Charles: *Ancient Cities: The Archaeology of Urban Life in the Ancient East, and Egypt, Greece and Rome* (London/NY 2003)
- Gauthier-Pilters, Hilde and Anne Innis Dagg: *The Camel: Its Evolution, Behavior and Relationship to Man* (Chicago IL 1983)
- Ginenthal, Charles: "Arguments of Straw: Dwardu Cardona and *Pillars of the Past*", *The Velikovskian*, vol. VII, no. 1, Supplement (2007)
- Ginenthal, Charles: "Scientific Dating Methods in Ruins," *The Velikovskian*, vol. II, no. 1 (1994)
- Ginenthal, Charles: *Pillars of the Past*, vol. I (Forest Hills NY 2003)
- Ginenthal, Charles: *The Extinction of the Mammoth* (Forest Hills NY 1997)
- Glass, *Encyclopedia Britannica Micropedia*, vol. 5 (Chicago IL 2002)
- Gleason, H.A. : *An Introduction to Descriptive Linguistics*, rev.ed. (NY 1961)
- Golden, Mark: *Sport in the Ancient World from A to Z* (London/NY 2004)
- Goldfrank, Walter L., et al., eds.: *Ecology and the World System* (London/NY 1999)
- Gordon, Cyrus H.: *Forgotten Scripts; how they were deciphered and their impact on contemporary culture* (NY 1968)
- Gordon, Cyrus H.: *The Common Background of Greek and Hebrew Civilizations* (NY 1965)
- Grayson, A.K.: "Mitanni," *The Penguin Encyclopedia of Ancient Civilizations*, A. Cotterell, ed. (London 1980)
- Grayson, A.K.: *Assyrian and Babylonian Chronicles* (Winona Lake IN 2000)
- Greenberg, Lewis M.: "Reply to Isaacson," *Kronos*, vol. I, no. 2 (2nd printing) (June 1975)
- Gribbin, John: *Future Weather and the Greenhouse Effect* (NY 1982)
- Haase, Wolfgang and Temporini, Hildegard, eds.: *Aufstieg und Niedergang der Römischen Welt. Geschichte und Kultur Roms im Spiegel der neueren Forschung*, part II, vol. 16, Berlin, Germany/NY 1978)



- Haddingham, Evan: *Early Man in the Cosmos* (NY 1984)
- Haldor, Alfred: *Who were the Amorites?* (Leiden, the Netherlands, undated)
- Hall, Jonathan M.: *A History of the Archaic Greek World* (Malden MA 2007)
- Hansen, Donald P.: "Art of the Royal Tombs of Ur: A Brief Interpretation," *Treasures from the Royal Tombs of Ur*, R.L. Zettler, L. Horne eds. (Philadelphia PA 1998)
- Harris, Marvin: *Cannibals and Kings: The Origin of Culture* (NY 1977)
- Hassan, Fekri: "Environmental Perception and Human Response in History and Prehistory," *The Way the Wind Blows*, R.J. McIntosh *et al.*, eds. (NY 2000)
- Hawkes, Jacquetta, and Sir Leonard Woolley: *Prehistory and the Beginnings of Mankind*, vol. I (NY 1963)
- Hawkes, Jacquetta: *Dawn of the Gods: Minoan and Mycenaean Origins of Greece* (NY 1968)
- Hawkes, Jacquetta: *The First Great Civilizations* (London 1973)
- Heinsohn, Gunnar: "Who were the Hyksos? Can archaeology and stratigraphy provide a solution to the 'enigma of world history'?" Proceedings of the *Sesto Congresso Internazionale di Egittologia*, vol. II (Torino, Italy, 1993)
- Henderson, Julian: "Part G" of "Glass, Frit and Faience," *Excavations at Tell Brak*, vol. I, David Oates, Joan Oates, Helen McDonald eds. (London 1997)
- Herodotus, *Histories* (transl. Aubrey de Séincourt, Harmondsworth MX, UK 1954/1978)
- Herodotus, *Histories*, (transl. George Rawlinson, London 1875)
- Hicks, Jim *et al.*: *The Persians* (NY 1975)
- Hoad, M.S.F.: *The Home of the Heroes* (London 1967)
- Hodson, F.R., ed.: *The Place of Astronomy in the Ancient World* (Oxford UK 1974)
- Hollander, Paul: *Lonely Planet Cyprus* (Footscray, Victoria Australia 2003)
- Hood, Sinclair: *The Arts of Prehistoric Greece*, The Yale University Press's *Pelican History of Art* (New Haven CT 1978)
- Hood, Sinclair: *The Minoans* (NY 1971)
- Huber, Peter: "Cuneiform Evidence for Venus", *Scientists Confront Velikovsky* (Cornell NY 1979)
- Humphrey, J.W., John P. Olszen, and Andrew N. Sherwood: *Greek and Roman Technology: A Source Book* (London 1998)
- Hunger, M.H. and D. Pingree: *Astral Science in Mesopotamia* (Leiden, the Netherlands 1999)
- Huntington, Samuel P. : *Clash of Cultures and the Remaking of World Order* (NY 1996)
- Hutten, E.H.: *The Origins of Science* (London 1962)

- Hyams, Edward: *Animals in the Service of Man* (Philadelphia PA/NY 1972)
- International Standard Bible Encyclopedia K–P* (Grand Rapids MI 1956)
- Introduction to Archival Materials Writing Links* (Richmond, Surrey UK 1998)
- Ishida, Tomoo: *The Royal Dynasties in Ancient Israel* (Berlin, Germany/NY 1977)
- Jackson, Stelios: *The Hellenic Book Service*, Book Review of *Minoans* by J. Lesley Fitton; Internet ([www.west-crete.com/book-review-minoans.htm](http://www.west-crete.com/book-review-minoans.htm))
- Jacobsen, Thorkild: *The Sumerian King List* (Chicago IL 1937)
- Jacobsen, Thorkild: *Toward the Image of Tammuz and Other Essays on Mesopotamian History and Culture*, W.L. Moran, ed. (Cambridge MA 1970)
- James, Peter *et al.*: *Centuries of Darkness* (London 1991 & New Brunswick, NJ 1993)
- James, Peter: “The Dendrochronology Debate,” *Minerva*, vol. 13, issue 4 (July/Aug 2002)
- Jay, Peter: *The Wealth of Man* (NY 2000)
- Jevons, W. Stanley: *Money and the Mechanisms of Trade* (NY 1984)
- Joannes, France: “Médailles d’argent d’Hammurabi?”, *Nouvelles assyriologiques brèves et utilitaires*, no. 4 (Décembre 1989)
- Joannes, France: “Private Commerce and Banking in Achaemenid Babylon,” *Civilizations of the Ancient Near East*, vol. III (NY 1995)
- Jonsson, Carl Olof: “The Foundations of the Assyro-Babylonian Chronology – Conclusion,” *Chronology & Catastrophism Review*, vol. IX (1987)
- Jonsson, Carl Olof: *The Gentile Times Reconsidered*, 4th ed., revised and expanded (Atlanta GA 2004)
- Jonsson, Carl Olof: *The Gentile Times Reconsidered: Chronology and Christ’s Return*, 3rd ed. (Atlanta GA 1998)
- Joseph, John: “The Bible and the Assyrians: It Kept Their Memory Alive,” *Journal of Assyrian Academic Studies*, vol. 12, no. 1 (1998)
- Jursa, Michael : “Observations on the Problems of the Median ‘Empire’ on the Basis of Babylonian Sources,” *Continuity of Empire (?) Assyria, Media, Persia*, Giovanni B. Lanfranchi *et al.*, eds., (Padova, Italy 2003)
- Kak, Subhash: “A Frequency Analysis of the Indus Script,” *Cryptologia*, vol. 12, no. 3 (1988)
- Kak, Subhash: “Indus Writing,” *Mankind Quarterly*, vol. 30 (1989)
- Keay, John: *India: A History* (NY 2000)
- Keenan, Douglas J.: “Anatolian tree-ring studies are untrustworthy” (Internet), pp. 7-8 [www.informath.org/ATSU04a.pdf](http://www.informath.org/ATSU04a.pdf)

- Keenan, Douglas J.: "Volcanic ash retrieved from the GRIP ice core is not from Thera," *Geochemistry, Geophysics, Geosystems*, vol. 4, no. 11 (Nov. 15. 2003), p. 7, Internet online publication ([www.informath.org/pubs/G%5E303a.pdf](http://www.informath.org/pubs/G%5E303a.pdf))
- Keenan, Douglas J.: "Why Early-Historical Radiocarbon Dates Downwind From the Mediterranean are too Early," *Radiocarbon*, vol. 44, no. 1 (2002)
- Keller, Werner: *The Bible as History* (reprint NY 1980)
- King, Leonard W.: *A History of Sumer and Akkad* (NY 1910), 1968 reprint
- Kirk, G.S.: *The Iliad: A Commentary* (Cambridge UK 2001)
- Kirk, William: "The Role of India in the Diffusion of Early Cultures," *Geographical Journal*, vol. 141, no. 1 (March 1975)
- Kloesel, Christian J.W., ed.: *The Essential Peirce: Charles Sanders Peirce, Selected Philosophical Writings* (Bloomington IN 1998)
- Kohl, Philip L.: "Transcaucasian 'Periphery' in the Bronze Age," *Resources, Power, and Interregional Interaction*, Edward M. Schortman, Patricia A. Urban, eds. (NY 1992)
- Konstan, David: *The Emotions of the Ancient Greeks* (Toronto 2006)
- Koppel, Tom: *Lost Worlds* (NY 2003)
- Kosmetatou, Elizabeth: "Hacksilber to Coinage...", *Bryn Mawr Classical Review*, 03.16 (2004)
- Kramer, Samuel Noah, quoted in Potts, Daniel T.: *Mesopotamian Civilization: The Material Foundations* (Ithaca NY 1977)
- Krebs, Paula V.: "Dendrochronology of Bristlecone Pine (*Pinus aristata* Engelm.) in Colorado," *Arctic and Alpine Research*, vol. 5, no. 2 (Spring 1973)
- Kristiansen, Kristian and Thomas B. Larsson, *The Rise of Bronze Age Society: Travels, Transmissions and Transformations* (Cambridge UK 2005)
- Kristiansen, Kristian: *Europe Before History* (Cambridge UK 2000)
- Kronos*, vol. V, no. 4 (Summer 1980)
- Kuhrt, A., and H. Sancisi-Weerdenburg: "Introduction," *Achaemenid History IV: Centre and Periphery*, H. Sancisi-Weerdenburg and A. Kuhrt, eds. (Leiden, the Netherlands 1990)
- Kuhrt, Amelie: "Achaemenid Babylonia: Sources and Problems," *Achaemenid History IV, Centre and Periphery* (Leiden, the Netherlands 1990)
- Kuhrt, Amelie: "Babylonia from Cyrus to Xerxes," *CAH*, 2nd ed., vol. IV (1988)
- Kuhrt, Amelie: *The Ancient Near East c. 3000-330 BC* (London, NY 1995)
- Kulke, Hermann and Rothemund, Dietmar: *A History of India*, 4th ed. (NY 2004)

- Kuniholm, P.I. *et al.*: "Anatolian Tree Rings and the Absolute Chronology of the Eastern Mediterranean," *Nature*, vol. 381 (1996)
- Kuniholm, P.I.: "Aegean Dendrochronology Project December 1996 Progress Report" (1996), Department of the History of Art, Cornell University, Ithaca, New York circular
- Kurinsky, Samuel: *The Eighth Day* (Northvale NJ/London 1994)
- Lace, O. Jesse: *Understanding the Old Testament* (Cambridge UK 1972)
- Laessoe, Jorgen: *People of Ancient Assyria* (London 1963)
- Lamb, H.H.: "Climate Vegetation and Forest Limits," *The Place of Astronomy in the Ancient World*, F.R. Hodson ed. (Oxford UK 1974)
- Lamb, Hubert H., *Climate, History and the Modern World* (London/NY 1995)
- Landsberger, Benno: "Tin and Lead: The Adventure of Two Vocables," *Journal of Near Eastern Studies*, vol. 24, no. IV (1965)
- Lanfranchi, Giovanni B. *et al.*, eds.: *Continuity of Empire (?) Assyria, Media, Persia* (Padova, Italy 2003)
- Lang, Andrew, quoted in Rosten, Leo: *Leo Rosten's Carnival of Wit* (1996)
- Lange and Swerdlow, *Planet's Visibility 2.0. and Planetary, Lunar and Stellar Visibility 3.1*
- Lange, Rainer, alcyone software, and Noel M. Swerdlow, University of Chicago: *Planet's Visibility*
- Latif, Syad Muhammad: *History of the Punjab from the Remotest Antiquity to the Present Time* (Eurasian Publ. House 1964)
- Laughlin, John Charles Hugh: *Archaeology and the Bible* (London/NY 2000)
- Lawler, Andrew: "Ur's Xena: A Warrior Princess for Sumeria?", *Science*, vol. 309 (Aug. 5, 2005)
- Lechtman, Heather: "Arsenic Bronze: Dirty Copper or Chosen Alloy? A view from the Americas," *Journal of Field Archaeology*, vol. 23, no. 4 (Winter 1996)
- Lehmann, Johannes: *The Hittites: People of a Thousand Gods* (NY 1977)
- Leick, Gwendolyn: *Mesopotamia: The Invention of the City* (London 2002)
- Leick, Gwendolyn: *The Babylonians: An Introduction* (London/NY 2003)
- Leopold, Arthur G.: *The Camel* (London/NY 1894)
- Leshem, Ayval: *Newton on Mathematics and Spiritual Purity* (Cambridge UK 2003)
- Lindholm, Charles: *The Islamic Middle East: Tradition and Change*, revised ed. (Oxford UK 2002)
- Linssen, Marc J.H.: *The Cult of Uruk and Babylon* (Leiden, the Netherlands 2003)

- Lippmann, Walter quoted in *Leo Rosten's Carnival of Wit*
- Littmann, Mark *et al.*, *Totality* (NY/Oxford UK 1999)
- Liverani, M.: "The Amorites," *Peoples of Old Testament Times*, D.J. Weiss, ed. (Oxford UK 1973)
- Longman, Tremper III: *Fictional Akkadian Autobiography* (Winona Lake IN 1991)
- Lovejoy, Paul E.: *Salt of the Desert Sun: A History of Salt Production and Trade in the Central Sudan* (Cambridge UK/NY 1986)
- Lucas, A.: *Ancient Egyptian Materials and Industries* (London 1962), Dover edition (Mineola NY 1999)
- Lucas, Alfred: *Ancient Egyptian Materials and Industries* (NY 1999)
- Lucas, Alfred: *Ancient Egyptian Materials and Industries*, 4th ed. (London 1962)
- Luce, J.V.: *Lost Atlantis. New Light on an Old Legend* (NY 1969)
- Lydekker, Richard: *The Horse and its Relatives* (NY 1912)
- Lynch, Thomas, in Tom Koppel: *Lost Worlds* (NY 2003)
- Maddin, Robert, ed.: *The Beginnings of the Use of Metals and Alloys* (Cambridge MA 1988)
- Maisels, Charles Keith: *Early Civilizations of the Old World* (London 1999)
- Mandelbrote, Scott: "Newton and Eighteenth Century Christianity," *The Cambridge Companion to Newton*, Bernard Cohen, George Edwin Smith, eds., (Cambridge UK 2002)
- Manning, Sturt W. *et al.*: "Anatolian Tree Rings and a New Chronology of the Eastern Mediterranean Bronze Iron Ages," *Science*, vol. 294 (Dec. 21, 2001)
- Manning, Sturt W. *et al.*: "Chronology for the Aegean Late Bronze Age 1700-1400 B.C.," *Science*, vol. 294 (Dec. 21, 2001)
- Manning, Sturt W.: *A Test of Time* (Oxford UK 1999)
- Marinatos, Spyridon: "The volcanic destruction of Minoan Crete," *Antiquity*, vol. 13 (1939)
- Marsman, Hennie J.: *Women in Ugarit and Israel* (Leiden, the Netherlands 2003)
- Martini, Marco: *Physics Methods in Archaeometry* (Amsterdam, the Netherlands 2004)
- Matossian, Mary Kilbourne: *Shaping World History: Breakthroughs in Ecology, Technology, Science and Politics* (Armonk NY 1997)
- Matthews, Donald: "Mitanni Seals from Tell Brak," *Excavations at Tell Brak*, vol. I, David Oates, Joan Oates, Helen McDonald eds. (London 1997)
- Matthews, Roger: *The Archaeology of Mesopotamia, Themes and Approaches* (London/NY 2003)

