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PREFACE

THE VELIKOVSKY PILLAR

In volumes I and II of *Pillars of the Past*, the scientific, technological, and other forms of evidence that support the short chronologies of Gunnar Heinsohn, Emmet J. Sweeney, and Lynn E. Rose were presented without reference to the great pioneering work of Immanuel Velikovsky whose historical reconstruction started these massive revisionist reconstructions of ancient Near Eastern chronologies. Since I am a proponent of their short chronologies, and of Velikovsky, parts of this final volume will be devoted to his theses. It will not cover all that might or should be included, because I have not, as yet, been able to find forensic evidence for these, or simply do not understand them. Nevertheless, the great chronological revision encompassed in these three volumes could not have been undertaken without Velikovsky’s earlier endeavors. Heinsohn, Rose, Sweeney, and I have each honored this great pioneer, even though we have partial or perhaps even strong disagreements with his chronology. To pay my debt of gratitude to Velikovsky, I cite historian Bruce G. Trigger as cited by Meres J. Weche, another Velikovskian revisionist, with whom I have profound disagreements:

“As I got more and more immersed into Velikovskian research, I soon realized that several other independent ‘new chronologists’ were continuously hard at work, long after Velikovsky’s death, to devise a revised chronological scheme for the ancient world. New chronologists hold among their ranks several *bona fide* historians and Egyptologists whom, whilst recognizing the validity of Dr. Immanuel Velikovsky’s call for a radical reshaping of the conventional chronology of the ancient world, often reject most of the latter’s solutions. Some of them have been labeled by strict Velikovskians as ‘the Peters of the revisionist movement’—for on the one hand working from Velikovsky’s foundations, and on the other hand failing to fully acknowledge their scholarly debt to him. Their principal motivation, it is often alleged, being to immune themselves from the Velikovskian stigma and ultimately shield themselves from the unavoidable backlash. New chronologists generally reply by saying that it isn’t fair to lump all chronological revisionists along-side the Velikovskian legacy. Currently, two of the most well-known of those new chronologists are British scholars Peter James [et al.] and David M. Rohl. …

“It is evident that their initial efforts have been sparked by Velikovsky’s *Ages in Chaos* [history] series. But from the late 1970’s on, there has been a general tendency for neo-catastrophists and new chronologists to go their own separate ways, after Immanuel Velikovsky’s attempts to make both revolutionary fields combine. The unfortunate result has been that Immanuel Velikovsky is being
ignored, and even denounced, by those who owe a lot more to him than they are willing to admit. Given the circumstance, I wholeheartedly agree with Professor Bruce G. Trigger of McGill University who, in personal correspondence, related to me that any such attempts to enact a chronological revolution in ancient history, without acknowledging the central importance of Velikovsky, is tantamount to trying to ‘reinvent the wheel’.”

This volume will also present catastrophic evidence that clearly correlates with Velikovsky’s hypothesis as it relates to the short chronology. In this respect, I wish to cite this mentor as he relates to my own contributions to the work he began:

“I claim the right to fallibility in details and I eagerly welcome constructive criticism. However, before proclaiming that the entire structure [of Pillars of the Past, volumes I, II, and III] must collapse because an argument can be made against this or that [non-scientific, non-technological, historical] point, the critic should carefully weigh his argument against the whole scheme, complete with all its evidence. The historian who permits his attention to be monopolized by an argument directed against some [historical] detail, to the extent of overlooking the work as a whole and the manifold [scientific and technological] proofs on which it stands, will only demonstrate the narrowness of his [or her] approach to [forensic] history. He will be like that ‘conscientious scientist,’ Professor Twist, in Ogden Nash’s verse, who went on an expedition to the jungles, taking his bride with him. When, one day, the guide brought the tidings to him that an alligator had eaten her, the professor could not but smile. ‘You mean,’ he said, ‘a crocodile?’”

With regard to these critics, Velikovsky also pointed out:

“The attempt to reconstruct radically the history of the ancient world, ... unprecedented as it is, will meet severe censure from those who, in their teaching and writing, have already deeply committed themselves to the old concept of history. And many of those who look to acknowledged authorities for guidance will express their disbelief that a truth could have remained undiscovered so long, from which they will deduce that it cannot be the truth.”

On the other hand, I paraphrase philosopher of science Del Ratzsch as his assertions relate to the historical community of scholars:

The traditional historians’ insistence that they and they alone possess the one and only valid understanding of ancient Near Eastern chronology and the history that flows from it is in itself intellectual imperialism. Their insistence that outsiders with revolutionary theories are to be seen through xenophobic filters, at best, or are to be barred from any serious consideration, at worst, reflects a form of academic isolation. Their insistence that we should be given little or no consideration and kept outside their deliberations is in itself a fortress mentality,

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3 ibid., p. VII
bristling with defensiveness, constituting their underlying fear that all is not going well with their chronological paradigm, which may be vulnerable and inadequate.

Historians have unknowingly arrogated to themselves and their institutions a unique place in the world, namely that there can be no other major chronology of the ancient Near East, eliminating a priori the legitimacy of other possible perspectives. Historians who share this pretension assume that they are the only ‘rational,’ objective, empirical practitioners of this discipline, that they stand as the one exception to irrational, subjective thought in all society as related to historical knowledge, and that they alone have a privileged vantage point that sees and understands historical reality instead of being fallible human beings who hold to a fallible construction of the ancient world.4

It is hoped that this appeal will not fall on deaf ears so the contents of these volumes will be taken seriously and dispassionately. Having worked with emotionally involved young people and knowing something of human nature, I suspect that what is being requested is almost impossible or highly improbable. Nevertheless, parts of this book will be devoted to showing that the scientific and technological evidence upholds not only parts of Velikovsky’s coordination of catastrophes with the short chronology, but also elements of his own chronology as these fit inside that revision. As for those who still maintain that the chronology of the ancient Near East is rather well established, David Henige has suggested that there are problems; these supposedly minor problems when taken together are not a mole hill but a mountain of contradiction. His biting style well fits the problematic nature of the established chronology which admits of a profusion of different dates across its entire spectrum:

“R.S. Poole [in his 1851 book, *Horae Egyptiacae: The Chronology of Ancient Egypt* (London)] … was willing to consider dynastic overlaps and co-regencies, and he dated Menes, the first ruler, later [or closer to the present] than almost anyone since, as late as ca. 2640 BCE … His objective was to validate biblical and Herodotean chronology, and he [like Velikovsky, Heinsohn, Sweeney, Rose, and I] made dynasties contemporary that we [assume to] know not to be the case, even subsumed some dynasties into others …

“Like Poole, early modern chronologists of the ancient Near East had only their credulity to guide them and this proved treacherous… it became clear that the easiest way to differentiate chronologies lay in deciding whether or not to accept Manetho’s implication that dynasties succeeded one another in linear fashion. If they did [as is still assumed], modern scholarship could establish the chronology of ancient Egypt by adding up the figures Manetho provided, subtract them from the date of Alexander’s visit [to Egypt] and determine the date of the accession of Menes.”5

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5 David Henige, *Historical Evidence and Argument* (Madison WI 2005), pp. 135-136
That is all that the historians/chronologists have been doing for the past 150 years, following Manethon and trying to fine-tune the documentary and archaeological evidence to make these parts mesh, to no avail. The results were disastrous. It was inferred that Egyptian history began as far back as the sixth millennium B.C. In this regard, W.M. Flinders Petrie in the 1890’s had to date Menes to 5546 B.C., and in his final work in 1934 moved him to 4320 B.C. Petrie adamantly held out against practically all other Egyptologists, who had reduced Menes’ date to around 3400 B.C. or even closer to the present. His approach, as Henige states, was “peculiar and circular but revealing.”6 As Petrie explained, “‘the [ancient Egyptian] historian [Manethon] would not include a duplicate line, however great it was and [t]he principle therefore seems clear that, where there were contemporaries, only one line was selected [by Manethon] and others ignored, in order not to upset the continuous reckoning.’”7

“The discovery of such texts as the Palermo Stone naturally led to another flurry of confident reckoning. [Seymour] de Ricci captured this in 1917, when he wrote that ‘[t]he day when it will be possible to combine the six known fragments of this text, the chronology of the earliest rulers of Egypt will rest on bases as certain as those of the twelve [Roman] Caesars.’ Writing after more than eighty years of further work, however, the author of the most extensive analysis of this source could only write that ‘[i]t seems unlikely that a definitive, or even plausible reconstruction of the annals [of ancient early Egypt] will ever be possible, infuriating as that may be.’”8

In fact, there exist today three dates for Menes. One group offers 3400 B.C., another suggests 2950 B.C. and the latest posits 3100 B.C., a range of 450 or more years.9 Yet with an almost 500-year variation in their chronology for Menes, Egyptologists suggest that only a little tinkering around the edges is necessary to make the chronology work, and each group maintains that radiocarbon dating supports their contentions. They do so by simply using 5730 or 5568 years for the half-life disintegration of Carbon 14 so that science, properly manipulated, supports their three different dates.10 Egyptologist Kenneth A. Kitchen, however, holds after 150 years of such debate “[i]n Ancient Egypt the earliest fixed date is 664 BC.”11 As we will see below, even this date is spurious.

6 ibid., p. 136
7 loc.cit.
8 loc.cit.
9 loc.cit.
10 loc.cit.
11 loc.cit.
Thus, after a century and a half, the supposedly only fixed date for Egyptian history is 664 B.C., and so we revisionists, it is believed, have no proper cause to make a major amendment to that problematic chronology.

During the period between 1987 and 1997, many complete and/or partial chronologies were put forward, none of them agreeing with the others, and even in some cases the proponents of one chronology were arguing against the earlier position they had taken.\(^\text{12}\) Jürgen von Beckerath presented three different dates for the New Kingdom between the years 1984-1997 (see Henige, p. 262). The disagreements were over dynastic lengths and the reign lengths of New Kingdom pharaohs as well. Henige explains why this chaos must exist:

“The reasons for this chaos are obvious: there are too many choices and too little independent evidence. For early periods there are few synchronisms with other parts of the ancient world, and authors must treat co-regencies, concurrent dynasties and lengths of reigns in virtual isolation.

“The imputed chronology of the better-documented XVIII and XIX Dynasties (ca. 1550-1190 BCE) underscores the fragility of these exercises. Speaking of the former [18th Dynasty], Patrick O’Mara concludes rather wistfully: ‘Must we not be content with some sort of multiple [choice] solution conveying a grid of probabilities?’ He offers four dates … with some evidence in favor of each for Akhenaten’s first year. Modern scholars credit Seti I of XIX Dynasty with a reign from 10 to 19 years and Merenptah from 9 to 19 years and put forward five different years (1304, 1301, 1290, 1279, 1276 BCE) for the accession year of Ramses III, who intervened between them.”\(^\text{13}\)

The situation in Mesopotamia’s early chronology is even more problematic. Again, following Henige: H.C. Rawlinson in 1862 claimed he was “glad to be able to announce to those who are interested in the comparative chronology of the Jewish and Assyrian kingdoms, the discovery of a cuneiform document [the limmu list] which promises to be of the greatest possible value in determining the dates of all great events which occurred in Western Asia between the beginning of the ninth and later [sic] half of the seventh century B.C.”\(^\text{14}\) Henige continues: “Only a few of the dates and names that Rawlinson went on to propose are now accepted. In fact like W.F. Albright … Rawlinson went on to change his own mind several times before he finally ceased to conjecture.”\(^\text{15}\)

In 1884 a number of cuneiform tablets giving the names of many kings from different dynasties were discovered. And, as with the case in Egypt, it was taken as fact that these kings ruled one after another and that these, as in Egypt, could be

\(^{12}\) loc.cit.

\(^{13}\) ibid., pp. 136-137

\(^{14}\) ibid., p. 137

\(^{15}\) loc.cit.
used, by backdating from that period into the past, again to build a firm chronology. However, a number of different dates were then found in the first two decades after analysis and nearly all analysts accepted the possibility that these kings reigned at the same time.\textsuperscript{16} But worse was to come. The hoped-for solidity of these dates crumbled when further tablets were discovered that created other correlations of these kings, which was seen as not at all possible, given the then established chronology. This excursion into Mesopotamian chronology ended when Leonard W. King carefully examined the various documents for that period that suggested 15 different dates between 1888 and 1903 for Hammurabi.\textsuperscript{17}

King’s work suggested that there were three early dynasties called Dynasty I, Dynasty II, and Dynasty III: the Hammurabi Dynasty, followed by the Sealand Dynasty, followed by the Kassite Dynasty, one coming after the other for 304, 368, and 576 years. Back-dating these from the middle of the twelfth century B.C. required that the Hammurabi Dynasty began 2400 B.C. Because this could not be accommodated within the established chronology, the Sealand Dynasty was shoved aside, which lowered the Hammurabi Dynasty to around 2100 B.C. This reduction continued and momentarily arrived at a point in time where Sargon of Agade was placed more than 1000 years closer to the present.\textsuperscript{18} Henige concludes his analysis of Mesopotamian chronology thus:

“In 1921 Stephen Langdon and A.T. Olmstead expressed wildly divergent opinions about our grasp of early Mesopotamian chronology. Langdon was confident: ‘[We] now possess, in almost complete form, trustworthy material for reconstructing the chronology of the early history of Mesopotamian civilization.’ Olmstead was feeling quite the opposite: ‘… [new discoveries] force a complete re-writing of almost every page in the earlier Assyrian history.’ The differing opinions are not surprising; that they were expressed within a few months of each other is not very surprising either.

“Three years later the new *Cambridge Ancient History* summed up matters: ‘[a]lthough the discovery that the first three dynasties are not be [sic] reckoned consecutively has narrowed the extent of the divergence in modern computations, the chronological schemes that have been proposed vary according to their reliance upon trustworthiness’ of the later inscriptive durations ‘and of the figures in the Royal Lists and other summaries.’ The number of schemes approximately matched the number of scholars proposing them. Ernst Weidner’s bibliography covered less than nine years but included over 1800 items. In 1946 Böhl reported that in the preceding seven years ‘as many as eighteen treatises’ on the date of Hammurabi had appeared. There was also a constant thread that sought

\textsuperscript{16} loc.cit.
\textsuperscript{17} loc.cit.
\textsuperscript{18} ibid., p. 138
to base the chronology of the ancient Near East on the chronographic data in the bible, Herodotus, Berosus, Eusebius, and others. The presumption was that there must have been good reasons why these ancient chroniclers offered the numbers they did.”\textsuperscript{19}

In his chapter titled “We’re Changing Everything … Again” Henige states:

“The study of ancient Near East chronology is an epistemological purgatory. It is not possible even to capture the latest orthodoxy in toto, nor to claim finality in even a single case before ca. 700 BCE, if not later. Despite two centuries of ebb and flow, assertion and retraction, hope and despair, despite the tremendous accretion of evidence, the editor of an inventory of ancient Mesopotamian inscriptions, could still make the chastening observation that ‘[o]f the nearly sixty rulers represented by inscriptions in this volume, we are certain of the length of reign of only one … Since the accumulation of evidence is responsible, it allows progress toward a final solution, even if that will remain a chimera barring the most miraculous of eventualities. One consequence is that authors routinely use one chronological system or another with no hint that there are differences of opinion, and carry out non-chronological arguments wielding different dates.

“Discussions of ancient Near East chronologies resemble panels in a geodesic dome. Sometimes, changing opinions have little ripple effect, but most are interconnected and proposed changes need to meet global as well as local specifications. Perhaps the first footnote of any article in which dates appear should carry an appropriate warning, however tiresome this might seem to the combatants. The phase of the study ending in 1940 now seems like prestidigitation. We encounter hundreds of textual and astronomical calculations, culminating in Pallis’s efforts to fix datings in the third millennium. This posturing took place in contexts where authors were either suggesting or reporting rectifications of previous chronological systems, sometimes their own. Yet not a single conclusion regarding absolute chronology from that period is deemed correct today.

“In sum, it is easy to agree in principle with Manning when he writes that ‘[a] fixed correlation is all that is necessary. However, when available, it is quite likely we will find neither the High, Middle nor Low chronologies to be right; instead a new chronology will begin to emerge independent of the unsatisfactory Venus Tablets, and the contradictory king-lists’.”\textsuperscript{20}

Henige hopes that things will get better. But this is so much wishful thinking:

“Despite this, will we continue to assert confidently time and again that certain dates are incontrovertible? The number of dogmatic assertions that pepper the literature of the past century or so–of which the examples cited here are but a tiny fraction–serves only to remind us that assurance is not a trait lacking in those interested in this subject. Still, and despite the aura of déjà vu all over again,

\textsuperscript{19} loc.cit.
\textsuperscript{20} ibid., pp. 146-147
evidence for ancient Near East chronology continues, if often fitfully, to converge, but not quite on a single focal point. The astigmatism remains uncorrected, but the prescription is getting better.”

For this tiny degree of progress, Henige has given us nothing but words. The evidence in these three volumes of *Pillars of the Past* shows that the chronographic patient is blind, and that corrective glasses will not help. What is required is major surgery! But things being what they are, historians will be “Changing Everything Again” and again and again for centuries to come. Like the blind men examining the elephant, they will never get that job done right.

The short chronology argues that there was contemporaneity of many Egyptian dynasties over the 800-900 years from ca. 1200-330 B.C. That concept was also discussed in a cautionary note by Dame Kathleen Kenyon regarding prehistory:

“In trying to fit into place the cultures these communities represent, we should learn a lesson from the progress of research in European prehistory. Early European scholars tried to place each culture observed into a regular [serial] sequence. Now it is recognized that many cultures represent regional developments, and several may have existed side by side. The older sequence method tended to produce very inflated chronologies, which have had to be considerably reduced now that the picture has become more coherent. This we shall bear in mind in trying to piece together the jigsaw puzzle which our present state of knowledge in Palestine [and Egypt] represents, and in fact some of the new pieces of the jigsaw which almost every year emerge … do suggest that the whole picture will eventually portray a number of groups of [prehistoric] people living side by side each with their own culture, but with just enough links with other groups to suggest contemporaneity.”

This is what will be undertaken in the next chapter—and Chapter 9. However, the evidence employed to outline these connections of Egyptian history with prehistory and its peoples will be based on forensic historical foundations—the only foundations that matter.

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21 ibid., p. 147
CHAPTER 1

PART I – EGYPT

EGYPTIAN PREHISTORY, CHRONOLOGY, CLIMATE, AND CATASTROPHISM

“In the course of a single century’s research, the earliest date in Egyptian history – that of Egypt’s unification under King Menes – has plummeted from 5876 to 2900 BC, and not even the latter year has been established beyond doubt.”


Having established in Volume I of Pillars of the Past, based on Sothic and lunar dating, that the 12th Dynasty ended with the coming of Alexander the Great (which will be discussed again below), it is time to turn to the beginnings of Egyptian history and chronology to scientifically establish the period when it began. In this way the chronology of Egypt will be scientifically sandwiched between these two anchor points! This, it is suggested, cannot be done without integrating Velikovsky’s catastrophic thesis as it affected the tilts of the Earth’s axis and the climatic changes that these tilts brought with them. These parameters are also based on scientific evidence and will further establish the relationship of his catastrophic theory with the short chronologies of Heinsohn, Rose, and Sweeney, as well as that of this author, based on all their work.

Velikovsky’s analysis of ancient myths and legends led him to suggest that there were two major pole shifts during the Hypsithermal dated ca. 6000-1000 B.C. Of these the first occurred around 1500-1400 B.C. and will be the focus of our attention now, while the 800-750 B.C. pole shift/climate shift accompanying it will be discussed toward the end of this volume. These events had to have profound effects on the climate of North Africa and therefore should also be reflected in Egypt. Nevertheless, based on Heinsohn, Sweeney and Rose’s chronologies, there were no advanced civilizations in existence during this first major global catastrophe. Sweeney, however, places this event at around 1100 B.C., thus moving this catastrophe into historical times [Emmet Sweeney, “Chapter Two,” The Pyramid Age (NY 2007)]. On the other hand, keeping that catastrophic pole shift/climate shift in Velikovsky’s timeframe suggests it should still be moved closer to the present, but only into late Egyptian history to perhaps between 1450-1350 B.C. Precision in this respect is presently not attainable.

If Velikovsky’s hypothesis is valid, there had to be a second shift around 800-750 B.C. and there should be scientific evidence for a two-step climate shift caused by an
in-tandem two-step pole shift. The evidence for this I also treated in my book, *The Extinction of the Mammoth* (Forest Hills, NY 1997), pages 202-252 and other parts of that volume. It must be admitted here that, at the time I wrote that book, I had not made a serious, prolonged analysis of ancient Near Eastern history and chronology, so that it was difficult to determine which pole shift/climate shift was represented by the evidence. In terms of the chronology presented in these three volumes, it is suggested that Velikovsky’s first pole shift/climate shift occurred in late prehistory just before the dawn of civilization, while the second occurred, just as Velikovsky claimed, around 800-750 B.C. To explain this, it is necessary to turn to scientific evidence; the arbiter for this thesis is plant biogeography.

Plants can only live in biomes where seasonally they receive the proper amount of sunlight and the proper amount of rainfall, etc. Trees outside oases in the Sahara, for example, do not thrive, and, more importantly in terms of the hypothesis presented here, cannot reproduce in that highly arid environment because the necessary conditions for them to do so do not exist there. Thus, if Velikovsky is correct, the types of flora and to a great extent fauna associated with such vegetation, should show a direct two-step change brought about by this two-step pole shift/climate shift. And this is exactly what scientists have found to be the case as reported by Georg Gerster:

“Professor [P.] Quézel showed me pollen-seeds [from the Sahara] under the microscope … he explained that one was from an Aleppo pine, another from an evergreen oak. He [also] found pollen from the following species and varieties of plants: cypress, sandarac cypress, juniper, Aleppo pine, Atlas cedar, corn [tree], black or grey alder, evergreen oak, southern nettle tree, Italian daphne, lime and possibly winter lime, French tamarisk, the jujube tree, ash or jasmin and olive …

“The pollen-seeds came from the heart of a desert which more than any other on this planet comes nearest to being completely barren.

“Such evidence as we have,’ said Professor Quézel, ‘is too slight to warrant any far-reaching conclusions, but it does seem a fair assumption that in the strata which represent the most recent periods, conifers and particularly cypress predominate. On the other hand, we can trace an extremely interesting qualitative change in the general picture. Lime and alder are essentially northern trees. There are no limes in North Africa today and the black alder is only found in certain well-watered areas of the Riff and Eastern Algeria. We found traces of lime and alder pollen only in the lowest stratum, which, to judge by the early paleolithic stone implements it contains, goes back some eight to fifteen thousand years. [In this short chronology analysis it goes back 8000 or more years before the present.] And this pollen is mixed with typical Mediterranean plants, Aleppo pines and evergreen oaks. This seems to me to indicate a humid but changeable climate. Trees fond of warm dry weather–olive, jujube and cypress–are only found in the
topmost stratum, which is certainly no more than five thousand years old. [In this short chronology, about 3500–3300 years old.]”¹

In essence we have, just as Velikovsky’s thesis requires, two distinct climate regimes in which two distinct forms of trees, requiring two distinct biomes, grew in the Sahara. The first was a very wet, cool period which allowed essentially northern type trees such as lime and alder to thrive with typical Mediterranean flora. During the second period closer to the present, trees fond of warmer and dryer conditions such as olive, jujube and cypress are found very tentatively dated to around 1000 B.C. Thereafter climatic conditions became so arid that none of these trees could germinate from seeds and take root in the Sahara.

We find a very similar climate change in Israel wherein two distinct types of trees grew before the region became highly arid, as reported by A. Issar and Mattayah Zohar:

“A. Horowitz … in 1971 concluded from pollen assemblage found in cores from the Lake Hula Basin in Northern Israel … the climate changed from cold and humid to warm and dry … the percentage of oak pollen was reduced because of climate deterioration. The sediment of the Sea of Galilee shows a remarkable reduction of oak and pistachio pollen with a parallel increase in olive pollen … and followed by a [climatic] reversal … Issar et al. interpreted these changes as an indication of human response to climate change; i.e. during the humid phase of the Early Bronze Age, the inhabitants had cut [down] the natural forest and replaced it with olive [trees], but once the climate [completely] deteriorated and the olives gave no profit, they were abandoned and replaced by natural vegetation [common to that region today which is that which grows in a highly arid climate].”²

That is, there were first forest-type trees which gave way to olive trees, as in the Sahara, which gave way to desert-like vegetation. Israel is north of the Sahara and its vegetation is of more northerly-type trees.

In terms of fauna associated with the trees and other vegetation of the earlier, wetter period, Marq de Villiers shows:

“Once, there was plenty of water in the Sahara, which was covered in verdant grasslands. The evidence is everywhere; not just the cave paintings or the occasional hippo fossils, but the middens [feces] of cattle, bones and signs that grazing animals [which need copious amounts of vegetation to live] had once roved here in large numbers. So did elephants, giraffes, rhinoceroses, gazelles, and ostriches. Crocodiles and fish stirred the water of lakes and rivers …”³

With respect to the size of the shift of this vegetation from Europe and the Mediterranean basin south into Africa, Alessandra Nibbi, speaking of Professor Quézels research, reports:

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“His study of core samples from a limited area confirmed what many scientists have been saying for some time. At the time of this highest precipitation, the Mediterranean climate moved southward into North Africa for approximately 1250 miles [2000 kilometers], thus bringing the Mediterranean vegetation in the Sahara as far south as Hoggar and Tibesti…”

Here we have an entire suite of European and Mediterranean trees moving south by about 1250 miles which indicates a change in latitude of about 16 to 17 degrees. Trees do not normally move such distances unless there is a great change in the position of the Earth’s axis. Also significant to the chronology presented here, Nibbi gives evidence that this highly rainy period ended not around 5000 years ago, as we were told, but about 3000 years ago, just as Velikovsky suggested. Nibbi speaks of

“G.E. Wickens in his study of the climate and vegetation of the Sudan in the last 20,000 years.

“Wickens quotes from an impressive number of specialists who show that there is now a great deal of evidence for large regional climate changes across the whole of north Africa in the comparatively recent past which must have affected the paleo-biology of the region as late as perhaps the Middle Kingdom of Egypt [conventionally dated to ca. 2000-1800 B.C.]. Much of this work is based on a study of lake-levels in Africa … This shows both dry and wet fluctuations in the climate, including a moist period, which may have extended almost into the New Kingdom [conventionally dated to ca. 1500-1300 B.C.], according to their dating using climatic, geomorphic and biological evidence. Wickens concluded that there was a wet phase during this Period IV in the Sudan which extended from 6000 BP [= Before the Present] until 3000 BP [1000 B.C.]. After that time, the climate though fluctuating became gradually drier and finally attained the conditions that prevail today. This seems to agree with Quézel from whom we quoted above.”

That is, based on climatic, geomorphic, and biological evidence as all of these relate to lake levels in the Sudan and the rest of the Sahara, the final drying out of the Sahara, when the Sahara changed from a verdant environment, happened about 1000 B.C.; Velikovsky dates this change to 800-750 B.C. This evidence we will go into more deeply below to show that this is the case.

In terms of the lakes and rivers across north Africa, Elizabeth Isichei outlines the scope of the evidence which indicates the size of the lakes and the rivers that were flowing across these regions as well as the life in these:

“From about 10,000 BCE, rainfall increased with dramatic consequences. Lake Chad became a vast inland sea, fed by rivers rising in what is now the Sahara Desert, and flowing through the Benue to the sea. The Riff Valley lakes expanded, and Lake Turkana flowed through the Pibor river into the Nile.