- Maunder, Michael and Moore, Patrick: *The Sun in Eclipse* (Berlin, Germany and NY 1998)
- Mauny, Raymond: "Trans-Saharan contacts and the Iron Age in West Africa," *The Cambridge History of Africa*, J.D. Fage ed. (Cambridge UK 2002)
- Mavor, James W. Jr.: *Voyage to Atlantis* (NY 1969)
- McK Camp, John II: "Walls and the Polis," in *Polis & Politics: Studies in Ancient Greek History* (Copenhagen, Denmark 2000)
- McLaughlin, D.L., D.E. Corrigan, W.D. McIlveen: "Etiology of Sugar Maple Decline at Selected Sites in Ontario (1984-1990)," report prepared by the Phytotoxicology Section Air Resources Branch, Ontario Ministry of the Environment (Ontario, Canada 1992)
- Meadow, R.H., in Maisels, *Early Civilizations of the Old World* (London 1999)
- Mellersh, H.E.L.: *The Destruction of Knossos: The Rise and Fall of Minoan Crete* (NY 1970)
- Meltzer, Milton: *Slavery: A World History* (NY 1993)
- Merrett, Stephen, ed.: *Water for Agriculture: Irrigation Economics in International Perspective* (London/NY 2002)
- Mertz, Barbara: *Temples Tombs and Hieroglyphics* (NY 1978)
- Mewhinney, Sean: "Ice Cores and Common Sense," Parts I and II, *Catastrophism and Ancient History* (Jan. and July 1990)
- Millard, Alan: "Amorites and Israelites: Invisible Invaders—Modern Expectation and Ancient Reality," *The Future of Biblical Archaeology: Reassessing Methodologies and Assumptions*, James Karl Hoffmeier, ed. (Grand Rapids MI/Cambridge UK 2004)
- Millard, Alan: *The Eponyms of the Assyrian Empire 910-612* (Helsinki, Finland 1994)
- Minns, E.H.: *Scythians and Greeks* (Cambridge UK 1913)
- Moorey, P.R.S.: *Ancient Mesopotamian Materials and Industries* (Oxford UK 1994)
- Morris, I.: "Archaeology as Cultural History," *Archaeological Review from Cambridge*, vol. 14, no. 1 (1997)
- Moscato, Sabatino: *The Face of the Ancient Orient* (NY 1962)
- Mosshammer, A.: "Thales Eclipse," *Transactions of the American Philological Association*, vol. III (1981)
- Muhly, J.D. Review of Giovanni Lanfranchi, Michael Roaf, Robert Rollinger, *Continuity of Empire (?)...*, *Bryn Mawr Classical Review*, vol. II, no. 11 (2004) (Internet)  
<http://ccat.sas.upenn.edu/bmcr/2004/2004-11-11.html>
- Muhly, James D.: "Sources of Tin and the Beginnings of Bronze Age Metallurgy," *American Journal of Archaeology*, vol. 89, no. 2 (April 1985)

- Muhly, James D.: "The Beginnings of Metallurgy in the Old World," *The Beginnings of the Use of Metals and Alloys*, Robert Maddin, ed. (Cambridge MA 1988)
- Muhly, James D.: "The Bronze Age Setting," *The Coming of the Iron Age*, T.A. Wertime, J.P. Muhly eds. (New Haven CT 1980)
- Muller, Herbert J.: *The Uses of the Past* (NY 1952)
- Murchie, Guy: *The Seven Mysteries of Life: An Exploration of Science and Philosophy* (NY 1978)
- Muret, Pierre: *Rites of Funeral, Ancient and Modern* (London 1683)
- Nair, K. Shadanan: "Role of Water in the Development of Civilization in India—A Review of the Ancient Literature, Traditional Practices and Beliefs," *The Basis of Civilization. Water Science?* John C. Rodda, Lucio Ubertini, eds. (Wallingford, Oxfordshire UK 2004)
- Needham, Joseph: *Science and Civilization in China*, reprint (Cambridge UK 1997)
- Nemet-Nejal, Karen Rhea: *Daily Life in Ancient Mesopotamia* (Westport CT/London 1998)
- Neugebauer, Otto: *A History of Ancient Mathematical Astronomy* (NY 2004)
- Neugebauer, Otto: *Astronomy and History Selected Essays* (NY/Berlin/Heidelberg/Tokyo 1983)
- Neugebauer, Otto: *The Exact Sciences in Antiquity* (NY 1969)
- Neugebauer, Otto: *The Exact Sciences in Antiquity* (Providence, RI 1957)
- Newton, R.R.: *Ancient Astronomical Observations and Accelerations of the Earth and Moon* (Baltimore MD/London 1976)
- Newton, R.R.: *Ancient Planetary Observations and the Validity of Ephemeris Time* (Baltimore MD/London 1976)
- Newton, R.R.: *Medieval Chronicles and the Rotation of the Earth* (Baltimore MD 1970)
- Newton, R.R.: *The Crime of Claudius Ptolemy* (Baltimore MD/London 1977)
- Niehr, Herbert: "The Rise of YHWH in Judahite and Israelite Religion: Mythological Religio-Historic Aspects, in Diana Vikander Edelman, editor, *The Triumph of Elohim: from Yahwisms to Judaisms* (Kampen, the Netherlands, 1995)
- Nimchuk, Cindy L.: review of "Andrew Ramage, Paul Craddock, *King Croesus' Gold ...*" in *Bryn Mawr Classical Review* 8:10 (2001)
- Ninkovich, D. and B.C. Heezen, "Santorini Tephra," *Submarine Geology and Geophysics*, vol. 17 (1965)
- Oates, J.: *Babylon* (London 1979)
- Oates, Joan and David: *Nimrud, An Assyrian Imperial City Revealed* (London 2001)

- Odenwald, Sten F.: *Back to the Astronomy Café...* (Boulder CO 2003),
- Olmstead, A.T.: *History of Assyria* (London 1923)
- Olmstead, A.T.: *History of the Persian Empire* (Chicago IL/London 1948)
- Oppenheim, A. Leo: "The Babylonian Evidence of Achaemenian Rule in Mesopotamia," *Cambridge History of Iran*, I. Gershevitch ed., vol. 2 (London 1985)
- Oppenheim, A. Leo: "The Cuneiform Texts," in *Glass and Glass Making in Ancient Mesopotamia* (Corning NY 1988)
- Oppenheim, A. Leo: *Ancient Mesopotamia* (revised ed., completed by Erica Reiner, Chicago IL/London 1977)
- Osborne, Esmenia Simmoes: *Climate Change: Critical Concepts in the Environment and Physical Geography* (London/NY 2002)
- Ostler, Nicholas: *Empire of the Word* (NY 2005)
- Overman, Frederick: *A Treatise on Metallurgy: Comprising Mining and General and Particular Metallurgical Operations*, 6th ed. (NY 1865)
- Oxford Atlas of World History*, P. O'Brien ed. (NY 1999)
- Oxford Encyclopedia of Archaeology in the Near East*, E.M. Meyers, ed. (Oxford UK 1997)
- Palmer, Leonard R.: *Mycenaeans and Minoans* (NY 1965)
- Palmer, Trevor: "In Search of Alter Egos," *Chronology and Catastrophism Review* (2004:3+4)
- Pannekoek, A.: *A History of Astronomy* (NY 1961)
- Papaodysseus, C. *et al.*, "Distinct, Late Bronze Age (c. 1650 BC) Wall-Paintings from Akrotiri, Thera, Comprising Advanced Geometrical Patterns," *Archaeometry*, vol. 48, no. 1 (Feb. 2006)
- Parker, Richard R. and Dubberstein, Waldo H.: *Babylonian Chronology 626 B.C.–AD 75* (Providence RI 1971)
- Parry, Adam in Frederick Ahl, Hanna M. Roisman: *The Odyssey Re-Formed* (Cornell NY 1996)
- Pasachoff, Jay M., in Mark Littman *et al.*, *Totality* (NY/Oxford UK 1999)
- Past Worlds, The Times Atlas of Archaeology* (Maplewood NJ 1988)
- Pearce, Nicholas J.G. *et al.*, "Identification of Aniakchak (Alaska) tephra in Greenland ice core challenges the 1645 BC date for Minoan eruption of Santorini," *Geochemistry, Geophysics, Geosystems*, vol. 5, no. 3 (Mar. 20, 2004), p. 1 (Abstract) of Internet online publication (<http://www.agu.org/pubs/crossref/2004/2003GC000672.shtml>)
- Peirce, Charles Sanders: "The Logic of Drawing History from Ancient Documents," *Collected Papers of Charles Sanders Peirce*, vol. 7 (Cambridge MA 1966)



- Peirce, Charles Sanders: *The Essential Peirce: Charles Sanders Peirce, Selected Philosophical Writings*, Christian J.W. Kloesel, ed. (Bloomington IN 1998)
- Pellegrino, Charles: *Unearthing Atlantis* (NY 1991)
- Peregrine, Peter N. and Melvin Ember: *Encyclopedia of Prehistory* (New Haven CT 2002)
- Philip, Graham: "The Metal Objects," *Excavations at Tell Brak*, vol. I (London 1997)
- Pielou, E.C.: *After the Ice Age* (Chicago 1992)
- Piggott, Stuart: *Prehistoric India* (Baltimore MD 1961), quoted in Donovan A. Courville, *The Exodus Problem and Its Ramifications*, vol. I (Loma Linda CA 1971)
- Pingree, David and Hunger, Hermann: *Astral Science in Mesopotamia* (Leiden, the Netherlands 1999)
- Pipkin Bernard *et al.*: *Geology and the Environment* (Independence KY 2005)
- Pittman, Holly: "Jewelry," in Zettler, R.L. and L. Horne, eds.: *Treasures from the Royal Tombs of Ur: A Brief Interpretation* (Philadelphia PA 1998)
- Pleiner, Radomir and Bjorkman, Judith K.: "The Assyrian Iron Age," *Proceedings of the American Philosophical Society*, vol. 118, no. 4 (1974)
- Plutarch's Lives, Harvard Classics no. 12* (NY 1909–14)
- Poole, Stanley Lane: "Metrological Notes on Ancient Electrum Coins," *The Numismatic Chronicle and Journal of the Numismatic Society*, vol. XV, John Evans, ed. (London 1875)
- Porter, Barbara Nevling: "Language, Audience and Impact in Imperial Assyria," *Israel Oriental Studies XV Language and Culture of the Near East* (Leiden, the Netherlands; NY; Cologne, Germany 1995)
- Possehl, Gregory L.: *Ancient Cities of the Indus* (Durham NC 1979)
- Postel, Sandra: *Pillar of Sand. Can the Irrigation Miracle Last?* (NY 1999)
- Potts, Daniel T.: *Mesopotamian Civilization: The Material Foundations* (Ithaca NY 1977)
- Pulsifer, William Henry: *Notes for a History of Lead* (NY 1888)
- Quotes about History* (Internet)
- Raikes, R.: *Water, Weather and Prehistory* (London 1967)
- Ramage, Andrew, Craddock, Paul T. *et al.*, eds.: *King Croesus' Gold*, (Cambridge MA 2000)
- Rapp, George Robert: *Archaeomineralogy* (Berlin Germany 2002),
- Raymond, Robert: *Out of the Fiery Furnace: The Impact of Metals on the History of Mankind* (Pennsylvania State 1986)
- Readers Digest Atlas of the World Rand McNally Maps* (Pleasantville NY 1987)

- Rehren, Thilo and Edgar B. Pusch, "Late Bronze Age Glass Production at Qantir-Piramesses, Egypt," *Science*, vol. 308 (June 17, 2005), pp. 1756-1758
- Reinach, S.: "La représentation du galop dans l'art ancien et moderne," *Revue archéologique*, vol. XXXVIII (1901)
- Reiner, Erica: "Babylonian Celestial Divination," in *Ancient Astronomy and Celestial Divination*, Noel M. Swerdlow ed. (Cambridge MA 1999)
- Reiner, Erica: *A Linguistic Analysis of Akkadian* (London/The Hague/Paris 1966)
- Renfrew, Colin and Whitehouse, R.: "The Copper Age of Peninsular Italy and the Aegean," *Annual of the British School of Archaeology in Athens*, vol. 69 (1974)
- Renfrew, Colin: "Kings, tree rings and the Old World," *Nature*, vol. 381 (June 27, 1996)
- Renfrew, Colin: *Before Civilization* (NY 1973)
- Rice, Michael: *The Archaeology of the Arabian Gulf* (London 1994)
- Rich, Vincent: *The International Lead Trade* (Cambridge UK 1994)
- Roaf, M.: *Sculptures and Sculptors at Persepolis* (Iran 21) (London 1983)
- Roaf, Michael : "The Median Dark Age," *Continuity of Empire (?) Assyria, Media, Persia*, Giovanni B. Lanfranchi *et al.*, eds., (Padova, Italy 2003)
- Robson, Eleanor: *Mesopotamian Mathematics: Some Historical Background* ([www.hps.cam.ac.uk/dept/robson-background.pdf](http://www.hps.cam.ac.uk/dept/robson-background.pdf))
- Rochberg, Francesca: *The Heavenly Writing: Divination, Horoscopy, and Astronomy in Mesopotamian Cultures* (Cambridge UK/NY 2004)
- Rodda, John C. and Ubertini, Lucio, eds.: *The Basis of Civilization. Water Science?* (Wallingford, OX, UK 2004)
- Root, Margaret Cool: "Art and Archaeology of the Achaemenid Empire," *Civilizations of the Ancient Near East*, vol. II, J.M. Sassoon *et al.*, eds. (NY 1995)
- Rose, Lynn E. and Vaughan, Raymond C.: "The Venus Tablets: A Fresh Approach?," *Kronos*, vol. X, no. 2 (Winter 1985)
- Rose, Lynn E.: "A Lowered Chronology for the Twelfth Dynasty," *The Velikovskian*, vol. II, no. 4 (1994)
- Rose, Lynn E.: "From Calendars to Chronology," in *Stephen J. Gould and Immanuel Velikovsky. Essays in the Continuing Velikovsky Affair*, Dale Ann Pearlman, ed., (Forest Hills NY 1996)
- Rose, Lynn E.: "Just Plainly Wrong: A Critique of Peter Huber," *Scientists Confront Scientists Who Confront Velikovsky*, *Kronos*, vol. IV, no. 2 (Winter 1978)
- Rose, Lynn E.: "The Lunar Data from the Reign of Nabopolassar", in press
- Rose, Lynn E.: *Ptolemy the Shadow Maker* (unpublished)

- Rosicrucian Digest 1950* (2004)
- Rosten, Leo: *Leo Rosten's Carnival of Wit* (1996)
- Rostovzeff, M.: *Iranians and Greeks in Southern Russia* (Oxford UK 1922)
- Roux, Georges: *Ancient Iraq*, 3rd ed. (Berkeley CA 1999)
- Sagan, Carl: *Broca's Brain* (NY 1979)
- Saggs, H.W.F.: *Babylonia* (hardcover ed.) (Berkeley CA, 2000)
- Saggs, H.W.F.: *Civilization Before Greece and Rome* (New Haven CT/London 1989)
- Saggs, H.W.F.: *Everyday Life in Babylonia and Assyria* (London/NY 1965)
- Saggs, H.W.F.: *The Greatness That Was Babylon* (NY 1962)
- Saggs, H.W.F.: *The Might That Was Assyria* (London 1984)
- Salisbury, Frank B.: "Biological Clocks," *Discoveries in Plant Biology*, vol. I, Sham-Dow Kung and Shang-Fa Yang, eds. (Singapore/London 2005)
- Salunke, D.K. and Kadem, S.S.: *Handbook of Vegetable Science and Technology* (NY/Basel, Switzerland 1998)
- Samuel, Alan E.: *Greek and Roman Chronology: Calendars and Years in Classical Antiquity* (Munich, Germany 1972)
- Sánchez, Jean-Pierre: *The Classical Tradition and the Americas* (Berlin Germany/Washington DC 1994)
- Sancisi-Weerdenburg, H. and A. Kuhrt, eds.: *Achaemenid History IV: Centre and Periphery* (Leiden, the Netherlands 1990)
- Sancisi-Weerdenburg, H.: "Was there ever a Median Empire?" *Achaemenid History III. Method and Theory*, A. Kuhrt, H. Sancisi-Weerdenburg eds., (Leiden, the Netherlands 1988)
- Sancisi-Weerdenburg, Heleen: "Darius I and the Persian Empire," *Civilizations of the Ancient Near East*, vol. II, J.M. Sassoon *et al.*, eds. (NY 1995)
- Sancisi-Weerdenburg, Heleen: "The Quest for an Elusive Empire," *Achaemenid History IV Centre and Periphery*, H. Sancisi-Weerdenburg, A. Kuhrt, eds. (Leiden, the Netherlands 1990)
- Sanderson, Ivan: *Investigating the Unexplained* (Englewood Cliffs NJ 1972)
- Sarton, George: *Ancient Science Through the Golden Age of Greece* (NY 1952)
- Sassoon, J.M. *et al.*, eds.: *Civilizations of the Ancient Near East* (NY 1995)
- Sayce, A.H., quoted in J.P. Mallory: *In Search of the Indo-Europeans* (London 1989)
- Schmidt-Nielsen, Kurt: "The Psychology of the Camel," *Scientific American*, vol. 201, no. 6 (Dec. 1959)

- Schove, D. Justin: *Chronology of Eclipses and Comets* (NY 1984)
- Schulman, E.: "Bristlecone Pine, Oldest Known Living Thing," *National Geographic Magazine* (March 1958)
- Sham-Dow Kung and Shang-Fa Yang, eds.: *Discoveries in Plant Biology*, vol. I (Singapore/London 2005)
- Sharma, Mano: *History of World Civilization* (New Delhi, India 2005)
- Sharon, Ilan: "Phoenician and Greek Ashlar Construction Techniques at Tel Dor," *Bulletin of the American School of Oriental Research*, no. 267 (Aug. 1987)
- Sherman, David M.: *Tending Animals in the Global Village* (NY 2002)
- Silver, Morris: *Economic Structures of Antiquity* (Westport CT 1995)
- Singh, Gopal: *A Geography of India* (Atma Ram India 1976)
- Singh, Nimal Tej: *Irrigation and Soil Salinity in the Indian Subcontinent: Past and Present* (Cranbury NJ 2005)
- Smith, George: *The Assyrian Eponym Canon* (London, undated)
- Smith, Homer W.: *Man and His Gods* (Boston MA 1952)
- Smith, Laurence E.D.: "Drainage Needs in the Lower Indus, Pakistan," *Water for Agriculture: Irrigation Economics in International Perspective*, Stephen Merrett ed. (London/NY 2002)
- Smith, Sidney: "Ashurbanipal and the Fall of Assyria," *CAH*, vol. 3 (Cambridge UK 1961)
- Smith, Sidney: *Alalakh and Chronology* (London 1940)
- Smith, Sidney: *Babylonian Historical Texts* (London 1924)
- Smith, Sidney: *Babylonian Historical Texts: Relating to the Capture and Downfall of Babylon* (NY 1975)
- Snell, Daniel C.: "Methods of Exchange and Coinage in Ancient Western Asia," *Civilizations of the Ancient Near East*, vol. III (NY 1995)
- Snell, Daniel C.: *Life in the Ancient Near East* (New Haven CT 1997)
- Sommerfeld, Walter: "The Kassites of Ancient Mesopotamia: Origin, Politics, and Culture," *Civilizations of the Ancient Near East*, vol. II (NY 1995)
- Spencer, Colin: *The Heretic's Feast* (Hanover NH 1995)
- Steel, Duncan: *Eclipse* (Washington D.C. 2001)
- Steele, John M.: "Eclipse Prediction in Mesopotamia," *Archive for History of Exact Sciences*, vol. 54, no. 2 (2000)
- Steele, John M.: *Observation and Prediction of Eclipse Times by Early Astronomers (Archimedes)* vol. 4 (Boston/London 2000)