5 *ibid.*, pp. 6-7
“There was a second, less extreme period of aridity later, which has usually been placed about 6000 BCE, though recent research suggests a later date (4400-3400 BCE).

Along the ancient shorelines of once enlarged lakes many relics of past aquatic civilizations have been found—bone harpoons, stone weights for fishing nets (or lines), and pottery decorated with [actual] catfish spines [embedded]. Bone harpoons … have been found near Khartoum and Lakes Victoria, Nyasa, Nakura and Chad, as well as many parts of the Sahara.

[According to H. Lhote]⁶, ‘Right in the heart of one of the ergs (sand dunes) … I have come across the remains of fisher’s encampments marked by formidable collections of fish bones (enough to fill several farm carts), of hippopotamus and elephant bones … Over three hundred miles farther south I discovered in more than ten camp sites, fish bones, tortoise shells, and those of mollusks, bones of hippopotamus, giraffe and antelope amid which lay human skeletons … delicate arrowheads in gagues for fishing nets and also superb bone harpoons.’

“In about 2500 BCE, a final period of desiccation began…”⁷

Not only does the Sahara west of the Nile show that it was verdant but this condition was prevalent throughout the ancient Near East, from Africa to Asia as noted by Bruce G. Trigger, B.J. Kemp, D. O’Connor, and A.B. Lloyd:

“The rock pictures which occur along the Nile Valley and the deserts on either side are a further important product of the [nomadic herding] peoples, but the problem of dating them makes them difficult to use historically. There seems to be widespread agreement, nevertheless, that a large portion of the cattle drawings, which predominate in the rock art of Nubia and the eastern desert and are found widely spread in the deserts to the west as well, are contemporary with periods under consideration and attest to the existence of a widespread cattle-orientation …”⁸

This cattle culture spread from the Sahara across into Arabia. For the eastern Sahara, Robert Schoch with R.A. McNally informs us:

“Recent research in the Eastern Desert of Upper Egypt—now a forbidding region between the Nile and the Red Sea—has uncovered a series of elaborate rock paintings. Dated to circa 4000 B.C., the paintings have been called ‘the Sistine Chapel’ of predynastic Egypt by Toby Wilkinson, the Cambridge University archaeologist who made the discovery. The paintings show a wetter, more abundant land, and they use a number of [painted] symbols, such as a boat for the voyage through the underworld and figures with plumes in their hair later known in Dynastic Egypt.

“The recent excavation of the Nabta Playa archaeological site, located … 65 miles west of Abu Simbel in southernmost Egypt’s Western Desert, shows that

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more was going on in predynastic Egypt than we previously suspected. Beginning in about 9000 B.C., nomadic cattle herders brought their animals to the playa during the wet season and let them graze until water and grass dried up. By 7000 B.C. the nomads had settled in the area digging deep wells to allow year-round habitation in the desert and building organized villages of small huts arranged in straight lines. Following a major drought the people disappeared to be replaced circa 5500 B.C. by a people with a social system more complex than any yet seen in Egypt. Their religion centered on sacrificing young cows and interring them in roofed chambers marked by burial mounds. By the fifth millennium B.C. they were erecting large stone alignments, building a calendar circle to mark the summer solstice ... and constructing over 30 complex structures. Nabta [Nubia] grew into a ceremonial center that drew people from all over the Western Desert.”

Although I disagree with the dates given by Schoch, it should be pointed out that these people sacrificed cattle and built calendar circles. These suggest, yet do not prove, that, in terms of Velikovsky’s thesis, they had previously experienced an event where they observed a bull-like body in the sky that affected them, and that the seasons were deranged so that they needed to rearrange their calendar. On this assumption, it is suggested that the event they experienced and survived was a major celestial catastrophe. The evidence for this will be discussed in the unit below titled “Catastrophe.” In terms of climate change in the southern Sahara, Fekri A. Hassan reports similar climatic evidence but dates the climate change to about 1000 B.C.

“Around this period there was an acute dry episode in the Sahara. It was particularly marked in its southern part ... this arid period ... seems to have provided radical changes in the organization of human settlement.

“This was the last [pluvial] Holocene Optimum. It was associated with an exceptionally high population in the southern Sahara and the northern Sahel [the strip of land just south of the Sahara that does get some yearly rainfall, but is still desert-like]. For the last time, hydrological networks [rivers, small streams, brooks, lakes, ponds, and swamps] expanding from Saharan basins were functional ...”

“Here are a few examples from Sudan [south of Egypt]. Between 3800 and 3300 bp [1800-1300 B.C.] in Kerma ... statistics regarding the percentage of cattle and ovicaprines [sheep and goats] diverge radically ... The same observation is valid [for other areas] ... Property was gradually reduced to the vicinity of Wadi Howar’s permanent pools where there are still giraffes around 3800 [B.P. or 1800 B.C.] and cattle towards the end of the millennium, confirming the progressive
character of desertification, as well as suggesting that the vegetation cover remained dense in the wadi [up to … ] 3000 bp [1000 B.C.].”

In terms of rock art across the Sahara and into Arabia, it has been assumed that the dates for the peoples living in these regions follow the older dates and not those of P. Quézel and G.E. Wickens dating the final drying out to 2300 B.C., or over a millennium farther back in time. The older dates place the drying out to 2300 B.C. or over a millennium farther back in time. This older date requires that these early peoples existed over 1300 years before the onset of Egyptian civilization and therefore they should not share highly similar or identical practices. The problem is that these nomadic desert people who supposedly lived over a millennium prior to Egyptian civilization did share highly similar arts and practices. Above, we pointed out that they employed the symbol of the boat for voyages through the underworld, a symbol found in Egypt, and “figures with plumes in their hair later known in Dynastic Egypt.” With respect to the western Sahara, Harry Thurston informs us that among the rock drawings there was found:

“Anubis … [the Egyptian] jackal-headed god who presided over the embalming procedure. Mask-wearing priests also assumed the roles of ibis-, falcon- and lion-headed gods. The image of the sun/ram with a disc on its head appears in Saharan rock art long before it becomes a stock image in pharaonic art.”

I will present more on this below to show how close in time these links between Saharan art and practice were to those of dynastic Egypt. What is being maintained here is that these various aspects of the Saharan people which appear in ancient Egypt did not go back as far into the past as is generally assumed but came about the same period as the onset of Egyptian civilization. In terms of climate change in Arabia, Michael Rice, as cited in volume II of this series, described the Arabian Desert for this same period as being comparable to that of the green Sahara. To repeat:

“… one of the least anticipated results of [a] recent archaeological survey of Saudi Arabia, [is] the discovery of 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 and in parts of western Arabia representations of cattle are pecked and engraved on rock surfaces.”

10 Fekri A. Hassan, “The Arid Crisis … 4000 bp,” Droughts, Food and Culture: Ecological Change and Food Security in Africa’s Late Prehistory (NY 2002), p. 55; Hassan adds a great deal of additional evidence related to this aridification event
11 Harry Thurston, Secrets of the Sands (NY 2004), p. 102
That is, there was a period just prior to and overlapping Egyptian civilization when people from the western Sahara across to the Arabian desert moved as pastoralists with their cattle etc. throughout this vast region, painting and carving pictures of their world on rock surfaces. As will be shown below they evidently spoke a common language or dialects of it which can be traced throughout this stretch of the Afro-Asiatic world, which strongly indicates their similar background and origin.

The dating of these peoples to the older period rather than closer to the onset of Egyptian civilization has been invoked to date this rock art that survives. In this respect Isichei unequivocally states: “Rock art is difficult to date.” More emphatically, Douglas J. Brewer and Emily Teeter state these “rock drawings are notoriously difficult to date.” The entire problem of dating ancient rock art has been analyzed in Robert G. Bednarik’s “Logic in direct dating of rock art,” in the journal *Sahara* for July 1995. Here Bednarik provides a healthy dose of science and logic to show how highly improbable and deeply flawed it is. I would also recommend to the interested reader Bednarik’s review article “Only Time Will Tell: A Review of the Methodology of Direct Rock Dating” in *Archaeometry*, vol. 38, no. 1 (1996). In short, he and others maintain that unless rock art engravings and paintings have been dated by solidly based scientific methods, which he maintains has not yet been accomplished, all determinations of the ages of these representations are subjective, contentious, dubious and must therefore be rejected or at least accepted with a great deal of skepticism.

For example Christian Züchner, in dealing with radiocarbon dates of microorganisms that have been extracted from engraved crusts overlaying these, shows they cannot be accepted

“... because dates from beneath lichen colonies [growing on rock art] give a significantly younger age than those directly adjacent to them [that should date older]. Everyone working in the Sahara will confirm that one and the same rock may have a different desert varnish at its opposite faces and that it may weather [erode away some or all the lichen in the varnish] and form anew.”

We are further informed in the *Encyclopedia of the Archaeology of Ancient Egypt*, Kathryn A. Bard., ed. (London 1999), p. 197:

“A … problem in the Western Desert is that there now are large numbers of radiocarbon dates, but ‘geological’ and ‘archaeological’ dates are difficult to separate, creating a circularity of reasoning in regard to the interrelationship between paleoenvironment and settlement: sites are commonly dated by clusters of age assays on materials that also date sediments, and dispersal dates on geological

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13 Isichei, op.cit., p. 43
sediments such as playa beds typically lead to searches for some artifact scatters that may or may not be contemporary. Systematic study of good stratigraphic sequences [at one site] has yielded comparatively little direct archaeological association with the critical sedimentary units, while the model sequence at [another site] lacks settlement evidence entirely ... Furthermore, plotting all radiocarbon dates from the Egyptian Western [Desert] together suggests above-average settlement density for the period 7,100-6,600 BP [5100-4600 B.C.] when the climate was relatively dry in most areas [and securing a living from the land by a large population was therefore difficult], and a low density 6,200-5,800 [4200-3800 B.C.] when it was wetter [and the land would support a larger population]. The large number of radiocarbon dates from the Western Desert creates an illusion that the archaeology and prehistoric settlement ecology are firmly established. In fact, given the time spans and distances [between these dated sites] involved, research is still in an exploratory phase.”

It thus becomes clear that the dates given for the peopling of the Sahara and the climate shifts and ecology associated with these settlement periods is far from secure at best. It is, in general, based on circular reasoning and ignoring dates contradictory to the generally accepted chronology.

Again we encounter how archaeologists manipulate dates or ignore them to support the established chronology. Now the dating method largely employed to achieve this chronology is that of radiocarbon. As shown in volumes I and II of this series, it is clear that this method is riddled with problems. However, one final analysis of radiocarbon needs to be exposed. The technique was invented in the mid-twentieth century by W. Libby and has been refined over the next 40 years; hundreds/thousands of dates based on it were published in journals, books etc. that in large measure tended to support the established chronology, albeit with a great deal of culling of data. Nevertheless, those involved in this research were evidently uncomfortable with all that was going on and wanted to determine if the method was not only reliable but accurate from laboratory to laboratory. Thus they proposed and carried out a blind test to determine this. Frank Wallace kindly sent me the following material related to this test.

Scientific testing is one of the great cornerstones of the scientific method, and to that end a major experiment was carried out to determine the validity of radiocarbon dating. Over a period from 1982 to 1989, 51 of the world’s 130 radiocarbon laboratories performed this test. The results were published 1990 in the premier dating journal, *Radiocarbon*. Austin Long, the editor of *Radiocarbon*, reports:

“Our bright September day in Scotland, 50 producers, consumers and analysts of 14C dates began a workshop to ponder a darker topic: the results of a series of intercomparisons among radiocarbon laboratories ... Beginning in 1982 Marian Scott and colleagues distributed a series of samples to willingly participating 14C labs. The 14C activities [actually known as dates] of these samples were unknown
to the labs. The purpose of the study was to obtain a quantitative appraisal of how reliable 14C dates are throughout the world, and, if inaccuracies exist, what are the causes …

“All classes of [radiocarbon] counting technologies [gas counting, liquid-scintillation and accelerator ion counting] participated in the intercomparison, as did well-established and newer labs. Representatives of all of the above attended the workshop.

“It is clear that an accuracy problem pervades all technological classes of 14C labs.”

Long does show that there was close agreement between the laboratories, but gives us no evidence that the dates achieved were accurate; that was far from the case, as will be shown below. He concludes, “It may be yet a few years before the radiocarbon community can repolish its somewhat tarnished image.” The authors of the study admit:

“Preliminary reports presented by organizers of this study … lead us to the conclusion that the present status of the 14C dating is unsatisfactory. The observed scatter in five sets of duplicate samples significantly exceeds 1000 yr. and in the other 500 yr. Values of correlation coefficients of 14C dates obtained on duplicate samples are greater than 0.9 in four cases [90 percent off] indicating significant systematic biases.”

The results in fact were so bad the authors were driven to say:

“The results of the ICS, if evaluated on the basis of ranges [of dates] listed … may lead to extreme opinions that after 40 years of improving the radiocarbon method, we are still at the starting point of Libby’s solid carbon counter with an accuracy of 300-500 yr … In any case, future, systematic projects are urgently needed.”

This first double blind test of radiocarbon conducted after 40 years of dating materials and publishing these results was an abject failure. The researchers say it would take years to correct these problems, which moves the possible corrections well into the late 1990s. That means that up until and into the late 1990s radiocarbon dating was quite unreliable. Nevertheless, it was this admittedly unreliable dating technique during all the four and more decades that created the established chronology that, based on several other dating grounds or criteria, has been shown in these three volumes to be in a state of disarray, or more accurately in a state of collapse.

In terms of dating rock we are further told:

“In the 1950s and 1960s, efforts were made by Henri Lhote … and F. Mori … to refine and date the development of the art. Development stages in the material

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18 ibid., pp. 292-294
were established by detailed typological studies, based on analyses of style and technique augmented by inferences from patina, overlay, and archaeological and historical correlations, and occupation deposits in decorated caves or rock shelters were excavated for material for radiocarbon dates …

“These studies indicated five main styles or periods: (1) Big Game, with incised pictographs of animals such as elephant, buffalo, crocodile, hippopotamus, giraffe and rhinoceros, a savannah fauna indicating a period of much greater moisture in the Sahara than today; (2) Round Heads—paintings of round-headed humans; (3) Pastoral or Cattle—paintings and engravings of humans herding cattle; (4) Equid—incised and painted scenes with horses; and (5) Camel—incised and painted scenes with camels. Radiocarbon dates in the Lybian Tadrart Acasus indicated dates of c. 7000 [uncalibrated] B.P. for the Round Head style and c. 5000 [uncalibrated] for the Pastoral style … The Horse and Camel styles also included depictions of chariots and other motifs indicating contact with the Egyptian and classical civilizations.”

It is well-known, as we pointed out in volume II, that the camel did not come to Egypt before about 600 B.C. or later. The question is: do these five artistic styles represent great depth in time or do they all fall closer to the present as all the other evidence presented above indicates? The authority we just cited shows there was contemporaneity of these forms with one another and thus the dates of these must be moved closer to the present:

“The validity of these typological sequences, however, is now very doubtful. Muzzolini, in particular, has argued in a series of studies that the Big Game motifs … form a style or school of design, not an initial phase of rock art, secondly, that they were in fact contemporary with the Pastoral or Cattle Motif along with many of the ‘Round Head’ paintings, and thirdly that the main corpus of early rock art in general was contemporary with the transition from hunting to herding in the Sahara … The Equid and Camel motifs are certainly late in the rock art tradition, but here again there are many indications of contemporaneity, and there is general agreement that most of them probably date about 1500 B.C. onward …”

That is, the dates of most of these fall about 1500 B.C. or just after the first catastrophe. The contradiction is that the camel was not known in the Sahara around 1500 B.C. but about 1000 or more years closer to the present, which also moves the other styles 1000 or more years closer to the present as well. Even the style of the rock art shows that the climate of Egypt became arid around 1500 to 800 B.C. Interestingly we are told “The period of the transition from hunting to herding in the Sahara was characterized by significant climate fluctuations, with increasing aridity over time.”

Prior to the 1500-1400 B.C. pole shift/climate shift,
the people were hunter-gatherers and after it nomadic herders, again in good agreement with Velikovsky’s chronology which I support. But there is a great deal more along dating lines that confirms the short chronology.

Related to the dating of rock art in the Sahara is the fact that a black mummy was discovered in the western Sahara which was reported on the Discovery Channel in a program titled “The Mystery of the Black Mummy” that was aired Friday, May 2nd, 2004. The contents of this program were later reported on the Internet. On page 1 of that report we read about:

“… the discovery of the black mummy, [named after the place it was found] Uan Muhuggiag. It soon became obvious that these people [in the Libyan Sahara] were responsible for an extraordinary array of innovations which later became famous under the Egyptians [such as animal burials, the symbol of the boat for voyages through the underworld; figures with plumes in their hair; Anubis, the jackal-headed god; the ibis-, falcon- and lion-headed gods; and the image of the sun/ram with a disc on its head]. Their presence re-writes the history of Egypt ... By the time the culture reached its pinnacle around 6000 years ago [4000 B.C.] these people had invented rituals which indicate a fairly complex world view. They were communicating with the heavens and using funerary rituals like mumification to treat their dead.”

What is being suggested here, however, is that there was a close overlapping of these supposedly predynastic desert cultures with that of Egypt around 1300-1100 B.C. or even somewhat closer to the present. Hence we should find desert cultures that supposedly came well prior to Egyptian civilization that were actually contemporaneous with each other and therefore shared many cultural as well as artistic traits and affinities. Charles S. Finch III and Bruce Williams, a research associate at the Oriental Institute of the University of Chicago who worked in the Sudan, tell us that

“… the Ta-Seti Kingdom was discovered at Qustul. The site was excavated for the first time in 1962 by Keith Steele who, even then, thought that the size and wealth of the tombs marked them [as] royal … Systematic analysis of the artifacts, however, did not take place until 15 years later by Bruce Williams … adding a hitherto unknown chapter to Nile Valley history. In one sense, however, the artifacts confirmed what Egyptian annals had already attested: there were whole dynasties that immediately preceded the 1st Dynasty under Menes.

“Qustul lies in Nubia, more than 100 miles [160 km] south of Aswan and the first cataract, and is now covered over by [the waters of] Lake Nasser. Qustul was a well-known site for what is known as A-group Nubian culture, contemporary with and related to the Gerzean pre-dynastic complex of Upper Egypt. The archaeological context of the tombs found in a burial spot known as ‘Cemetery L’ showed clearly [presumably] that they were older than the 1st Dynasty. When the

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tombs were opened, a startling variety of goods and materials were present including five different kinds of pottery [and] expensive, finely crafted jewellery.”

What is further significant, and which I claim shows that this culture and early Egyptian culture do overlap and are contemporary, is explained by Ivan Van Sertima:

“About a dozen black kings reigned at Ta-Seti and all the major religious and political symbols of later Egypt were found in this kingdom … On a stone incense burner … were carvings of the falcon-god Horus, the uniquely-shaped crown that was to adorn the later Egyptian kings, the sacred boat-litter of the pharaohs, the elaborate palace serikhs and façades–everything already in place among these royal blacks. Most important, the excavators found inscriptions—the earliest in the hieroglyphic system—in the tombs of Qustul, a system … to be the mother of [the] Egyptian [language] …”

As proof that Ta-Seti was contemporary with the Old Kingdom Van Sertima elsewhere shows: “Moreover, some of the [black and red ware] vases in the [Ta-Seti] tombs were of Syro-Palestinian manufacture.” There is absolutely no evidence to prove that at 3300 B.C. and earlier Syria and Palestine were trading with Egypt and vice versa. This evidence clearly shows that there was overlap of the Old Kingdom with this Nubian culture at Ta-Seti and that there was no long period separating these cultures. As Molefi K. Asante and Ama Mazama [Marie-Josée Cérol] point out: “The people of Ta-Seti had the same funeral customs, POTTERY, musical instruments and related artifacts as the Egyptians. [Bruce] Williams (1987 173, 182).”

Bruce Williams, the excavator of Ta-Seti, in arguing with William Y. Adams who held that the A-Group culture at Qustul was either contemporary with early pharaonic Egypt or came after it, suggests the A-culture was closely related to dynastic Egypt and states:

“Adams appears to believe that Pharaonic iconography and the appearance of Monarchy were contemporary. In fact, specifically Pharaonic motifs occur well before the monumental development of the early Thinite Period, including, for

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example, features of the Hierakonpolis Painted Tomb and a red crown shown in relief on a black-topped vessel of earlier Naqada II times.”

It is evident, nevertheless, that the Ta-Seti culture was closely related in time with dynastic Egyptian development. According to historians the same forms of pottery have always been taken to mean that the cultures in which these forms are found were contemporary. In this respect the climatological, rock art, cultural, and pottery evidence all concur with a great shortening of Saharan African/Egyptian chronology.

A further way to correlate these matters with the short chronology is the migration of the humble goat from Mesopotamia across northern Africa and into the western Sahara. Since the goat came from Mesopotamia, to reach the western Sahara it should have first crossed the Sinai desert into Egypt before migrating farther west. The problem is that, based on the established chronology of Egypt and of these desert cultures, the goat supposedly arrived in the western desert long before it seemingly arrived in Egypt. Thurston informs us that among the fossils found in that desert region:

“... the majority of bones ... beginning about 6500 years ago [4500 B.C.] belong to cattle and goats ...

“Cattle were native to North Africa, but goats were not, their wild ancestors being indigenous to the Near East (namely Mesopotamia). This raises the troubling question: where did the domestic goat come from in the Western [Sahara] Desert? Until recently, it was assumed that goats arrived in Egypt by a route across the northern Sinai to the Nile Delta, then along the Mediterranean [coast westward] and southward up the Nile Valley. The problem with this theory is that goats appear in the Western Desert around 7000 years ago [5000 B.C.] ... a thousand years before they do along the Nile. New research in the southern Sinai strongly supports a different migratory route: goats could easily have swam or been rafted across the 22, narrowest strait of the Gulf of Suez, which was a relatively risk-free sea passage. They then diffused through southern Egypt into the desert.”

The obvious problem inherent in this scenario is that the goat only diffused/migrated west. While Thurston claims that the goat would have migrated “southward up the Nile Valley,” neither he nor anyone else explains why, after the goat swam or was rafted across the Gulf of Suez, it didn’t then migrate “northward as well down the Nile Valley.” It is as if these goats were in touch with some ancient Horace Greeley whose words I paraphrase “Go west, young goats, go west!” One must assume for some incomprehensible reason that the goat, as it migrated and reproduced, never ventured north after swimming the Gulf of Suez. By moving the chronology of the cultures of the Sahara and Egypt into a close

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29 Thurston, *op.cit.*, pp. 106-107
overlapping timeframe, the goat comes to Egypt in predynastic times and very shortly thereafter migrates to the western Sahara. There is no problem, thus, inherent with the westward migration of the goat.

At this point it is necessary that a scientific method be employed as an anchor to date the onset of Egyptian civilization. But how does one date this on a scientific basis? Since the climate discussed above, from Arabia across to the Western Sahara, permitted trees to grow in these regions, plant biogeography and most significantly the dendrochronological or tree-ring dating of living trees will inform us as to just when the Sahara dried out. Since the types of trees that last grew in the Sahara before the final desiccation are known to be olive and cypress, the ages of these surviving/living trees will empirically decide this ultimately and make this determination scientifically. Once the Sahara became a desert, any seedling from any of these surviving/living trees would have fallen into sand or onto stony ground and thus could not germinate (except in oases). Therefore the ages of the oldest living trees in the Sahara (not around or in oases) will have tree-rings that go back directly to the period prior to this great desiccation.

According to the established chronology, the Sahara turned from a savannah into a desert around 2300 B.C. or 4300 years ago. At present, the oldest living tree in the world is named Methusela and is located in the American southwest. This bristlecone pine tree “had begun growth more than 4,600 years [ago]. Thus it stands right now as the oldest known living thing.” It can be argued that since neither olive nor cypress trees could live to such a great age, the expectation of them existing in the deep Sahara from about 4300 to 4400 years ago is obviously not a realistic possibility. This is granted, but based on such an understanding there should be no trees whatsoever living in the deep Sahara! If the great desiccation began 4300 years ago, after that time—100 to 200 years at most—no living tree would survive because there was practically no rainfall for their roots to absorb water. They would have died. Furthermore, after the drying up of the Sahara any seeds that fell would not germinate in sand or on stony ground.

Here then is the quandary for those proponents of the 2300 B.C. date for the final aridification of the Sahara: the fact of the matter is that there are indeed living trees there, and most importantly they are of the olive and cypress variety, the last known types that grew prior to the final drying up of the Sahara! This, by its very nature, presents a massive contradiction to the chronology that holds the Sahara dried out about 2300 B.C. or 4300 years ago. These trees, as we will soon see, are some 3000 years old. If the Sahara was a desert just after 4300-4200 years ago, no trees would have survived prior to that time because they would lack rain water.

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And further, they would not, over a thousand years later, have dropped seeds that germinated. And even if they miraculously did drop seeds, these would never germinate in the dry, sterile sands or stony surfaces of that desert. This is asking for the scientifically impossible. Yet we learn from Robert Silverberg about the oldest surviving olive tree still growing in the deep Sahara:

“There is no doubt that the Sahara of 5000 B.C. was a green and pleasant place, at least in comparison to what it is today … the stumps of ancient [dead] sycamore and acacia trees still rise from the sand in what is now a lifeless desert west of the Nile. In the Air Highlands [of the south central Sahara] … there stands a living olive tree more than twenty-five feet in diameter. It may be as old as three or four thousand years, and is the lone survivor of what formerly was an extensive grove [of olive trees which all died].”

This age is an estimate based on the diameter of its trunk, which is about 25 feet or 7.5 meters. Olive trees, as they age, rot at their cores and leave a hollow there which cannot tell us the exact age of the tree. What dendro-chronologists do is to estimate how many tree-rings would have grown there by comparing this hollow core with young trees that have no hollow cores. By adding the outer, visible, tree-rings to those estimated to have existed in the hollow core, they have estimated that this ancient olive tree is between 3000 and 4000 years old. There are, however, other olive trees in that desert and their ages have also been estimated. Kate Hennessy tells us in a well-respected travel book that in the Sahara there are “three thousand-year old olive trees in the mountains, [and] ancient cypresses fifty-five feet tall [the height of a five and a half story building].”

The olive tree does not naturally grow in the Sahara but needs care as shown to be the case by J.L. Quiles et al.:

“Egypt’s climate is not suitable … for the development of the olive [tree], and thus it was necessary to acclimatize the crop … The evidence available shows that the olive did not acclimatize until the 12th Dynasty … or even the 18th Dynasty [had them], very late compared to the eastern zones [of Palestine, Syria, etc.] … The oldest botanical vestiges come from Memphis (12th Dynasty), but it cannot be determined if they are autochthonous [indigenous rather than imported] …”

Therefore, if the Sahara desert had dried out 4300-4200 years ago, 1300 to 1200 years later olive trees could not have grown there naturally. Yet there are olive trees around 3000 years old which requires, nay demands, that 3000 years ago the climate of the Sahara was cool and wet enough for olive trees to grow there naturally. That is to say, the Sahara was a verdant enough environment 3000 years ago.