- Stein, Diana L.: "Art and Architecture," *The Hurrians*, Gernot Wilhelm ed. (Jennifer Barnes transl.) (Warminster UK 1989)
- Stephenson, F. Richard: *Historical Eclipses and Earth's Rotation* (Cambridge UK 1977)
- Stern, Ephraim: "Cities of the Persian Period," in E. Meyers (ed.), *The Oxford Encyclopedia of Archaeology in the Near East*, E.M. Meyers, ed. (Oxford UK 1997)
- Stern, Ephraim: *Archaeology of the Land of the Bible*, vol. II (NY/London et al. 2001)
- Steve Farmer, Richard Sprout, Michael Witzel, "The Collapse of the Indus-Script Thesis: The Myth of a Literate Harappan Civilization," *Electronic Journal of Vedic Studies (EJVS)*, vol. 11, no. 2 (Dec. 13, 2004)
- Stiebing, William H., Jr.: "Cosmic Catastrophism", *Aeon* vol. II, no. 6 (May 1992)
- Stiebing, William H., Jr.: "Heinsohn's Revised Chronology," *Aeon* vol. II, no. 5 (1991)
- Stieglitz, Robert R.: "Commodity Prices at Ugarit," *Journal of the American Oriental Society*, vol. 99, no. 1 (Jan-Mar 1979)
- Stronach, David: "Independent Media: Archaeological Notes from the Homeland," *Continuity of Empire (?)*, G.B. Lanfranchi et al., eds. (Padova, Italy 2003)
- Swaddings, Judith: *The Ancient Olympic Games*, 2nd ed. (Austin TX 1999)
- Sweeney, Emmet J.: "Did Artaxerxes III Despoil the Temple in Jerusalem?," *Chronology & Catastrophism Review* (2002-2)
- Sweeney, Emmet J.: *Ramessides, Medes and Persians* (Forest Hills NY 2000)
- Talbot Rice, Tamara: *Scythians* (NY 1957)
- Temporini, Hildegard, in Haase, Wolfgang and Temporini, Hildegard, eds.: *Aufstieg und Niedergang der Römischen Welt. Geschichte und Kultur Roms im Spiegel der neueren Forschung*, part II, vol. 16, Berlin, Germany/NY 1978)
- Theological Dictionary of the Old Testament*, G. Johannes Botterweck et al., eds. (Grand Rapids MI 1988)
- Thiele, Edwin R.: *A Chronology of the Hebrew Kings* (Grand Rapids MI 1977)
- Thomas, William John et al.: *Notes and Queries* (London 1864)
- Thompson, J.A. : *The Bible and Archaeology*, 3rd ed., fully revised (Grand Rapids MI 1973)
- Thompson, John Jason: *Sir Gardner Wilkinson and His Circle* (Austin TX 1992)
- Thureau-Dangin, F. and Maurice Dunand: *Til-Barsib* (Paris 1936)
- Treister, Michael Yu.: *The Role of Metals in the Ancient Greek History* (Leiden, the Netherlands 1996)

- Trigger, Bruce C.: "Writing Systems: a case study in cultural evolution," *The First Writing Script Invention as History and Process*, Stephen D. Houston, ed. (Cambridge UK 2004)
- Twain, Mark: "Preface" *The tragedy of Pudd'nhead Wilson and the Comedy of Those Extraordinary Twins* (Oxford UK 1996)
- Tyldesley, Joyce: *Hatshepsut the Female Pharaoh* (London 1996)
- van de Mieroop, Marc: *A History of the Ancient Near East ca. 3000-323 B.C.* (Victoria, Australia 2004)
- van de Mieroop, Marc: *A History of the Ancient Near East ca. 3000-323 B.C.*, 2nd ed. (Oxford UK 2006)
- van de Mieroop, Marc: *Cuneiform Texts and the Writing of History* (London 1999)
- van den Bergh, G.: *Periodicity and Variation of Solar (and Lunar) Eclipses* (Haarlem, the Netherlands 1955)
- van Sertima, Ivan: *The Golden Age of the Moor*, 2nd ed. (Piscataway NJ 2004)
- Veenhof, K.R.: *Aspects of Old Assyrian Trade and its Terminology* (Leiden, the Netherlands 1972)
- Velikovsky, Immanuel: "From the End of the Eighteenth Dynasty to the Time of Ramses II," *Kronos*, vol. III, no. 3 (1978)
- Velikovsky, Immanuel: *Oedipus and Akhnaton* (NY 1960)
- Velikovsky, Immanuel: *Peoples of the Sea* (NY 1977)
- Velikovsky, Immanuel: *Worlds in Collision* (NY 1950)
- Vikander Edelman, Diana, editor: *The Triumph of Elohim: from Yahwisms to Judaisms* (Kampen, the Netherlands, 1995)
- Vitaliano, Dorothy B.: *Legends of the Earth* (Bloomington IN/London 1973)
- von Soden, Wolfram: *The Ancient Orient: An Introduction to the Study of the Ancient Near East*, Donald G. Schley, transl. (Grand Rapids MI 1994)
- Waldbaum, Jane C.: "The First Archaeological Appearance of Iron, *The Coming of the Iron Age*, T.A. Wertime, J.P. Muhly eds. (New Haven CT 1980)
- Walker, C.B.F.: *Cuneiform* (Berkeley CA 1987)
- Waterman, Leroy: *Royal Correspondence of the Assyrian Empire*, vol. I (1930-1936)
- Weaver, Warren quoted in *Leo Rosten's Carnival of Wit*
- Webster Wilde, Lyn: *On the Trail of Women Warriors* (NY 2000)
- Weiner, Malcolm H.: "The Absolute Chronology of Late Helladic IIIA2," *Sardinian and Aegean Chronology: Proceedings of the International Colloquium 'Sardinian Stratigraphy and Mediterranean Chronology'*, Tufts University, March 17-19, 1995

- Weiner, Malcolm H.: "The Absolute Chronology of Late Helladic III A2 revisited" in *Sardinian and Aegean Chronology towards the Resolution of Relative and Absolute Dating in the Mediterranean*, M. S. Balmuth, R. H. Tykot, eds., *Annual of the British School at Athens*, vol. 98 (Oxford UK 1998)
- Weiss, H.: "Beyond the Younger Dryas Collapse as adaptation to abrupt climate change in ancient West Asia and the Eastern Mediterranean," *Environmental Disaster and the Archaeology of Human Response* (University of New Mexico, NM 2000)
- Wells, Ronald A.: Review of Lynn E. Rose. *Sun, Moon, and Sothis: A Study of Calendars and Calendar Reforms in Ancient Egypt*. *Journal of Near Eastern Studies*, vol. 61, no. 4 (Oct. 2002)
- Wenke, Robert J.: *Patterns in Prehistory: Mankind's First Three Million Years* (NY/Oxford UK 1980)
- Wertime, T.A. and Muhly, J.P., eds.: *The Coming of the Iron Age* (New Haven CT 1980)
- Wheeler, Sir Mortimer, quoted in William Kirk: "The Role of India in the Diffusion of Early Cultures," *Geographical Journal*, vol. 141, no. 1 (March 1975)
- Wheeler, Sir Mortimer: *Civilizations of the Indus Valley and Beyond* (London 1966)
- Whewell, William quoted in *The New Dictionary of Thoughts* (NY 1961)
- Widengren, Geo: "The Persians," *People of Old Testament Times*, O.J. Weissman, ed. (Oxford UK 1973)
- Wilde, Oscar, in Ringo, Miriam: *Nobody said it better* (Edison NJ 1984)
- Willets, R.F.: *The Civilization of Ancient Crete* (London/NY 1976)
- Wilson, A.J.: *The Living Rock: The Story of Metals Since Earliest Times and Their Impact on Civilization* (Cambridge UK 1996)
- Wilson, Colin: *From Atlantis to the Sphinx* (NY 1996)
- Wilson, Ian: *Before the Flood* (NY 2004)
- Wilson, John A.: *The Culture of Ancient Egypt* (Chicago IL 1962)
- Wolpert, Stanley: *A New History of India*, 5th ed. (NY/Oxford UK 1997)
- Wood, Michael: *In the Footsteps of Alexander the Great* (Berkeley CA 1997)
- Woodward, William E.: *Meet General Grant* (NY 1928)
- Woolley, Sir Leonard: *Alalakh, An Account of the Excavations at Tel-Atchana in the Hataq* (Oxford UK 1995)
- Yener, K. Aslihan: "The Archaeometry of Silver in Anatolia: The Bolkardag Mining District," *American Journal of Archaeology*, vol. 90, no. 4 (Oct. 1986)
- Yener, K. Aslihan: *The Domestication of Metals: The Rise of Complex Metal Industries in Anatolia* (Leiden, the Netherlands, et al., 2000)

- Yoffee, Norman: *Collapse of Mesopotamian States and Civilization* (Tucson AZ 1988)
- Zangger, Eberhard: *The Future of the Past: Archaeology in the 21st Century*, transl. Storm Dunlop (London 2001)
- Zarins, J.: "The Camel in Ancient Arabia: Further Note," *Antiquity* (March 1978)
- Zebrowski, Ernest: *Perils of A Restless Planet. Scientific Perspectives on Bronze Age Natural Disasters* (Cambridge UK 1997)
- Zeilinge de Boer, Julie and Donald Theodore Sanders: *Volcanoes in Human History. The Far-Reaching Effects of Major Volcanoes* (Princeton NJ 2002)
- Zettler, R.L. and L. Horne, eds.: *Treasures from the Royal Tombs of Ur: A Brief Interpretation* (Philadelphia PA 1998)
- Zielinski, Gregory A., and Mark S. Germani: "New Ice-Core Evidence Challenges the 1620s BC age for the Santorini (Minoan) Eruption", *Journal of Archaeological Science* vol. 25, issue 3 (March 1998)



## **APPENDIX**

### **THE ASTRONOMICAL EVIDENCE FOR A SHORTENED CHRONOLOGY IN FIRST-MILLENNIUM MESOPOTAMIA**

**Lynn E. Rose**

This Appendix begins with my essay on the Saturn Tablet of Kandalanu. That essay was my first effort in Neo-Assyrian astronomy, and can be regarded as laying the foundation for all that followed. Next is a summary of the numerous cases in which Reports or other documents dating the Neo-Assyrians to the eighth or seventh century have been successfully moved to the fifth or fourth century. Then I discuss the month-length evidence pertaining to Nabopolassar, as well as the eclipse of the Moon in Year 2 of Nabonidus. This is followed by a discussion of the important difference between those contemporary Neo-Assyrian and Neo-Babylonian documents that can, with due effort, be moved to the fifth, fourth, or third century and those other documents (including several Diaries as well as various retrospective Seleukid texts) that contain dates that prove very difficult if not impossible to move at all. The Appendix then closes with a personal rant.

The Kandalanu essay has not been significantly modified, although the References that were originally at the end of the essay have been moved to the end of the Appendix and merged with References pertaining to other sections of the Appendix.

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#### **THE SATURN TABLET OF KANDALANU**

The Saturn Tablet, BM 76738 + 76813, which was previously known as AH 83-1-18, 2109 + 2185, is a joined fragment that gives the lunar dates of the first appearances and of the last appearances of Saturn during Years 1-14 of

Kandalanu. (A few traces of the “alanu” part of the name Kandalanu have survived in line 1.) The join was made by Irving L. Finkel. According to the conventional chronology, Kandalanu ruled in Babylon following the death of Shamash-shum-ukin, which occurred roughly mid-way through the reign of Ashurbanipal.

This Saturn Tablet has been principally analyzed by Christopher B. F. Walker, first in 1983, as part of a more general project, and then in 1999, in a dedicated treatment of the Saturn Tablet itself, replete with a drawing of the joined fragment, a transliteration, a translation, and various astronomical retro-calculations, both Saturnian and lunar. There is no photograph, however. A photograph might have been very helpful, especially with regard to the traces of the name Kandalanu. In my opinion, Walker’s reading of those traces as an “a” followed by a bracketed “lanu” is correct, but one does wish that the name of the king were a bit less problematic.

Walker’s own position regarding those traces is neither very clear nor even consistent. On the one hand, he says (page 63) that “the name of Kandalanu is to be restored from only a few traces in the first line”. This seems to mean that one can indeed read those traces as the final portion of the name Kandalanu. On the other hand, he says (page 74, note 1) that “The name Kandalanu is restored in the light of astronomical considerations, but the position in the line of the sign ITU [meaning “month”] already indicated that it was preceded by a royal name.” This might be taken to mean that the name itself *cannot* be read from the tablet, and that those “astronomical considerations” are the *sole* grounds upon which the association with Kandalanu rests. If the astronomical fit is bad, or if there is a significantly better astronomical fit somewhere else, does that mean that the association with Kandalanu goes by the boards? These issues never cross Walker’s mind. To him (page 63), it is “certain that we are dealing with Saturn and Kandalanu”, and “there is no difficulty in determining the date of the present text”. I can agree that “we are dealing with Saturn and Kandalanu”, but the *date* is quite another matter. (Perhaps he means that one *can* read the traces as the name of Kandalanu, but that one can *also* date the Saturn Tablet to Kandalanu on the basis of astronomy. I might agree with that, too — though I favor a fourth-century date rather than a seventh-century date!)

Even the provenance of the Saturn Tablet is unrecorded. Like so many other such treasures, it was acquired somewhere in what is now Iraq, then

shipped back to England, and finally deposited somewhere in the storerooms of the British Museum — only to lie ignored for nearly a century.

According to Walker, although the “AH” would be Abu Habba (Sippar), the Saturn Tablet might be from Babylon or perhaps even from Borsippa. On this point, Walker cites Julian E. Reade (1986), page xxxiv, to the effect that Hormuzd Rassam’s “AH” or Sippar collection did also include items from Babylon and Borsippa.

Determination of provenance can be notoriously difficult at times — as well as bizarre. Sometimes the only clue as to which expedition might have found something, and thus as to *where* it might have been found, is the date of the newspaper in which it has lain wrapped since the nineteenth century.

I shall retrocalculate for Babylon, with Carl Schoch’s Tables (1928), which were specifically designed for Babylon, anyway. The other two possible sites for the Saturn Tablet are not that far away in any case.

Walker (1999, page 63), states that an identical fit between the Saturn reports and the retrocalculated positions of Saturn and of the Moon would not recur for over seventeen hundred years. (Actually, in his 1983 paper, page 20, Walker had said “approximately every 1770 years”.)

Saturn will return to the same part of the sky at about the same time of the year after two sidereal periods, which would be a little under 59 years. In order for the heliacal phenomena to fall on about the same days of the lunar month, however, an additional factor of 29 or 30 would be involved. Walker used the formula  $59 \times 30 = 1770$ . But 1770 is of course a very *rough* figure, and one could quibble about the exact number of years. Two sidereal periods of Saturn would actually be just over 58.9 years, and if we multiply that by 29.53059, which is the mean lunar month, we get something in the neighborhood of 1739 or 1740 years. Walker must eventually have realized the inaccuracy of the 1770 years, and he retreated from that figure. In his second paper, he is content to say “rather more than 17 centuries” (page 63), and to let it go at that.

After having seen that second paper, some time prior to its publication, Carl Olof Jonsson (1998, pages 169-171), stresses Walker’s figure of over seventeen hundred years, and argues that the Saturn Tablet shows that the usual dating of the reign of Kandalanu (from -646 to -625) is correct. Walker

himself is not overly concerned about chronological issues; that is perhaps why he could so boldly and blithely infer from the recorded observations that “there is no difficulty in determining the date of the present text” (1999, page 63). Walker seems to be mainly interested in assessing the accuracy of the recorded observations, while Jonsson and I are primarily interested in using those observations to investigate chronology. Unlike both Jonsson and Walker, however, I do *not* find that the Saturn Tablet itself proves that the traditional dating of Kandalanu’s reign is correct. Indeed, I shall on strictly astronomical grounds be considering a *fourth-century* placement of the Saturn Tablet — and of Kandalanu himself — despite all of the problems that may arise with such a placement.

For the Saturn calculations, Walker dutifully mentions Schoch, but finds it easier to use the tables of Bartel L. van der Waerden (1943), together with the tables of Hermann Hunger and Rudolf Dvorak (1981). The Hunger and Dvorak tables are a backward extension of Bryant Tuckerman’s tables (1962 and 1964), except that, for the positions of Saturn and Jupiter, the equations of Pierre Bretagnon (1978) are used in preference to the equations of Aimable Gaillot (1888). Walker’s real problem is not with Saturn conjunctions, however, or even with the lunar New Crescents. His real problem is that when the Saturn conjunctions are supplemented with van der Waerden’s highly suspect intervals between the conjunctions and the last and first visibilities, the results cannot be trusted. I have looked at the Saturn output from the programs of Rainer Lange and Noel M. Swerdlow, but I have preferred to rely upon Schoch’s own Saturn Tables, *directly*, and *without* the van der Waerden “improvements”. The results are significantly different from those of Walker. It might also be noted that the Hunger and Dvorak tables themselves have been rather severely criticized. See Peter J. Huber’s review (1983, pages 228-229).

For the lunar calculations, Walker relies upon Huber’s unpublished work. I am not on good terms with Huber (it must have been something that I wrote), and I have perforce relied for the most part upon Schoch’s own dates for the Moon. I have frequently used *Starry Night* as well, and I have looked at but not relied upon the programs of Lange and Swerdlow, which are very convenient, and are therefore quite useful as a quick guide or check, despite the fact that I have found that they sometimes allow too much visibility. For the fourth century only, I have primarily used Richard A. Parker and Waldo H. Dubberstein (1956), who start their Schoch-based tables with 626 B.C. historical or, as I prefer to say, with -625 astronomical. That is the year when Nabopolassar is thought to have

succeeded Kandalanu, following two short reigns *within* -625. If there seem to be any discrepancies, I have relied upon Schoch's lunar Tables to settle the matter.

It should be noted at the outset that retrocalculation of the first visibility or the last visibility of a slow-moving outer planet like Saturn is a very delicate procedure. We cannot expect perfection or exactitude, and the results could easily be off slightly. Schoch estimates his own possible error for Saturn at no more than one day (page 96), but there is also the possibility that bad seeing might occasionally delay the onset of a lunar month by a day or so, which would in turn affect the lunar date of any Saturnian or other events that are recorded in the lunar calendar. The procedure that the ancients seem to have used is described as follows by Schoch (page 98):

“Both the Babylonians and the ancient Jews in determining the beginning of the month were interested only in observations made in the evening which closed the 29<sup>th</sup> day of the month. If the crescent was not seen, the day beginning at sunset was regarded as the 30<sup>th</sup> of the old month; if it was seen, that day was the 1<sup>st</sup> of the new month. The observation at the close of the 30<sup>th</sup> day had astronomical interest only, and had no bearing on the calendar.”

Schoch was eager to make his Tables as simple as possible for users, and he was quick to round off numbers whenever that was permissible in terms of the purposes at hand. The result was that his sample cases used varying degrees of simplification and rounding. Sometimes, for example, he might have been interested only in the *mean* conjunction. Even in dealing with lunar New Crescents, however, the answer is often clear-cut. Thus on page 100 he says that, “It is generally sufficient to take T [time of day] in each table to two decimal places and L [longitude] to one decimal place only”, and on page 101 he says that, “Where the difference between the [actual and the necessary] intervals amounts to 0.4<sup>h</sup> or less, it is desirable to repeat the computation with the full number of places given in the tables.” In working with Schoch's Saturn Tables, I have assumed that the time of day of the conjunction is significant, as it most assuredly is in the case of the Moon, and I have used *that* moment as the base from which the interval from conjunction to the last or to the first visibility is to be measured. I have also used fractional values for those intervals where required, and I have even taken into account the times of day when last and first visibilities would have occurred: last visibility was always a little after sunset and first visibility was always a little

before sunrise. My attention to these details may seem excessive. The verdict on that will depend upon how well my results conform to the Saturn Tablet itself, as well as to the great multitude of other astronomical documents from antiquity. So far, what I have been finding is that my attention to these details is both justified and well worth the bother.

It might be noted in passing that in Summer and Autumn van der Waerden more often than not shortens Schoch's intervals between the conjunctions and the last and first visibilities by one day. The result is that, at both ends of the intervals of invisibility, van der Waerden is recognizing too much visibility. Lange and Swerdlow, as noted, also allow too much visibility at times, even though they are far less off the mark than van der Waerden. My own fussiness about the time of day of the conjunctions and about the time of day of the last visibilities and the first visibilities will of course permit *less* visibility than one gets from van der Waerden.

The difficulty of calculating the first and last visibilities of Saturn can also be appreciated when we realize that the sidereal position of the Moon changes at a rate of over half a degree per *hour*, while Saturn (ignoring retrogradation) advances with respect to the stars only about one degree per *month*. Even Saturn's angular distance from the Sun changes by only about one degree per *day*, and nearly all of that is because the motion of the Sun with respect to the stars is so rapid as compared to that of Saturn. Thus the visibility conditions for Saturn do not change very much from day to day. No wonder that Schoch claimed accuracy only to within one day for Saturn! Subsequent work has not improved upon this. Too many of the circumstances affecting the viewing conditions are simply not known, and there is no way to compensate for our ignorance. Also, even though the gradual slowing of Earth's rotation since antiquity has been much studied during the past century or so, the estimates of the accumulated change in the clock-time are still rather rough. Schoch has by no means become outmoded.

Schoch is in fact much *better* at retrocalculation than many of his successors, even those with powerful computers and supposedly sophisticated programs, and Schoch's Saturn Tables will be more than adequate for our purposes here. As Parker and Dubberstein have noted, "it is possible that a certain number of dates in our tables may be wrong by one day, but as they are purely for historical purposes, this uncertainty is unimportant" (page 25). I might have been a little

happier with “sometimes unimportant”, but of course I am quite the nit-picker, as Parker himself once felt compelled to note!

The fourteen years that are covered in the surviving portion of the Saturn Tablet would have involved twenty-eight dates, fourteen for the first visibilities in the east in the morning and fourteen for the last visibilities in the west in the evening. The implied periods of invisibility would usually have been of about a month but sometimes a few days longer. Of the twenty-eight Saturn events that our surviving fragment would have covered, only twenty are ones that Walker actually uses. Most of the remaining dates are either missing entirely or too damaged to use.

Not all of these first or last appearances were observed; some have notations like “(because of) cloud not observed”. Presumably all of the ones not observed were computed or at least estimated. One of them explicitly *states* “(because of) cloud computed”. We must of course confine our analysis to the items that seem to have been actually *seen*, for our concern here is only with the *observations* on the Saturn Tablet, not with any calculations or estimates that may have been made.

Two of those remaining eight events are merely said to have occurred at the end of some named month, which is too imprecise for our purposes. Even on the questionable assumption that they meant the very last day of the lunar month, we would still not know whether it was day 29 or day 30 — and I do not see how to exclude their even having meant day 1 of the *next* month. One of these two end-of-month events was “not observed”, anyway, which may suggest that “end of month such-and-such” was a bluff answer used when they did not know the actual date. In any case, I agree with Walker’s decision to drop the other “end of month such-and-such” entry, too. Even if it *was* observed, the actual lunar *date* is unknown. We cannot fit nothing.

Two of the entries note that Saturn was “high”, which seems to mean that it was decidedly higher in the sky than it would have been at first appearance. In other words, for whatever reason, Saturn was not actually seen until some time after it should already have been visible. This delay could have been due to bad seeing, or to the press of other matters, or to who knows what. One of these “high” entries lacks a day of the month, and Walker has doubts about the month as well. He of course decides that he cannot use that report at all. The other “high” report has a complete date, and he keeps it, even though he counts it as missing by 5 days.

I find that this one misses for him by only 3 days. Coincidentally, my fourth-century placement of Kandalanu has it miss by 5 days. But none of that means anything, since a report of that sort is of no value for the exact matching that we are interested in here. So I am throwing this one out, too. There is simply no way that a “high” sighting can be expected to match up with retrocalculation, on *any* likely chronology.

Walker (1999, page 62), refers to these “high” sightings as off by only “a day or two”. I think that these sightings could easily have been off by many times that. Quite aside from bad seeing, the observers could have been kept from their posts by rebellion, purge, famine, plague, war, or who knows what else. I am not claiming that any of those things actually occurred, of course, but only illustrating by extremes that we have no basis whatsoever for *assuming* that the interval was only “a day or two”. In any case, since the scribe has made it plain enough that this is *not* an observational datum concerning astronomical first visibility, we must not treat it as if it were.

We can keep the “20+7/8”, since it is clearly either a day 27 or a day 28, and we can then simply allow *either* a retrocalculated 27 *or* a retrocalculated 28 to be recognized as a hit. (Taking it as 27½ is undesirable, because that would make it impossible for anyone to score less than ½ — in other words, *better than* ½. It should always remain *possible* for a recorded date to score a perfect 0, at least on *some* chronology.)

Walker rejects the “20+[x]” and the two occurrences of “10+[x]”, but he keeps the “[x]+1”, which happens to be the first visibility that follows the just-discussed last visibility that was on either day 27 or day 28. He does not explain himself here, but I assume that his reasons for keeping the “[x]+1” have to do with the fact that “[x]+1” (we do not know what, if anything, preceded the “1”) may be giving us the actual final digit of the day-number, whereas a “10+[x]” or a “20+[x]” offers us no clue as to what the final digit is. Another consideration is that reading “[x]+1” as day 1 leaves the interval between last visibility on day 27 or day 28 and first visibility on day 1 at 1 month 2 to 4 days, which may already be the largest determinable such interval that appears in the Saturn Tablet; the other three are 1 month exactly, 1 month 2 days, and then another 1 month 2 days. (This consideration would also favor “28” rather than “27” for that last visibility reported in the previous line of the Saturn Tablet, but we do not know.) Thus I concur with Walker’s decision to keep the “[x]+1”. An alternative way of approaching this is



that reading “[x]+1” as day 1 leaves the interval between that first visibility and the subsequent last visibility at 11 months 19 days, which is already one of the shorter determinable such intervals that appear in the Saturn Tablet; the other three are 11 months 20 days, 11 months 22 or 23 days, and what would be a *second* 11 months 19 days.

In summary, then, I am modifying Walker’s list of usable dates in only three respects: I reject the “high” date, the “cloud computed” date, and the “[cloud?], not [observed?]” date. None of these are actual *observations* that we can then compare to retrocalculated dates of heliacal events.

That leaves us with seventeen usable dates, and I am more than willing to let Walker’s fate and Kandalanu’s — as well as my own — rest upon those seventeen dates.

For ease of comparison, the following table is arranged somewhat like the middle and right-hand portions of Walker’s own table on pages 66-67. The first column lists the Julian year for each pair of events. The second column lists the retrocalculated successive Julian dates of the first appearance and the last appearance of Saturn. The third column lists the retrocalculated Julian date of the immediately preceding first visibility of the Moon. The fourth column lists the lunar day upon which the first appearance or the last appearance of Saturn would have occurred. The fifth column lists any surviving lunar day that we might wish to compare to the fourth column; this is expressed in terms of the regnal year, the Assyro-Babylonian lunar month (by number), and the day of that lunar month. (Some of those date-elements, absent but obvious, are bracketed.) The sixth and seventh columns give the number of days by which the recorded day for each sort of event — first visibility or last visibility, respectively — differs from the calculated day.

It might seem natural to us to group a last visibility with a first visibility. After all, any two such events are separated by only one month or just a little more, and they do here fall in the same regnal year as well as in the same Julian year. Furthermore, we usually compute the last and first visibilities from the intervening conjunction of Saturn with the Sun. But that is *not* how the Saturn Tablet arranges the various observations. Straight horizontal rulings across the face of the Saturn Tablet separate the entries into pairs: one first visibility and one last visibility, separated by nearly a year. The events thus being paired belong not only to different regnal years but also to different Julian years, which may seem

quite awkward to us. This practice does nonetheless make sense in that the focus was on periods of visibility. Instead of horizontal rulings I use blank lines, but I keep the same organization as the original. We should not allow our own preferred format to obscure the fact that the Saturn Tablet focuses on intervals of *visibility*, and *not* on periods of invisibility.

(One must also remember that the Julian date upon which the last visibility occurred was one day prior to the Julian date that is customarily equated with the Babylonian date that began with the last visibility in question. Hence the way in which the last visibilities are being scored may *appear* to be one day off. But such scores are really *not* one day off. The Babylonian day began at dusk, but the Julian day begins at midnight. Think all of that through before you try any of this at home.)

I shall present two tables, one for the seventh century, and one for the fourth century (which is of course where I shall be putting the Saturn Tablet — and Kandalanu himself).

I shall not present Walker's own table, since it is flawed. Suffice it to say that he lists the numbers of days by which the Saturn Tablet differs from retrocalculation. The absolute values of his twenty numbers add up to 40, and  $40 \div 20 = 2$  days, which is the average amount by which his retrocalculated dates differ from his recorded dates. His median score is -2. When we search his twenty numbers for the mode, we find a "tie" between -2 and -3. The algebraic sum of all of his scores is -20. Thus even on the basis of his own stated findings there is a *very severe* shift to the negative. Such large shifts are very much to be expected when someone tries to force-fit a set of astronomical data into a era where that data does not belong. We shall see that the algebraic sum of all of my own scores is only +2.

When I attempt to duplicate Walker's results, but with Schoch, I find that the twenty numbers add up to 36. Note that  $36 \div 20 = 1.8$  days. (Walker is counting the "high" date, the "cloud computed" date, and the "[cloud?], not [observed?]" date, and he would here be counting the "20 + 7/8" as a 28.)

The seventeen numbers that I prefer to use add up (for Walker) to 30. Note that  $30 \div 17 = 1.7647$ .

Either way I do this, his “fit” is in fact *significantly better* than he himself imagines! (Remember that the average amount by which his own retrocalculated dates differ from the twenty recorded dates that he uses was 2 days.)

When I repeat this for the fourth century solution, however, I get a total of only 16 for the misses. Note that  $16 \div 17 =$  only 0.941 days, or about 22 hours and 35 minutes. That is a fairly significant improvement, to say the least.

This is Walker’s situation if we use Schoch’s Tables:

Year	Saturn First/Last Visibility	Most Recent First Lunar Crescent	Retrocal- culated Lunar Day	Recorded Babylonian Year/Month/Day	Last Visibility Score	First Visibility Score
-646	6/12	5/25	20	[1]		
-646	7/20	6/24	26	[1]/4/24		-2
-645	6/26	6/13	14	2/4/10+[x]		
-645	8/2	7/13	20	[2]/5/“broken”, “not [observed]”		
-644	7/9	7/1	9	3/4/7	-2	
-644	8/13	7/31	13	[3]/5/16 “high”		
-643	7/22	7/20	3	4/“end of Tammuz”, “cloud not observed”		
-643	8/26	8/19	4	[4]/“high”		
-642	8/4	7/10	26	[5]/5/23	-3	
-642	9/8	9/7	1	[5]/“end of Ulul”		
-641	8/16	7/28	19	6/5/20	+1	
-641	9/20	8/27	24	[6]/6/22		-2
-640	8/27	8/15	15	7/6/10+[x]		
-640	10/1	9/14	17	[7]/7/15		-2
-639	9/9	9/3	7	8/6/5	-2	
-639	10/13	10/3	10	[8]/7/5		-5
-638	9/21	8/23	30	9/6/20 + 7/8	-2	
-638	10/23	10/22	1	[9]/8/[x]+1		0
-637	10/2	9/11	22	[10]/7/20	-2	
-637	11/4	10/11	24	[10]/8/23 “bright”		-1

-636	10/12	9/29	14	[11]/7/13	-1	
-636	11/14	10/29	16	[11/8]/15		-1
-635	10/23 or 10/24	10/18	6 or 7	[12]/8/5 “cloud computed”		
-635	11/26	11/17	9	[12]/9/5		-4
-634	11/4	10/8	28	13/8/26 “[cloud?], not [observed?]”		
-634	12/7	11/6	1	[13]/10/1		0
-633	11/16	10/27	20	14/8/20	0	
-633	12/19	11/25	24	[14/9]/20+[x]		

The retrocalculated last visibility in -635 may be too close to call. We could in such situations count either of the resulting lunar dates as acceptable. But this one is of course not a *datum*, anyway, since the text says “cloud computed”.

Bad seeing aside, the scores in the next-to-last column are expected to be 0 or below, and the scores in the last column are expected to be 0 or above. Bad seeing aside, any other scores are ones that Walker considers “impossible”. They amount to first visibilities that are earlier than retrocalculation permits, or last visibilities that are later than retrocalculation permits. I consider this use of the word “impossible” rather harsh for some of these recorded dates, and I shall not use such language. It is more important to concentrate on the data *set*, and to see how well it works as a *set*, rather than to worry prematurely about the alleged astronomical “impossibility” of individual dates.

However, I do agree with Walker that a score of -1 in the last column would, in the event of bad seeing, be acceptable — rather than what he calls “impossible” — in that “this allows for the possibility of the Babylonian lunar month starting one day later than calculated” (page 68). In that sense, any -1 score that turns up in the last column would be “acceptable” on the assumption that there was bad seeing. Nonetheless, it would still have to be *scored* as an astronomical miss. Walker has two of these; we shall see that I also have two, but not the same ones as his.

Walker’s disproportionate number of scores of -2 or lower might indeed be due in small part to the sort of bad seeing that would have kept the lunar month from being started on time. But most of this out-of-whack pattern is

obviously due to Walker's having put the Saturn Tablet in a time frame where it does not belong.

Another indication that the time frame must be wrong is that, aside from the three 0's and the single +1, all of the remaining thirteen scores are negative, ranging downward from -1 and including a -3, a -4, and a -5. Once again, this is the gross sort of imbalance that results whenever someone tries to force-fit a set of astronomical data into an era where it does not belong.

Every single one of Huber's relevant New Crescent dates for the seventh century (see Walker's page 67) agrees exactly with *Starry Night* and with the programs of Lange and Swerdlow, as well as with Schoch's Tables. I find it noteworthy that Schoch still holds his own (and more) in this digital age!