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33 J.L. Quiles *et al.*, *Olive Oil & Health* (Wallingford UK 2006), p. 6
ago to allow these trees to grow. Therefore, the Sahara desert dried up about 1000 B.C.—3000 years ago—and not 2300 B.C. or 4300 years ago. The scientific evidence prohibits that later dating and contradicts the established chronology.

What has been argued in all three volumes of Pillars of the Past from various forms of scientific evidence is that the final climate change that dried out not only the Sahara but all the various deserts in that belt across the African-Asian world happened finally around 800-750 B.C., in line with Velikovsky’s chronology. Therefore, not only should we find olive trees with estimated ages of around 3000 years, but more precisely there should exist other living trees that do not rot at their cores and can be tree-ring dated (dendrochronologically) to have lived there somewhat prior to 800-750 B.C. This then brings us, again, to the cypress tree, the last type that grew in the Sahara. The English naturalist Sir David Attenborough presents just this dendrochronological fact:

“Amazingly, one living organism has survived from that [lush] time [in the Sahara]. In a narrow rock-walled gorge stand a group of ancient cypress trees. Judging from the number of rings in their trunks, they are between 2000 and 3000 years old. … Their thick twisted roots have pushed their way through the sun-riven rock … as they have groped downwards for underground moisture. Their dusty needles manage somehow to be green. … Their branches somehow still produce cones and viable seed within them. But none germinate. The surrounding land is simply too dry.”

These trees, like the ancient olive trees in the Air Highland or in the Saharan mountains, had to have been firmly and deeply rooted before the onset of the great aridity. Once again, this scientific evidence proves that the final desiccation of the Sahara began about 1000 B.C. and not 2300 B.C. The seeds that germinated to give rise to these trees had to have sufficient moisture at the surface around 800 B.C. or after. If the Sahara actually had dried up ca. 2300 B.C., after a period of 1300 years any moisture in the soil, any stream flowing in that rock-walled gorge, any lake that supplied that river would have gone dry long, long before 800 B.C. These trees grew before 800 B.C. and their roots have penetrated deep enough below the surface to reach supplies of water which allows them to live. Based on this dendrochronological and botanical/biogeographical scientific evidence, the Sahara became fully deserted after 1000 B.C. and not 2300 B.C.

This being the case, it is evident that the environment in and around Egypt during the Old Kingdom, which I date around 1150-800 B.C., should show art work reflecting these conditions. These works should show savannah animals and vegetation, and at the end or near the end of the Old Kingdom this should disappear from their depictions of their world. Hermann Kees describes these early conditions:

34 David Attenborough, The Living Planet (NY 1984), p. 141 (emphasis added)
“Of major significance in Egypt itself is the faunal and floral evidence provided by pictorial representations. Between the end of the first and the beginning of the 4th Dynasties … elephant, rhinoceros, giraffe and Genenuk gazelle disappeared from the Nile Valley north of Aswan, and from the Red Sea Hills, to be increasingly restricted to the margins of the Nubian Nile [and] to the summer rainfall belt along the southern fringes of the Sahara. At the same time … Barbary sheep, lion and leopard became decidedly scarce in Egypt.”

He further states:

“It is interesting that several 5th Dynasty … hunt scenes from Saqqara and Abu Sir show scattered trees and shrubs or rocky desert surfaces, implying a desert savannah vegetation within reasonable proximity to Memphis …

“A second faunal change is apparent, with addax, ibex, and oryx becoming scarce …”

James Henry Breasted presents the following botanical information: “… the lily [is] the flower of Upper Egypt.” The Latin name for the lily is Lilium candidum or monadelum, known as fleur-de-lys in France. However, it does not grow in Africa. According to C.P. Khare: “Lilium candidum… [is] native to southern Europe and South-West Asia.”

Since the lily was clearly the symbol of pharaohs in Egypt but could never grow there unless the tilt of the Earth’s axis was different than at present, why was it pictured as an important decoration on the ancient Step Pyramid of 3rd Dynasty Djoser? Why would Egyptian kings employ as one of their main symbols a plant that grew about 800 miles or more [11 degrees] to the north and that they would never have seen?

The explanation suggested is the same as with the olive trees that grew in the Sahara about 1000 miles south of where they would naturally grow today, namely that at that early time the position of the Earth’s axis was different and allowed the lily to grow in African Egypt. We maintain that, like all other temperate plants and trees that grew in North Africa and around Egypt, these lilies grew locally and died out after the pole shift which turned the entire region into a desert.

M.M. Mandelkehr, who strictly adheres to the 2300 B.C. date for the drying up of the Sahara as well as all other sites across the Earth, cites Karl W. Butzer’s article on the same topic:

“Butzer … describes how the climatic change is graphically portrayed in Egyptian art. A number of Egyptian Fifth Dynasty [Old Kingdom] reliefs show

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35 Herman Kees, Ancient Egypt: A Cultural Topography (Chicago 1961), pp. 26-27
36 ibid., p. 27
37 James Henry Breasted, Egypt through the Stereoscope, (NY/London et al., 2006, p. 89)
38 C.P. Khare, Indian Medicinal Plants: An Illustrated Dictionary (Berlin 2007), p. 373
wild animals in the open on undulating desert sands studded with tree-sized sycamores and acacias as well as desert shrubs. After the Egyptian Fifth Dynasty, the desert reliefs show a sharply deteriorated picture. All hunting scenes are depicted within artificial enclosures and the vegetation is no longer included.”\(^{40}\)

But in order to keep these Old Kingdom dynasties in the time when trees were growing in the Sahara, and the olive and especially cypress which are 3000 years old, growing there around 1000 B.C., we must move the Old Kingdom to that 1000 B.C. time or just prior to it. Like the rock art found across the Sahara, the Egyptian reliefs correlate with this final climatic change.

The evidence for hunting in Egyptian reliefs also correlates directly with the evidence of wild bulls in Egypt. Since farmers would have killed these animals, had they trod down their crops, wild bulls could only have continued to survive if they dwelt outside the Nile flood plain. In terms of the short chronology, wild bulls should have been either exterminated or domesticated by around 800 B.C. After that date, as with nearly all the other animals depicted in Old Kingdom reliefs, they would disappear, having nothing to eat in the desert. Michael Rice shows

“… the wild bull itself became extinct in Egypt. The process had been underway for centuries. The kings, at least of the Old Kingdom, had hunted the wild bull. Sahure records one of the hunts in the twenty-fifth century B.C. The attrition of the wild breed had been further hastened by domestication. Most of the representations of bulls throughout the Old Kingdom are domesticated animals.”\(^{41}\)

Sing C. Chew further explains, “According to [Karl] Butzer (1976) there was a significant decline in Nile flow to the order of 30 percent or more after the First Dynasty right through to the Middle Kingdom.”\(^{42}\) This is a tremendous change in the flow of the Nile River and suggests quite strongly a vast climate change.

Further related to this evidence is the agronomy of the Nile flood plain. Because rainfall was 30 percent more plentiful prior to 800-750 B.C. and earlier, the level of the Nile would have remained high throughout the year with a very mild seasonal fluctuation. Under such a condition, irrigation agriculture would have led to soil salinization during the Old Kingdom. In this regard Bert De Vries and Johan Gouldsblom shows that this is just what occurred:

“Long term historical water level records in combination with archaeological finds provide a unique basis for studying human-environmental interactions in the … Nile Valley and the Nile Delta. … Early documents show administrators shifting from wheat to the more salt-resistant barley to combat increasing salinization resulting from irrigation, although much land had to be abandoned


through salinization. One millennium later, ... low [seasonal] floods with catastrophic droughts [which allowed the flood stage to leach the salts] explain the opposite...”

When the Nile River was high year round, of course, this salinization problem would occur. This increased rain fall from 1500-1400 B.C. to 800-750 B.C. also explains why the sphinx built during the Old Kingdom exhibits water erosion, as described in volume I of this series.

As we discussed above, there were high lake levels, which Quézel and Wickens date to 1000 B.C.; we encounter the very same high lake levels in Egypt in the Fayum basin lake. Barry J. Kemp states:

“In the Fayum, the maximum extent ... of Lake Moeris seems to have persisted into Old Kingdom times, covering the greater part of the depression ... by the mid-Twelfth Dynasty the level had dropped to below 18 m[eters or 20 yards], thus exposing a substantial area of [lake bottom] land for cultivation.”

Along with this drop in lake levels there would be changes in the Nile valley as well, and these are also presented by Sing C. Chew:

“There were also reports of the invasion of dune sands in the [Nile] Valley near Memphis, suggesting the increasing aridity of the landscape (Hassan 1997). In Middle Egypt, sand dunes also invaded the flood plain. Lack of high floods from the Nile along with dry climate led to severe pressure on the agricultural system as naturally irrigated areas of crop cultivation were reduced.”

This change would have affected the types of animals that could live in the Nile valley region such as wild bulls, etc.

In volume II of this series the pig (and the problem of climatic conditions associated with it) was presented. When populations rise and/or the land turns to desert or is destroyed by salinization, raising pigs becomes prohibitively expensive. The pigs cannot, in these circumstances, forage for themselves and must eat grain to live and grow. Prior to the final drying out of the Sahara, which occurred in late Old Kingdom times, keeping pigs would not have been a serious burden; they could have fed themselves in the swamps near the Nile. However, after that, with the growth of the Egyptian population and with the aridity that destroyed most of the tree cover in and near the Nile Valley, keeping pigs would have become too costly and we would expect there to be a sharp decline in pigs

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45 Chew, *loc.cit.*
towards the end of the Old Kingdom. And this is just what has been found. Brian Hesse and Paula Wapnish explain:

“Within those territories where swine husbandry is possible we note considerable historical variation … differences in the availability of water or the possibility that deforestation denuded pig habitat were considered. … In the Nile Valley, for example, the exploitation of pigs declined sharply after the Old Kingdom period. Richard Redding has argued … that this history is related to changes in the agricultural sector … In Redding’s model … pigs were a resource aimed at domestic consumption … However, subsequent intensification of agriculture [to feed the growing Egyptian population] meant a reduction in the amount of land allocated to pasturage, a change that pressured the mix of stock to favor those species which browse as well as graze. … Intensification [of agriculture to meet the needs of a growing population] meant plows and plows mean cattle. … Cattle compete with sheep for graze, and at the same time, because they yield milk, provide higher rates of protein return than the pig [because unlike pigs, cattle and sheep eat grass and not grain] per unit of labor. Thus the changes brought on by the decision to intensify grain production transformed village-based animal husbandry from an emphasis on sheep and pig raising to management that concentrated on cattle and goats.”

Now the dating of all this Old Kingdom climate evidence is correlated with the dendrochronological evidence that requires/demands that there was abundant rain fall in the Sahara to allow 3000 year-old olive and cypress to grow. This correlation is also aligned with the various forms of evidence in volumes I and II, as well as this volume, that scientifically and technologically shortens Egyptian history/chronology by about 1800 to 2000 years. Much of the present volume will take up this evidence. At the end of this book I will take up the final 800-750 B.C. climate change across the ancient Near East to India, the Deccan, south central Asia, Chinese Turkestan, and China.

This massive climate change may be reflected in the great famine that occurred around the end of the 6th Dynasty. Kemp reports: “… at the end of the 6th Dynasty … Ankhifty of Hierakonpolis … found himself distributing famine relief over a huge territory.” It may have been that people arriving in Egypt from the drying desert added to this problem. T.G.H. James adds:

“In Middle Egypt we have a particularly well-documented record for this [famine] … The period is particularly well represented by burials, so much so that it has been suggested the combination of famine and civil war could have sharply increased the mortality. Yet the grave goods show no sign of general impoverishment. …

“It is unreasonable to argue that this was mostly material robbed from earlier graves because much of it is in styles peculiar to the period [of foreign people]. Nor were these graves clustered around large centrally placed tombs of leaders and providers. They occur in a series of small cemeteries spread out along the edge of the desert as if representing the burials of a wide scatter of villages. It strains credulity to suppose that these people were passive recipients of a state redistribution system which was, by its nature, of limited flexibility.”

It might possibly be that desert people migrating to the Nile Valley as the desert dried up, were only allowed to live along the edge of the desert, with their cattle, goats, etc. They died there of starvation as these resources were consumed. It does seem to correlate with the other evidence, presented above, indicating that the Sahara was drying out at this time, i.e. around 1000 B.C.

**POLE SHIFT**

In terms of Velikovsky’s hypothesis there were two pole shifts/climate shifts as discussed above. Critics of Velikovsky claim that the climate shifts were not in any way credited by pole shifts, or that these climate shifts were temporary. Nevertheless, the movement of certain plants deep into the Sahara have driven certain scientists, at their wits’ end, to claim that there was, or even had to have been, some pole shift, perhaps involving a rearrangement of the Earth’s orbit, to account for these plant migrations. Martin Claussen and Veronika Gayler outline this concept:

“It has been hypothesized … that the wetter climatic conditions [in the Sahara] were probably caused by changes in the Earth’s orbit 6000 years ago, the perihelion [or closest point of the Earth to the Sun] was in mid-September [rather than early January as it is today], the tilt of the Earth’s axis was 0.7° of a degree [50 miles] stronger [more tilted] than today, and the eccentricity [deviation from circularity] of the Earth’s orbit around the Sun was a little bit larger (0.187 instead of 0.167 today). Numerical simulations using general circulation models … of the atmosphere revealed that changes in the Earth’s orbit led to increased solar radiation which amplified the African and Indian summer monsoon, thereby increasing the moisture transport to North Africa.”

Backing up this assessment, on page 2 of the Internet Discovery Channel program discussed above, “The Mystery of the Black Mummy,” the evidence for a pole shift appears rather obvious to certain scientists. Here is how they describe this wet phase in the Sahara:

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48 T.G.H. James, *Pharaoh’s people: Scenes from Life in Imperial Egypt* (London 1984), pp. 113-114 + 242-247
“... all of [the] evidence indicated an Eden-like place—one with trees, grasses and abundant running water. And yet nothing could be further from this picture than the Sahara today. Although archaeologists had already assembled the clues, the science of climatology solidly confirmed what all had suspected. Changes with the tilt of the earth’s axis had caused drought in the Sahara and brought this thriving society to an end.”

This, of course, is an opinion of a small minority, but, like all ideas thrown into the pool of scientific thought, it will have ripples that will grow. Yet the question not addressed is: why two distinct climate shifts without two distinct pole shifts? Of course critics will argue that these small pole shifts are in no manner whatsoever as large as those demanded by Velikovsky’s hypothesis. In this instance they are averting their gaze and attention away from the biogeographical evidence related to this pole shift described by Velikovsky and this author. The unimpeachable scientific evidence that requires a pole shift of many degrees—perhaps as much as 15°—is directly related not only to vegetation in the Sahara and other deserts in the north Afro-Asian desert belts, but more importantly to plants that grew well above the Arctic circle, which could never have lived there unless the Earth’s axis had tilted several degrees at the least or well over 10 degrees. Here I will present as briefly as possible some of the scientific facts that demand a major pole shift.

As pointed out above, Alexandra Nibbi, citing Professor P. Quézel’s studies in the Sahara, showed: “At the time of this highest precipitation, the Mediterranean climate moves southward into north Africa for approximately 1250 miles [2000 km], thus bringing the Mediterranean vegetation as far south as Hoggar and Tibesti…” This suggests a change in latitude of 16 to 17 degrees. The olive tree between 4000 and 3000 years old in the Air Highlands of the Sahara was somewhat over 1000 miles south of its range along the Mediterranean coast of north Africa. The cypress trees in central Africa are located about 9 degrees or 475 miles south beyond their normal biogeographical range. This evidence suggests the overall pole shifts were about 15 degrees taken together.

If this is the case, then there should exist the same form of plant biogeographical migration evidence for about an overall 15 degree or 1000 mile pole shift above the Arctic Circle. In my book, The Extinction of the Mammoth, evidence related to large trees growing well north of the Arctic Circle was presented on page 110ff, and in the chapter titled “Pole Shift.” Significantly, Velikovsky in his book Earth in Upheaval described trees that were dead but standing still rooted in the ground on the New Siberian Islands several hundred

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50 Discovery Channel, “The Mystery of the Black Mummy”, ibid.
51 Immanuel Velikovsky, Earth in Upheaval (NY 1955) and Charles Ginenthal, The Extinction of the Mammoth (Forest Hills NY 1997)
miles north of the Arctic Circle. The problem from the biogeographical view point is that such vegetation cannot grow at these latitudes today because of the basic requirement for temperate latitude photoperiodic (light) rhythms and temperatures.

In this regard, J.B. Charlesworth presented evidence that a bush, the Black Crowberry, Empetrum nigrum, was found in situ on one of the Spitsbergen islands, located about 1000 miles north of the Arctic Circle. The plant had ripe berries and Charlesworth admitted that these plants “no longer ripen in these northern lands”.52 That is, this plant was about 15 degrees north of the Arctic Circle and had gone through the process of sexual reproduction to generate flowers in spring, then fruit with seeds during the summer months, which would later fall to the earth to germinate and reproduce a new plant. But above the Arctic Circle these plants today reproduce by asexual means. Yet Charlesworth claimed that the Black Crowberry found in situ had “ripe fruit stones [seeds].”53 In order to do this: produce flowers, fruit and seed or “ripe fruit stones,” this plant had to live in an environment/biome with temperate zone seasons of different lengths of daylight and night. Significantly, Charlesworth was talking about the Holocene period and more specifically about the Hypsithermal period from around 8000 B.C. to 3000 B.C., the same time that the 3000-year-old olive and cypress trees were growing either 1000 or 500 miles, 15-7½ degrees south of their present day natural environments in Africa.

J.V. Bell and J.H. Tallis in their discussion of the present-day biogeographical range of the Black Crowberry bush have presented a map of the regions where this plant thrives and reproduces by sexual means.54 This plant only sexually reproduces south of the Arctic Circle and in only one very small area of northern Scandinavia–just slightly north of the Arctic Circle. Bell and Tallis inform us where the Black Crowberry’s sexual reproductive habitat occurs. It is

“… common on mountains and moorlands throughout Scotland, Wales, northern England and western parts of Ireland: present in suitable habitats on the higher ground of Dartmoor and Exmoor [southern England] …

“Almost circumpolar in distribution … Extending south in Europe to the Pyrenees [mountains between France and Spain], the Auvergne [of France], the northern Apennines and the Alps of Italy, France, Switzerland, Austria, Germany and Poland, and extending in Scandinavia to a general limit of 60° 30’ N, but with isolated sites as far at least as 68° 23’ N [or about 55 miles north of the Arctic Circle] … Also present in Iceland and the Faeroes [islands north of Scotland]. In Eastern Europe, E. nigrum occurs in the Carpathians, Montenegro and Bulgaria and thence across Russia to the southern limit of 65° N … extending across the Aleutian

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53 loc.cit.
Islands and along the Alaskan coast southward to c. 40° in California, with further occurrences in Eastern Quebec and the maritime provinces of Canada.  

In the Sahara, olive and cypress trees, and on the Spitsbergen Islands, Black Crowberry were living 1000-500 miles (15-7½ degrees) beyond their present-day natural habitats at the same time. This empirical evidence proves that at that earlier time the Earth’s axis had to be about 15 degrees more perpendicular to the plane of the Earth’s orbit to allow this. Plant biogeography proves this fact! Neither those scientists who suggest that there was only a 0.7 degree pole shift nor anyone else has given facts–scientific cited facts in print–that deny this. Tied as they still are to their uniformitarian paradigm, they simply cannot and will not present evidence–scientific evidence–that refutes this clear-cut biogeographical evidence so destructive to their version of reality.

These findings fully support Velikovsky. Yet it may be asserted that such evidence does not belong in a book on the chronology of the ancient Near East. That would simply be an evasion and escape mechanism, in order to deny these scientific facts and avoid what they clearly and directly indicate. Since science is the approach to chronology presented in these volumes of Pillars of the Past, this evidence along with all the other evidence will not be neglected and evaded. It is directly applicable to the behavior of the early people who created the great civilizations in the ancient world. It is directly related to their historical migrations because of the pole shifts/climate shifts. It is directly related to dating the chronology for the onset of civilization in the ancient Near East.

As an aside, I suggested in my book The Extinction of the Mammoth, page 182, that the polar ice cap over the Arctic Ocean had nearly melted away during the Holocene Hypsithermal, citing J.B. Charlesworth’s Quaternary Era vol. II (London 1957), pages 1483-4, where he showed that warm water mollusks inhabited shores all across that body of water, as well as C.E.P. Brooks’s Climate Through the Ages, 2nd ed. (NY 1970), page 143, which I cited in my book, pages 131-2, that the Arctic Ocean was mostly ice free, also based on Brooks’s showing warm water mollusks lived all around all the islands and shores there. Sean Mewhinney has argued against this as I pointed out in 1999.  

Now even members of the scientific community have been willing to suggest that the Arctic Ocean was ice-free at that time. In 1996, Saveli V. Tomirdiaro stated in clear and unambiguous terms that it was ice-free:

“The warming of the Arctic Ocean was so intense that an ocean lacking or nearly lacking ice might have taken its place in the Holocene [climate] optimum. Thus the forest growth of the Holocene optimum spread not only across the plains of Yakutia,

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55 ibid., pp. 290-291
but as far north as the northernmost islands [in the Arctic Ocean] of Novosibirsk [New Siberian Island] Archipelago. … The transformation of the ice-free Arctic Ocean of the Holocene optimum to the contemporary [ice-covered] Arctic Ocean occurred during the second half of the Holocene ... Contemporary landscapes of the arctic tundra [from the earlier forest environment] formed during this period likewise."57

Here it is fully admitted that forests grew on the New Siberian islands hundreds of miles north of the Arctic Circle where trees of this size and nature do not and cannot grow today without a pole shift. But of great interest, Tomirdiaro directly suggests that such a climate change involved a catastrophe of some sort or other and may be related to ancient man’s mythology. Nevertheless, he attributes these changes to the ocean’s conveyor system and not to a pole shift, without explaining how, say, the Black Crowberry, or temperate trees as well, could flower and produce fruit and ripe seeds 1000 miles above the Arctic Circle without the photoperiodism of temperate latitudes on Spitsbergen which requires a pole shift or on the New Siberian Islands which also requires a pole shift.

As has been shown, the deserts bloomed as did the Arctic region during the Holocene optimum or Hypsithermal ca. 8000-1500 B.C. and this was completely reversed after this time which I date to 800-750 B.C., along with Velikovsky. Slowly but surely the evidence for Velikovsky’s hypothesis continues to accumulate.

CATASTROPHE

To tilt the Earth’s axis twice, by an overall amount of about 15 degrees, cannot have been the outcome of uniformitarian processes. Historians will say that Velikovskian catastrophes are known to be astronomically impossible and therefore are to be completely disregarded. They may further argue that there is no evidence on Earth of celestial catastrophes of the size envisaged by Velikovsky. They can do this if they ignore the literature both from the Velikovskian scholars and from mainstream science. What follows is the new evidence presented by accredited scientists that indicates that there was a major celestial catastrophe that overwhelmed the Earth in prehistoric times but which I, with Velikovsky, date to around 1500-1400 B.C. The view that no such evidence exists is an attempt to convey misinformation as factual truth. The concept that major catastrophes befell the Earth in the past few thousand years is gradually coming into its own and will grow and grow. Velikovsky’s thesis is like a stone thrown into water which has created ripples that cannot be stopped and will, I believe, continue to widen and

surge. Let us begin. The following material regarding this was kindly sent to me by Clark Whelton in an article by Sandra Blakeslee from *The New York Times*:

“At the southern end of Madagascar lie four enormous wedge-shaped sediment deposits, called chevrons, that are composed of material from the ocean floor. Each covers twice the area of Manhattan [100 square miles or 259 square kilometers each, i.e. 400 square miles or 1036 square kilometers altogether] with sediment as deep as the Chrysler Building is high [which is 1046 feet or 319 meters high].

“On close inspection, the chevron deposits contain deep ocean microfossils that are fused with a medley of metals typically formed by cosmic impacts. And all of them point in the same direction — toward the middle of the Indian Ocean where a newly discovered crater, 18 miles in diameter, lies 12,500 feet below the surface.

“The explanation is obvious to some scientists. A large asteroid or comet, the kind that could kill a quarter of the world’s population, smashed into the Indian Ocean 4,800 years ago [2800 B.C.], producing a tsunami at least 600 feet [around 200 meters] high, about 13 times as big as the one that inundated Indonesia nearly two years ago. The wave carried the huge deposits of sediment to land.”

In terms of Velikovsky’s theory, when Venus was created from Jupiter with thousands of comets and possibly asteroids, those moving with it in the same direction as Venus and sufficiently close to it would have begun to orbit around it. This is fundamentally and gravitationally what would have occurred. Thus when Venus neared the Earth, many of those bodies in such orbits would have crashed into the Earth. Velikovsky actually presented evidence of thousands upon thousands of Tunguska-like events which left craters all across the east coast of the United States from New Jersey to Georgia, known as the Carolina Bays. The evidence of the Indian Ocean crater and the chevrons in Madagascar clearly reflect Velikovsky’s thesis.

Blakeslee continues:

“Most astronomers doubt that any large comets or asteroids have crashed into the Earth in the last 10,000 years. But the self-described “band of misfits” that make up the two-year-old Holocene Impact Working Group say that astronomers simply have not known how or where to look for evidence of such impacts along the world’s shorelines and in the deep ocean.

“The researchers, who formed the working group … are established experts in geology, geophysics, geomorphology, tsunamis, tree rings, soil science and archaeology, including the structural analysis of myth.

“This year the group started using Google Earth, a free source of satellite images, to search around the globe for chevrons, which they interpret as evidence of past giant tsunamis. Scores of such sites have turned up in Australia, Africa, Europe and the United States, including the Hudson River Valley and Long Island [both in New York].

“When the chevrons all point in the same direction to open water, Dallas Abbott, an adjunct research scientist at Lamont-Doherty Earth Observatory in Palisades, N.Y.,

uses a different satellite technology to look for oceanic craters. With increasing frequency, she finds them, including an especially large one dating back 4,800 years. But if Dallas Abbott is right and they find 10 such events, we’ll have a real contradiction on our hands.”

David Morrison, a strident critic of Velikovsky at NASA Ames Research Center in Mountainview, California, claims: “Given [the astronomers’] observations ‘there is no reason to think we have had major hits in the last 10,000 years … But if Dallas Abbott is right and they find 10 such events, we’ll have a real contradiction on our hands’.”

Although some scientists suggest the chevrons might be caused by other geological processes, William Ryan, a marine geologist at Lamont Observatory and co-author of the book *Noah’s Flood*, in which he claims that the Black Sea was suddenly filled with salt water from the Mediterranean Sea about 7500 B.C., remarks:

“Many of us think Dallas is really onto something… She is building a story just like Walter Alvarez … a professor [who] spent a decade convincing skeptics that a giant asteroid wiped out the dinosaurs 65 million years ago.

“Ted Bryant, a geomorphologist at the University of Wollongong in New South Wales, Australia, was the first person to recognize the palm prints of mega-tsunamis. Large tsunamis of 30 feet or more are caused by volcanoes, earthquakes and submarine landslides, he said, and their deposits have different features.

“Deposits from mega-tsunamis contain unusual rocks with marine oyster shells, which cannot be explained by wind erosion, storm waves, volcanoes or other natural processes, Dr. Bryant said.

“We’re not talking about any tsunami you’ve ever seen,’ Dr. Bryant said. “Aceh [another tsunami] was a dimple. No tsunami in the modern world could have made these features. End-of-the-world movies do not capture the size of these waves. Submarine landslides can cause major tsunamis, but they are localized. These are deposited along whole coastlines.”

Dr. Bryant found two chevrons more than 4 miles from the shore near Carpentaria, north central Australia, which point north. Dr. Abbott examined these chevrons and took deep sea sediment cores.

“The cores contain melted rocks and magnetic spheres with fractures and textures characteristic of a cosmic impact. ‘The rock was pulverized, like it was hit with a hammer,’ Dr. Abbott said. ‘We found diatoms fused to tektites,’ a glassy substance formed by meteors. The molten glass and shattered rocks could not be produced by anything other than an impact, she said. …

“Dr. Abbott and her colleagues have located chevrons in the Caribbean, Scotland, Vietnam and North Korea, and several in the North Sea.

“Hither Hills State Park on Long Island has a chevron whose front edge points to a crater in Long Island Sound …

“But Madagascar provides the smoking gun for geologically recent impacts. …

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59 *loc.cit.*
60 *loc.cit.*
61 *loc.cit.*
“Dr. [Bruce] Masse [like Velikovsky and others] analyzed 175 flood myths from around the world, and tried to relate them to known and accurately dated natural events like solar eclipses and volcanic eruptions. Among other evidence, he said, 14 flood myths specifically mention a full solar eclipse, which could have been the one that occurred in May 2807 B.C. [4807 years ago].

“Half the myths talk of a torrential downpour, Dr. Masse said. A third talk of a tsunami. Worldwide they describe hurricane force winds and darkness during the storm. All of these could come from a mega-tsunami.”

Scott Carney, in particular, deals with Dr. Masse’s view of the myths respecting this catastrophe in Discover Magazine:

“The serpent’s tails coil together menacingly. A horn juts sharply from its head. The creature looks as if it might be swimming through a sea of stars. Or is it making its way up a sheer basalt cliff? For Bruce Masse, an environmental archaeologist at Los Alamos National Laboratory, there is no confusion as he looks at this ancient petroglyph, scratched into a rock by a Native American shaman. ‘You can’t tell me that isn’t a comet,’ he says.

“In Masse’s interpretation, the petroglyph commemorates a comet that streaked across the sky just a few years before Europeans came to this area of New Mexico. But that event is a minor blip compared to what he is really after. Masse believes that he has uncovered evidence that a gigantic comet crashed into the Indian Ocean several thousand years ago and nearly wiped out all life on the planet. What’s more, he thinks that clues about the catastrophe are hiding in plain sight, embedded in the creation stories of cultural groups around the world. His hypothesis depends on a major reinterpretation of many different mythologies and raises questions about how frequently major asteroid impacts occur. …

“Masse’s epiphany came while poring over Hawaiian oral histories regarding the goddess Pele and wondering what they might reveal about the lava flows that episodically destroy human settlements and create new tracts of land. He reasoned [as did Velikovsky] that even though the stories are often clouded by exaggerations and mystical explanations, many may refer to actual incidents. He tested his hypothesis by cross-checking carbon-14 ages for the lava flows against dates included in royal Hawaiian genealogies. The result: Several flows matched up with the specific reigns associated with them in the oral histories. Other myths, Masse theorizes, hold similar clues.

“Masse’s biggest idea is that some 5,000 years ago, a 3-mile-wide ball of rock and ice swung around the sun and smashed into the ocean off the coast of Madagascar. The ensuing cataclysm sent a series of 600-foot-high tsunamis crashing against the world’s coastlines and injected plumes of superheated water vapor and aerosol particulates into the atmosphere. Within hours, the infusion of heat and moisture blasted its way into jet streams and spawned superhurricanes that pummeled the other

62 loc.cit.
side of the planet. For about a week, material ejected into the atmosphere plunged the world into darkness. All told, up to 80 percent of the world’s population may have perished, making it the single most lethal event in history.”

Here now is a discussion of mythic evidence that thoroughly follows Velikovsky on such matters:

“Why, then, don’t we know about it? Masse [like Velikovsky] contends that we do. Almost every culture has a legend about a great flood, and—with a little reading between the lines—many of them mention something like a comet on a collision course with Earth just before the disaster. The Bible describes a deluge for 40 days and 40 nights that created a flood so great that Noah was stuck in his ark for two weeks until the water subsided. In the Gilgamesh Epic, the hero of Mesopotamia saw a pillar of black smoke on the horizon before the sky went dark for a week. Afterward, a cyclone pummeled the Fertile Crescent and caused a massive flood. Myths recounted in indigenous South American cultures also tell of a great flood.

“These stories are all exactly what you would expect from the survivors of a celestial impact,’ Masse says … ‘When a comet rounds the sun, oftentimes its tail is still being blown forward by the solar winds so that it actually precedes it. That is why so many descriptions of comets in mythology mention that they are wearing horns.’ In India, he notes, a celestial fish described as ‘bright as a moonbeam,’ with a horn on its head, warned of an epic flood that brought on a new age of man.

“Among 175 flood myths, Masse found two of particular interest. A Hindu myth describes an alignment of the five bright planets that has happened only once in the last 5,000 years, according to computer simulations, and a Chinese story mentions that the great flood occurred at the end of the reign of Empress Nu Wa. Cross-checking historical records with astronomical data, Masse came up with a date for his event: May 10, 2807 B.C.”

There is no doubt, all these analyses and descriptions of a global catastrophe come right out of the works of Velikovsky. Though the time of the event is assumed to be accurate based on the established chronology, this is contradicted by the ages of living olive and cypress trees in the Sahara.

Another giant ripple of recent global celestial catastrophism has also been presented by another group of scientists, this time for North America. It states that a massive Tunguska event—like the bolide that exploded over Siberia—occurred over the great ice cap that had buried North America under perhaps a mile of ice. Thus a report in Science Daily, an Internet on-line magazine presents an article titled “Extraterrestrial Impact Likely Source Of Sudden Ice Age Extinction.”

“In the Proceedings of the National Academy of Sciences [an] international team lays out its theory that the mass extinctions in North America were caused by

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63 Scott Carney, “Did A Comet Cause the Great Flood?”, Discover (Nov. 2007), p. 66
64 loc.cit.
65 September 25, 2007
one or more extraterrestrial objects – comets or meteorites – that exploded over the Earth or slammed into it, triggering catastrophic climate change.”

The evidence for this was summarized in the Boston Globe by Colin Nickerson:

“Wooly mammoths, giant sloths, saber-toothed cats, and dozens of other species of megafauna may have become extinct when a disintegrating comet or asteroid exploded over North America with the force of millions of hydrogen bombs, according to research by an international team of scientists.

“The blast, which the researchers believe occurred 12,900 years ago, may have also doomed a mysterious early human culture, known as Clovis people, while triggering a planetwide cool-down that wiped out the plant species that sustained many outsize Ice Age beasts, according to research published online yesterday [September 25, 2007] in the Proceedings of the National Academy of Sciences.

“Scientists have long speculated that an impact from a comet or asteroid may have wiped out dinosaurs 65 million years ago.

“But the notion of an extraterrestrial object wreaking such havoc during human times is a bit unnerving even to researchers.

“‘What is sobering about this theory of ours is that this impact would be so recent,’ Peter H. Schultz, a Brown University planetary geologist who participated in the research, was quoted in a release from the university. ‘Not so long ago, something may have fallen from the sky and profoundly changed our climate and our culture.’

“The object, with a girth estimated to be 3 miles, appears to have exploded high above present-day Canada with such fury that detritus was spread from California to Belgium. The height of the blast and the cushioning effect of the ice layers that still covered the region would explain the lack of an immense crater.

“‘The comet may have broken up into small pieces as it neared the earth, and these pieces detonated in various places [such as the site of the Carolina Bays] above North America and northern Europe [where other such bays exist],’ Ted Bunch, professor of geology at Northern Arizona University and a retired NASA researcher who specializes in extraterrestrial impact research, said in an interview.

“‘The cataclysm occurred … when an array of fantastic mammals and birds – including camels, tapirs, and a condor with a 16-foot wingspan – shared North America with Clovis people, hunter-gatherers known for their distinctive stone spearheads.

“‘The detonation may have fried them or the shock wave would have compressed them,’ said Bunch, one of the authors of the paper, referring to creatures directly exposed to the blast. ‘Others would have been wiped out in massive fires and floods.’

“Indeed, fossil records of some of the most exotic beasts associated with the era, along with Clovis culture, abruptly disappear with a dark layer of dirt called ‘black mat’ [above them]. The mat was formed by algae-rich water containing soot and other remnants of burned material, according to the research.

“Just beneath the black mat layer, scientists found high concentrations of magnetic grains holding iridium, charcoal, soot, carbon spherules, and ‘glass-like carbon.’ Also found were tiny diamonds, known as nanodiamonds, and extraterrestrial helium.
“Nanodiamonds are formed only by the kind of incredible pressures you'd get from an extraterrestrial object slamming into earth,” Bunch said. ‘The other material, especially the helium, also strongly suggests [something] extraterrestrial,’ most likely a comet or ‘low-density, carbon-rich’ asteroid.

“The soot is indicative of immense fires that roared across North America, fanned by ‘hurricane-force winds,’ according to the scientists.

“The research, led by Richard Firestone of Lawrence Berkeley National Laboratory in California, is likely to fan controversy among scientists. That is partly because it flies in the face of recent research suggesting that North America’s big mammals were hunted to extinction by early humans, but mainly because the paper argues that the comet’s impact triggered a planetwide big chill, the so-called Younger Dryas cool-down, that lasted 1,000 years.

“This is fascinating research when it comes to the mass extinctions. They really seem on to something,’ said Jeffrey P. Severinghaus, a geochemist and expert in prehistoric climatology with the Scripps Institution of Oceanography in California. ‘I can imagine this sort of impact causing a cool-down of five years or 10 years, but 1,000 years – well, I'm skeptical. I don't think they have given good evidence for that.’”

The response on the part of scientists, still in large measure attached to the uniformitarian model for the history of the Earth over the past 10,000 years, was not long in coming. Richard A. Kerr, the editor of Science, was shrill and quick to enthusiastically report the demise of this celestial catastrophist theory and evidence in an article provocatively titled “Experts Find No Evidence For A Mammoth-Killer Impact”:

“It looks impressive on the [television] screen last spring [2007]. Nearly a dozen debris markers, found at 26 sites from the U.S. West Coast to Belgium testified to a huge impact followed by a continent-spanning wild fire. The catastrophe had taken place a geologic instant ago–closely coinciding with the disappearance of North America’s mammoths and the continent’s earliest [Clovis] human culture. (Science 1 June 2007, p. 1264) Then came the 26-author paper last October in the Proceedings of the National Academy of Sciences (PNAS), not to mention the hour-long National Geographic Channel documentary running on cable last October [2007], with more coverage on the way from the History Channel and PBS’s prestigious program NOVA.

“Although cosmically blasted mammoths make good copy, many impact specialists have lately swung from leeriness to thorough disbelief. ‘The whole thing is contrived,’ says geochemist and impact specialist Christian Koeberl of the University of Vienna, Austria. ‘Their data don’t agree with anything we know about impacts. It just doesn’t make sense. Occam’s razor has been put safely in a drawer somewhere …’

66 Colin Nickerson, “Cosmic blast may have killed off megafauna; Scientists say early humans doomed, too,” The Boston Globe, (September 25, 2007), p. A2
“Proponents [of the cosmic impact thesis], meanwhile, are defending some of their published claims and giving ground on others but promising ultimate vindication.”

The arguments, though somewhat complicated, can be narrowed down to a claim that all these markers of a celestial, Tunguska-like or impact event—nanodiamonds, soot, Bucky Balls, iridium, magnetic spherules, etc.—can be explained by other processes or that these materials are simply not there in the amounts expected. For example, archaeologist Vance Haynes, professor emeritus at the University of Arizona, Tucson, claims he could find in 300 grams of dust from the roof of his house many magnetic microspherules. “‘[W]hether they are the melted iridium-rich micrometeorites that continually drift down from the upper atmosphere or the product of high-temperature industrial processes such as coal-burning, he doesn’t yet know. Either way they could be trouble’ to the impact thesis.”

The problem the opponents of the impact theory seem to casually ignore with each of their uniformitarian processes is: why are these materials—nanodiamonds, soot, Bucky Balls, iridium, magnetic spherules, etc.—that came from these other processes found in their greatest abundance only in and just below the black mat, but not well below or well above it from America to Europe? What they are suggesting is that each of these individual uniformitarian or other processes all at the same time produced a welter of these materials. In terms of Occam’s razor, this unusual coincidence of all of these processes just happening at the same moment in time has so low a probability that what they are asking one to believe borders on the miraculous. The actual levels of these cosmic materials are outlined by Richard Firestone, Allen West and Simon Warwick-Smith who report:

“He [archaeologist Bill Topping] had to see if there were more magnetic particles in the Clovis level than there were above and below it. If they peaked in the Clovis layer, that would be the decisive factor. It would mean that somehow billions of these magnetic grains suddenly entered the Clovis world …

“There were far more grains in the Clovis samples than in the nearby levels, as many as 2,000 percent more …”

The same was also true for microspherules. Topping found “tens of thousands of them here [in the layers] … He checked the other layers making a comparative count of the number. What he found was that the Clovis layer had about 3,000 percent more microspherules than the nearby layers, compelling evidence supporting the cosmic [catastrophic North American] event.”

68 ibid., p. 1322
70 ibid., p. 32
What is of interest to the discerning reader is that two groups of scientists have uncovered evidence of two separate major global celestial catastrophes on two hemispheres of the planet within the last few to several thousand years, which drove many of the various forms of life, including humans, to either near or complete extinction. As I predicted over ten years ago in *The Extinction of the Mammoth*, page 294: “To be sure, the floodgates of recent catastrophism based on myths are opening and we can expect to be deluged by recent cosmic catastrophist theories from the scientific establishment.” Another example, in this respect, is presented by Alan Bond and Mark Hempsell’s book, *A Sumerian Observation of the Köfels Impact Event*. This short volume is described in a news release by Joanne Fryer titled “Cuneiform clay tablet translated for the first time” on the Internet:

“A cuneiform clay tablet that has puzzled scholars for over 150 years has been translated for the first time. The tablet is now known to be a contemporary Sumerian observation of an asteroid impact at Köfels, Austria and is published in a new book, ‘A Sumerian Observation of the Köfels Impact Event.’

“The giant landslide centred at Köfels in Austria is 500m thick and five kilometres in diameter and has long been a mystery since geologists first looked at it in the 19th century. The conclusion drawn by research in the middle 20th century was that it must be due to a very large meteor impact because of the evidence of crushing pressures and explosions. But this view lost favour as a much better understanding of impact sites developed in the late 20th century. In the case of Köfels there is no crater, so to modern eyes it does not look as an impact site should look. However, the evidence that puzzled the earlier researchers remains unexplained by the view that it is just another landslide.

“This new research by Alan Bond, Managing Director of Reaction Engines Ltd and Mark Hempsell, Senior Lecturer in Astronautics at Bristol University, brings the impact theory back into play. It centres on another 19th century mystery, a Cuneiform tablet in the British Museum collection No K8538 (known as “the Planisphere”). It was found by Henry Layard in the remains of the library in the Royal Palace at Nineveh, and was made by an Assyrian scribe around 700 BC. It is an astronomical work as it has drawings of constellations on it and the text has known constellation names. It has attracted a lot of attention but in over a hundred years nobody has come up with a convincing explanation as to what it is.

“With modern computer programmes that can simulate trajectories and reconstruct the night sky thousands of years ago the researchers have established what the Planisphere tablet refers to. It is a copy of the night notebook of a Sumerian astronomer as he records the events in the sky before dawn on the 29 June 3123 BC (Julian calendar). Half the tablet records planet positions and cloud cover, the same as any other night, but the other half of the tablet records an object large enough for its shape to be noted even though it is still in space. The astronomers made an accurate note of its trajectory relative to the stars, which to an error better than one degree is consistent with an impact at Köfels.
“The observation suggests the asteroid is over a kilometre in diameter and the original orbit about the Sun was an Aten type, a class of asteroid that orbit close to the earth, that is resonant with the Earth's orbit. This trajectory explains why there is no crater at Köfels. The incoming angle was very low (six degrees) and means the asteroid clipped a mountain called Gamskogel above the town of Längenfeld, 11 kilometres from Köfels, and this caused the asteroid to explode before it reached its final impact point. As it travelled down the valley it became a fireball, around five kilometres in diameter (the size of the landslide). When it hit Köfels it created enormous pressures that pulverised the rock and caused the landslide but because it was no longer a solid object it did not create a classic impact crater.

“Mark Hempsell, discussing the Köfels event, said: ‘Another conclusion can be made from the trajectory. The back plume from the explosion (the mushroom cloud) would be bent over the Mediterranean Sea re-entering the atmosphere over the Levant, Sinai, and Northern Egypt.

‘The ground heating though very short would be enough to ignite any flammable material – including human hair and clothes. It is probable more people died under the plume than in the Alps due to the impact blast.’”

The dating of this event, like all the others, is highly speculative and it might very well have happened earlier than assumed, or in late prehistoric times.

Such great upheavals would have triggered immense earthquakes, etc., and these would have led to other catastrophes over the globe. In this respect one such event is outlined in an article that appeared in Geophysical Research Letter, vol. 33, no. 22 (Nov. 2006) by Maria Teresa Pareschi, Enzo Boschi and Massimiliano Favalli titled “The Lost Tsunami,” which presents evidence that the flood legends of the ancient Near East in fact reflected a real event that occurred in the Mediterranean about 8000 years ago, in 6000 B.C. They suggest that a volcanic eruptive landslide on the eastern flank of Mount Etna in Sicily caused a volume of land about the size of Manhattan island–13 miles long, 2 miles wide or about 25 cubic miles–to cascade into the sea. Such a massive volume would have induced a great tsunami that inundated shorelines all around the Mediterranean Sea to a greater or lesser extent according to their locations.

The basis for this finding is in part that an ancient fishing village, Atlit-Yam, near the coast of Israel was submerged beneath the Mediterranean and exhibits signs of sudden abandonment 7000-8000 years ago. Their evidence includes a heap of gutted fish that had evidently been processed and stored away, but which was uncovered beneath a covering of clay laid down by a flood.

72 Alan Bond, Mark Hempsell, A Sumerian Observation of the Köfels Impact Event (Bristol UK 2008), pp. 93-97
Further evidence of this disaster that they point to are mainly highly disturbed sediments lying on the bed of the Ionian Sea to the east. Tsunamis do disturb these soft submarine sediments that wash ashore and leave clay deposits. The deposits they examined clearly imply Mount Etna as the source of the tidal wave, and these researchers clearly disassociate themselves with either a cosmic impact or deep sea earthquake as the cause of the event.

They suggest this tsunami generated waves up to 165 feet or 50 meters high off southern Italy as it left that region moving east which became a sea surge of about 43 feet or 13 meters when it struck Greece and Libya with smaller waves farther away which could have penetrated miles inland from the coast. Pareschi and her team estimated that the tsunami would have been moving at a speed of about 450 miles or 725 kilometers an hour when it struck land.

This event would have been similar to the modern one of December 2004 that struck Indonesia. That tsunami actually moved the axis of the Earth one inch instantly. Thus we have in the ancient past Indian Ocean and Mediterranean tsunamis that occurred fairly close in time to one another, or more probably at the same time. The impact in the Indian Ocean would have sent seismic waves through the Earth that triggered the Mount Etna landslide. Yet it is assumed that such a devastating impact in the Indian Ocean did not affect other, more distant regions in this way.

Related in time to these ancient tsunamis is the research by Walter Pitman and William Ryan of the Lamart-Doherty Observatory offered in their book *Noah’s Flood* (NY 2000). In it they claim that in about 7500 B.C. the Black Sea was a fresh water lake separated from the Mediterranean Sea by a plug of land. Something, a gradual leakage or an earthquake caused that plug to weaken or break, and salt water inundated the Black Sea lake, flowing past the plug with the force of a hundred Niagara Falls, filling the Black Sea basin in a relatively short time. The evidence for this event is rather large. Cores taken of the sea bed showed a distinct change from fresh water laid materials to salt water ones, showing the change was rapid.\(^73\)

In 1988, Bob Karlin aboard the Woods Hole research vessel, *Knorr*, made an important discovery:

“Just where the Bosporus joins the Black Sea, Karlin discovered evidence of a one-time enormous underwater sedimentary avalanche. As revealed by his echo soundings, the sharp-sided canyon ... led into an enormous sedimentary apron that fanned out hundreds of kilometers into the Black Sea. It was just as if an immense powerful torrent of pent-up water from the Mediterranean, after scouring out the

\(^{73}\) Ian Wilson, *Before the Flood: The Biblical Flood as a Real Event and How It Changed the Course of Civilization* (NY 2004), p. 41
Bosporus canyon, had surged northwards to break into the Black Sea at this point. But still it was unclear when and why such a breakthrough might have happened.”

Furthermore Dr. Petka Dimitrov from Varna, Bulgaria sent a fax message to Pitman and Ryan containing the following:

“I found an old shoreline about 110 meters [358 feet] under the surface. Then I found ancient beaches. The old dune formations were preserved. This proves that they had been covered suddenly by a huge volume of water.”

“According to Dimitrov, this old shoreline, which would of course have been that of the former freshwater lake, dated back, according to the best calculations he was able to obtain, to c. 7750 B.C.”

Moreover, Pitman and Ryan’s research revealed that the Don River, which enters the Black Sea, had earlier extended well out into it when it was a fresh-water lake. And lastly, Candace Major was able to show that in the cores of the seabed the upper layer contained shells of Mytilus, a saltwater variety, but in the deeper parts of the cores were Dreissena rostriformis, a gastropod [snail] that lives in fresh water but not in the open sea.

Thus, we have been offered by various teams of scientists evidence that there was giant cosmic impact in the Indian Ocean about 4800 years ago sweeping a wall of water ashore in Madagascar that left four 1000 foot high chevrons pointing to the crater, and that other such chevrons have been tentatively located all over the globe, implying other such massive movements of water across the oceans of the world, possibly at the same time.

Aligned with this evidence we have, also around or at the same time, a gigantic landslide on the eastern flank of Mount Etna that sent an estimated 165-foot wall of water racing across the Mediterranean at 450 miles an hour, inundating shorelines and flowing inland for miles around that basin. And somehow, perhaps even earlier, some unknown event broke the land plug at the Bosporus so that the Mediterranean deluged the Black Sea fresh water lake with the force of 100 Niagaras. Yet many scientists do not see these as related events and suggest that there is no evidence of major celestial catastrophes in these ancient times. Yet we still have the explosion evidence for North America which wiped out the megafauna, leaving cosmic debris in a layer of a black mat across this vast region while in Köfels in Europe there is a landslide 500 meters or 1600 feet thick with a 5 kilometer or almost 3 mile diameter, also supposedly happening at close but separate times. That is we now have evidence for five major catastrophes in recent
prehistoric times—the dates of none of them being secure—but all are supposedly separate and unrelated to each other.

Everything about massive pole shifts and climate shifts that Velikovsky presented almost 60 years ago is now in various guises being put forth by scientists: pole shifts, immense floods, mass extinctions, stupendous hurricanes, great forest conflagrations, animal burials under flood debris and large climate shifts, and these, they say, are reflected in man’s ancient myths. And this trend will, I believe, continue to grow. The dating of these events still needs more exacting work but gradually modern science is inexorably groping in Velikovsky’s direction and playing the game of catch-up with this great pioneer.

The negative views by J. Derral Mulholland and others of Velikovsky’s catastrophism are gradually being undermined:

“If a planet-sized object were to pass close by the Earth, then giant tides would be raised; there would be global earthquakes, the north pole would change direction; the day, the month, the seasons, the year would all change. Faith is not involved here; these are unavoidable consequences of the laws of motion as we presently know them. We must accept that the dynamical aspects of Velikovsky’s vision of hell on Earth are largely acceptable. This is not to admit that the events he described ever happened.”

In a later chapter we will deal with the evidence of the 8th century B.C. Mars catastrophes. What is clear, however, is that there is scientific evidence of a pole shift and global catastrophes in prehistoric times, and teams of scientists are providing evidence for this fact.

It is therefore particularly loathsome to read, while all this new research is going on, this unjust and ignorant statement by Dwardu Cardona, some of whose mythological work is a direct offshoot of Velikovsky’s, in his problematic book, *God Star*, where he calls Velikovsky a “discredited scholar.” As Lewis M. Greenberg told me in a telephone conversation, “Where would Cardona be without Velikovsky?” Ignorant of this new, burgeoning catastrophic research, in a certain sense, Cardona is in the same position as Carl Sagan and all the other critics of Velikovsky. Scientists, contrary to these critics, are now saying that there were great celestial catastrophes that devastated the Earth in the past thousands of years, based on direct physical evidence, and that these events are reflected in ancient myths and legends. Sadly, Cardona, like so many of these other critics, is yet another footnote in the Velikovsky Affair.

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THE AFRO-ASIATIC LANGUAGES, CLIMATE, AND CHRONOLOGY IN PREHISTORY

Directly related to the chronology of the onset of Egyptian civilization and to the two climatic changes in the Sahara and the rest of the ancient Near East is the linguistic history of the nomadic pastoralists who drove their herds of cattle etc. across this vast expanse of North Africa, Arabia to Mesopotamia while it was yet a rich savannah. As was pointed out above, the catastrophes which created these pole shifts/climate shifts had reduced the populations of animals and peoples throughout the globe. Yet the regions that did not experience the worst aspect of these catastrophes clearly had relatively large populations that came through these events intact. Although they were clearly affected, these few groups were able to repopulate the regions adjacent to them where they pastured their flocks. North Africa was one of these regions that did not lose the same number of genera as others to this first extinction event. It still had a highly diversified assemblage of megafauna such as elephants, hippos, rhinoceroses, into and through ancient times. As these pastoralists migrated with their herds, they carried their language with them.

However, when these regions turned from savannah to desert, the people in the various areas became separated and isolated from each other and their languages continued to evolve. In a period of a few hundred years they became dialects of the original language, and in much less than a thousand years, these languages would have become hardly related to any of the others. If they evolved for over a thousand years, each language would have become as different and unrelated to the original language and to one another as happened over the thousand years that separates Anglo-Saxon from English. That is, if the Sahara, Sinai, Arabian, and Syrian deserts formed in 2300 B.C. and literate civilization began, as posited in the short chronology, around 1200-1100 B.C., there would be almost nothing to link these languages to each other. The linguistic evidence related to this will be analyzed toward the end of this volume in the chapter on “The Indo-European Language, Climate and Chronology in Prehistory.” The following chapters on Egyptian history/chronology will be devoted to giving scientific and technological and other evidence that Egyptian history is much, much shorter than is assumed by the historians of our era. The evidence for the close linguistic relationships found for languages across North Africa to Mesopotamia is well outlined by Thomas L. Thompson:

“For origins we need to go far back to the North African ancestors of the speakers of Semitic languages. They lived in the ‘Green Sahara’ until late in the seventh millennium B.C.E., when a long and relentless drought [Velikovsky’s 1500-800 B.C. pole shift/climate shift]–responsible for the creation of the desert of the ‘Great Sahara’ that we know today–forced the farmers and herders of North Africa to leave their homes and villages to emigrate to the Berber lands of the
west, to the Chad areas to the south, to the Nile Valley to the east, and finally crossing the Nile to Palestine by way of the Sinai.