Walker's own handling of lunar New Crescents is at best sloppy. On page 67 Walker lists thirty New Crescent dates that he got from Huber. These cover Years 1 through 14 of Kandalanu. (Walker here includes two alternative dates, where it is not immediately clear which New Crescent should be used.) The fourteen Julian years are expressed with astronomical rather than historical dates. The days of the months are the actual Julian dates on which the New Crescents are calculated to have occurred (this would be shortly after Sunset). On pages 70-71 Walker gives what he calls an extension of "Parker and Dubberstein's tables for Babylonian chronology back in a continuous series from the reign of Nabopolassar to the beginning of the reign of Kandalanu" (page 70). For reasons that I do not understand, Walker's table on pages 70-71 covers only Years 2 through 14, despite his own statement that it should go all the way back "to the beginning of the reign of Kandalanu". This means that the table on pages 70-71 contains nothing from Year 1, and that we can compare only 28, not 30, of the dates on page 67 to the corresponding dates on pages 70-71. Walker's intention of "following the same principles as the tables in Parker and Dubberstein" (page 69) complicates matters in two respects. (1) Parker and Dubberstein use historical rather than astronomical dating. (2) Parker and Dubberstein give Julian dates that share their daylight hours with the Babylonian dates on which the New Crescents would have occurred, so that the Julian dates that they give are actually one day later than the Julian dates on which the New Crescents would have occurred. These differences may be tricky, but they can be safely handled, if one is careful. Walker was not very careful. Of the 28 comparable New Crescents given in the table on page 67, I

count no fewer than *six* (10/3, 10/22, 10/11, 10/29, 11/17, and 12/6) that contradict the dates given in the table on pages 70-71!

There are still some retrocalculation issues that I do not yet know how to resolve. Even Lange and Swerdlow follow Schoch in using an *arcus visionis* of  $10^\circ$  for the last visibility of Saturn and an *arcus visionis* of  $13^\circ$  for the first visibility of Saturn about a month later. (That is not to say that output from Lange and Swerdlow *always* agrees with Schoch.)

Schoch (who died in 1929) did not know about the Saturn Tablet, which was still deteriorating in the humid and salty air of some storeroom in the British Museum, and was first described by Walker in 1983. If Schoch *had* known about the Saturn Tablet, he might have reexamined his *arcus visionis* values for Saturn, and our overall situation might be somewhat better. In any case, I still wonder why these *arcus visionis* figures are not more nearly equal, with the last visibility and the first visibility more nearly symmetrical.

Robert D. Purrington (1988), page S79, writes:

“One should also note that in practice the observed values of  $\Delta h$  [*arcus visionis*] for heliacal setting are less than for heliacal rising because it is easier to follow an object into the sun than to look for its appearance, since one may not know precisely, in altitude or azimuth, where to look. . . .”

Indeed, “one may not know” this, but might not *another* know it? It seems to me that a skilled and careful observer might well beat the conventional spread between the *arcus visionis* of the last visibility and the *arcus visionis* of the first visibility, and thereby reduce if not virtually eliminate the usual asymmetry.

Thus I do wonder whether Schoch might have overestimated the *arcus visionis* of the morning or eastern appearances of Saturn. He did base his *arcus visionis* values both on his own observations and on ancient observations. But some of those ancient observations — or *possibly* even his own — may have been late because the ancients — or *possibly* even he himself — did not know exactly where to look or exactly when to look. Either way, we should not be surprised that some of his retrocalculated last visibilities are what Walker considers “impossibly” late, and we should especially not be surprised that some of his retrocalculated first visibilities are what Walker considers “impossibly” early. As we are about to see, I

have three positive scores for first visibility. For whatever it may be worth, *each* of them is in a position to be improved if Schoch's *arcus visionis* of 13° is eventually reduced. Because of his own very severe shift to the negative, Walker has *no* positive scores for first visibilities, and cannot hope to benefit in this way at all!

This is my own situation in the fourth century:

Year	Saturn First/Last Visibility	Most Recent First Lunar Crescent	Retrocal- culated Lunar Day	Recorded Babylonian Year/Month/Day	Last Visibility Score	First Visibility Score
-351	6/18 or 6/19	6/3	16 or 17	[1]		
-351	7/25	7/3	22	[1]/4/24		+2
-350	7/2	6/22	12	2/4/10+[x]		
-350	8/8 or 8/7	7/22	15 or 16	[2]/5/"broken", "not [observed]"		
-349	7/16	7/11	6	3/4/7	+1	
-349	8/20	8/9	11	[3]/5/16 "high"		
-348	7/29	7/28	2	4/"end of Tammuz", "cloud not observed"		
-348	9/1	8/27	5	[4]/"high"		
-347	8/10	7/17	25	[5]/5/23	-2	
-347	9/13	8/16	28	[5]/"end of Ulul"		
-346	8/22 or 8/23	8/5	18 or 19	6/5/20	+1	
-346	9/26	9/4	22	[6]/6/22		0
-345	9/4	8/24	12	7/6/10+[x]		
-345	10/8	9/22	16	[7]/7/15		-1
-344	9/15 or 9/16	9/11	5 or 6	8/6/5	0	
-344	10/18	10/10	8	[8]/7/5		-3
-343	9/27	8/31	28	9/6/20 + 7/8	0	
-343	10/30	10/29	1	[9/8]/[x]+1		0
-342	10/8	9/20	19	[10]/7/20	+1	
-342	11/10	10/19	22	[10/8]/23 "bright"		+1
-341	10/20	10/9	12	[11]/7/13	+1	
-341	11/22	11/7	15	[11/8]/15		0
-340	10/30	10/27	8	[12]/8/5 "cloud computed"		

-340	12/2	11/26	6	[12]/9/5	-1
-339	11/11	10/16	28	13/8/26 “[cloud?] not [observed?]”	
-339	12/14	11/15	29	[13]/10/1	+2
-338	11/23	11/4	20	14/8/20	0
-338	12/26	12/4	22	[14/9]/20+[x]	

The retrocalculated first visibility in -350 and the retrocalculated last visibilities in -351, -346, and -344 may all be too close to call. In each case, we can count either of the resulting lunar dates as acceptable. (Of these four, only -346 and -344 are being counted.)

If we ignore the signs, my average miss of  $16 \div 17 = 0.941$  days is much better than Walker's  $30 \div 17 = 1.7647$  days. I have beaten him by well over nineteen hours *on the average*. The *total* by which I have beaten him is exactly fourteen whole days for the seventeen events. I continue to stress the point that, if we include the signs, the algebraic sum of Walker's 17 scores is -28, which is severely tilted toward the negative, and the algebraic sum of my 17 scores is +2, indicating a quite satisfactory degree of balance. Walker's median is -2, mine is 0. His mode is -2, mine is 0. (In all of these situations, zero is good.) It is obviously the *fourth-century* fit that is on target here.

I have four last visibilities with scores of +1. Walker considers these “impossible”. I consider that sort of language a grotesque exaggeration. With an error margin of  $\pm 1$  day, how can we be so quick to call this “impossible”?

I have four cases outside Schoch's range of  $\pm 1$  day. Walker has *ten*. This may not be such a big deal, but if *Walker* had beaten *me* on it. . . .

Balance is not the whole story, of course. In addition to balance, which is indicated by an algebraic sum near zero, we are also interested in a reasonably close fit, which is indicated by an average miss that is fairly low. That is why it is so important that Walker's and my algebraic sums are -28 days and +2 days, respectively, and that his average miss and my average miss, ignoring the signs, are 1.7647 days and 0.941 days, respectively.

I do not need the seventeen centuries plus that both Walker and Jonsson emphasize. I have found a significantly better fit that is less than *three* centuries later. They said that it could not be done, and yet I have just done it.



What makes this possible is that Walker does *not* have an acceptable fit. As we saw, his calculation of the seventeen centuries plus amounted to nothing more than  $59 \times 30 = 1770$ . (The figure of 1770 is from the 1983 paper, page 20.) Even if that sort of calculation were correct, however, the seventeen centuries plus would pertain only to very nearly identical fits. Walker and Jonsson are of course assuming that we already have a very *good* fit in the seventh century. That assumption is false: Walker's fit is *not* a good one at all. The fact is that replacing his very poor fit with a much better and much more likely one does *not* require seventeen centuries plus. Perhaps that seventeen centuries plus needs to be measured from *my* placement, rather than from Walker's. The point is that we might indeed have to look some seventeen centuries plus away from my own fit before we found one equally as good.

Saturn seems to have come back to *about* the right ball park in a little less than *three* centuries — 295 years, to be more precise. Fortunately, that meant that I had to survey only about three centuries' worth of lunar and Saturnian material before I struck gold.

If, in working on the Saturn Tablet, I was able to do something that supposedly could not be done, what might *others* do with *other* materials that have also been offered in support of the entrenched chronology? I cannot emphasize this point enough.

We are now in a better position to test the accuracy of van der Waerden's intervals between the conjunctions and the last and first visibilities. There is relatively little disagreement about the times of the conjunctions, upon which the heliacal phenomena of course depend. But van der Waerden has well over half of the relevant heliacal phenomena occurring with too much visibility. Walker was most ill-advised to forsake Schoch for van der Waerden.

It should be noted that in the surviving portion of the Saturn Tablet, all of the conjunctions are in the Summer and Autumn. In that general sector of the sky — actually, for longitudes ranging from about  $50^\circ$  to about  $245^\circ$  — van der Waerden has many of his intervals one or even two days shorter than Schoch's. In the remaining part of the sky (for longitudes of about  $245^\circ$  to about  $50^\circ$ ), van der Waerden has many of his intervals one or even two days longer than Schoch's.

What is of crucial importance for this investigation is that, of the 14 last visibilities, van der Waerden has *ten* that are one day later than Schoch, and

that, of the 14 first visibilities, van der Waerden has *eight* that are one day earlier than Schoch, while one other is *two* days earlier than Schoch. For further details, see van der Waerden's Table 4 on page 50 and Schoch's Table C on page XIV.

The principal test of any empirical theory is how well it "saves the phenomena", as Plato is reported to have said. Thus the ultimate test of any retrocalculation device applicable to antiquity is how well it fits the ancient astronomical observations. In general, I have found that Schoch comes out very well, that *Starry Night* and Lange and Swerdlow come out moderately well, and that van der Waerden comes out not so well at all. For me, the evidence lies not only in the quality of the fourth-century fit that I have found for the Saturn Tablet, but also in the quality of the fits that I have found for other data sets, such as the El-Lahun papyri.

It turns out that my astronomical dating of Kandalanu is a stunning, if only approximate, confirmation of Gunnar Heinsohn's own chronology, although that was neither my intention nor my expectation. Heinsohn identifies Ashurbanipal of the Neo-Assyrian period with Artaxerxes III Ochus of the Persian Empire. In the past, I have fought this sort of idea, though my principal objection was to Heinsohn's parallel idea of identifying Sennacherib of the Neo-Assyrian period with Darius II Nothos of the Persian Empire. (See my 1996, 1997, and 1998 papers in the References.) Now I find myself obliged to point out that my study of the astronomy of the Saturn Tablet has resulted in evidence that Heinsohn is correct about the need for lowering Ashurbanipal by about three centuries, and, more generally, about the need for lowering the entire Neo-Assyrian period into the time of the later Persian Empire.

Many problems remain, of course. Not the least of these is the lunar eclipse tablet B.M. 35789, which is also known as LBAT 1417. This tablet, which has been discussed by Jonsson, pages 274-278, mentions a lunar eclipse from Year 16 of Kandalanu, Simanu (month III), day 15. In this case, there *was* an appropriate lunar eclipse on May 23, -631; there was not *any* lunar eclipse in the spring of -336. What can be done about LBAT 1417? That still remains to be seen. (I do have an explanation for this, which will be discussed in a later section, along with other such matters.)

Little is known about Kandalanu himself. Presumably he was merely a satrap or viceroy under Ashurbanipal (= Artaxerxes III Ochus?). Or maybe Kandalanu was the Babylonian name of the multiply-named Artaxerxes III Ochus

himself. That is an idea that we should probably resist. For if it is correct that Kandalanu “reigned” for twenty-two years, then he appears to have outlived Artaxerxes III Ochus by seven years. It is interesting that the latest two attested dates from Kandalanu say “after Kandalanu” rather than “of Kandalanu”. That may mean that he had died or was for some other reason no longer serving, but that he had not yet been replaced and that his year count was being continued for a while. (See Parker and Dubberstein, page 11.) As Charles Ginenthal has pointed out to me, Kandalanu would apparently have remained in power (or at least his year count would have remained in effect) right up until Alexander the Great entered Babylon in the Autumn of -330. Indeed, Ginenthal called to my attention an article by Dubberstein (1944), who acknowledges on page 40 that the “reign” of Kandalanu might indeed have continued into the Autumn of -625, which would have been in Year 22. According to Dubberstein, however, the tablet in question, “which was originally reported by Pinches to Oppert”, could not subsequently be located, and consequently “the text has never been published and must be considered dubious evidence” (page 40). Or maybe the later scholars were simply never told where the tablet could be found!

Just for the record, the tablet that could not be found is B.M. 40039. It was published by Donald J. Wiseman (1956), Plate 19 and page 89. Thus it is definitely no longer appropriate to say that “the text has never been published and must be considered dubious evidence”! Its date is “VIII/2/22”, which means the month of Arahsamnu, day 2, Year 22. In -625, this would have been October 30. But in -330, it would have been November 7, which may suggest that Alexander did not enter Babylon until sometime later in November.

It is always possible, of course, that a scribe might have continued to use the “after Kandalanu” year count, even after Alexander reached Babylon. The wishes of the conqueror regarding regnal year counting may not yet have been made known. Nonetheless, there is a little-appreciated document, *Diary -330*, that sheds further light on this matter. According to it, Alexander the Great entered Babylon either on or shortly after the 14<sup>th</sup> day of a lunar month. The lunar month in question would have begun at nightfall on November 9, so Alexander would presumably not have entered Babylon until November 23 or later. See Abraham Sachs and Hermann Hunger (1988), page 179. (They give the crucial part of the text as “Alexander, king of the world, [came in]to Babylon”.)

In 1956, without even batting an eye, Parker and Dubberstein cited Wiseman, who had of course in that very year published the “after Kandalanu” tablet in question. Presumably the tablet was therefore in question no longer; in any case, they did not even mention Dubberstein’s earlier skepticism about it, though Wiseman himself made quite a point of doing so!

From a variety of ancient sources, the battle of Gaugamela appears to have been either on September 30 or (more likely) on October 1 of -330. Some modern scholars have assumed that there was a great rush to enter Babylon and to declare the victory of Macedonia over the Persian Empire. This ignores the military situation. They are not the strategist that Alexander was. With hindsight, we know that Darius was in desperate circumstances and was eventually left with only a handful of supporters. Alexander did not yet know any of that; as far as Alexander knew, Darius might have been deploying additional armies against him. Darius would have been somewhere at his back, and Alexander did not like to have enemies at his back. Once he found that he could not immediately capture Darius, Alexander did indeed make his way to Babylon, but probably very slowly and cautiously.

The full chronological repercussions of putting Kandalanu in late Persian times are not my concern here. Many of those repercussions have long since been discussed by Heinsohn, anyway.

This is of course not the first time that I have surprised myself by finding astronomical evidence strongly supportive of Heinsohn’s chronology. It has happened three times before.

When on astronomical grounds I moved the Twelfth Egyptian Dynasty to Persian times, I had to move the First Babylonian Dynasty with it, because the Twelfth Egyptian Dynasty overlapped and was approximately contemporary with the First Babylonian Dynasty; see, for example, Sidney Smith (1940, pages 13-16). Thus I found that I had come up with evidence that Heinsohn was correct in having said some years earlier that the First Babylonian Dynasty was the same as the Persian Empire. Among other things, this claim by Heinsohn means that Hammurabi of the First Babylonian Dynasty was Darius the Great of the Persian Empire, and that Ammisaduqa of the First Babylonian Dynasty was Artaxerxes III Ochus of the Persian Empire. (In recent years, Heinsohn may have softened his position from strict identity to something like contemporaneity.)

Similarity of names is usually very weak or even misleading evidence of identity; nonetheless, I would point out that Artaxerxes III Ochus was also called Umasu, at least according to older sources like Albert Ten Eyck Olmstead, pages 541 and 554, and that aside from the vowels this might seem suspiciously similar to the first part of the name Ammisaduqa. But later sources, like Sachs and Hunger, pages 142-143, 146-147, and 156-157, spell it Umakus, which is far less impressive. I would also point out that the name Ammisadiq occurs in late Persian times; see Olmstead, pages 444-445. In any case, we should not try to make too much of these sorts of things, fascinating as they may be.

When I later looked at the occurrences of thirty-day months in the reigns of Ammisaduqa of the First Babylonian Dynasty and of Artaxerxes III Ochus of the Persian Empire, I found further astronomical evidence that Heinsohn was correct, both in having said that the First Babylonian Dynasty was the same as the Persian Empire and in having identified two kings usually thought to have been separated by well over a millennium.

Some time later still, when I looked at the occurrences of thirty-day months in the reigns of Hammurabi of the First Babylonian Dynasty and of Darius the Great of the Persian Empire, the result was much the same: Heinsohn once again seemed to be correct, this time in having identified two further kings usually thought to have been separated by well over a millennium. There have been many attacks on Heinsohn's equation of Hammurabi and Darius, but *none* of the critics have ever even *mentioned*, let alone attempted to *answer*, my astronomical defense of that equation. (Once again, it should be noted that Heinsohn's views on some of these "equations" may have shifted over time, leading him to think of them now more in terms of contemporaneity than strict identity.)

For all three of these unexpected confirmations of Heinsohn's work, see *Sun, Moon, and Sothis*, pages xxvi, xxxiv, 202, and 299-303, and "From Calendars to Chronology", pages 722-723.