“As early as 1950, German scholars had begun to recognize close similarities between verbs in the Akkadian language and in some of the languages of North Africa. This was apparent in the Berber language of the northwest, but also in Libyan [the language of the region of the ancient Garamantes people]. The similarities in both made it difficult to see the Semitic languages as entirely independent of these. Scholars also connected the two languages in North Africa that were separated from each other by more than a thousand miles of desert. Further study of language families by the 1960’s made it clear that the earliest characteristics of Semitic languages were closely related to a number of African languages, not only Berber and Libyan, but also ancient Egyptian and the later Coptic language spoken in Egypt. These languages were also closely allied with Cushite of the modern Sudan and with Chad south of the Sahara.”

Thompson adds to this that:

“The close association of archaeology with linguistics brought strength to this direction of research by bringing a clear historical dimension for what had always been primarily a linguistic theory without a chronology. Semito-Hamitic (also known as Afro-Asiatic), and proto-Semitic were no longer merely theoretical models, they began to look like actual historical languages. Though we have no texts written in this language, we know it existed! Proto-Semitic was to be dated to the period before Akkadian went its own way and took on its own character as a language. We [believe we] know when that happened: when Semitic speakers first entered the Tigris and Euphrates valleys and joined with Sumerians [i.e. the Chaldeans in the short chronology] of South Mesopotamia in the course of the third millennium BCE.”

Along the same lines, Samuel Kurinsky states:

“Many scholars today accept that at some time during the pre-dynastic period (possibly c. 3400 B.C.), a new group of people arrived in Egypt and their advent resulted in profound changes in many aspects of civilization,’ states Rosalie David ... Sir Alan Gardiner concurred: ‘Many affinities (of the Egyptian language) with Hamitic and in particular with Berber dialects have been found...’ Will Durant acknowledged the consensus: ‘The further back we trace the Egyptian language the more affinities it reveals with Semitic tongues of the Near East. The pictographic writings of the predynastic Egyptians seem to have come from Sumeria’."

Here then we come to a massive contradiction to the chronology of the Afro-Asian language and the drying up of the Sahara. The linguists have found clear affinities of this proto-Afro-Asian language from the lands of the Berbers in north-
west Africa to Mesopotamia where it shares these affinities with Akkadian. That is, in order to chronologically correlate this linguistic development, both Egyptian and Mesopotamian chronology must date back to at least 3000 to 3500 B.C. But in volumes I and II of this series, I pointed to a number of linguistic correlations along with scientific and technological evidence that demand that the chronology of Mesopotamia be shortened by about 1800 to 2000 years. Although it is tedious to point these out again, they must be re-exhibited to expose the linguistic time frame of Mesopotamian history and how it contradicts the accepted chronology of the development and evolution of the Afro-Asian languages.

1. The dig at Tall Munbaqa contained cylinder seals supposedly dating from about 2300 B.C. with Akkadian writing on them, but as I have repeatedly shown, they were the same supposedly for 2300 B.C. and also for 750 years later. That is, there was no change in the Akkadian language for over 700 years. No linguist would accept such a condition to have existed over so great a span of time. This linguistically shortens Mesopotamian history by some 700 years.

2. The evolution of Akkadian itself is fraught with this same chronological problem. Over the 1500 years or more of its evolution the two forms of it–Babylonian/Akkadian spoken in southern Mesopotamia and Assyrian/Akkadian in the north, separated in time by over 1000 years and several hundreds of miles in space should have evolved into totally new languages. But as we cited Georges Contenau, “The two forms of the language are practically identical in grammar and vocabulary, and probably differed more in their method of pronunciation.”

Therefore we must linguistically lower Mesopotamian history/chronology by about another thousand years. The Akkadian linguistic evidence from Tall Munbaqa lowers Mesopotamian history/chronology by about 700 years and the Akkadian spoken thereafter changed so little that it requires an additional lowering of that history/chronology of about 1000 years or 1700 years altogether. In this regard, the Afro-Asian language could not have come to Mesopotamia around 3400 B.C. because by 1300 B.C. or 2100 years later there would not exist a clear, straightforward correlation between Akkadian and its parent language. In 2100 years any correlations and “close affinities” would have evolved beyond probable recognition. This requires/demands that the parent Afro-Asian language came to Mesopotamia at least about 1700 years closer to the present as well. The linguistic evidence of the Afro-Asian language does not support, or correlate with, the established chronology, but clearly does so with the short chronologies of Heinsohn, Rose, and Sweeney.

Thompson graphically describes this aridification that descended across this vast territory in his chapter titled “Paradise lost 6500–4500 BCE”:

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“A radical change in climate took hold of the entire Mediterranean Basin by the end of the seventh millennium BCE. Sea levels fell steadily, as did the water-table in most regions. Higher temperatures and longer dry summers dominated the climate, especially to the east and south of the Mediterranean. Rainfall diminished sporadically. An increasing frequency of drought years brought famine, overgrazing and the gradual abandoning of agricultural lands. North Africa entered a long transition from a Mediterranean climate to steppelands. These changes continued in an extended and intensifying series of droughts that lasted for well over a thousand years. What started out as agricultural collapse and famine in marginal zones became a region-wide disaster. People were forced to abandon completely what had been rich agricultural lands. The Sahara relentlessly encroached on every front, eventually leaving only small pockets of farmers and herders near oases fed by the few and distant springs that survived the drastic lowering of the water-table. As the Green Sahara collapsed and forever disappeared, leaving only a few traces in the drawings and tools of the farmers from this once prosperous region, the great Sahara of today, with its shifting dunes of sand, nearly impassable expanses of empty wilderness and wholly unforgiving climate, took its relentless hold across the entire northern rim of the continent of Africa.

“The desiccation of the Sahara did not only affect the farmers of the Green Sahara, it also impinged on their close neighbours of the steppe zones. The expansion of the Sahara sand-dunes was particularly unfavourable to the steppe-dwelling herders of sheep and goats. These were denied not only the stands of wild grasslands of their homelands, but also the support of the patch agriculture that normally formed such a staple of pastoral nomadism. As a result, such groups were driven to the fringes and highlands bordering the expanding desert.

“Many of the refugees from the drought were driven westward and southwestward. They settled down in what became the Berber lands in the extreme northwest corner of Africa. Still others moved southward into North-central Africa. They settled in the area of Chad and in the Darfur region. Many more moved eastward to the central Nile Valley and laid the foundations of the Egyptian language. The migrants became geographically and physically isolated from each other as the expanding Sahara cut off further contact. The greater the isolation, the more distinctive were the languages that developed. As the migration spread over larger and ever more complicated regions, whole families of languages were created over time. The expansion of the Libyan dunes separated the people of the Berber lands from Egypt, and both from North-central Africa. These separations led to the historically distinct but related languages that we know in North Africa.

“Most major areas of the ancient Near East were altered by this great change in the climate. In addition to North Africa, the Sinai Peninsula that separates Africa from Asia shifted from what was dominantly a steppeland to a desert, comparable to what we find today. Arabia, apart from the region of Arabia Felix in the southern corner of the peninsula, changed like the Sahara into a vast
expanse of desert where occupation was limited to its few scattered oases. However, while this extended period of drought destroyed agriculture over areas of both Africa and Arabia, turning the survivors of starvation, famine and disease into refugees, the falling water-table and more arid climate also dried up many great swamps and marshes. This opened new areas to farming and sedentary occupation for the first time. The Delta marshlands at the mouth of the Tigris and Euphrates valleys drained off and exposed one of the ancient world’s richest expanses of agricultural lands. This was to become the heartland of ancient Sumer, the earliest of the great ancient Near Eastern civilizations.**83

We therefore have two scientific anchor points for Egyptian history and chronology. The first is Sothic and lunar dating of the 12th Dynasty as outlined in volume I, to which we will return below, which ends with Alexander the Great. The second anchor is not nearly as precise, yet the dendrochronology of olive and especially cypress trees for the final desiccation of the Sahara dates the onset of Egyptian civilization to some time just prior to this drying up by no more than 400 to 500 years, or to around 1200-1100 B.C. The aim of this book will be to present scientific and technological as well as other forms of evidence that encompasses Egyptian history between these two scientific chronological pillars.

YEARS OF DARKNESS

Before proceeding there is yet another aspect of Velikovsky’s theory regarding this final desiccation of the Sahara and all the other deserts around the world that I wish to address. In Worlds in Collision (1950), pages 126ff, Velikovsky spoke of “The Shadow of Death”. These were not years of total darkness but as Velikovsky claimed “years of this gloom, when the world was covered with clouds shrouded in mist … enveloped in a somber fog …” because “there was little light” (p. 128). He also claimed that during the period “None knoweth that midday is there; the shadow is not discerned … he [the sun] is beheld; he is in the sky like the moon” (p. 128). At times “the disc of the sun was not clearly visible, and only its diffused light made the day different from the night. The gloom gradually lifted with the passing years as the clouds became less thick; little by little the sky and the sun appeared less and less veiled” (pp. 128-9).

No matter where any dust and other material in the atmosphere comes from, it settles out in weeks and, in very rare cases, months, even from a comet. As Kenneth Hsu states:

“A large comet need not even hit the earth to produce dust; a near miss would leave enough debris in earth’s atmosphere to produce a complete blackout.

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83 Thompson, op.cit., pp. 112-113 [emphasis added]
“Toon figured that the dust would settle quickly and photosynthesis could resume by about three months after the initial blackout …

“Even when computations are made for larger volumes of dust—trillions or tens of trillions of tons—the sky would be bright as moonlit night three months after the impact, and bright enough for photosynthesis to resume in four months’ time … If [the dust particles] should remain separate and therefore settle more slowly, darkness might last longer than a year, but this possibility was considered highly improbable. Most likely darkness could not have lasted more than a few months no matter how massive a comet or asteroid had hit …”

Yet Velikovsky spoke of years of darkness and gloom and this requires a scientific explanation. With the final drying up of the world’s deserts, as described above, a geologic process had to operate which would create long and short episodes of darkness ranging from total blackouts to highly overcast days. That is, with the final desiccation of the world’s deserts in the 8th century B.C., the Earth would experience a great drought quite similar to, but immensely greater than, the great drought that befell the American Midwestern states during the decade of the 1930’s. That severe drought killed much of the vegetation in that region and the winds blowing over the parched soil picked up minute to small dust particles during certain periods of the year. The winds blew away the topsoil and shrouded whole regions of North America in various levels of darkness. Lawrence Svobida, a farmer who lived in the Midwest through this decade, describes these events:

“Only those who have not been caught out in a ‘black blizzard [dust storm]’ can have more than a faint conception of its terror. When the soil has become finely pulverized … followed by wind, when the surface is blown dirt from a previous storm, the dust begins to blow with only a slight breeze. As it continues to rise into the air, it becomes thicker and thicker, obscuring the landscape and continuing to grow in density until vision is reduced to a thousand yards [3000 feet, 900 meters] or less. If this is to be a real dust storm, a typical blizzard of the Dust Bowl, the wind increases its velocity until it is blowing at forty to fifty miles [60 to 80 km] an hour. Soon everything is moving—the land is blowing, both farm land and pasture alike. The fine dirt is sweeping along at express-train speed and when the very sun is blotted out visibility is reduced to some fifty feet; or perhaps you cannot see at all, because the dust has blinded you.”

Svobida points out that airplanes that flew “over the Dust Bowl during a dust storm used to try to get over the rolling dust clouds but pilots soon learned that they had to fly either around them or through them. They reported that the

84 Kenneth Hsu, *The Great Dying* (NY 1986), pp. 190-191
85 Lawrence Svobida, *Farming the Dust Bowl* (Lawrence KS 1968), pp. 123-124
atmosphere two or three miles \([10,560 \text{ – } 15,740 \text{ feet or } 3.2 \text{ to } 4.8 \text{ km}] up was still laden with dust."\(^{86}\) He further shows:

“There have been occasions at the height of a blow season when, in the small towns, the residents have not known for days at a time when the sun rose or set.

“Do you wonder that week after week during the blow season the congregation in the churches devote much of their time imploring the Higher Power to bring an end to the dreaded dust menace.\(^ {87}\)”

Hubert H. Lamb describes these clouds:

“On 12 May 1934 \textit{The New York Times} reported that the cloud of dust coming from ‘drought-ridden states as far west as Montana 1500 miles [2400 km] away filtered the rays of the sun for five hours yesterday.’ New York was in a half-light condition like an eclipse of the sun and the dust-cloud was thousands of feet high.\(^ {88}\)”

The extent of the area covered by this dust cloud was great. According to Michael Allaby:

“Inside the White House in May 1934, dust settled on the desk of … President [Franklin D. Roosevelt] and as fast as it was cleaned away, more settled. Outside in New York and Baltimore as well as Washington, the sky was so dark with clouds of dirt that in some places chickens roosted, thinking it was night. Dust settled on ships 300 miles [480 km] out to sea. Ducks and geese fell from the sky, choked to death by the dust through which they flew. People called the storms ‘black blizzards.’ At their height, a single cloud 3 miles high covered 1.35 million square miles, from Canada to Texas and Montana to Ohio.\(^ {89}\)”

That immense cloud of soil was caused by “soil blown from farm lands covering 150,000 square miles.”\(^ {90}\) These “black blizzards” occurred during the spring–summer season. But this seasonality would not be the case with respect to Velikovsky’s hypothesis. Deserts all over the world in both hemispheres would have been eroded both in the northern hemisphere in spring, summer, and fall, and in the southern hemisphere during these seasons there, different from those in the north, creating year-round clouds of dust. The deserts all over the world would be losing their veneers of top soil. The Sahara itself covers an area about the size of the United States. The soil blowing off all the deserts on Earth could easily darken a continental-sized area.

This dust would have left layers in various places on Earth and therefore there should exist clear evidence that in historical times immense dust storms raged across the planet. The evidence for this material was discussed by Mark Bowen.

\(^{86}\) \textit{ibid.}, p. 134

\(^{87}\) \textit{ibid.}, p. 144


\(^{89}\) Michael Allaby, \textit{Dangerous Weather Droughts} (NY 19989), pp. 76-77

\(^{90}\) \textit{ibid.}, p. 77
The dating he employs is that of the established chronology which has no scientific or technological basis to it, and thus in our discussion of this evidence that dating system will be omitted:

“The Akkadian empire [in the short chronology, the Assyrian empire of the 8th to 7th centuries B.C.] suddenly collapsed. Your encyclopedia may tell you that Akkad was overrun by tribal Gutians from the Zagros Mountains [in the short chronology the Scythians and Second Chaldeans] … and that Mesopotamia descended into chaos …

“But Egypt’s Old Kingdom collapsed at precisely the same time …

“In 1993, after fifteen years of excavation at Tell Leilan, the urban center on the Sibir plain … Harvey Weiss announced that he had uncovered a layer of fine dry wind-blown dust just above the last signs of the Akkadian [Assyrian] occupation and proceeded to touch off a tempest in the archaeological teapot by suggesting that mighty Akkad had fallen not to lowly mountain tribesmen but to a social upheaval brought about by a remarkable three-century drought.

“This notion was treated as a curiosity or local anomaly at best, until 1998, when Lamont paleoclimatologists Heidi Cullen and Peter deMenocal found a similar dust layer in a seabed core from a site about 1,400 miles [2250 km] away, in the Gulf of Oman [between major deserts] …

“Thirty-two and a half meters [35 yards] down in the … longest ice core from Kilimanjaro’s northern ice field we found a black layer of dirt more than an inch thick, interrupting very clear ice above and below it …

“But when [it was] finally nailed down … Kilimanjaro’s dust layer lines up precisely with the layer Harvey Weiss found at Tell Leilan …

“Other records show that the lakes of central Africa retreated at this time. The speleothem [calcium deposit] from the Soreq cave shows that precipitation in Israel dropped by 20 to 30 percent for about four hundred years. A core [of mud] retrieved from the bed of Lake Van in eastern Turkey … displays a ‘sharp five-fold dust spike …’ There is evidence for a simultaneous drought in Greece, Italy and Spain. Spikes show up in pollen records from West Asia and in seabed sediments from the Arabian Gulf off the coasts of Pakistan and India. Lakes also dropped in Ethiopia, the all-important head-waters of the Nile.”

This being the case, we would expect to find similar evidence in the Americas. On this Bowen reports:

“And there are signs in the New World. There’s a dust spike at 2100 B.C., give or take two centuries, in a sediment core from Elk Lake, Minnesota, and its magnitude indicates that there was three times as much dust in the air back then than there ever was during the 1930s Dust Bowl … Droughts occurred in Mexico. And finally [the] Huascaralan ice cores display an enormous dust event … lasting more

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than a century which appears to mark the greatest drought Peru has experienced in
the last seventeen thousand years … Harvey Weiss has continued to build his case
that whatever felled Tell Leilan probably reached around the globe …

“Furthermore, every record with fine enough resolution shows that this global
eruption of dust and drought came to a head very rapidly, reaching its peak within
two or three decades at most.”  
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These dust layers only reflect the drought that removed the top-soils of the
deserts. Once these had been removed, during those decades, there was little if any
new top-soil to blow away. The period after the dust layer was still in drought for
these numerous deserts, but there was no top-soil to be blown away to create new
layers, except perhaps from other localized regions where droughts occurred.

Hence, it can be seen once again that the scientific community is slowly
groping in the direction that Velikovsky pioneered some 60 years ago. Evidence
for catastrophic celestial events during the Hypsithermal period 8000–3000 years
ago over various places all around the globe is being put forth and the scientists are
suggesting this can be correlated with the myths of ancient man. Pole shifts that led
to major climate shifts are also being proposed to explain these shifts, just as
Velikovsky earlier had put forth. Lastly, there are scientists claiming, based on
evidence, that there were years of dust and darkness that accompanied these pole
shifts/climatic changes, though they fail to discuss this “darkness” as a necessary
ingredient of their scenario.

The evidence for Velikovsky’s hypothesis has continued to grow despite
decades of scientists and critics of Velikovsky ignoring these findings, or throwing
dust into the eyes of those who may be interested in this work. They will, I
daresay, continue to offer ad hoc hypotheses to explain this evidence away along
strict uniformitarian lines, but they cannot stop the slow but steady movement by
that community of scientists as they uncover more and more evidence that may, in
the end, culminate in the vindication of this great and tragic figure as it did during
the scientific revolution for the great, tragic figure of Galileo. That time may never
come but science may, in time, accept the Velikovsky revolution.

As noted above, the Sahara dried out after 1000 B.C. The people living there, as
has been shown, interacted to some degree with Egyptian civilization. But the
Saharan people did not completely vanish; rather, they evolved from a largely
pastoralist society into one that turned to agriculture by adapting to the arid
condition that overtook them, and continued to thrive alongside Egypt and well into
later Greek and Roman times and beyond. Since the final desiccation of the Sahara
began around 800-700 B.C., the people living there had to change their methods of
growing food. During this period the Saharans were surrounded by people who had

92 *ibid.*, pp. 385-386
begun to develop an ancient Industrial Revolution and to some degree would have taken part in it via trade. These people developed their own agricultural revolution which permitted them to build a large stable society that interacted with the surrounding world. Thus we read on page 3 of “The Mystery of the Black Mummy”:

“The Fezzan project, headed by Professor David Mattingly (University of Leicester) focuses on the Garamantes civilisation which thrived from 1500bc-500ad. The Garamantes were known by the Romans as barbarians but evidence from the Sahara shows a large, sophisticated civilisation. Remains show substantial architecture and a complex society replete with numerous luxuries. Almost 100,000 tombs [without mummies] litter the Fezzan escarpment – to date these bodies are the most concrete testimony to this little-known people.”

This evidence suggests that, prior to about 800 B.C., the people of the Sahara lived as pastoralists and small marginal farmers harvesting dates and olives, etc., but when the Sahara ultimately dried up after 800 B.C., they were forced to migrate or adapt their way of life to the new harsh conditions that confronted them. How did they continue to thrive in the Libyan desert for so long? According to Felipe Fernandez-Armesto:

“What is today one of the most inhospitable regions of the Sahara … the Fezzan, deep in the Libyan interior, conceals nearly a thousand miles of irrigation galleries, rough-hewn out of limestone, to channel the flow [of water] from underground springs. These fed the fields of one of antiquity’s tantalizing ill-documented civilizations ruled by people known in Greek and Roman reports as Garamantes. Their cities in the Fezzan were bounded on all sides by desert. Theirs was not a conventional oasis settlement, for it relied on elaborate hydraulic engineering to tap the vast water table of the Sahara, which is a triple-decked desert: the sand lies on limestone which covers the water, which drains from the surrounding mountains into a subterranean sea. Dates—the historic native staple of the desert—were not part of the basic diet in the region to judge from Garamantine middens [human fossil feces] nor even millet, instead the Garamantes or their slaves or peasants grew wheat where water could be easily delivered, and barley (which they exported to Roman territory) [and which they grew] on less favorable grounds.”

The chronology as to when the Garamantes made this great adaptation which brought on their efflorescence from pastoralists/agriculturalists to a sedentary civilization is summarized by Graeme Baker and D.D. Gilbertson:

“The Fezzan Project will hopefully make an important contribution [to our understanding of the history of the African Sahara] … It is likely, though has not been demonstrated, that Neolithic farmers [in the Sahara] grew their crops in small patches of soil naturally irrigated by higher groundwater levels, in contrast with

the floodwater farming systems developed on the northern margins of the Sahara fringe by Romano-Libyan farmers …

“With pastoralism and small-scale cultivation established, there is then little evidence for significant change in subsistence through the third and second millennia B.C. The period of the Garamantes, however, (between 900 BC [the approximate time for the drying up of the Sahara] and AD 500), marked a dramatic development in farming technologies and systems, associated with transformations in cultural complexity. These transformations included:

- the rise of a major polity and civilization in the Sahara;
- the development of urbanism;
- the evolution of a hierarchical and probably slave-using society;
- the adoption of a written script for the Libyan language;
- the further development of agriculture to encompass a range of Mediterranean and desert crops that require intensive irrigation (cereals, grapes, olives, dates);
- the introduction of the horse, the camel and wheeled transport to the Sahara;
- the creation of trade and political relations that extended north to the Mediterranean, east to Egypt and south to sub-Saharan Africa; and
- a massive demographic expansion to a level that was probably not equalled again until the last forty years … [It is] estimated that there were at least 120,000 Garamantian burials in the el-Agial alone.

“The Garamantes represent in part a continuation of the local neolithic tradition, as is clear from lithic and ceramic finds at their early settlements. But they probably comprised a great confederation of tribes, and there are indications that some elements may have migrated from oases further [sic] east, nearer Egypt, bringing with them knowledge of improved technology for oases civilization—notably the foggara [or underground irrigation and water storage system]. There are clear parallels, for instance between the Libyan tribesmen on Egyptian reliefs and in rock art of southern Libya and Algeria … Botanical remains from sites like Zinchecra dating to the first half of the first millennium B.C. demonstrate that irrigated cultivation had begun by that early date.”

The Garamantes chronology and development surely fit with the desiccation of the Sahara 800-700 B.C. when around that time (“900 B.C.”) they developed advanced irrigation agriculture, urbanization, hierarchical society, a written script, horse, camel and wheeled transport, and distant trade which encompasses Europe, North Africa, and the rest of the Near East.

We were told there was “little evidence for significant change through the third and second millennium B.C.” and that around “900 B.C. … marked a dramatic development in farming technologies and systems associated with [this transformation] …” Here, then, is the great problem of the Garamantes as it relates

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to climate and chronology. The Sahara supposedly dried up around 2300 B.C. while
the Garamantes changed from a pastoralist/agriculturalist society around 900 B.C.
That is, for some 1400 years the Garamantes lived in the Sahara while it was a
desert, or, after 1400 years moved into that desert to establish an irrigation-based
civilization. Why did these people wait 1400 years before suddenly turning to
irrigation, or why did they move into a totally inhospitable land to begin this
irrigation process? People do not move into deserts to establish a civilization. What
we have once again is evidence that the Sahara had to have dried up, not about 2300
B.C., causing a 1400-year separation from a pastoralist/agriculturist to an intensive
irrigation way of life, but that it dried out around 800 B.C. and the Garamantes
living there began to immediately adapt their environment to irrigation agriculture.
In this respect the Garamantes development correlates with the chronology of the
drying up of the Sahara about 800 B.C. and there is no 1400-year-long period of
their waiting before adapting their way of life to the new, harsh conditions.

Evidence of their far-flung trade can be seen with the transport of sub-Saharan
apes to Knossos on Crete, Akrotiri on Thera and even as far east as the Indus
Valley, all of which have been scientifically and technologically dated to the first
millennium B.C. With respect to the Minoans, A.J. Wilson points to an unusual
aspect of this trade as noted by R.F. Tylecote who

"... drew attention to the fact that in one of the frescoes in the palace of
Knossos in Crete, dated to Late Minoan times ... a species of monkey known only
in the Lake Chad area of Africa is depicted. ‘If such a monkey, or reliable
representations of them, could travel such distances at this time,’ observed
Tylecote, ‘it would not be difficult for ... travel from Nigeria’ [to the Garamantes,
thence to Crete]." 96

But in fact the trade was not only in depictions of these sub-Saharan apes but of
the apes themselves. As Ian Wilson shows:

"While casually exploring a part of Thera’s eastern coast an American called
Edward Loring turned up a piece of black rock [which] seemed to have a face on
it, like that of a tiny baby or a young animal ... A Greek professor ... gave it to
anthropological specialists [who] X-rayed it, [when] the mystery was explained. It
was the head of a monkey semi-fossilized because it was coated with andesite and
quartzite – part of the ejecta of a volcanic eruption. The genus was identified as
forming part of the sub-family Colobinae, native to the belt of tropical Africa,
extending from the west across the Congo to Ethiopia. But what was such a
monkey doing on Thera? ... Great traders that they were, the Minoans ... brought
back to Crete as pets monkeys they acquired in the course of their trading missions
[with Africa]." 97

96 A.J. Wilson, The Living Rock (Cambridge, UK 1991), p. 27
97 Ian Wilson, The Exodus Enigma (London 1985), p. 96
In this same vein, S.R. Rao unearthed a terracotta figurine of a gorilla at Lothar in the Indus Valley near the coast, a species that lived in sub-Saharan Africa.\(^{98}\)

As we proceed, these examples of various animals from all across the ancient Near East will be shown to have been traded throughout this vast region, but most significantly, like the various animals discussed in this and the preceding volumes, they did so only in the first millennium B.C. and not the second or third. In each of the cases to be discussed below, there are problems, contradictions, etc., that arise when the established chronology is employed to explain these movements via trade of domesticated animals, but none when the short chronology is employed.

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CHAPTER 2

THE CHRONOLOGY OF THE NUBIAN/25TH DYNASTY

A major aspect of this volume is to chronologically integrate various nations of the ancient Near East with one another. Instead of doing as the historians have done, using Egypt as the true historical/chronological template or gauge for cross-dating other nations, here scientific and technological facts will be employed to make these cross-references. Having correctly established the absolute chronology of one ancient nation and that of its monarchs first, one can then begin to outline the historical events that derive from it. Once these forensic foundations have been established, these will determine the historical narrative for surrounding nations with which these nations interacted. Instead of using documents and archaeological interpretive materials as has previously been done, there are now many nations that will be the gauges and anchor points with which one can organize and explicate the structure of ancient historical reality. That means we can begin to use these anchors to make forays into rewriting the history of the ancient Near East. Those critics who attack these forays—these historical revisions—without establishing scientific, technological, linguistic foundations, etc. upon which their criticisms are based will simply argue that their criticisms are valid because the established chronology is valid, or, especially, Peter James et al.’s or David Rohl’s chronologies are valid. Since direct support for these by forensic historical evidence is lacking (as will be shown below to be the case also for James et al. and Rohl), these critics will be reasoning in a circle. Their attacks lacking these foundations, they are in effect saying that they are right because they are right, which is of course merely an authoritarian conceit and of no evidential value.