Since this sort of thing has happened to me continually, I do take Heinsohn's overall chronology very seriously, even though I continue to object to some features of his work, such as his acceptance of the idea that 297 years of conventional medieval history should be deleted, and his tendency to throw all of the astronomical babies out with the conventional chronological bath water. (Actually, "bath water" is much too nice a term for most of the conventional chronology.) Thus I have suggested that Heinsohn's rejection of Sothic dating,

and of astronomical dating in general, should be confined to the *other* side of the Velikovsky Divide. On *this* side of the Velikovsky Divide, it seems to me that Sothic dating, and astronomical dating in general, are entirely legitimate. Indeed, it is the successful astronomical confirmation of the great bulk of *ancient* materials pertaining to astronomy that precludes any shortening of *medieval* history or of any other stretch of history within, say, the so-called Common Era. Heinsohn and his supporters cannot avail themselves of *any* of my four astronomical proofs for Heinsohn's chronology if they insist upon shortening medieval history by 297 years!

I am of the opinion that all religious beliefs are totally bogus. Thus I have nothing good to say about any kind of religion, especially fundamentalism. Merely in the interest of historical accuracy, however, and without any intention of defending fundamentalism, I have long been objecting to Heinsohn's attempts to put onto *fundamentalism* a considerable part of the blame for the original acceptance of the now-entrenched chronology for antiquity. Velikovsky was right when he identified the "Three Pillars" that are allegedly supportive of conventional Egyptian chronology — and thereby supportive of the conventional chronologies of most other peoples in that part of the world. For Velikovsky, those "Three Pillars" were Manethon, Menophreus, and what Immanuel Kant would call a "dialectical" use of Sothic dating beyond its proper range. In terms of the conventional chronology, fundamentalism is irrelevant. Though shameful in other respects, it deserves no place on that list of shame.

The *proper* range of Sothic dating is of course on *this* side of the Velikovsky Divide. I do acknowledge that any use of Sothic dating on the *other* side of the Velikovsky Divide is, as Kant would put it, "dialectical". To that extent, at least, Heinsohn is on the right track even about Sothic dating, though I would insist that *all* Sothic dating is still astronomical — and not "pseudo-astronomical" as Heinsohn often puts it. Maybe he should say that there have been "dialectical" applications of otherwise perfectly good astronomical procedures. If, however, he insists upon denigrating *all* such astronomical procedures as "pseudo-", then, once again, it seems to me that he cannot avail himself of *any* of my four astronomical proofs for his chronology! He would be endorsing what he himself has labeled pseudo-evidence.

Before anyone else points it out, I might as well acknowledge that *if* 297 years were removed from medieval history, then my fit for Kandalanu would

put him right back in the seventh century before the now-truncated Common Era, only two years earlier in that seventh century than where Walker and others had him to begin with! That option is unacceptable to me. It is too big an if, for reasons that I have already explained elsewhere. My usual response to such suggestions is that if pigs had wings, wishful beggars would ride them across the sky! On the other hand, perhaps I have inadvertently provided something that will be cited in favor of the 297-year theory as well. I certainly hope not.

As was mentioned earlier, LBAT 1417 is a serious problem for a fourth-century placement of Kandalanu. There are *many* serious problems for a fourth-century placement of Kandalanu. That is the way it is for *any* proposed chronological revision. In this case, as was already mentioned, there was indeed a partial eclipse of the Moon on May 23, -631, which was indeed day 15 of the lunar month, and yet there were no eclipses at all in the spring of -336. (Some might therefore suggest that the Saturn Tablet should no longer be associated with Kandalanu himself, but I suspect that Walker was correct in reading the surviving traces as the last part of the name Kandalanu. For reasons to be explained later, I think that there was indeed no lunar eclipse in month III of Year 16 of Kandalanu, and that such an eclipse came to be associated with Kandalanu only after Seleukid rewriters of history had shifted Kandalanu and many others to centuries earlier.)

Finally, let me try to put this fourth-century placement of the Saturn Tablet and of Kandalanu in some sort of perspective. I cannot be expected to resolve all of the difficulties faced by revised chronologies. We need to go over such difficulties one by one. I have no idea how many of those difficulties can be effectively handled. Before I wrote this paper, however, there was one more difficulty faced by those who would move the Neo-Assyrians. The Saturn Tablet was held by Walker and Jonsson and presumably by many others to provide strong, independent evidence for the conventional chronology, all on its own. It was also held by Walker and Jonsson and presumably by many others that the observations on the Saturn Tablet could not recur for seventeen centuries plus. All of that has now collapsed. The fit with a seventh century placement is very poor. The fit with a fourth century placement is very good. I have taken away a major foundation stone upon which the conventional chronology rests, and I have found that it is much more effective as a foundation stone supporting the Heinsohn chronology. I have not done anything here about the numerous other foundation stones that one might mention. There are enough foundation stones remaining to occupy all of us for some time to come.

## NEW DATES FOR OLD EVENTS

Most but not all of the following items are Reports from Hermann Hunger, *Astrological Reports to Assyrian Kings*, 1992. A few are Letters from Simo Parpola, *Letters from Assyrian and Babylonian Scholars*, 1993. Most of the Reports and Letters that are discussed here have conventionally been given dates in the eighth or seventh centuries. My self-assigned task has been to find new homes for them in the fifth or fourth centuries. In all cases I have found placements that are at least as good as the conventional placements, and in a number of cases I have found placements that are considerably *better* than the conventional placements (if there are any).

Report 42, which is dated to Elul (VI) 13, is by Nabu-ahhe-eriba. Emphatically and repeatedly, he predicts that the lunar eclipse will pass by and that the Sun and the Moon will be seen together on day 15. An eclipse was said to pass by if it occurred entirely below the horizon and therefore remained invisible. At a point not long after the Full Moon, the Sun and the Moon could first be seen together on the morning when Sunrise finally preceded Moonset by a sufficient period of time. Hunger dates the eclipse to 23 August -672, but the margin by which that partial eclipse passes by is less than half an hour, which would make it very difficult for Nabu-ahhe-eriba to predict with such bombastic confidence that the eclipse would pass by. How could he be so sure, two days in advance, that *none* of the eclipse would be visible in those last minutes before Moonset? A far better choice is the total eclipse of 8 August -383, which was much closer to mid-day — and could therefore be counted on to be completely out of sight below the horizon. According to Peter J. Huber and Salvo De Meis (2004, page 193), this eclipse began just before noon and lasted just over 3 1/3 hours. The last contact was over *four hours* before Moonrise! This one would very definitely have passed by! The seen-together was on August 9, which was indeed day 15 of the lunar month.

Report 64, which is also by Nabu-ahhe-eriba, describes four consecutive months (unnamed) that complete the day, that is, four consecutive months with a full day 30. The text then says, according to Hunger's reconstruction, that the Sun and the Moon will be seen together on day 13. We are also told that



Mars is “bright”, and “comes close to Gemini”. Hunger does not offer any date for this Report. We are not told exactly *when* Mars was “bright”, and “close to Gemini”. Was this just after the four months had been completed? Was it at the time of the seen together? Presumably if Mars was *in* Gemini, they would have said so. In any case Mars was just next-door in Cancer at about the right time in -453 and in -374. If Nabu-ahhe-eriba wrote Report 42 in -383, that seems to favor -374 over -453. (It is possible, but very unlikely, that the same person could have written two Reports seventy years apart!)

Report 100 describes an occultation of Jupiter. Hunger dates it to 27 April -675, when there was indeed an occultation of Jupiter by the waxing gibbous Moon. I date it to 31 October -382, when there was an occultation of Jupiter by the waning gibbous Moon. Hunger’s dating works, but so does mine. It’s a tie.

Report 102 is the important text by Akkullanu that deals with Mars and Saturn. My detailed discussion of Report 102 has been quoted at length in Chapter 2. 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. What is particularly noteworthy here is that this Report fell somewhere within the reign of Esarhaddon.

Report 104 describes an eclipse of the Sun on the 28<sup>th</sup> of Nisanu at 2½ double-hours of the day. There is a fragmentary reference to “2 fingers towards”, but it is not entirely clear whether this means that 2/12 of the Sun is eclipsed or that 2/12 of the Sun remains uneclipsed. Hunger gives the date as 15 April -656. Ancient reports of solar eclipse magnitudes are notoriously rather rough. The retrocalculation programs differ slightly among themselves, and the magnitudes at the various likely cities (Nineveh, Ashur, and Babylon) are also slightly different. In any case, I favor 2 May -378, which works just as well. Another tie.

Report 207 has an eclipse of the Moon that passed by on day 13, with a solstice on day 15. My detailed discussion of Report 207 has been quoted at length in Chapter 2. Hunger does not attempt to give a date, and I could not find anything at all between -725 and -625 that would help him. I have an acceptable fit in -363 and an excellent fit in -344.

Report 300 has a lunar eclipse on Simanu (III) 14 that passes the evening watch and extends into the middle watch. My detailed discussion of Report 300 has been quoted at length in Chapter 2. Hunger’s date for Report 300 as well as for Report 336 is 22 May -677, but that eclipse does not even reach the middle watch!

My date is 6 June -380, where the eclipse *does* extend into the middle watch. As I wrote to Charles Ginenthal in my letter of 24 March 2007 (already quoted in Chapter 2):

“At best, Hunger’s dating here is desperate, wrong, disappointing, and pathetic. At worst, it is deceptive, dishonest, and fraudulent. The -677 lunar eclipse simply does *not* fit Reports 300 and 336, but the entrenched chronology that Hunger embraces seems to *compel* him to come up with *something, anything*, here, whether it actually fits or not.

“It has become increasingly clear that Gunnar Heinsohn was correct in placing the Neo-Assyrians in the fifth and fourth centuries, rather than in the eighth and seventh centuries. This is the first time, however, that I have ever found anything in the various scholarly studies of the surviving astronomical materials that might be regarded as an entrenchment *scandal*.”

Report 384 has a large solar eclipse on Iyyar (II) 28 or 29. The Sun at rising is described as a crescent, and as having horns. Hunger puts this on 27 May -668, which is unacceptable. This -668 eclipse was a very small partial, hardly anything that could be described as a crescent, or as having horns. I put Report 384 on 31 May -435, which reached seven fingers or more, and could much more plausibly be described as a crescent, and as having horns. Charles Ginenthal and I have even speculated that this large partial eclipse of 31 May -435 might in fact be the famous Eponym Canon eclipse.

Report 396 names five consecutive months (Iyyar through Elul) *for* which the Sun and the Moon were *not* seen together on day 14. This very likely means that each day 14 *preceded* the month in question. That is exactly what occurred in -462, but in -374 each day 14 occurred *in* the named month. At the risk of belaboring the obvious, what is noteworthy about this Report is that Hunger did *not* manage to find a fit for it in the eighth or seventh centuries, and that I have found *two* plausible fits for it, one in the fifth century (which I very much prefer) and one in the fourth century.

Report 487, by Nadinu, predicts a lunar eclipse on Iyyar (II) 14 in the morning watch that will set “darkly”. Hunger dates this to “-648 May 1?”. Citing Parpola, he then says that this is the “only eclipse in morning watch in Iyyar”. This

partial eclipse would have begun before midnight, in the *middle* watch, and extended into the morning watch. Also, the eclipse would have ended well over two hours before Moonset. So how was it possible for the Moon to set “darkly”? The total eclipse of 17 April -340 is much better. It does begin in the morning watch, as the text suggests, and it does set while still 95% eclipsed. Once again, Hunger’s behavior . . . well, I’ve already said it.

Report 501 is dated to “*Kislev (IX)*, 14<sup>th</sup> day, year 1, Sargon king of Babylon.” On that day, the Sun was seen with the Moon, and Saturn was standing in the “halo” of the Moon. Hunger dates this to “-708 Nov. 27”. It would also fit in -443 or -442, or in -413 or -412. Thus I do have fifth-century fits that are as good as Hunger’s. Recently, however, I have become increasingly suspicious of the *Kislev*. In the Introduction, Hunger tells us: “Uncertain or conjectural translations are indicated by italics.” Note that he has “*Kislev*” in italics. Is it possible that Hunger conjectured that it was “*Kislev*” because that is what would be required in -708? Unless and until I find out that that is *not* what Hunger did, I am unwilling to make chronological use of *any* of my fits in -443 or -442, or in -413 or -412, since they, too, are based on Hunger’s “*Kislev*”. Campbell Thompson (1900, page 44) apparently saw *nothing* of the month-name. In any case, I have lately been trying to find a better date on the basis of the conventional reign lengths. Those reign lengths put the Accession Year of Esarhaddon thirty years after the Accession Year of Sargon as King of Babylon. If -391 fell during the reign of Esarhaddon, then Year 1 of Sargon in Babylon must have been between -431 and -420 inclusive. Unfortunately, there is no unique fit during that stretch: -429, -428, and -426 are all good possibilities. That would at least put Year 1 of Sargon in Babylon within that four-year stretch. (Actually, -426 is much better than the other two.) Pending further developments, however, this is another tie.

Report 506 mentions four consecutive months (Elul through *Kislev*) in which the Moon became visible on day 1 (as opposed to a brief day 30 that would promptly have been switched to a day 1 if the New Crescent had been spotted). Hunger offers no date here. My date is -356. Another possibility is -399.

Letter 148 has the Sun rising two fingers eclipsed. Parpola dates this to 27 May -668. That is also the very date to which both Parpola and Hunger assign Report 384. The two fingers seems rather high for their date. Besides, since Letter 148 speaks of a mere two fingers eclipsed, and since Report 384 speaks of a crescent and of horns (which would suggest a considerably greater magnitude), I take it that

they are *not* one and the same eclipse, anyway. I date Letter 148 to 4 October -414, where the magnitude of two fingers at Sunrise would be just about right, and I date Report 384 to -435, where the magnitude at Sunrise would be just over 7 fingers.

Letter 159 says that there was an eclipse of the Sun on Tishri (VII) 28, and that an eclipse of the Moon in Marchesvan (VIII) passed by. Parpola does not date these eclipses, but there are at least two fits in the seventh century. I am not identifying the years in question, because I refuse to do Parpola's work for him; I leave this as an exercise for Parpola and others. There are a number of likely spots in the fifth and fourth centuries, but by far the most likely fit is 4 September -376 for the solar eclipse (magnitude 60%) and 18 September -376 for the total lunar eclipse that passed by. (The somewhat early placement of Tishri and Marchesvan suggests only that they had neglected to make a needed intercalation either after Adar at the end of the previous year or after Elul of the current year. That is nothing very surprising.)

Letter 362 describes an appearance of Jupiter on Iyyar (II) 22, a disappearance of Jupiter the next year on Nisanu (I) 29, and then an appearance of Jupiter on Simanu (III) 6 in the area of Orion and in the path of the Anu stars. Letter 363 then corrects this, and says that the Simanu appearance of Jupiter was in fact "under the constellation Chariot in the path of the Enlil stars". The Chariot corresponds to our Perseus. According to the *Mul Apin*: "From III 1 to V 30 the sun is in the path of Enlil." Parpola puts these Letters in -668; I put them in -441. Those two placements are equally acceptable; another tie.

There is much more to be done, but that is where I am now with regard to Hunger's Reports and Parpola's Letters. All things considered, the fifth and fourth centuries seem to be working out better than the eighth and seventh centuries.

## NABOPOLASSAR

In a long, unpublished article entitled "The Lunar Data from the Reign of Nabopolassar", I have argued that Nabopolassar belongs in the fourth century, not in the seventh. Here I shall for the most part merely summarize the main results of that article.

D. A. Kennedy (1986, page 222) gives us concise but very useful lists of attested intercalary months and of attested 29-day and 30-day months from the reign of Nabopolassar, who is conventionally thought to have reigned from -625 to -604. I began by fitting Kennedy's materials into the conventional chronology of Nabopolassar as found in Parker and Dubberstein (page 27). The resulting sequence hit only 13 out of the 27 attested month-lengths, not even half. One should be able to average 50%, just by chance.

Attested month-lengths are far more tenuous than lunar dates. A lunar date depends upon only one lunar event, the New Crescent at the beginning of the month. A month-length depends upon *two* lunar events, the New Crescent at the beginning of the month and the New Crescent at the beginning of the next month. The vulnerability of month-lengths can be well illustrated by supposing that there are three consecutive New Crescents that delineate two consecutive months, and that those months are of 29 days and 30 days, respectively. If there is bad seeing at the end of day 29 of the first month, the astronomical sequence of 29-30 will be reported as a sequence of 30-29. A few situations like that can change an overall score rather drastically. Month-length reports need to be treated very carefully.

I take the *Mul Apin* much more seriously than Parker and Dubberstein do. That revered document provides a strong and early precedent for trying to keep the vernal equinox in Nisanu, at least most of the time.

Just for curiosity, I tried having each of the Babylonian months one month earlier. I was expecting something closer to an even split, but in fact there were 19 hits, and only 8 misses. That is definitely above 50%.

The beginnings of both of these sequences seem to reflect the residual chaos of the interregnum. It took a while to get the intercalations in order. But let us look more carefully at the stretch from, say, the end of Year 5 on. The earlier sequence has *seven* cases of Nisanu ending *before* the vernal equinox. That is too *early*. One Nisanu even ends 17 days too early. The later sequence has *seven* cases of Nisanu starting *after* the vernal equinox. That is too *late*. One Nisanu even starts 15 days too late. Some sort of intermediate sequence would be much better.

The 19 hits for the earlier sequence are obviously better than the 13 or 14 or so that I expected, but I am by no means prepared to accept those 19 hits as settling the issue in favor of the earlier sequence. I have major reservations, of three different sorts.

(1) As I have already indicated, a serious problem with the earlier sequence is that it runs much too early to ensure that the vernal equinox will usually occur in Nisanu. What this amounts to is that the intercalation practices make no sense. Indeed, there should have been at least *one more* intercalary month. And the intercalary months should have been *much* more effectively placed. (If they really were trying to keep Nisanu starting *after* the vernal equinox, matters would of course have been even worse!)

(2) The 19 hits out of 27 may still be a random outcome. After all, eight misses is rather a lot, even when we take into consideration the several factors that might be responsible for such errors: bad seeing, poor observation, scribal error, and, lest we forget, faulty retrocalculation by us. But the role of each of these is likely to be none at all, or very small. For a correct chronology, I might have expected a score at least a little closer to 27.

(3) There are three consecutive 29-day months reported in Year 19: these are months IX, X, and XI. That sort of thing does happen — every six or seven years, on the average (see Huber, 1982, pages 24-25) — and is thus not all that unusual. There is a general consensus among scholars that when there was bad seeing at the end of day 29 of a lunar month, the ancients would recognize the new day as day 30 of the old month, but that if they *saw* the New Crescent, they would of course count that new day as day 1 of the new month. (This generalization does not apply to the Maya, however, who used a completely different procedure.) Let us ask ourselves how a month that *we* retrocalculate as having been astronomically a 30-day month might nonetheless have been counted by the Babylonians as a 29-day month. The simplest way in which this might happen is if there were an astronomical sequence consisting of a 29-day month followed by a 30-day month. Suppose that months VIII and IX of Year 19 constituted such a sequence. If there was bad seeing as the 29-day month VIII ended, they would have counted it as a 30-day month, and their day-count would consequently have been running one day late all through the astronomical 30-day month IX that followed. When they got to the end of what they counted as day 29 of that month IX, they might have *seen* the New Crescent and immediately started a new month. Thus an astronomical sequence of 29-30 would have been reported as a sequence of 30-29. That would explain how the *first* of the three reported 29-day months might be wrong. But how could they get the next two wrong? Scribal error might be involved, but I find that unlikely at best. Presumably they did not count a month as having 29 days unless they *saw* the New Crescent. I grant that they might have counted it as having 29 days, in *spite* of bad seeing, if there had been a number

of days of invisibility and if the New Crescent had consequently been considered a sure thing; this circumstance might have been considered almost the equivalent of an actual sighting. But our best bet is to assume that if they cut the month off at 29 days, it was because they *saw* the New Crescent. Besides, at least if we look at the earlier of the two seventh-century sequences, the one that scored 19 hits, the three relevant invisibilities in -606 and -605 were *each* of two days only! (The later of the two seventh-century sequences runs into a 3-day invisibility at the end.) Now I ask the crucial question: How could they be wrong about the final two months of the four-month sequence, namely, months X and XI? If the astronomical sequence of VIII-IX-X-XI was 29-30-29-29, and if there was bad seeing after the initial 29, it would be very easy for them to mistake this for a sequence of 30-29-29-29. But they would not have called the second one a 29 unless they *saw* the New Crescent that ended it (or had some other astronomical assurance that that must indeed have been the time when the New Crescent would become visible). Similarly, they would not have called the third and fourth ones 29-day months unless they *saw* the New Crescents that ended them (or had some other appropriate astronomical assurance regarding the matter). In other words, except for the remote possibility of scribal error, *I do not see how they could have been wrong about the lengths of the third and fourth months in this sequence.* It seems to me that they must have *seen* those two consecutive 29-day months, namely months X and XI of Year 19. Yet retrocalculation clearly shows New Crescents on 11/26/-606, on 12/26/-606, on 1/24/-605, and on 2/23/-605; thus the earlier sequence features a definite 30-29 for months X and XI of Year 19, and the later sequence features a definite 29-30 for months X and XI of Year 19. I cannot for the life of me see how it is possible for either month X or month XI of Year 19 to have an astronomical length of 30 days. And what that amounts to is that I cannot for the life of me see how the conventional dating of Nabopolassar can be correct.

Nonetheless, I realize that historians are deeply committed to the conventional dating of Nabopolassar, and that they will simply continue to follow the Parker and Dubberstein dates. A few others may go with the 19 hits, even if that means abandoning Parker and Dubberstein. But they will not do anything radical, and will be content with shifting Nabopolassar back by merely that one month. I do grant that 19 hits is much better than what I would have expected, that is, much better than somewhere closer to 50-50. So, in terms of persuading people, my arguments against the -625 to -604 dating of Nabopolassar may depend in the end upon whether I can find another set of dates that is superior *in more than one way* to the seventh-century dating. In other words, I need to find a placement of Nabopolassar that perhaps scores at least a little higher than 19, that puts all or almost all of the vernal

equinoxes from, say, Year 6 to Year 21 in Nisanu, and that has both month X and month XI of Year 19 as astronomical 29-day months. Another way of expressing this last point is that I need to find two New Crescents, separated by 58 days, and occurring not far from December 10 and not far from February 6 — or a bit earlier as we move into later centuries, because of the way the vernal equinox slips earlier in the Julian calendar by somewhat less than one day per century. (*Vice versa* for earlier centuries.)

In my opinion, this set of lunar data from the reign of Nabopolassar has integrity, accuracy, and verisimilitude. It reflects good seeing, careful observation of the New Crescents, and reasonably close datings of the vernal equinoxes (equinoxes are notoriously harder to date than solstices). There is also what I for one perceive as a meticulous and sustained effort to keep Nisanu as the month containing the vernal equinox.

So when did Nabopolassar *really* reign? Our next task, and it is no small one, is to find a worthy home for this magnificent and priceless data set. Its true and proper epoch cannot hide forever.

To make a shortened story shorter, I eventually found the answer. With Nabopolassar reigning from -341 to -320, there is *no* Nisanu after Year 5 ending more than one day *before* the vernal equinox and *no* Nisanu at all beginning *after* the vernal equinox! Of the twenty-seven attested month-lengths, there are 20 hits, for a score of just over 74 percent.

My score of 20 hits is presumably better than 19 hits, but all that I claim here is that my score is just as respectable as the score of 19 hits from the seventh century. At least one of these two scores *must* be a chance result, a statistical fluke, no matter how unpalatable that is. We obviously cannot settle the issue on the basis of scores that are not decisive in themselves. (Remember, anyway, how sensitive the month-lengths are.) Instead, I shall rely upon the fact that I hit both of the final two attested 29-day months in Year 19 while the two sequences from the seventh century do not, and upon the fact that, after the 4-day miss at the beginning of Year 5, I have only one Nisanu missing the vernal equinox (and that by one day only), while the earlier and later sequences from the seventh century have *seven* such misses each, which range from 2 to 13 days and from 3 to 15 days, respectively.

Here are the details for my fourth-century sequence:



Calculated First Visibilities	Calculated Lengths of Months	Attested Lengths of Months	Months and Years	Attested Intercalary Months	Implied Dates of Nisanu 1, by Regnal Years	Days Nisanu Follows Equinox by	Hits and Misses
					2/14/-341 = I/1/acc. 2/3/-340 = I/1/1	-9 -20	
				XII <sub>2</sub> in Year 1			
3/23/-339 4/21/-339	29 days	≠ 30 days	II in Year 2		2/21/-339 = I/1/2	-2	miss
12/14/-339 1/13/-338	30 days	= 30 days	XI in Year 2				hit
				XII <sub>2</sub> in Year 2			
					3/12/-338 = I/1/3 3/2/-337 = I/1/4		
4/30/-337 5/29/-337	29 days	= 29 days	III in Year 4				hit
				VI <sub>2</sub> in Year 5			
					2/20/-336 = I/1/5 3/9/-335 = I/1/6	-4	
4/8/-335 5/7/-335	29 days	= 29 days	II in Year 6				hit
12/29/-335 1/28/-334	30 days	= 30 days	XI in Year 6				hit
1/28/-334 2/27/-334	30 days	= 30 days	XII in Year 6				hit
				XII <sub>2</sub> in Year 7			
					2/27/-334 = I/1/7 3/17/-333 = I/1/8 3/5/-332 = I/1/9		
4/4/-332 5/4/-332	30 days	= 30 days	II in Year 9				hit
5/22/-331 6/21/-331	30 days	= 30 days	IV in Year 10		2/23/-331 = I/1/10	-1	hit
				VI <sub>2</sub> in Year 10			
					3/14/-330 = I/1/11		hit
4/12/-330 5/12/-330	30 days	= 30 days	II in Year 11				
4/2/-329 5/1/-329	29 days	≠ 30 days	II in Year 12		3/3/-329 = I/1/12		miss
5/31/-329 6/29/-329	29 days	= 29 days	IV in Year 12				hit

9/26/-329	30	=	30	VIII in		hit
10/26/-329	days		days	Year 12		
					XII <sub>2</sub> in Year 12	
					3/21/-328 = I/1/13	
4/20/-328	29	=	29	II in		hit
5/19/-328	days		days	Year 13		
5/19/-328	29	=	29	III in		hit
6/17/-328	days		days	Year 13		
					3/11/-327 = I/1/14	
					2/28/-326 = I/1/15	
					VI <sub>2</sub> in Year 15	
11/20/-326	30	=	30	IX in		hit
12/20/-326	days		days	Year 15		
					3/19/-325 = I/1/16	
12/9/-325	30	=	30	X in		hit
1/8/-324	days		days	Year 16		
1/8/-324	30	≠	29	XI in		miss
2/7/-324	days		days	Year 16		
					3/7/-324 = I/1/17	
3/7/-324	30	=	30	I in		hit
4/6/-324	days		days	Year 17		
					2/24/-323 = I/1/18	
5/24/-323	30	=	30	IV in		hit
6/23/-323	days		days	Year 18		
					VI <sub>2</sub> in Year 18	
9/20/-323	29	=	29	VII in		hit
10/19/-323	days		days	Year 18		
10/19/-323	30	≠	29	VIII in		miss
11/18/-323	days		days	Year 18		
					3/15/-322 = I/1/19	
7/11/-322	30	≠	29	V in		miss
8/10/-322	days		days	Year 19		
11/7/-322	30	≠	29	IX in		miss
12/7/-322	days		days	Year 19		
12/7/-322	29	=	29	X in		hit
1/5/-321	days		days	Year 19		
1/5/-321	29	=	29	XI in		hit
2/3/-321	days		days	Year 19		
					3/5/-321 = I/1/20	
6/30/-321	30	≠	29	V in		miss
7/30/-321	days		days	Year 20		

9/28/-321	30	=	30	VIII in	
10/28/-321	days		days	Year 20	hit
				XII <sub>2</sub> in Year 20	
				3/23/-320 = I/1/21	

Months VIII, IX, X, and XI of Year 19 are indeed a 29-30-29-29 sequence, which would have been recorded as a 30-29-29-29 if there had been bad seeing at the end of the first 29 and good seeing thereafter.

(A matter that is discussed in more detail in my paper is whether the intercalations in the rival sequences are *sensible* and *natural*. Suffice it to say here that the seventh-century sequences are far more mysterious and puzzling in terms of the intercalation decisions that would have to have been made than is the fourth-century sequence.)

All this constitutes very strong evidence that Nabopolassar reigned from -341 to -320. The repercussions of those new dates are quite interesting. Nebuchadnezzar and Nabonidus and the other Neo-Babylonians would be vassal kings under the Macedonians. Nabopolassar himself would have begun under the Persians and continued under the Macedonians. Nabopolassar's reign would have overlapped that of Kandalanu. I do not know just where Gunnar Heinsohn puts the Neo-Babylonians, but he identifies a number of the Neo-Assyrians — including Sennacherib, Esarhaddon, and Ashurbanipal — with later rulers of the Persian Empire (1992 and 1994). This is now the *fifth* time that I have analyzed a set of astronomical material and found that it seems to support Heinsohn's chronology! Since I am placing most of the Neo-Babylonian period just after the Persian Empire, that would be roughly consistent with the traditional view that the Neo-Babylonians followed the Neo-Assyrians.

I have made every reasonable effort to resuscitate the conventional dating for Nabopolassar, but it must now be pronounced dead.

## NABONIDUS, *THIRD-CENTURY KING OF BABYLON*

Charles Ginenthal has called my attention to a 1999 article by Erica Reiner entitled “Babylonian Celestial Divination” that begins as follows:

““On the thirteenth of the month of Ululu, the moon became eclipsed and set while eclipsed. It was a sign that the moon god requests a high priestess.” So speaks Nabonidus, the last king of the Babylonian empire before its conquest by Cyrus, in his account of the selection and installation of his daughter as high priestess of the moon god, Sin.<sup>1</sup> The date of the eclipse can be established as 26 September 554 B.C., and its interpretation was given to the king by the scholars in his entourage who had at their disposal a compendium of celestial omnia in which they were expected to look up the significance and the prediction associated with the event observed.”

In the footnote, Reiner cites the “detailed analysis of the inscription of Nabonidus and the interpretation of the eclipse” from Chapter 1 of her 1985 book, *Your Thwarts in Pieces, Your Mooring Rope Cut: Poetry from Babylonia and Assyria*, where she does go over a great deal of literary and historical and textual material, but gives us no further astronomical detail. For a discussion more focused on the significance of the eclipse itself, see Carl Olof Jonsson, 1998, pages 109-111. Among other things, Jonsson explains how W. G. Lambert established that the eclipse was in regnal year 2 of Nabonidus.

Since the previous first visibility of the Moon would have been on September 13, Ululu 1 can be equated with September 14, on the customary basis of their common daylight hours, in which case Ululu 13 would indeed have been on September 26, -553. Thus there is no problem about the day of the lunar month. (In this section, I follow the “Ululu” of Reiner rather than the “Elul” of Hunger that I used earlier. The month-names that one finds in the scholarly literature, and especially their spellings, are extremely varied. I have long since given up any hope of imposing consistency on this subject-matter. To those who have not given up, I offer my best wishes — and my sympathy.)

There was indeed a partial eclipse of the Moon on September 26, -553, shortly before dawn. Whether the Moon did actually *set* eclipsed is more difficult

to determine. *Planet's Visibility 2.0* does have the umbra still covering about 29 percent of the Moon at the moment of setting. *Starry Night* also shows the Moon partially eclipsed at the moment of setting, but with a magnitude of not much more than 10%. But *Planetary, Lunar and Stellar Visibility 3.1* has the magnitude drop to 0% just as the formerly-eclipsed part of the Moon — now fully illuminated — is setting below the horizon! (In all three programs, it is the lower right edge of the Moon that is eclipsed.) Huber and De Meis (page 187) put the maximum magnitude at 0.84, with the Sunrise at 5.84 = about 5:50.4 a.m., with the Moonset at 6.00 = 6:00 a.m., and with the last contact (not visible) at 6.16 = about 6:09.6 a.m.; thus the eclipse lasted some 9.6 minutes *after* Moonset.

The disagreement among the programs raises at least the possibility that observers may not have been able to tell whether the Moon was or was not eclipsed as it set. They would have been worried that the eclipse had been running its course for some time, and that it was nearly over. In those circumstances, they might have needed a very clear view of events occurring right at the horizon in order to have been able to tell whether the Moon was or was not eclipsed as it set. Thus if *Planetary, Lunar and Stellar Visibility 3.1* is correct, as opposed to the other programs, there may be a considerable problem for the conventional dating of the eclipse that is espoused by Reiner and most others. How did the ancients *know* that the Moon set eclipsed?

A further complication with the -553 date is that Sunrise would have preceded Moonset by a number of minutes. Sunlight would have filled the sky, making their observation of the setting Moon at least slightly more difficult. Again, how did they *know* that the Moon set eclipsed?

In my unpublished paper on the subject (see the preceding section), I have presented astronomical evidence that Nabopolassar reigned from -341 to -320. This presumably means that his son Nebuchadnezzar reigned in the late fourth century and perhaps even into the early third century, and that Nabonidus himself reigned in the early third century.

Is there any lunar eclipse in the early third century or thereabouts that occurred close enough to dawn that the Moon really did set eclipsed? Yes. There was a total eclipse of the Moon on October 2, -293. This one clearly set while still *totally* eclipsed, as opposed to the -553 eclipse, which was only partial, anyway, and which, at least according to the *Planetary, Lunar and Stellar Visibility 3.1* program, was no longer even partially eclipsed at the actual moment of setting.

The 2 October -293 eclipse is the *only* relevant eclipse anywhere in the vicinity. All three of the PC programs that were mentioned have the Moon setting totally eclipsed on that date. Even Huber and De Meis (page 195) put the magnitude at 1.36, with the first totality at 5.74 = about 5:44.2 a.m., with the Sunrise at 5.98 = about 5:58.8 a.m., with the Moonset at 6.07 = about 6:04.8 a.m., and with the last totality at “6.98\*” (the asterisk connotes invisibility) = about 6:58.8 a.m. Besides, the daughter of the *king* was involved, and it only makes good sense that the gods would have sent a *total* lunar eclipse as their sign that the Moon-god Sin was requesting a high priestess!

Astronomically, the lunar date would have been Ululu 14, but if the start of the month of Ululu had been delayed by bad seeing on August 19 and again on September 18, the eclipse could still have been dated to Ululu 13. This sort of delayed day-count did commonly happen from time to time, and is not seen by anyone as a serious problem.

The eclipse in question is known to have been in Year 2 of the reign of Nabonidus. With allowance for the customary Accession Year that would have preceded the official Year 1, Nabonidus would have begun his reign in -295. The twenty-five-year interval between -320 and -295 would include the reign of Nebuchadnezzar, as well as the much shorter reigns of Amel-Marduk and Nergal-shar-usur (Nergilissar) together with the extremely short reign (only a couple of months) of Labashi-Marduk.