To return now to the period of Egyptian history just prior to the coming of Alexander the Great, we arrive at the Persian domination of Egypt and the Nubian/25th Dynasty. In order to anchor this era one must have a scientific base upon which it can be placed. As has been demonstrated in volume II, chapter 2 of this series, there are a great many astronomical anchors that place the Neo-Assyrians in Persian times; the Persian rulers of Assyria having Neo-Assyrian vassals, any conquests of Egypt by these kings are in reality conquests by the Persians. It was thus demonstrated that the astronomical data required that all Neo-Assyrian history must be moved closer to the present by about 300 years, and therefore it follows that all their various conquests and/or military Neo-
Assyrian/Persian expeditions into Egypt must be similarly down-dated by this same approximate 300 years.

In this respect, we are not using Egyptian history/chronology as has always been the case, as the sheet anchor from which to date the surrounding nations of the Near East. Since, as has also been demonstrated in volume I of *Pillars of the Past*, the scientific and technological evidence does not support the established chronology for Egypt, one must employ the scientific anchor points available, especially astronomical data points. Since these scientific anchors are the correct, absolute ones, these will now be used to date historical events in Egypt to untangle the chronology that presently exists. By this methodology Egyptian and Mesopotamian chronology mesh, are brought into scientific/astronomical alignment, and thus represent chronological reality.

It must be pointed out that the established chronology proponents, as well as Peter James *et al.* and David Rohl, all maintain that the Nubian/25th Dynasty is properly dated in terms of secure evidence. James *et al.*, who shortened the chronology/history of Egypt by 250 years, state:

> “Within Egypt the chronology of the later 25th Dynasty kings is fairly securely established through their connections with [that of] the [Neo-]Assyrian emperors. [The Nubian king] Shabaqo reigned for fifteen years and his successor, Shebitqo, for ten or twelve. Taharqo’s [Tiharka’s] active reign of twenty-six years is well attested from buildings, inscriptions and documents from both Egypt and Nubia. The later years of his reign were disrupted by [Neo-]Assyrian invasions led by Esarhaddon … and Ashurbanipal … .”

David Rohl, the other major revisionist, in shortening the chronology/history of Egypt by about 300-350 years is even more emphatic than James *et al.* about the dating of this chronological connection between Ashurbanipal and Tiharka as well as with other materials directly connecting them:

> “The first [chronologically absolute connection] is beyond reproach as a secure date in history—and is not in dispute in this thesis. In 664 BC Ashurbanipal, king of Assyria, brought an army to Egypt and sacked the sacred city of Thebes as the punishment for a revolt by Pharaoh Taharka against the recent [Neo-]Assyrian occupation of Egypt. This date of 664 BC is supported by a whole network of interlocking data supplied by independent sources including [the statements of] Berosus, Manetho, and the [Neo-]Assyrian and Babylonian chronicles, Apis [bull] stelae and Egyptian regnal dates on the monuments. We can state without reservation that this crucial anchor point in Egyptian chronology is our first real ‘fixed’ point in history.”

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121 Peter James *et al.*, *Centuries of Darkness* (New Brunswick NJ 1993), p. 208
Kenneth A. Kitchen stated about this date:

“There is no reason to doubt the date 664 BC for the commencement of the 26th dynasty with Psamtek I, after Tantamam of Nubia had slain his (Ps I’s) father Necho I, and raised a revolt against Assyria, resulting in Assurbanipal’s fierce punitive campaign in 663. The year 664 also saw the death of … Taharqa after a full 26 years of reign, which thus began in 690 BC, a date which should now be given equal status with 664 …”

That is, James et al., who shorten the history of the ancient Near East by 250 to 200 years, and David Rohl, who shortens it by 350 to 300 years, and Kenneth A. Kitchen, who generally accepts the established chronology with few if any reservations, all maintain that the 664 B.C. date is absolute, absolutely. As an aside, Carl Olof Jonsson also claims “the chronology of the Twenty-Sixth Dynasty of Egypt is soundly and independently established. The results are summarized in the following table: CHRONOLOGY OF THE TWENTY-SIXTH DYNASTY: Psammetichus I … 664–610 B.C.E.” As will be demonstrated below, this is not the case!

The position taken here is that James et al., Rohl along with Kitchen and all the proponents of the established chronology are fundamentally in error. It is not beyond reproach as a secure date in history. This date of 664 B.C. is not supported by a whole network of interlocking data supplied by independent sources, including the statements of Berosus, and Manetho, and the Neo-Assyrian and Babylonian chronicles, Apis bull stelae and Egyptian regnal dates on the monuments. We can state without reservation that this crucial anchor point in Egyptian chronology IS NOT our first real “fixed” point in history. The reason that we can affirm this is that the astronomical evidence presented in volume II of this series categorically contradicts the established dating of Esarhaddon and Ashurbanipal, who each invaded Egypt, and upon which this 664 B.C. date is determined.

I am adding the following information on the dating of Taharqa/Tiharka at the behest of Lynn Rose who has since apprised me of this new data.

In his “Appendix,” as previously in his letter of 19 April 2007, which was quoted extensively in Chapter Two of Pillars of the Past, Volume Two, pages 242-254, Rose had dated Hunger’s Report 102 to 14 March -391. Since Report 102 mentions the king, the crown prince, and “Shamash-“, it is usually assumed that

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the king was Esarhaddon, that the crown prince was Ashurbanipal, and Shamash-shum-ukin was mentioned because it had already been decided that he would inherit Babylon.

Report 501 is dated to Year 1 of Sargon in Babylon. Since Year 1 of Sargon in Babylon is supposed to precede Year 1 of Esarhaddon by thirty years, and on the assumption that 14 March -391 did indeed lie within the reign of Esarhaddon, Rose tentatively concluded (“Appendix,” pages 623-624) that Year 1 of Sargon in Babylon might have been either -429, -428, or -426, with -426 being much more likely than either of the other two possibilities.

Subsequently, in a series of further letters, Rose modified his interpretations of Reports 102 and 501. This was necessitated by the fact that he had in the meantime discovered that the eclipse associated with the death of Psammetichus I in his Year 55 is likely to have been the total solar eclipse that passed across northern Egypt on 4 July -335. If Year 55 overlaps -335, then both Year 27 of Taharqa and Year 1 of Psammetichus I would have overlapped -389. It was Ashurbanipal, not Esarhaddon, who replaced Taharqa with Psammeticus I. This was several years or more into the reign of Ashurbanipal. That means that Report 102 on 14 March -391 fell in the reign of Ashurbanipal. Thus we must take the unnamed king in Report 102 to be Ashurbanipal, the unnamed crown prince to be a young son or other designee of Ashurbanipal, and the “Shamash-“ again to be Shamash-shum-ukin, by then already in power in Babylon.

This pushes the Sargonids back a few years. Rose now dates Report 501 to 27 March -433, which suggests that Year 1 of Sargon in Babylon would have overlapped -433. Thus Rose’s dates for the Sargonids are 274 years later than the conventional dates. It would have been in Ashurbanipal’s own Year 4 that he placed Psammetichus I on the throne of Egypt—or at least on the throne of some part of Egypt.

As I pointed out:

“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 [C.B.F. ] Walker and [Carl-Olof] Jonsson suggest.

“According to Lynn E. Rose …, 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 -3! 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!

“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 …’ Parker and Dubberstein report that this occurred by extending Kandalanu’s reign after his death one year later. That is, Kandalanu’s extended reign ended in late October. But it is also held that Alexander the Great entered Babylon in late October. That Alexander should enter Babylon at about the time that Kandalanu’s reign ends is an extraordinary fit.”

Thus, the 664 date for Ashurbanipal’s conquest of Egypt must be moved about 280 years closer to the present and the same must be done for Tiharka of Egypt. That is what this scientific evidence demands. In this respect, not only is the established chronology upheld by Kitchen invalid, but the chronologies of Peter James et al., and David Rohl as well: all the chronological connections listed by Rohl as independent confirmations and corroborations can only be valid if the astronomical/scientific data confirm and corroborate these connections, but they don’t; they completely invalidate these and therefore all those extraneous materials must be moved to fit the astronomical/scientific evidence, not the other way around.

Nevertheless, this approximate 300 year shortening of Egyptian chronology/history is just the beginning of this revision. There is an additional period of about 250 to 350-year Dark Age in Nubian history that must also be removed. Donald B. Redford describes this Dark Age thus:

“By the time of the first millennium B.C., the Southland [Nubia] had entered a ‘dark age’ … By the close of the twentieth Dynasty, if not before, the inscriptions cease in Nubian temples, and signs of an active administration are absent. The last texts at Kawa belong to the viceroy Nebmarenkht ([con-][temp[orary of] Ramesses VI and VII), at Buher … and at Napata temple service peters out under Ramesses IX. The last taxes from [Nubian] Kush are recorded late in Ramesses XI’s reign

125 Charles Ginenthal, Pillars of the Past vol. II (Forest Hills NY 2008), pp. 197-198
(year 23) … and for centuries we must suffer an almost total absence of archaeological material …”

James et al., describe the problem inherent in having this Dark Age:

“All discussions of Nubian history in the late New Kingdom and the period following have been concerned with one of the two major issues: either the collapse of the [Egyptian] viceregal administration [over Nubia], or the rise of the Kingdom of Kurru [suddenly out of nowhere, and] its conquest of Egypt and rule there as the 25th Dynasty. These have nearly always been considered as separate and unrelated phenomena. Indeed, all the writing on this period has been Egyptocentric, obsessed by those problems directly related to Egypt, and using only Egyptian-type evidence. Thus in any period where there are no large stone monuments or hieroglyphic texts historians can only envisage a hiatus, or ‘Dark Age’. At best the region is said to have ‘regressed’ to a ‘tribal level’.

“The period between the end of the viceregal administration during the last ten years of the 20th Dynasty (c. 1070 BC) and the first datable inscriptions of the 25th Dynasty in the 8th century BC has become one of the most significant gaps in Nubian history. The apparent lack of archaeological material in this Dark Age is balanced by a corresponding increase in the use of colourful language [to explain it]. The American archaeologist William Adams … characterizes the period as follows:

“Nubia vanished entirely from history. Its erstwhile Egyptian conquerors had returned to their native soil, and the indigenous population had retreated somewhere into the wilderness of Upper Nubia, whence they were to emerge with a vengeance [and conquer Egypt] three centuries later.”

To explain this further, they add:

“Having created a Dark Age in Nubia, it is not surprising that historians have treated the appearance of the Egyptianized ‘Kingdom of Kurru’ in the mid-9th century BC as a new beginning, largely unrelated to the end of the [previous] viceregal period. So firmly entrenched has this idea become that Adams was forced to make the bizarre comment that it ‘took some time for the lesson of the pharaohs to sink in.’

“Indeed, few writers considering the end of the viceregal administration and the [subsequent] rise of the Kingdom of Kurru discuss the Dark Age itself; most restrict themselves to a passing comment on the lack of evidence from this period. Accordingly, the sudden [military] expansion of Kurru power in the second half of the 8th century BC has baffled Nubian archaeologists.”

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126 Donald B. Redford, *From Slave to Pharaoh: The Black Experience of Ancient Africa* (Baltimore MD 2006), p. 58
127 James et al., op.cit., pp. 206-207
The Nubians vanish from history for over 250 years. They stop writing, building, worshipping the gods in their temples, halt paying taxes to the central government, and fail to communicate with the rest of Egypt. They became invisible in Nubia; but then 250 or more years after their vanishing act they return from out of the blue as a vibrant, literate, building nation with all the paraphernalia of an advanced Egyptian culture, militarily overwhelm Egypt and administer this realm as though they had never lost any of these capacities for over two and a half centuries. Like the Greeks with their own Dark Age around this same time, the Nubians disappear into oblivion and then return fully developed as a literate, architectural, military and administrative entity with ‘a vengeance’ to dominate Egypt.

The explanation for this efflorescence of an almost non-existent Nubian people is not only lacking, but it is not even an explanation; it is an excuse, a failure on the part of historians to come to grips with reality. As Alfred North Whitehead said, “It requires a very unusual mind to undertake the analysis of the obvious.”129 What is obvious is that a people who fail to write, to build, to tax, to grow crops and livestock, to keep an army, to maintain an administrative branch of government, do not overnight start writing, building, taxing, growing crops and livestock, maintain a military force and develop a fully functional administrative capacity.

Where there is no evidence—archaeological, literary, documents of a people, monuments, pottery, buildings, or anything else that establishes the existence of any civilization—there is no history and therefore there is no chronology. Civilized people build houses, palaces, monuments, temples, statues; they produce pottery and other artifacts, and keep records. When historians encounter such a Dark-Dark-Dark Age, they fill it with theories, assumptions, etc. But as Heinsohn contends, totally empty periods in history are totally empty because they do not exist. As we have repeatedly observed, such totally empty Dark Age periods are endemic with the established chronology, and are filled with cut-and-paste non-existent materials created in the minds of historians.

Therefore, not only must Tihaarka be moved about 300 years closer to the present, based on astronomical/scientific grounds, but in addition 250 or more years must also be removed from Nubian/Egyptian chronology because of the false Nubian Dark Age. In toto, we have reached a point where about 600 years of Nubian/Egyptian history do not exist thus far just in the first millennium B.C.

But that is not the end to the reductions in Egyptian chronology that must be made. Since the Nubian/25th Dynasty ended only sixty years prior to the conquest of Egypt by Alexander the Great, the Nubian/25th Dynasty cannot be followed in

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129 Alfred North Whitehead, in Leo Rosten’s Carnival of Wit (NY 1994), p. 413
succession by the 26th, 27th, 28th, 29th, and 30th Dynasties. If they all did indeed follow the 25th Dynasty, they would have to have reigned well into Ptolemaic times and this concept is simply not credible. Since these five dynasties did exist, they had to have been contemporary with others and possibly some with each other, or possibly as governors under the Ptolemies. Nevertheless, the overall lengths of Dynasties 26 through 30 must also be largely removed from Egyptian history/chronology. This covers an additional 200 to 250 years. When we add this 250 years to the 550 to 600 years already removed we achieve a total of about 800 years that must be taken away from Egyptian chronology in the late second and first millennium B.C. Amazingly, this 800-year reduction correlates almost exactly with Velikovsky’s 800-year reduction of Egyptian history/chronology. This will be dealt with in the next chapter.

With the Nubian Dynasty placed in the mid to latter part of the first millennium B.C., it shared this time with other dynasties, namely the 12th, the 19th, and others. This being the case, what we would expect to find is that it shared many cultural developments with these classic dynasties. Historians have explained away these close affinities by arguing that the Nubians slavishly copied these supposedly older cultures rather than that they shared these developments as part of a chronologically common culture. I.E.S. Edwards and T.G.H. James present the conventional explanation:

“A remarkable renaissance in the culture of Egypt followed the conquest of the land by the Nubian kings who established the Twenty-fifth Dynasty … The effects of this renaissance were to be felt in all branches of the fine arts, in the use of the written word, and in religion. It was strongly permeated with the archaizing tendency which evidenced itself in the studying, copying, and adaptation of ancient forms. Under the stimulus of this remarkable movement the skills of the ancient crafts, in which the Egyptians of the earlier great periods had excelled, were revived; the techniques of stone-working, in particular, reached notable heights. The artistic advances made during the Nubian dynasty continued to be maintained during the Twenty-sixth Dynasty … but the products of Egyptian artists and craftsmen in this later period lacked some of the freshness and spirit of the preceding dynasty. The spirit was in the end to be almost completely extinguished by the Persian conquest of Egypt in 525 B.C.”

But since the Nubians were contemporary with these classic dynasties and not far removed in time from them, it only stands to reason that they developed and maintained these arts, architecture, written forms etc. in common. That is, the art of

130 Immanuel Velikovsky, Peoples of the Sea (NY 1971)
the Nubians which supposedly comes 700 years after the 18th and 19th dynasties shares close affinities with them. According to Sweeney:

“The art of the Ethiopian [Nubian] age shows striking affinities with the art of the late 18th and 19th Dynasties. This is illustrated for example in the decoration of the tomb of Mentuemhet, High Priest of Amon at Thbes in the time of Tirhaka and Ashurbanipal. In 1947 ‘a fragment of limestone relief of exceptional quality’ from the tomb was purchased by the Brooklyn Museum. Ignorant of the relief’s origin, the Museum’s Egyptian Department evaluated it as a product of the late 18th Dynasty. The work contained scenes of peasant life already known from paintings of the 18th Dynasty tomb of Menna in the Theban necropolis. However, ‘only a few months later two other fragmentary reliefs were offered to the Museum’, and these were assessed by Professor Cooney of the Museum to the late 7th century [B.C.]. At first the museum did not suspect a link between the two pieces. In the words of Professor Cooney ‘I was so convinced of the early date of the relief with the peasant scenes that I failed even to consider a relationship between it and the Saite pieces.’ Yet, to the astonishment of the Museum authorities, it was subsequently discovered that all three reliefs were from the tomb of Mentuemhet.

“Because of the close similarities between the scenes in the tombs of Menna and Mentuemhet, Professor Cooney assumed that the artists of the Ethiopian epoch must have had access to the tomb of Menna and its paintings, to serve as models, after more than seven hundred years had elapsed. In Cooney’s words, ‘The lucky preservation of the Eighteenth Dynasty original, which served as a model to the Sai’te sculptor, provides an ideal chance to grasp the basic differences between the art of these periods separated by a span of almost eight centuries.’

“But the artists of the Ethiopian epoch had no need to break into ancient tombs to find inspiration for their work. Between the end of the 18th Dynasty and the beginning of the Ethiopian epoch there were not seven centuries…”

In fact, there is clear-cut epigraphic evidence that connects several dynasties at the same time as the Nubian. As Sweeney pointed out, the Nubian/25th Dynasty dominated Egypt into Saite/26th Dynasty times. And here we encounter several epigraphic forms of Egyptian from different periods supposedly separated by long periods of time in one place, all dated to one day. The 25th, Nubian Dynasty was the period when a form of Egyptian writing developed known as “demotic.” “Demotic is the further simplification of hieratic and is much more cursive and more difficult to read. It first occurred around the 25th Dynasty.”

132 Emmet Sweeney, Ramessides, Medes, and Persians (NY 2005), pp. 66-67
133 Geoffrey W. Bromley, The International Standard Bible Encyclopedia (Grand Rapids, MI 1988), p. 34
However, as was pointed out in vol. II, Eugene Cruz-Uribe stated that there is “… 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.”134

This means that in Nubian times, scribes were writing in scripts that go back to the Late New Kingdom, that is the 18th and 19th Dynasties, as well as in a form of script developed around Nubian times. They obviously did not go back to some tomb to copy these scripts since they were being used in legal documents. To suggest these were copied from the past would be to suggest that people today wrote in the script used in Chaucer’s time and at other times between then and now for legal matters. Again the obvious conclusion is that these cultures/dynasties were either contemporary or closely contemporary and that the various scripts were all being used for legal purposes at around the same time. On the scientific/astronomical level and the artistic/architectural level as well as on the epigraphic level the Nubians and the New Kingdom must be brought closer to the present, and the earlier ones must be brought closer by as much as 800 years, just as Velikovsky suggested. To deny this fact is to deny scientific/astronomical evidence which is fundamental to the issue. To deny this fact is to deny both scientific/astronomical and epigraphic evidence and well-understood artistic/architectural evidence. On all these levels the established chronology as well as the chronologies of Peter James et al., and David Rohl fail. On all these levels the short chronology of Heinsohn, Rose, Sweeney, and, in great measure, Velikovsky are substantiated and validated.

Martin Bernal has written “Velikovsky’s … drastic chronology [has] rightly been discredited.”135 But, as will be shown below, Velikovsky’s drastic historical revision in many, many cases fits into the short chronology and is vindicated because it is supported by forensic historical evidence.

135 Martin Bernal, Black Athena Writes Back. Martin Bernal responds to Critics (Durham NC 2001), p. 335
CHAPTER 3

IDENTITY AND CHRONOLOGY OF THE PEOPLES OF THE SEA

As was shown in volumes I and II of this series, whenever the historians have created a civilization that does not exist or is misdated, they give it stratigraphic materials that belong to another culture well known to ancient peoples. The culture deprived of these materials disappears. When the Sumerians, who are the Chaldeans, were dug up and their relics were labeled Sumerian, the Chaldeans vanished from the archaeological record. When the Mitanni, who are the Medes, were dug up and the relics were labeled Mitanni, the Medes vanished from the archaeological record. When the Old Babylonians, who are the Persian overlords of Persia, were dug up and their relics labeled Old-Babylonian, the Persians practically vanished from the archaeological record, etc. In addition, because of these false identifications, a whole slew of problems arose about these newly discovered civilizations: their homelands could not be archaeologically found, their original language could not be determined. Their original religion could not be found, etc. On top of all this, their conquests of the lands they took over happened in Dark Ages and their fall from power happened in other Dark Ages. They came upon the historic scene out of nowhere and vanished into that same nowhere. Their technological developments such as having iron, tin, etc., are either millennia or centuries too early, and these many problems for the past two centuries have stubbornly failed to be resolved by new advanced research. All this, of course, has been based on historians adopting an overly lengthy chronology which historians were forced to fill with a great number of ad hoc explanations that are fundamentally excuses to paper over the gaps and unresolved problems in that chronology. The entire process has led to a history and chronology that are deeply dysfunctional and will remain dysfunctional as long as historians, archaeologists, philologists, etc., try to stretch about 900 years of history into a 3000-year framework.

In terms of the short chronology, two major nations conquered or attempted to conquer Egypt in the first millennium B.C. prior to Alexander the Great: the Old Assyrians/Akkadians/Hyksos, who will be dealt with in Chapter 4, and the Pereset and Sea Peoples, whom we will now examine. The Pereset and Sea Peoples are assumed to have played a major role that supposedly occurred in Egypt and much of the Near East around 1200 B.C. These never before known marauders invade
out of the blue, and are identified and referred to as the Sea Peoples. They arrive on the historical scene from some unknown land or lands, wreak havoc and total destruction upon numerous civilizations, are finally repulsed by the Egyptians, and vanish from history. According to Velikovsky, these people, the Pereset, were the Persians and the Sea Peoples were the Greeks whom he dates to around 390 B.C. With the 800-year shortening of Egyptian history as described above, the 1200 B.C. date of this invasion of Egypt by the Sea Peoples occurs around 400 B.C. Velikovsky summarizes the evidence thus:

“The scheme of things as offered in every book on ancient history presents the beginning of the twelfth century before the current era as a time of great convulsions in the life of nations around the eastern Mediterranean, the region usually known as the Ancient East. This scheme has it that out of the gloom of the north hordes of peoples swept over the lands of ancient civilization and in each of them left in ruins everything that had been rich and glorious; order was changed to chaos, abundance turned to want, and destitute populations, leaving behind them the glory of their own past, followed the crest of migration and transgressed frontiers of other nations. The Mycenaean culture, which was centered around Mycenae in Greece and which embraced the Aegean isles, came to an end [followed by a 500-year Dark Age]; the Trojan War is regarded by many scholars as one of the terminal effects of the vast dislocation caused by the migratory waves that uprooted populations of whole lands.

“This scheme also has it that soon after the Trojan War, in a matter of only a few years, armed hordes, sufficiently organized to be called armies, reached Egypt, which was ruled at that time by Ramses III [of the 20th Dynasty]. …

“According to the reckoning of modern historians, Ramses III started to reign in the year –1200 before the present era, or only a short time later. The major event of his reign was the successful opposition to the [Sea Peoples’] armies coming from the north. In their sweep of conquest, the northern hordes came to the very gates of Egypt … But Ramses III rose to the occasion. He battled the invaders on land and sea and turned back the tide that threatened to envelop Egypt.

“… But of the sweep of the invading troops across the lands of the Near East before their arrival at the frontier of Egypt nothing is known from any historical source, literary or archaeological. It is only by inference that the conclusion is made: Mycenaean Greece, the Hittite Empire, and many lesser kingdoms were swept out of existence by the wandering and conquering Peoples of the Sea. This inference is made on the basis of the fact that all these kingdoms and empires were found to have been terminated in about –1200. For the next four or five centuries there is no record and no relic of their existence and scarcely any vestige of the surviving population in these lands. …

“The centuries from –1200 to –750 are called Dark Ages. They were not dark in the sense in which this term is applied to the period of European history
between the end of the Roman Empire in +475 and the end of the Crusaders’ wars in the East: these centuries from the end of the fifth to the middle of the thirteenth of the current era represent a regression in learning, in commerce, in administration and law, when compared with the time of the Roman Empire, but they abound in historical relics and literary testimonies; whereas the Dark Ages between –1200 and –750 before the present era are dark because no document survived from that time in Greece, in Crete, in the Aegean world, or in Asia Minor.”

The only reference to the Sea Peoples is in the Medinet Habu inscriptions and pictorial reliefs of Ramses III in Egypt. Outside of this single reference there is not another that mentions the Sea Peoples. What historians have done to fill in this void is interpret any mention of a threat to these other cultures as being that of the Sea Peoples. While the Sea Peoples rampaged across this part of the Near East, they left nothing of themselves in the way of relics, or anything else. Thus the historians self-validate their belief in the Sea Peoples’ existence and all they supposedly did is expanding the reference of Ramses III and saying, without anything else to support their contention, that the Sea Peoples were a horde that did what they claim for them. Their thesis is a massively circular rationalization. For example, how do we know what caused all these peoples to suddenly invade these regions? The migration hypothesis, like everything else about them, is, as Robert Drews claims, “a rash conjecture”:

“… the migrations hypothesis, sprung from a rash conjecture made by [Gaston] Maspero in 1873, was by the 1920s no longer a hypothesis but a generally accepted historical fact. Subscribed to by archaeologists, historians, Old Testament scholars, and even Egyptologists, it seemed a self-evident ‘reading’ of the Egyptian inscriptions, and few remember that until Maspero read his folk migrations between the lines, the inscription made perfect sense as a record of battles with… invaders aided by warriors recruited from all the northern lands.”

Eric H. Cline and David O’Connor, in speaking of the great destruction wrought by the Sea Peoples, admit:

“This dramatic historical account [of the Sea Peoples], based almost on a handful of Egyptian inscriptions, provides an explanation for major change in the archaeological record, [yet] the Sea Peoples remain curiously difficult to identify. As a result, the Sea Peoples continue to perplex and mystify historians and archaeologists … The Sea Peoples, on present evidence, seem to suddenly come from nowhere, cause widespread disruption, take on some of the greatest powers

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1 Immanuel Velikovsky, *Peoples of the Sea* (NY 1977), pp. 3-5
of the region, and equally abruptly disappear from history save for one or two historic peoples [who historians conjecture were Sea Peoples] of later times."

What the names of these peoples were is likewise a major unresolved issue since the only reference which names them is Ramses III’s inscription. Cline and O’Connor explain the depth of the problem:

“The Egyptian sources provide the names of at least nine Sea Peoples. With those names the problems of the historians begin, for the Egyptian scripts record only consonants [without vowels], whereas the contemporary cuneiform scripts of Mesopotamia records syllables; from the start, it is difficult to be sure that a name in one script [Egyptian] corresponds to a name in another [Mesopotamian cuneiform] [and] there are often uncertainties in reading, particularly for foreign names. [Egyptian] New Kingdom writing introduces a selection of signs in combinations that appear to echo the syllabic structure of cuneiform, but there remains extensive debate over the vocalization of each name … The nine names attested are, in the text of Ramesses III … the Peleset, Tjekru, Shekelesh, Danuna or Da’anu, and Washosh, and, in the other sources, the Eqwosh, Lukki, Shardana and Teresh. Given the problems in reconstructing the vowels between the consonants in the Egyptian writings, it is not surprising that these names usually have not been identified yet with specific regions, whether those from which the Sea Peoples [supposedly] originated, or those in which they [supposedly] resettled themselves after the events described during the reign of Ramesses III. The Sea Peoples are bound to remain elusive, as long as they exist only as names rendered in Egyptian scripts. Identifications with names in other scripts and languages may help, though they grow more speculative the greater the gap in time; without corroboration in the material unearthed in secure archaeological contexts, it is difficult to know whether a name is even intended to denote a separable ‘people’ rather than a less distinct part of a larger movement … The questions remain: who is giving which names, and on what grounds, to whom in this Egyptian reflection of history? Equally mysterious are the cultures and organizations of the Sea Peoples, and the degree of their diversity when at least nine different ‘ethnic’ names were involved. Although the Sea Peoples are depicted in Egyptian art, and perhaps appear in sources from various parts of the Levant and the Aegean, and although Egyptian texts provide some glimpses of the Sea Peoples’ political and military structure, and of their material culture, all in all the data remains meagre.”

As for the dates of their conquests, this is another unanswered problem. Michael Mann admits even the dates attributed to these are dubious. “To make sense of all this [Sea Peoples’ conquests], the exact dates matter a great deal. In

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4 Ibid., p. 109
what order did Troy, Mycenae, Boghazköi, and Babylonia fall? We do not know. With only [supposed] exact chronology we are left floundering.”

According to David Noel Freedman et al., “The Sea People were not just engaging in a foray into Egypt, but were looking for a place to settle.”

But if these people were looking for another homeland, why didn’t they settle down in Anatolia or Syria, regions that were large enough to accommodate them? This, it is well-known, did not occur. In fact, Trevor Bryce suggests that

“... theories of dramatic invasions by bloodthirsty northern barbarians against long-established sophisticated civilizations are no longer fashionable in Bronze Age scholarship. We need to reexamine very carefully the meagre information available to us before drawing any firm conclusions as to who the Sea People were and what role they played in the final decades of the Bronze Age.”

Not only are the names, homeland, dates and reasons for the Sea Peoples’ movements unknown from Ramses III’s inscription, the inscription itself is questionable according to Donald B. Redford:

“The text is anything but a sober, fact-filled record. The speaker (Ramesses III?) couches a smattering of detail in a metrical structure larded with high-flown figures of speech, and clearly strives for rhetorical effect rather than reasoned argument. A superficial reading which takes metaphor, simile and metonymy at face value can only result in egregious error in attempting to reconstruct the event … Though steps have been taken in this direction … a more rigorous textual and historical criticism awaits application to the Medinet Habu [Ramses III’s] reliefs. For example: to what extent can we trust the representations of the Sea Peoples? … why does the text of year 8 [of the battle] lack calendrics [names of seasons, months, and days] while other historical texts show them? Does the Medinet Habu text telescope events? How precise a chronology is it possible to build?

“What has been said is borne out by the state of present research: problems of interpretation abound not only at the level of minutiae [tiny details], but also in the heady expanse of the overview. The political historian might well opine that the rush to [any] conclusion is ‘jumping the gun,’ that the evidence is not yet all in.”

In fact, in terms of the battle, Redford shows “The written record fails to note the site of the final encounter between the Sea Peoples and the Egyptian forces.”

The depths of this problem echo throughout the literature. Michael Wood asks the

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6 *Eerdmans Dictionary of the Bible*, David Noel Freedman et al., eds. (Grand Rapids MI 2000), p. 1176
question that Velikovsky answered decades earlier: “Were the Sea Peoples in part composed of Aegean warriors? It seems possible, even likely, but at present these events are shrouded in mystery.”

Before proceeding, let us briefly recapitulate the intractable problems which have remained unresolved with regard to the Sea Peoples. They are a migratory people based on a “rash conjecture made by Maspero in 1873” which became historical gospel ever since. Yet the migratory theory in force for over 125 years is no longer “fashionable in Bronze Age scholarship.” Thus there is no explanation for their movements except that they moved to conquer because they were looking for a homeland but failed to settle in those they supposedly first encountered which were large enough to accommodate them. The only historical evidence of them comes from the Medinet Habu inscription reliefs of Ramses III, but from nowhere else, and they are totally absent in the archaeology of the lands they passed through. Therefore they are “curiously difficult to identify,” and “continue to perplex and mystify historians and archaeologists.” The names of the nine peoples listed by Ramses III are filled with problems, as Cline and O’Connor noted, as well as their origin. They “suddenly come from nowhere … and equally abruptly disappear from history.” The historical approach regarding practically every aspect of the Sea Peoples employed by modern historians has led to dead ends and indicates the entire approach as well as the chronology attached to resolving these enigmas is dysfunctional, just as it was in so many other cases outlined in these volumes. Redford brings forth the rational approach that seems necessary for unraveling this panoply of problems:

“The Sea Peoples as a topic spans so many disciplines that it defies the conventional training of a modern student. Yet it is precisely the multidisciplinary approach that will bear fruit. Nearly twenty years ago the author argued … for a sort of Wagnerian Gesamtkunstwerk [total work of art] … in which every conceivable technique would be brought to bear upon an intractable and multifaceted problem… ; and to the extent that it fits such a model, the Sea People too are ready for this kind of concerted attack.”

Nevertheless, because of the historians’ attachment to the established chronology that “concerted attack” has not born fruit. Since Redford wrote this in the year 2000 and Cline and O’Connor in 2003, Trevor Bryce in 2005 as cited above gives the lie to any assertion that any real progress has been made in penetrating the wall that stands in the way of understanding and solving the problem of the Sea Peoples. Yet, what Redford suggested is precisely the approach that was taken by Velikovsky and will be taken up here. It will be demonstrated that there are several forms of

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11 Redford, op.cit., p. 7
scientific and technological evidence as well as other forms that, when applied to this multi-faceted problem in a concerted frontal attack on the Sea Peoples’ identity and chronological place in ancient history, will bear clear-cut truths related to this intractable problem. Every conceivable technique, field of study and interdisciplinary method of examination are what is needed for the resolution of the Sea Peoples’ identity and their chronological place in history.

**PHILOLOGY AND LINGUISTICS**

If Ramses III of the 20th Dynasty lived in the 12th century B.C., he could not have known of the Greeks nor have used Greek letters. But if he lived in the 4th century B.C., this certainly could have been the case. As Velikovsky pointed out in *Peoples of the Sea*, pages 6 through 12, that is precisely the case. The Greek alphabet did not exist in the 12th century B.C. Velikovsky states:

“The problem of the Greek letters on the tiles of Ramses III [at Tell el-Yahudiya] cannot be solved even by assuming that the Greek alphabet derived from the Phoenician originated not in the seventh, eighth, or ninth century but a number of centuries earlier. What really matters is the fact that the Greek letters on the Egyptian tiles do not look like early Greek letters of the seventh century but like the classical letters of the age of Plato.

“Judging by these letters, the tiles must have been made in one of the later centuries before the present era. The peculiar form of the *alpha* was introduced only then; and the forms of some other letters also indicate that they are of a late century. Thus *sigma* was designed Δ and not Σ. Following these obvious facts, scholars at first felt sure that the tiles had been made in the last century of the Late Kingdom (the fourth century before the Christian Era), possibly even during the period of Greek rule there after Alexander the Great, under the Ptolemies.

“The [Emil Brugsch wrote] ‘The Greek letters, and especially alpha, found on the fragments and disks leave no room for doubt [ne laissent aucun doute] that the work was executed during the last centuries of the Egyptian empire and probably in the time of the Ptolemies …’”

Critics of Velikovsky’s chronology regarding these late capital alphabetic Greek letters on tiles that are clearly from Ramses III’s time have simply ignored this evidence as though it did not exist.

Let us now examine other linguistic materials from the hieroglyphic inscriptions of Ramses III at Medinet Habu. Again, based on the established chronology, we would expect the linguistic evidence of the texts to reflect Egyptian language of the 12th century B.C. and certainly not the language of a time much closer to the present.

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12 Velikovsky, *op.cit.*, pp. 7-8
Based on the chronology of Heinsohn, Sweeney, and Rose, we would expect the Egyptian language to display anachronisms that reflected it linguistically being far closer to the present. By the 4th century B.C., the Egyptians had developed three shorthand scripts: hieratic, abnormal hieratic, and demotic. This being the case, the ability to write in the older formal signs would to some extent have deteriorated. Furthermore, at this later time Egypt had a large population of both Hebrews and Syrians that would also carry their influences into the Egyptian language, as is generally the case when large numbers of foreigners come to any land; for example the French Normans influenced the English language after their conquest of England. As in such cases, foreign loan-words would have infiltrated the Egyptian language and become part of the vernacular. Again, Velikovsky informs us:

“For over two hundred years after the fall of Jerusalem [in the 600s B.C.] there existed a Jewish colony in Egypt. The influence of the Hebrew-Syrian language, conspicuous in the sixth century, must have become prominent in the time of Ramses III. This is indeed the case. In many instances Semitic words displaced Egyptian words, and the scribes of Ramses III ‘often abandoned a perfectly good Egyptian word’ in favor of a Hebrew equivalent. In the inscriptions of Medinet Habu—to take one example at random—the Semitic word barekh, ‘to bless,’ is used instead of the corresponding Egyptian word. [according to J.H. Bondi, *Dem hebräisch-phönizischen Sprachzweige angehörige Lehnwörter in hieroglyphischen und hieratischen Texten* (Leipzig, 1886)]

“‘The Medinet Habu texts are extreme in their choice of words. They exhibit a straining after the unusual word or phrase. ... They take an especial relish in employing foreign words, borrowed usually from the Semitic tongues. ... That Semitic words should be so profusely present in Medinet Habu points to cultural interrelations on a very brisk scale throughout the ancient Near East.’ [according to J.A. Wilson, “The Language of the Historical Texts Commemorating Ramses III,” *Medinet Habu Studies, 1928-29* (Chicago, 1930), p. 32]

“This characteristic is understandable when the influence of the Jewish colony in Egypt is taken into account; it becomes embarrassing, however, if Ramses III was a contemporary of the Judges [in the Bible] ... since in the entire Book of Judges there is no mention of any contact with Egypt.”

Velikovsky further stresses that the literary style of the Medinet Habu inscriptions of Ramses III does not “exhibit a close relationship with that of the two dynasties [18th and 19th] preceding it on the conventional timetable.” He cites J.A. Wilson’s *Medinet Habu Studies 1928-29*, pages 24ff, pointing to the

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13 Ibid., pp. 69-70
14 Ibid., p. 70
great dissimilarity between these earlier scripts, supposedly prior to the 12th century B.C. and that of Ramses III’s inscriptions:

“A cultured Egyptian scribe of the twelfth century B.C., well versed in the classics of his literature, might have bewailed the degenerate style of the temple scribes of his day. … he would shudder at the florid bombast with which Ramses III choked his records. … He would be oppressed by the straining artificiality evidenced by a profusion of foreign words and far-fetched metaphor. Remembering, if he could, the more rigid rules of grammar which defended the purity of the classical literature, he would feel a lofty pity for these scribes who labored to employ the old grammar but whose efforts were defeated by ignorance, haste, and the sheer weight of the spoken language.’

“… ‘The temple compositions of his day’—as judged by the Medinet Habu texts—‘are turgid, careless, and grammatically irregular.’ Besides, they are ‘stupidly pompous.’ …

“As to the grammar, the scribe of Ramses III ‘was groping after a style which had passed out of general use.’ …

“On the paleographic side, ‘the cutting of signs is coarse and careless. … Evidence of haste is universal.’ The scribes who prepared the outlines [of the hieroglyphs] for the stone engravers were clearly more familiar with the hieratic signs, generally used on papyri but not on stone, than with the hieroglyphs and thus disfigured the latter. Their hieroglyphs ‘entirely lost sight of the genesis of individual signs.’” 

Douglas J. Brewer and Emily Teeter in this respect show:

“Middle Egyptian is considered to be the ‘classical’ stage of the language, and it was employed for literary compositions long after it was replaced by Late Egyptian and even demotic. By the late period [the time of Ramses III] it it was clearly anachronistic and would have sounded even more archaic than Chaucer’s English sounds to modern English speakers.”

That is, the language employed on the walls of Medinet Habu was a language barely accessible to the scribes of Ramses III’s day. Therefore, they used it so coarsely and in such a rough manner.

Finally we come to the hieroglyphic signs at Medinet Habu as these relate to the “Pereset” as Persians or as “Peleset,” which the historians translate as Philistines. On this point Velikovsky, who interprets the word to mean “Persian,” explains:

“The Pereset were obviously not Philistines but Persians. This interpretation of the name is subject to control. For almost two hundred years Egypt was under Persian domination—or warred against it—and it should not be difficult to find by what name the Egyptian texts of the Persian and Ptolemaic periods refer to Persia and the Persians.

15 Ibid., pp. 71-72
16 Douglas J. Brewer, Emily Teeter, Egypt and the Egyptians (Cambridge UK, 2004), p. 115
“In the hieroglyphic texts of the Persian epoch between -525 and ca. -390 when Egypt won temporary independence, there are a number of references to Persia—it is always called P-r-s (in Hebrew Persia is also called P-r-s, or Paras); the name, as usual in Egyptian writing, is supplied with the sign ‘foreign land.’

“Under the third Ptolemy, in -238, a priestly conclave made a decree and had it cut in stone … A reference is found in the decree to the Persians as a nation and, significantly for our thesis, it is written P-r-s-tt. …

“The Canopus Decree is written in three scripts on a slab of stone, in Greek, in demotic Egyptian (cursive) and in hieroglyphics. The Greek text reads: ‘And the sacred images which have been carried off from the country by the Persians, the King, having made an expedition outside Egypt, brought them back safely unto Egypt…’ [according to E.A. Wallis Budge, The Rosetta Stone in the British Museum (London 1924), Appendix: “The Decree of Canopus”, pp. 256, 283]

“If there could be a doubt as to the meaning of Pereset, the Greek text of the Canopus Decree dissipates it.”17

In terms of epigraphy, at the time of Ramses III there were classical Greek letters on the back of tiles, which date not to the 12th century B.C. but to the 5th or 4th century B.C. In terms of loan words from Hebrew and Syrian in the Medinet Habu inscriptions, these can only be attributed to late Egyptian times and not to the 12th century B.C. In terms of the literary expressions in these inscriptions, they reflect an ignorance of classic Egyptian from the 18th and 19th Dynasties that supposedly directly preceded Ramses III’s 20th Dynasty and are imitated poorly and crudely as if these dynasties did not closely precede them or were isolated from them. In terms of the leaders of the Sea Peoples—the P-r-s-tt—the hieroglyphs of the Medinet Habu inscriptions are identical to the Canopus Decree of Ptolemaic times and both mean Persia or Persian. On all the levels of philology, epigraphy, linguistics, and literary style, Ramses III lived in the 4th century B.C. and was warring with Sea Peoples whose leaders were Persian.

The final piece of linguistic evidence clearly and directly links the Sea Peoples to the Greeks. In volume I of Pillars of the Past, page 173 refers to the Cambridge Ancient History, vol. I pt. 2 (1971), pp. 180-181, where W. Stevenson Smith discussed the fact that the Egyptian word “Haunebut” refers to the Greeks of the Aegean. The fact of the matter is that James Breasted stated that the “Haunebu (peoples of the distant North in the Mediterranean) [were] conquered by … Ramses III …”18

Along these same lines, Hanswilhelm Haefs writes:

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17 Velikovsky, op.cit., pp. 34-35
“In der Inschrift aus dem Neuen Reich … heißt es ‘Alle Länder der Haunebu, der Fremdländer des großen Ozeans.’ In den Texten von Medinet Habu unter Ramses III … werden ‘die Haunebu, von denen der Bernstein kommt’ … genannt.”

This translates to: “The inscription of the New Kingdom speaks of ‘all the lands of the Haunebu, the foreign lands of the great ocean.’ In the texts of Medinet Habu under Ramses III we find ‘the Haunebu from whom comes amber’ …” We are told directly that the Sea Peoples were “Haunebu” or “Haunebut” which means Greek or Greeks. In this respect, Sweeney informs us: “The Sea Peoples of Ramses III are also described as Haunebut, a term well attested from Ptolemaic times as referring to the Greeks.”

The linguistic, epigraphic evidence names the Pereset–P-r-s–Persians and the other foreigners of the sea or Sea Peoples “haunebu” which refers to the Greeks. What historians have done is say that the identical words P-r-s-tt on the Canopus Decree and the Ramses III inscription have two different meanings. In the Decree it means Persians, in Ramses III’s inscription it means Philistines. They also say that the words “Haunebu” or “Haunebut” in both Ptolemaic inscriptions and those of Ramses III have two different meanings and identifications. In Ptolemaic inscriptions it means Greece or Greeks, but in those of Ramses III it does not mean Greeks. In other words, the very same words referring to the Persians and Greeks mean whatever the historians want them to mean.

This interpretation is made explicit by A.T. Olmstead who admits “among the chiefs of the Haunebu … feared men of the sea but now disdainfully assigned to their former allies, the Greeks.”

**IRON WEAPONS**

One of the great advantages that the Sea Peoples enjoyed was that they possessed iron weaponry. This fact echoes throughout the literature. Here are a few such citations: Military historian Richard A. Gabriel speaks of “the Sea Peoples with their straight iron swords …” Thomas D. McGonigle and James F. Quigley say “the ‘Sea Peoples’ … probably came from the north and east of Anatolia because they had developed iron weapons. The bronze weapons of the Egyptians

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20 Emmet Sweeney, *Ramessides, Medes and Persians* (NY, 2000), p. 8
and the Kassites were no match for them.”

I.M. Diakonoff tells us of the Sea Peoples “They brought with them … iron technology and iron weapons.”

Here, then, is the technological problem: the Sea Peoples had to conquer and then obtain their knowledge of making iron which makes superior weapons from the very people that they conquered—the Hittites—and apparently overwhelmed with inferior copper or bronze weapons. They then immediately learned all the processes of manufacturing iron, and mining many tons of iron ore from unknown mines to cast thousands upon thousands of iron swords, shields, and armor, almost overnight, as it were, burst upon the rest of the eastern Mediterranean world and destroyed it. But in terms of historical reality, the Sea Peoples are the only ones in that part of the world who ever did such a thing.

As was pointed out in volume II, pages 295-302, the Neo-Assyrians did not develop the ability to mine and process iron ore and manufacture sufficient iron to achieve a full-fledged Iron Age quickly, as Radomir Pleiner and Judith Bjorkman show on page 293 of that volume:

“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.”

The Sea Peoples, in an instant of time, learned and developed the methods for mining iron ore, building furnaces to process it, and manufacture thousands of military implements of this metal around 1200 B.C. Yet the Assyrians took 700 years or so to accomplish the same thing. Were the Neo-Assyrians slow learners compared to the semi-barbaric Sea Peoples? But this great time-lag not only applies to the Neo-Assyrians; it also applies to other peoples. As cited in volume I, pages 222-223, Robert Drews reports:

“Anthony Snodgrass’ [Early Greek Armour and Weapons from the Bronze Age to 600 B.C., (Edinburgh 1964)] survey of early Greek weaponry showed that few iron weapons were used in twelfth century Greece. Although areas to the east may have been slightly in advance of Greece in iron working, in the twelfth century iron weapons were also rare in the Near East. According to the tabulations of Jane Waldbaum, of all the weapons and armor found in twelfth century contexts in the eastern Mediterranean, only a little more than three percent were iron while over 96 percent of them are bronze (for the eleventh century the proportions are 80

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percent bronze and 20 percent iron, and for the tenth century 46 percent bronze and 54 percent iron.”

Again, the Sea Peoples develop a total capacity to make iron weapons in the 12th century B.C. while the Greeks and other advanced peoples of the Near-East take several centuries to learn how to mine and process iron ore and forge utensils, weapons, armor, etc., i.e. to do the very same thing. On page 223 of volume I we cited Garland and Bannister that “Egypt was the last country in the Near East to enter the iron age.” And why, after the Egyptians knew of iron, did it take them several hundred years to accomplish what the Sea Peoples did in such a short time? Were the Egyptians also very slow learners compared to the Sea Peoples?

What historians are putting forth is simply assumptions. They say that the Sea Peoples with bronze weapons conquered the Hittites who had iron weapons. They assume the Sea Peoples in a blink of historical time achieved a mastery of iron production that took all the advanced societies of the eastern Mediterranean hundreds of years to achieve. However, they never admit to these problems or explain them. They assume everything they need to prove, and explain these developments and events on this assumption, and then treat these assumed explanations as historical fact. They have given us no proofs that these assumed explanations (or, as I suggest, excuses) are realities, and build from there.

However, when we move these events related to the Sea Peoples to the 4th century B.C., the fact that they have weapons of iron and armor in massive amounts is not an assumed explanation but a historical reality. By the 4th century B.C., iron weapons and armor were rather commonplace. Both the Greeks and Persians of the 4th century are well understood to have had iron weapons and armor. Again the established chronology exhibits its dysfunctional nature. Historians also assume that the Philistines prove their case regarding iron production at that early time but as we will see, this too is based only on assumptions contrary to the evidence.

**STRATIGRAPHY**

As readers of volume I should recall, the excavations carried out at Tall Munbaqa by geologist Ulrike Rösner proved that the 700-800 year settlement gap placed there by archaeologists and historians did not exist. What the archaeologists did was simply to write the word “hiatus” or other such word or words to claim the settlement gap existed without determining whether there were aeolic/wind-blown materials separating the cultures above and below the gap. Ramses III reigned over a city built in the Nile Delta, today’s Tell el-Dab’a, also known by its Greek name of Avaris or that of its close neighbor, Piramesse, built by Ramses II. Here, too, we
have a comparable situation to Tall Munbaqa because, as we will see, the archaeologist who excavated there put a “hiatus” in the strata; in doing so, he correlated this settlement gap with the established chronology without showing intervening wind-blown materials between the two cultures.

This stratigraphic evidence was found by Manfred Bietak and his team who have been excavating this site for decades. Although other regions of Egypt rarely if ever show Egyptian dynastic chronology such that many dynasties’ relics lie in strata above or below the other in such a manner that they give a picture of which ones come first, second, third and so on for centuries, Avaris in Egypt is therefore unique in this respect. It is located in the eastern Nile Delta, which is not desert, but was a somewhat swampy delta region. Buildings, walls, temples in such a setting will in time be covered by soil or wind-blown sand from the nearby deserts. Grasses, bushes, trees, reeds will grow over any abandoned city and leave a layer of material in the streets and abandoned houses that will become deeper over time. Bietak explains that “Tell el-Dab’a is the largest and most impressive of all sites, and by its fine stratigraphic series and abundant excavated materials the most representative [of the chronology of Egypt].”

Therefore, if Ramses III of the 20th Dynasty lived in the 12th century B.C., the stratigraphy there should exhibit this by showing Ramses III’s relics and stratum separated from the Ptolemaic strata by either intervening dynasties or, if abandoned, by about 700-800 years of materials laid down between the two cultures. In this instance the situation is quite similar to that at Tall Munbaqa with its 700-800 year hiatus. However, if Ramses III’s stratum is found beneath Ptolemaic strata with neither several dynasties’ relics above it nor a deep layer of material separating the two, this will be clear evidence that Ramses III lived less than a century before the Ptolemies, around 390 B.C., where Velikovsky placed him. One cannot presume that if Ramses III’s 20th Dynasty stratum was abandoned for 700-800 years there would not exist a deep layer of materials to indicate this fact, especially as this city was on the flood plain. That Nile floods for 700-800 years would have failed to deposit silt over the site, that vegetation also failed to grow and decay there, leaving dead grasses, bushes, leaves, trees, and even here and there the remains of wild animals, is clearly impossible.

The fact of the matter is that Ramses III’s 20th Dynasty materials are located directly beneath Ptolemaic strata with no evidence reported by anyone of such a deep layer of material existing to separate them. As with Tall Munbaqa, the word “hiatus” is simply inserted between the Ptolemies and the 20th Dynasty. Bietak states of the

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topmost two strata at Piramesse that the topmost is Ptolemaic, labeled stratum “A”, and directly beneath it stratum “B” is that of Ramses III’s 20th Dynasty:

“The stratigraphy of Tell el-Dab’a can be presented for the time being as follows:

“Stratum A: Early Ptolemaic settlement 3rd century BC, limited area.

“hiatus

“Stratum B: Settlement, large temple, stores, fortification (?) from the end of the 18th dynasty to the 20th [Ramses III] dynasty (+ 1310-1080 BC) and scanty evidence of 21st dynasty [materials].”

That is, the 20th Dynasty came less than a century before the Ptolemaic one and therefore in that very short time the 21st Dynasty and 22nd would have left some relics here. This of course had to be explained away and here is that explanation presented by John Van Seters:

“The city of Piramesse was largely abandoned at the end of the 20th Dynasty. Probably because of the silting-up of the watercourse on which it was located [for trade] and the shift of the marine traffic to a new watercourse through Tanis. It was at this time that Tanis became the new capital of the 21st Dynasty. Piramesse became a quarry for valuable stone blocks and monuments to be used at Tanis and other sites, especially Bubastis. Yet the name and remembrance of Piramesse did not entirely disappear. It appears in a list of place names of the 21st Dynasty date, along with Tanis. Under Sheshonq I (Shishak) of the 22nd the city of Piramesse seems to have had a brief revival …”

That is, for 600 years after Sheshonq I, Piramesse was abandoned and even over this lengthy period an appreciable layer of debris should have accumulated between the Ptolemies and these dynasties. No such report, as far as I have been able to research, has been put forward to contradict Velikovsky’s placement of Ramses III in 390 B.C.

When we come to Tanis, there is another unusual stratigraphic form of evidence that supports Velikovsky’s thesis. Velikovsky maintains that the 21st Dynasty, whose capital was at Tanis, not only followed Ramses III’s 20th Dynasty, but did so into Ptolemaic times. Velikovsky discusses a king from the 21st Dynasty at Tanis named Psusennes and one from the 22nd Dynasty named Osorkon II whom he dates prior to the 21st Dynasty. Velikovsky thus dates the 22nd Dynasty prior to the 21st, and the 21st Dynasty during Ptolemaic times. He shows:

“Psusennes [of the 21st Dynasty] built an enclave [at Tanis] … surrounded by a massive wall of bricks. The temple area was explored by Pierre Montet; the identity of the builder of the enclave was immediately obvious to him: in the

26 ibid.
northeast corner of it there was a foundation bearing the name of Psusennes; that name was also on many bricks of the walls of the enclosure. [according to P. Montet, Tanis (Paris 1942), pp. 43, 55-6]

“In a corner between the temple and the brick wall Montet discovered the tomb of the same priest-prince. But instead of being strengthened in his first expressed view that the enclave was erected by Psusennes, Montet found himself obliged to revoke it: ‘This view expressed in our recent publications is not correct. Now we know that the great temple in its final form dates from a much later date because under the northeastern and southwestern corners we have found deposits of Osorkon II [874-850 B.C.] and in the southeastern corner a deposit of Nectanebo I [Nekht-nebef] [380-362 B.C.].’