There are numerous problems about which personages did or did not reign just before or just after Nebuchadnezzar. Many of these difficulties have to do with Nergal-shar-usur (Nergilissar). For an extremely careful analysis of the relevant materials and issues, see Immanuel Velikovsky, *Ramses II and his Time*, pages 104-113, 121-124, and 131-139.

Still other problems abound, awaiting our attention. One major item that must be dealt with is Diary -567, which describes itself as being from Year 37 of Nebuchadnezzar (with no numeral, of course). Astronomically, it does indeed fit in -567, which is conventionally recognized as Year 37 of Nebuchadnezzar II, and it fits nowhere else. But once the Neo-Assyrians are lowered to Persian times, a lowering strongly advocated by Gunnar Heinsohn, and once the Neo-Babylonians are lowered to late Persian and to early Macedonian or Hellenistic times, a lowering that I have been trying to support on astronomical grounds, it may be possible to lower the Nebuchadnezzar of the late second millennium all the way down to the

seventh and sixth centuries and to attribute Diary -567 and other astronomical documents to *his* reign. How that will work out remains to be seen. How, if at all, these radical lowerings will affect the events of Jewish history likewise remains to be seen. Charles Ginenthal has even pointed out that it may be possible to keep the reign of 43 years for the Nebuchadnezzar in the seventh and sixth centuries, and to use the reign of about 21 years or so conventionally assigned to the Nebuchadnezzar of the late second millennium for the Nebuchadnezzar that we are putting at the end of the fourth century. The 25 years between -320 and -295 would be at least roughly correct for a two-decade-or-so reign of this Nebuchadnezzar followed by the shorter reigns of Amel-Marduk and Nergal-shar-usur (Nergilissar) and the extremely short reign of Labashi-Marduk. Those latter three reigns add up to less than six years, anyway.

### THE EASY AND THE IMPOSSIBLE

Hunger's Reports and Parpola's Letters are much like the Saturn Tablet of Kandalanu, in that it was at least *relatively* easy to lower all of them by several centuries. Nabopolassar and Nabonidus were likewise easy to lower by several centuries.

LBAT 1417 assigns lunar eclipses to Sennacherib (the regnal year is lost), to the Accession Year and to Year 18 of Shamash-shum-ukin, and to Year 16 of Kandalanu. These four eclipses are part of a Saros series, and are separated by 18 years. The description of the Kandalanu eclipse is very precise, and it would be very difficult if not impossible to move LBAT 1417. As we have seen, my placement of Kandalanu in the fourth century does not allow any lunar eclipse at all in Year 16.

Another such text is that same Diary -567, which is from Year 37 of a Nebuchadnezzar. Diary -567 contains so many astronomical details that it would be very difficult if not impossible to place it anywhere other than in -567/66.

A surprisingly small number of other such texts, equally immovable, are described by Stephenson (1997, page 149) as follows:

“All of the surviving observations (and predictions) of lunar eclipses from earliest times (731 BC) to 609 BC — as well as many later observations down to 317 BC — are recorded on a series of five British Museum tablets. Their reference numbers are: BM 32238 (= LBAT 1414), BM 45640 + 35115 + 35789 (= LBAT 1415 + 1416 + 1417: three joining pieces) and BM 32224 (= LBAT 1419). The major compilation, which lists eclipses at 18-year intervals, originally covered 24 saros cycles or 432 years and extended from some time between 749 and 740 to between 317 and 308 BC.”

The royal canon, as it is called, is a list of kings, with reign-lengths, running from Nabonassar in the eighth century down to well into Roman times. If we lower the Neo-Assyrians to the fifth and fourth centuries, and if we separate Shamash-shum-ukin from Kandalanu by some period of years, then we must take the position that LBAT 1417 was written after the conventional and canonical list of kings and reign-lengths was worked out. That Fabricated Chronology, as I want to call it, had Shamash-shum-ukin immediately preceding Kandalanu, and indeed had Year 20 of Shamash-shum-ukin being equivalent to the Accession Year of Kandalanu. A Saros sequence could easily have been added to a stretch of reigns from this newly fabricated canon; indeed, that fabrication itself could in part have been *based upon* an existing store of dated astronomical records. For example, when they decided to move Kandalanu back to what we call the seventh century, they could have found a description of an eclipse that occurred in what we call -631, and from then on described it as having occurred in Year 16 of Kandalanu.

The LBAT documents are sometimes centuries after the fact. The Reports and Letters and Diaries, which concern contemporary events and are the work of dedicated and responsible observers, are much more reliable.

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 these lines *must* have happened; otherwise, there would have been no Ages in Chaos in the first place.

This would have been a Hellenistic enterprise, not only because some of the documents had to have been written in Hellenistic times, but also because we



already have the Neo-Babylonians extending into the third century. They were, after all, *among* those peoples who were temporally reassigned when the Fake Chronology was worked out. The motivation could have been much like the motivation of Berossos (and Manethon). “Before you Macedonians and Greeks, and even before the Persians, we in Mesopotamia [and Egypt!] were free and independent, with our own vast Empires that were in no way inferior to yours.” Like so many others at the time, they took what were originally contemporary stretches of history and put them end to end, thus lengthening their histories, all to the greater glory of themselves and their various rulers.

I propose to call the unknown person (or persons) who accomplished the fabrication Winston, after the *1984* character whose job at the Ministry of Truth was rewriting history. He, too, was under a lot of pressure from the powers that be. It is entirely possible that Berossos was our Winston. We do have some indication that Berossos was much interested in astronomy, especially in his later years, when he founded a school on Kos that taught both astronomy and astrology (which were the same thing then) — although some have suggested that this was a *second* Berossos.

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 eighth and seventh and perhaps the sixth centuries were duly plugged into the appropriate reigns, with whatever adjustment of the reigns was necessary. The resulting eclipse of Year 16 of Kandalanu is just one example of this chicanery. The deception worked. It fooled everybody for over two thousand years. So it is that the Assyrians and Babylonians, who were wont to transport a conquered people from their homeland to some distant place, have themselves been transported, albeit to a different time. Perhaps their long temporal exile can now be brought to an end.

## A CLOSING RANT

In “From Calendars to Chronology” (1996, page 718), I seriously misjudged most of my fellow Velikovskians. Concerning the use of the El-Lahun papyri as astronomical evidence for radically lowering the Twelfth Egyptian Dynasty, I wrote:

“People will readily accept this. It is cut and dried. The evidence is solid and unanswerable. Anyone who *looks* at the data will inevitably be convinced.”

Some lines later, I added this:

“*All* Velikovskians worthy of that name will accept it as soon as they examine the evidence.

“I could have sold it to Velikovsky in five minutes, and I can sell it to any of his supporters in fifteen.”

I still believe what I said about Velikovsky, but his supporters are another matter. The informed Velikovskians who agree with me about the Twelfth Dynasty can be counted on the fingers of one hand — with enough fingers left over for a *corna* gesture toward the rest of them.

But that is by no means my principal lament. My main complaint is that no one else has been doing the sort of work that needs to be done. There are many within and without this movement whose technical capacities vastly exceed my own. What have they been doing? Nothing. They have left it all to me. For decades now, I have been working on ancient astronomy as it broadly relates to Velikovsky. Such work is central, critical, and crucial. Anyone who professes to be interested in interplanetary catastrophism and/or in revisionist chronology *must*, in my opinion, recognize the importance of ancient astronomical records. Such records are powerful, double-edged, and lethal. They can and do destroy false theories, and they can and do confirm true theories.

If the rest of you had been doing what you needed to do, and not left it all to me, I could have spent more time doing what I would rather be doing, anyway, such as studying Anaximander, Melissus, Plato, Democritus, Bruno, Leibniz, Freud, Velikovsky, Renault, and Hoyle. Fred Hoyle in person was a total jerk, who made it plain that he hated my guts, but that has not stopped me from placing him in that august company. He shed more light on the nature of our universe *as a whole* than anyone since Bruno. Hoyle's principal shortcomings were only two. (1) He was a thoroughly brainwashed uniformitarian, which meant among other things that he had no use for Velikovsky. (2) He sought to insult his enemies by saying that they believed in what he sarcastically called a Big Bang; unfortunately, they turned around and accepted that as a suitable name for their silly theory, which made Hoyle bitter to the very end of his life. (He was also angry that the Bangers kept reporting, falsely but for many years, that he himself had abandoned the Steady State theory, something that he *never* did. All such reports are either wishful thinking or flat-out lies.)

For too many, the study of catastrophism and chronology amounts to browsing through picture books concerned with antiquity, visiting museums, sailing about on the Nile, or trudging through empty tombs. I have done all of those things, and they are not where it's at. What does cry out to be done is the difficult, boring, and tedious analysis of ancient astronomical texts. And when the analysis is done, we need to look at the evidence and judge accordingly. The fact is that the ancient astronomical evidence contradicts many of the alleged "facts" that we find in conventional sources. When it comes to discrediting and disproving those conventional sources, I have done far more than my share of the work. But I should not have been doing *any* of that work. *You*, if you were serious, should have been doing it.

I do not say that *all* Velikovskians should have been doing what I have been doing for forty years. I do say that *many* of them should have been doing it, and that *all* of the rest should have been paying close attention.

This book that you have been reading has no doubt made you aware of Charles Ginenthal's position, which is one that I strongly endorse: literature, propaganda, political and other rhetoric, and even historiography are secondary and subservient when it comes to establishing firm chronologies for antiquity; what we must be mainly and primarily guided by are the scientific and technological sorts of

evidence (the *hard* stuff), which would of course include all of the evidence from astronomy.

*You* have a problem. When it comes to chronology, the ancient astronomical records contradict everything that you have been taught, everything that you stand for, and everything that you find it comfortable to believe. That is not my problem. I believe what the ancient astronomical records imply. I do not believe anything that is contradicted by what those records imply, especially if the discordant materials have come to us with the compliments of entrenchment scholars from the various softer disciplines, such as philology, speculative archaeology, epigraphy, historiography, and the like. That is why I do not believe what you have been taught, what you stand for, or what you believe. So do not ask me to pull you out of your slough of contradiction and despair. What do I care if you sink?

## REFERENCES

- Pierre Bretagnon, *Sur une Solution Globale du Mouvement des Planètes*. Thèse de doctorat d'état, Paris 6. 1978. (I have not seen this.)
- Tieje 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. I mention this paper only in order to state that while I *have* read it, I find that it sheds no light whatsoever on any of the issues that I have been addressing here.
- Waldo H. Dubberstein, "Assyrian-Babylonian Chronology (669-612 B.C.)", *Journal of Near Eastern Studies* III:1 (1944), pages 38-42.
- Aimable Gaillot, *Théorie Analytique du Mouvement des Planètes*, Paris, Gauthier-Villars et Fils, 1888. (I have not seen this.)
- Charles Ginenthal, *Pillars of the Past: History, Science, Technology As These Relate to Chronology*, in *The Velikovskian*, VI:1, 2, and 3 (2003). Actually, it was Ginenthal who persuaded me to study the Saturn Tablet. I am very glad that he did!
- Charles Ginenthal, "Science, History, Ramses II and Velikovsky", *The Velikovskian*, V:4 (3003), pages 60-88.
- Albrecht Goetze, "Additions to Parker and Dubberstein's Babylonian Chronology", *Journal of Near Eastern Studies* III:1, January, 1944, pages 43-46.
- Gunnar Heinsohn, *Perserherrscher gleich Assyrerkönige?*, Gräfelfing, Mantis Verlag, 1992.
- Gunnar Heinsohn, "The Restoration of History", presented and distributed at the Canadian Society for Interdisciplinary Studies, Haliburton, Ontario, August 28, 1994, pages 5 and 22.
- Peter J. Huber, unpublished computer program for the Moon. (I have not seen this.)

Peter J. Huber *et al.*, *Astronomical Dating of Babylon I and Ur III, Monographic Journals of the Near East, Occasional Papers*, I:4, June, 1982, page 51.

Peter J. Huber, review of Hunger and Dvorak, *Journal for the History of Astronomy*, Volume XIV (1983), pages 228-229.

Peter J. Huber and Salvo De Meis, *Babylonian Eclipse Observations from 750 BC to 1 BC*, Milano, Mimesis, 2004.

Hermann Hunger and Rudolf Dvorak, *Ephemeriden von Sonne, Mond und hellen Planeten von –1000 bis –601*, Wien, Österreichische Akademie der Wissenschaften, 1981.

Carl Olof Jonsson, *The Gentile Times Reconsidered: Chronology and Christ's Return*, Third Edition, Revised and Expanded, Atlanta, Commentary Press, 1998, and my review thereof, in *Chronology & Catastrophism Review* 1999:1, pages 56-57. A slightly revised Fourth Edition appeared in 2004.

D. A. Kennedy, "Documentary Evidence for the Economic Base of Early Neo-Babylonian Society", Part II, *Journal of Cuneiform Studies* XXXVIII:2, 1986, pages 172-244, especially page 222, which may well be the most important page ever published in the *Journal of Cuneiform Studies*.

W. G. Lambert, "A New Source for the Reign of Nabonidus", *Archiv für Orientforschung*, Volume XXII, 1968/1969, pages 1-8. (I have not seen this.)

Stephen H. Langdon, John K. Fotheringham, and Carl Schoch, *The Venus Tablets of Ammizaduga*, London, Oxford University Press, 1928. Often referred to as "LFS".

Rainer Lange and Noel M. Swerdlow, *Planet's Visibility* version 2.0 and *Planetary, Lunar and Stellar Visibility* version 3.1, both programs downloadable as freeware from [alcyone.de](http://alcyone.de).

Albert Ten Eyck Olmstead, *History of the Persian Empire*, Chicago, University of Chicago Press, 1948.

Richard A. Parker and Waldo H. Dubberstein, *Babylonian Chronology 626 B.C.–A.D. 75*, Providence, Rhode Island, Brown University Press, 1956.

Robert D. Purrington, "Heliacal Rising and Setting: Quantitative Aspects", *Archaeoastronomy* XII (1988), pages S72-S85.

Julian E. Reade, "Rassam's Babylonian Collection: The Excavations and the Archives", in Earle F. Leichty, *Catalogue of the Babylonian Tablets in the British Museum*, VI (1986), xii-xxxvi.

Erica Reiner, "Babylonian Celestial Divination", in *Ancient Astronomy and Celestial Divination*, edited by Noel M. Swerdlow, Cambridge, Massachusetts, The MIT Press, 1999, pages 21-37.

Erica Reiner, *Your Thwarts in Pieces, Your Mooring Rope Cut: Poetry from Babylonia and Assyria*. Michigan Studies in the Humanities, Volume 5, Ann Arbor, Horace H. Rackham School of Graduate Studies at the University of Michigan, 1985, Chapter 1.

Lynn E. Rose, "From Calendars to Chronology", in *Stephen Jay Gould and Immanuel Velikovsky: Essays in the Continuing Velikovsky Affair*, Forest Hills, New York, Ivy Press Books, 1996, pages 663-726.

Lynn E. Rose, "Current Issues in Catastrophism", in *Proceedings of the Immanuel Velikovsky Centennial Celebration 1895-1995*, Forest Hills, New York, Ivy Press Books, 1997, pages 89-102.

Lynn E. Rose, "In Defense of Higher Chronologies", *Chronology & Catastrophism Review* 1998:2, pages 4-10.

Lynn E. Rose, *Sun, Moon, and Sothis: A Study of Calendars and Calendar Reforms in Ancient Egypt*, Deerfield Beach, Florida, KRONOS Press, 1999, pages 299-303.

Lynn E. Rose, "Schoch, Tuckerman, and Goldstine: A Defense of Their Lunar Retrocalculations for Antiquity", unpublished.

Lynn E. Rose, "The Lunar Data from the Reign of Nabopolassar", unpublished.

Abraham J. Sachs, *Late Babylonian Astronomical and Related Texts*, Providence, Rhode Island, Brown University Press, 1955. Often referred to as "LBAT".

Abraham J. Sachs, *Astronomical Diaries and Related Texts from Babylonia*, completed and edited by Hermann Hunger, Wien, Verlag der Österreichischen Akademie der Wissenschaften, 1988, Volume I.

Carl Schoch, Chapter XV, "Astronomical and Calendrical Tables", pages 94-109, and the "Tables" that follow, on pages I-XVI, in Stephen H. Langdon, John K. Fotheringham, and Carl Schoch, *The Venus Tablet of Ammisaduga*, London, Oxford University Press, 1928.

Sidney Smith, *Alalakh and Chronology*, London, Luzac and Company, 1940.

*Starry Night*, from starrynight.com.

F. Richard Stephenson, *Historical Eclipses and Earth's Rotation*, Cambridge, Cambridge University Press, 1997.

Bartel L. van der Waerden, "Die Berechnung der Ersten und Letzten Sichtbarkeit von Mond und Planeten und die Venustafeln des Ammisaduqa", *Berichte über die Verhandlungen der Sächsischen Akademie der Wissenschaften zu Leipzig, Mathematische-Naturwissenschaftliche Klasse XCIV* (1942), pages 23-56, especially pages 25 and 29, Leipzig, S. Hirzel, 1943.

Immanuel Velikovsky, *Ramses II and His Time*, Garden City, New York, Doubleday, 1978.

Christopher B. F. Walker, "Episodes in the History of Babylonian Astronomy", *Bulletin of the Society for Mesopotamian Studies* V (1983), pages 10-26. (I have not seen this.)

Christopher B. F. Walker, "Babylonian Observations of Saturn During the Reign of Kandalanu", in *Ancient Astronomy and Celestial Divination*, edited by Noel M. Swerdlow, Cambridge, Massachusetts, The MIT Press, 1999, pages 61-76.

Donald J. Wiseman, *Chronicles of Chaldean Kings (626-556 B.C.) in the British Museum*, London, British Museum, 1956, pages 7, 51, 89-90, and 93.