“Of course, a pharaoh of the eleventh century before the present era [Psusennes] could not have built on foundations from under which comes a deposit made by a king of the ninth or eighth century [Osorkon II]; he could not continue a building started by a king of the fourth century [Nectanebo I]—unless he himself is counted as an eleventh-century king only through error and actually belongs in the fourth century at the earliest. This thought did not occur to Montet but the problem was not resolved by a mere revocation of the assessment made in his earlier publications: it must be shown how Psusennes of the eleventh century could build on foundations laid down in the fourth century. If the following is an explanation, let us set it down for the record [in Montet’s words as translated from French]:

“The temples of Tanis were rebuilt so many times and so mishandled in modern times that not one stone of the Old, Middle, and New Kingdoms occupies its original place.”

Montet assumed, in order to maintain the established chronology, that the particular temple was rebuilt repeatedly and from this process a unique situation occurred, namely that the bricks from the earlier centuries were placed above those of the 4th century B.C. The fact that the bricks of the 21st and 22nd Dynasties were laid above those of the 4th century B.C. is far too improbable an accident to even consider. But when we combine the evidence at Piramesse—where the 20th Dynasty stratum lies directly below the Ptolemies strata—with the brick stratigraphy at Tanis—where the 21st and 22nd Dynasty bricks are found directly above those of a 4th Dynasty structure—it is quite clear that the 20th Dynasty of Ramses III reigned only a short time, perhaps 60 years, before the Ptolemies and that the 21st Dynasty existed into Ptolemaic times while the 22nd Dynasty preceded it (i.e., it did not come after the 21st).

If bricks from these dynasties at Tanis were found in an order that had the 21st followed by the 22nd, followed by the 30th Dynasty of Nectanebo I, historians would never suggest that the brickwork became disarranged by constant

28 Velikovsky, op.cit., pp. 152-153
rebuilding. No, they would claim that the evidence is clear-cut and fully supports the established chronology. At this point it is evident that the historians are prepared to invent any possible excuse or rationale to maintain the established chronology. Velikovsky in these cases is right and Egyptian history in the first millennium to 1200 B.C. must be shortened by about 800 years.
WEAPONS, ARMOR, AND HELMETS OF THE PERESET AND SEA PEOPLES

From the foregoing evidence it becomes clear that Ramses III’s wars with these Sea Peoples could only have taken place in the 4th century B.C. Therefore the Pereset who took part in these last two wars against Egypt could not be the Philistines of the 10th century B.C. but had to have been a nation of the 4th century that made war on Egypt. Historically it is well-known that the nation that fits this chronology is Persia. This raises an interesting question: How did these important wars which saved Egypt from enslavement fail to be seen by historians of Egypt? The reason, of course, is that the established chronology forced them to place Ramses III in the 12th century B.C. so that the actual wars fought against the Persians and their Greek allies were misinterpreted as wars against the Sea Peoples. This would be comparable to the British forgetting about the Battle of Dunkirk or the Americans forgetting about the battle of Yorktown or the attack of the Japanese on Pearl Harbor. The inscriptions on the walls of Medinet Habu and the numerous illustrations of the wars graphically describe the battles and it is from these that one can begin to fill in the details that enable us to understand how this evidence clearly portrays these Sea Peoples as Persian/Pereset and Greeks.

Furthermore, on the Greek side of the equation there exists a direct history of these wars by Diodorus written in 375 B.C. that fully outlines these events. If these wars were fought by the Persians and the Greeks, then Diodorus’ history should be illustrated in the inscriptions and engravings of Ramses III. And that is just what is found to be the case as Velikovsky showed in chapters II and III of Peoples of the Sea. The aim here, however, is to show that the military weapons, armor, helmets, and headdress depicted on the walls of Medinet Habu are those of the Greeks and Persians of the 4th century B.C. Before commencing, though, it must be pointed out that many of these weapons, armor, and certain shields are also described as being those of the Philistines and thus historians have taken these and the name Peleset to refer to the Philistines. But here is the contradiction: although it is known that the Sea Peoples had these iron implements and the Philistines also supposedly had a monopoly on iron, the fact of the matter rarely discussed is: the Philistines did not use iron. Simcha Shalom Brooks makes this fact abundantly clear:

“It is generally assumed that it was the Philistines who brought the knowledge of iron-working into the country [of Palestine], and that they retained their monopoly of that knowledge. This assumption is based on 1 Sam. 13:19-22 which describes the situation in the reign of Saul; vs. 19-20 state: ‘There was no smith to be found throughout the land of Israel; for the Philistines said the Hebrews must not make
swords for themselves. So all the Israelites went down to the Philistines to sharpen their plowshares, mattocks, axes or sickles.’ But this assumption derived from Samuel is mistaken. Although the Iron Age I period is generally accepted as beginning ca. 1200 [B.C.], the use of iron only came at a later time. It first appeared during the 14th-13th century [B.C.] in the Hittite Kingdom, where it was protected as a monopoly … and it started to spread with the fall of the Hittite empire at the end of the 13th century, but it penetrated Palestine only later. In fact the word ‘iron’ does not appear in these [biblical] verses at all, and the word ‘smith’ means simply ‘craftsman’ or ‘artisan’. These [biblical] verses do not deal with iron-working: the tools which have been discovered at the Iron I sites [of ca. 1200 B.C.] were made of bronze. Hence these verses probably refer to metal-working in general. Neither is there archaeological evidence of the use of iron in Philistia at this period. In the only Philistine capital that has been extensively excavated (at Ashdod) iron implements were not found. In fact, during the Iron I, bronze was still the most prevalent metal. At Tell Qasile [in Philistia], no iron was found in the Early Strata (XII-XI), except for a single iron blade with an ivory handle in Stratum XII. …

Moreover, most iron artifacts in the early period are of inferior quality because techniques of smelting were not sufficiently developed. … This is apparent in most parts of the ancient Near East until the ninth or eighth century B.C.E. Also, it took decades before iron distribution grew, and therefore iron technology could not be associated with the [Sea Peoples’] general upheaval in the region.

“The accumulation of archaeological evidence regarding the scant appearance of iron in the [Philistine] region during the Iron I period, as well as the re-evaluation of its geopolitical distribution would, therefore, indicate that the Philistines did not, as was previously thought … maintain … controlling … production and distribution of metal weapons.”

Samuel Kurinsky furthers this analysis:

“No evidence has been produced that attests to the iron working propensities of the Philistines, of … ‘Sea Peoples’ or their probable progenitors, the Mycenaean, prior to their appearance on the Canaanite coast. No evidence exists that would indicate that they developed such a capability soon after their incursion into lower Canaan, not even in the early strata of Ashdod, the extensively excavated Philistine capital. An iron axe and a chisel were among a few iron tools found in a room of the fortification of the city from stratum 9 of the excavation, but they date from the late seventh or early sixth century B.C.E.”

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29 Simca Shalom Brooks, Saul and the Monarchy (Aldershot UK/Burlington VT 2005), pp. 31-32
30 Kurinsky, op.cit., p. 215
It is evident that the Philistines could not be the warriors depicted on the walls of Medinet Habu. Their lack of a full-fledged iron technology precludes any such consideration.

As regards the headdress and helmets worn by the Pereset/Persians and Sea Peoples/Greeks, Velikovsky writes:

“On the murals of this temple at Medinet Habu the Pereset and their allies, the Peoples of the Sea, are easily recognizable by their apparel. The Pereset wear crown-like helmets on their heads and are dressed in rich garments. The soldiers of the Peoples of the Sea have horned helmets sometimes with a ball or disk between the horns. …

“If we page through the records of excavations in various countries and photographs of ancient art, we find again the crown-like helmets that we know from the bas-reliefs of Ramses III as the headgear of the Pereset. We find them on the heads of Persian soldiers.

“In Persepolis, the ancient capital … of the … kings of Persia, … many figures of Persian soldiers are preserved on the walls of the staircases in relief. They have crownlike helmets on their heads: this headgear consists of a number of facets or petals set on a strap wrought with ornaments around the head, and a small protective screen for the back of the head. The crownlike headgear of the Pereset on the Egyptian murals also consists of a number of facets or petals set on a strap wrought with ornaments around the head, and a small protective screen for the back of the head.

“… The tomb of [the Persian king] Darius has carved rock bas-reliefs showing the guard of the Persian monarch. Again it is easy to recognize the headgear of the Pereset.”

As for the armor worn by the Pereset/Persians, Velikovsky further reports:

“Ammianus Marcellinus, describing much later the armor of the Persians, tells us that they were clad from head to foot in pieces of iron fashioned like feathers. …

“The climate of Egypt forbids, for the larger part of the year, the use of mail. The soldiers of the Pereset were clad in light tunics, a few strips of mail, and helmets made of scales. At the excavations of Daphnae (Tahpenes of the Scriptures), the Greek military colony in Egypt, iron scales were unearthed. ‘The scale armour is the most unusual find of all.’ [according to W.M. Flinders Petrie, Tanis, Pt. II, “Nebeshesh and Defenneh (Tahpenhes),” p. 78] But identical armor, actually ‘a corselet of scales, is shown in the tomb of Ramessu [Ramses] III,’ Flinders Petrie observed not without wondering. [Ibid.]

“The fact that iron scales were unearthed by Petrie at Daphnae, founded in the seventh century for Greek mercenaries serving in Egypt, and a corselet of similar scales is shown in the tomb of Ramses III belongs to the growing collection of anachronisms with which the time of this pharaoh now seems to abound.”

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31 Velikovsky, op.cit., pp. 31-32
32 Ibid., p. 34
With regard to the helmets and armor of the Sea Peoples, Velikovsky shows:

“The armor of the Peoples of the Sea is also a good earmark. The helmets, the tunics, the corselets, the swords, the targets, the spears are those of Greek mercenaries in Persian service in the fourth century. A Greek warrior in a helmet with two horns is pictured on a vase found on the Acropolis of Athens. But a helmet with horns and a crest was originally the headgear of the Lydian contingents in the army of [the Persian king] Xerxes. One hundred years after Xerxes came to the throne the helmets with horns but without ears apparently became part of the uniform of the Greeks in the Persian army and in overseas service in general.

“Horned helmets were worn by Athenian soldiers of the fourth century in mercenary service … There is a difference between the Athenian helmet worn by those who served under [the leadership of the Greek] Chabrias (a disc between the horns) and the helmet worn by the troops under [the Greek leader] Iphicrates (no disk between the horns). …

“Forty years later a two-horn helmet was worn by Alexander in his campaign in Asia and Egypt. …

“An even more precise means for placing the Peoples of the Sea in their proper time is offered by their offensive and defensive armor …

“In the pictures of Medinet Habu we have the opportunity to see … the Greek army … The soldiers of the Peoples of the Sea are in tunics, some in light corselets, none in heavy armor. The development of sword, spear, javelin, and shield may be observed. We see rather short swords, as well as very long swords like those introduced [in the 4th century by the Greek] Iphicrates; short spears and long ones; also shields of two different designs—a square one that covers most of the body, and a smaller round target [shield]. …

“Although heavy coats of mail were not used in the mercenary army, as shown on the bas-reliefs of Ramses III, next to battalions with the newly-introduced arms—very long swords and round shields or bucklers—there are still soldiers with antiquated short swords and heavy shields, straight on the lower edge, curved at the top. …

“The Peoples of the Sea ‘engaged in the Land and Sea fight under Ramses III had immense swords,’ writes [H.L.] Lorimer, Homer and the Monuments (1950), p. 267, and this is also what we had to expect from Greek mercenaries of the [4th century] days when the reform of Iphicrates was introduced …”

“Ramses III himself described the unusual swords employed in the Libyan campaign as five cubits long.” [according to F.W. Bissing, Studi Etruschi (1932), IV, 75.] Breasted reads “five feet.” …

“He [Iphicrates] changed likewise the fashion of their spears and swords. The spears he caused to be made half as long again as they were before, and swords longer almost by two parts. … Lastly, he altered the very soldiers’ shoes, that they might be sooner put on, easier to march with, and more readily cast off; …’ [according to Diodorus, XV, 5]. The pictures in the temple of Ramses III at
Medinet Habu illustrate Diodorus’ description [of all these accoutrements of war of the Sea Peoples].”

The armor, helmets, weapons of the Pereset and Sea Peoples portrayed at Medinet Habu show clearly and directly that these were the very armor, helmets, weapons of the Persians and Greeks of the 4th century B.C. Here is the obvious problem facing proponents of the established chronology. If the wars of the Pereset and Sea Peoples were fought _ca._ 1200 B.C., with these forms of armor, helmets, weapons, etc., why aren’t they exhibited throughout that entire time down to the 4th century B.C. by other warring nations of that region? Yes, the Philistines have some of these, but the Philistines did not have iron in abundance so they could not be the Pereset, who did. Henry Brugsch-Bey as long ago as 1880 knew this identification and makes it obvious that the Sea Peoples were clad with weapons, helmets, etc., which are “unmistakably of Greek type” when discussing these depictions at Medinet Habu:

“The second group [not the Pereset] is formed by the kindred peoples of the Shardana, Shakalsha, and Uashash, with the epithet ‘of the sea,’ that is valiant warriors on sea. Their armament is essentially distinguished from that of the first group. Helmets surmounted with horn-like crests, coats of mail, armlets, shields with handles and bosses, long swords, sandals on their feet—all give them a chivalrous appearance … the Greek type is unmistakable.”

Historians wed to their chronological constraints could not see the obvious and have attached these events anachronistically to the 13th century B.C.

John Bagnell Bury, the great Greek historian, illustrates precisely the chronological thinking which created this historical dogma that the Sea Peoples could not be the Greeks, and claims:

“And it is just possible that some curious records which have been discovered in distant Egypt bear upon the occupation of the [Aegean] islands. We learn that the throne of [pharaoh] Mernphat was shaken by a joint invasion of Libyans and the peoples of the north. A generation later another invasion is recorded in the reign of Ramses III … The Egyptian records mention the names of some of the northern peoples in both invasions, but the names teach little. There is not much likelihood in view that some of the invaders were Greeks. The day was yet to come when Greeks would fight in Egypt as mercenary soldiers … but the twelfth century [B.C.] is too early an age to find Greek adventurers on the shores of Africa.”

The rigidity of the historians on this point has made it impossible for them to see the evidence that so clearly demonstrates that the Sea Peoples were indeed

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33 Velikovsky, _ibid._, pp. 56-60
35 John Bagnell Bury, _A History of Greece to the Death of Alexander the Great_ (NY 1900), p. 49
Greeks. On the other hand, John Chadwick “speculates” “it is hard to dismiss from our minds the attacks on Egypt by … ‘the Sea Peoples,’ there is little doubt that these ‘Sea Peoples’ came from the Aegean area.”36 Even knowing this possibility, the established chronology is a crushing obstacle for historians to abandon.

As a brief aside to this discussion, Velikovsky claimed that the Lydians, whom Emmet Sweeney identifies with the Hittites, also used “a helmet with horns and a crest.”37 This being the case for the Lydians, the same should also be the case for the Hittites, and it is. According to Kristian Kristiansen and Thomas B. Larsson, “Hittite rock art from Boghazkøj and Yazilikaya from the fourteenth and thirteenth century B.C. … a closer look at the famous rock relief from the King’s Gate in Boghazköy … The relief possibly represents the God of War … and he is portrayed in profile with plumed [crested] helmet with horns…”38 And again the evidence places the Hittites in the first millennium as Lydians on top of all the other evidence given in these volumes.

Not only does Velikovsky show that the Lydians of the 5th century wear Greek armor and carry Greek weapons, and Kristian Kristiansen and Thomas B. Larsson claim that Hittite soldiers wore a plumed [crested] helmet with horns; John Garstang gives this further description of Hittite soldiers:

“The Hittite soldier of the Late Period [ca. 1200 B.C.] wore armor which anticipates almost exactly the Athenian [warrior’s] panoply of the fifth century [B.C.].”39

There are others who report this common direct closeness between the Hittite/Lydian soldiers’ panoply of weapons and armor and that of the Greeks. These we will return to below. These direct connections are not fortuitous nor unrelated to chronology. Along with all the others they cement the short chronology to historical reality, a reality that opposes all that conventional historical thought has failed to address or explain, except by circular reasoning. Since it is presumed that the established chronology is correct, as does Bury, therefore the warriors of the Hittites and the Greeks wore almost identical armor, though separated by almost 800 years. This is self-induced blindness.

37 Velikovsky, op.cit., p. 56
THE SHIPS OF THE SEA PEOPLES

If the Sea Peoples are the Greeks who sailed to Egypt in the 4th century B.C., we would expect them to do so in Aegean type ships and that too is the case, as shown by A. Raban:

“The ships of the Sea Peoples depicted in the scene of the ‘Naval Battle’ on the north wall of the Mortuary Temple of Ramses III at Medinet Habu were recently discussed by several scholars. Most of them agree that the angular shape of the hull and the birdhead decoration of the stern and sternposts clearly belong to Aegean tradition.”40

This, too, indicates that the Sea Peoples were Greek.

THE MEDINET HABU TEMPLE AND CHRONOLOGY

Another problem that contradicts the 12th century B.C. placement of Ramses III and his Mortuary Temple at Medinet Habu is the evident fact that it is still standing. The problem is that the Persian king Cambyses, who conquered Egypt supposedly hundreds of years after Ramses III had died, destroyed all the temples in Egypt save one, and that one was not Medinet Habu. Here Velikovsky asks:

“How did it happen that the temple of Ramses III survived while temples of the ‘later’ dynasties are in ruins? ‘This building [Medinet Habu] is the most completely preserved temple of Egypt, antedating the Ptolemaic period. … The Medinet Habu temple is therefore unique.” [according to J.M. Breasted, Ancient Records, (Chicago IL 1906), Vol. IV, Sec. 1] …

“An Elephantine papyrus relates that ‘when Cambyses came to Egypt, he found the [Jewish] temple [of Elephantine] already built. The temples of the gods of Egypt were demolished, all of them, only the said temple suffered no harm.’ [according to Eduard Sachau, Aramäische Papyri and Ostraka (Leipzig Germany 1911), p. 21].

“The fact that the presumably twelfth-century buildings of Ramses III survived in a good state of preservation is in conflict with the information contained in the papyrus dating from the year -407 that all the temples of Egypt that stood when Cambyses entered the country (-525) were ruined by this king. Medinet Habu, the mortuary temple of Ramses III, and the temple of Khonsu erected by him in Karnak are among the best-preserved structures of Egypt. Buildings of the twelfth century could hardly have escaped destruction in the seventh … and if, by chance,

one or a few of the temple and palace structures of imperial Egypt escaped destruction at the hand of the Assyrians, they were not likely to have survived the Persian conquest [of Cambyses], too…”

The historians, of course, will argue that these kings lied about their exploits and their statements are not to be trusted. With this I would agree; however, the pylon or front of the Medinet Habu temple of Ramses III, supposedly of the 12th century B.C., is practically identical in form to the pylons of the Egyptian temples of the Hellenistic era built in the 4th century B.C. Velikovsky reproduced the pylon or portals to Ramses’ mortuary temple at Medinet Habu and that of a 21st Dynasty temple of Khonsu in Karnak 100 years later with the pylon of the Temple of Edfu from Ptolemaic times, and the almost identical features of these structures are striking. See figure I

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41 Velikovsky, *op.cit.*, pp. 74-75
These were purposely not labeled other than A, B, and C to show the difficulty of determining which temples belong to Ramses III and which to the time of the Ptolemy. Pylon A is that of Ramses III at Medinet Habu. Pylon B is also of the 21st Dynasty, but of the Khonsu Temple, 100 years later. Pylon C is of the Temple of Edfu from Ptolemaic times. Again, the historians will argue that the later Greek Ptolemy copied temples from earlier times to curry favor with their Egyptian subjects. But what is difficult to explain away is that the names of the Sea Peoples are listed on the Ptolemaic temples. Here Velikovsky shows:

“From our point of view it is only natural that the texts on the walls of the Ptolemaic temples should, too, bear close resemblance to the texts of the temples of Ramses III. Jean Yoyotte, who examined the Ptolemaic temple at Edfu, was surprised to find on the walls of this temple … a text that speaks of the king destroying his enemies, ‘hacking to pieces the Meshwesh, slaughtering the Shasu, massacring the Tjeker.’ Yoyotte wondered at the use of names of people that Egypt knew almost a thousand years earlier and that supposedly were no more present … An inscription in the Kom Ombo [Ptolemaic] temple speaks of the Meshwesh, the Shasu, the Tjeker, the enemies known from the wars of Ramses III. ‘At the present state of historical documentation, the only massive attack of the Tjeker, one of the Peoples of the Sea, against Egypt, took place in the reign of Ramses III,’ writes Yoyotte. Then why should some of the Peoples of the Sea be called by name and referred to as enemies in the temple of one of the later Ptolemies?

“The answer is: The war of the Peoples of the Sea took place only half a century before Ptolemy I … mounted the throne of Egypt and founded a new dynasty.”

The Greek Ptolemies would not have commemorated a battle that took place 1000 years before their time and had probably been all but forgotten. But to curry favor with their Egyptian subjects they would do so for a pharaoh–Ramses III–who lived close to that time and was remembered as the savior of Egypt.

In addition to this there is a direct artistic link between the Persians and Ramses III’s temple at Medinet Habu. As pointed out above and in volume II of this series, the Neo-Assyrians ruled Assyria for the Persians and thus lived in Persian times. Therefore, Ramses III, supposedly living 600 to 700 or more years before the Neo-Assyrians and Persians, would not and could not know of their art and employ it on the walls of his temple. However, if he lived in Persian times, as Velikovsky claims, then he would have been able to use the artistic motifs of the Persian epoch. Velikovsky, of course, did not know of this Neo-Assyrian/Persian connection; nevertheless, his analysis is even more cogent given this relationship of the Neo-Assyrians to the Persians to Ramses III’s art on the walls of his temple:

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42 Velikovsky, op.cit., pp. 76-77
“Ramses III ordered his artists to decorate his edifices at Medinet Habu with hunting scenes as well as military ones. These hunting scenes show many features in common with [Neo-]Assyrian bas-reliefs depicting royal game as executed by the artists of Assurbanipal and, before him, of Ashurnasirpal. ‘It is regularly assumed that the scenes of the Assyrian chase of the ninth-seventh centuries [B.C. in the established chronology but astronomically dated by Rose in volume II to the 6th–4th centuries B.C.] were inspired by the Egyptian scenes which served them as originals. Yet no proof for this was ever presented. The problem needs a thorough re-examination.’[according to Louis Speleers]”

Because the Neo-Assyrians are the rulers of Assyria of the 6th through 4th centuries B.C., they would not portray hunting scenes in a similar style to those of the 20th Egyptian Dynasty 600 to 800 years earlier. Likewise, the Egyptians of 1200 B.C. could not portray hunting scenes of a foreign nation in a similar style which came 600-800 years after them.

“Speleers saw clearly that the [Neo-]Assyrian scenes were more realistic, with better portraiture of animals in their various poses, and that the motifs had been developed by the [Neo-]Assyrians in an original way. This cannot be said of the scenes of Medinet Habu. Although these have certain features that can be traced to older Egyptian models … many new details reveal themselves as of Asian [Mesopotamian] origin.

“The difficulty of harmonizing the results of the analysis with the chronological sequence was, of course, seen from the start. ‘In order to grip the problem properly we must remind ourselves that the [Neo-]Assyrian hunting scenes are many centuries later than those of Egypt [of Ramses III].’ The author [Speleers] had to repeat this warning to himself several times because everything pointed to a borrowing by Ramses III from [the Neo-Assyrians]. …

“The hunting scenes of Ramses III have disclosed upon detailed examination their dependence on Asiatic motifs; but as soon as we disclaim the twelfth century [B.C.] for Ramses III we are no longer beset by the difficulty of explaining how twelfth-century motifs could have been borrowed from scenes created in the [Neo-Assyrian/Persian epoch].

“Speleers was also struck by the similarity in the portrayal of animals (lions) by the artists of Ramses III and by artists under the late Ptolemies—a characteristic case is seen in the figures created at Kom Ombo.

“This, too, is of no embarrassment for the chronological timetable in which Ramses III preceded by only half a century the beginning of the Ptolemaic Dynasty. It would be natural for art forms used in Egypt of the fourth century to be copied by the succeeding Ptolemaic kings.”

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43 Velikovsky, op.cit., p. 72
44 ibid., pp. 72-73
Is one to assume that these various nations separated by several centuries had an archaizing love of the forms of various other nations, in terms of the art, architecture, weapons, armor of those foreign nations? One must assume some such explanation or believe the entire panoply of highly similar to identical forms of evidence were all highly similar to identical though separated by several centuries from each other by accident or coincidence. Yet when archaeologists dig up or find pictures of highly similar to identical forms of pottery, these prove that there was a chronologically direct connection between these nations. But with all these other highly similar to identical forms of armor, weapons, architecture, art, etc., that contradict that chronology they deny this evidence as a chronological marker. In essence they want to have it both ways. When highly similar to identical relics support their chronology, that is proof of its validity, but when highly similar to identical relics across a broad spectrum contradict their chronology, that direct connection is denied.

Velikovsky presented a great deal more evidence to show the direct chronological connection between Ramses III and the events related to the Pereset/Persians and the Haunebut/Sea Peoples/Greeks and his book *Peoples of the Sea*, should be carefully read to glean this evidence. Here then is a recapitulation:

**Linguistics/Philology:** These show Ramses III had tiles with Greek letters from classical Greek times. Yet historians claim Ramses III lived 1200 B.C. and not in and around classical Greek times after 500 B.C. They thus claim that these are not Greek letters, and that they are being rational and logical when they are saying Greek letters are not Greek letters. The language of the Ramses III inscriptions clearly reflects the Egyptian language not of 1200 B.C. but of times far closer to the present when Hebrew and Syrian influences were prominent in the 6th century B.C. and even replaced Egyptian words with Hebrew words long before the nation of Israel arose. But the historians claim that these influences belong to the 12th century B.C.; that is, they claim to be logical and rational when they say 6th century B.C. Hebrew-Syrian forms expressed in Egypt are not 6th century Hebrew Syrian language forms. The word “pereset” in the Egyptian language of Ptolemaic times is formed of hieroglyphs that transliterate to p-r-s-tt, which means Persian[s]. And on the walls of Medinet Habu the identical word p-r-s-tt exists but is interpreted by the historians as Philistines. Thus the historians claim that identical terms in these writings do not have identical meanings. They claim that the term p-r-s-tt which means Persian does not mean Persian and they are being logical and rational when they do this. The Egyptian word “haunebu” in Ptolemaic times is well-known to mean Greece or Greeks. But the very same word to denote the Sea Peoples on the walls of Medinet Habu is also “haunebu” or Greeks. Again the historians claim that Haunebu Greeks does not mean Haunebu Greeks, and this conclusion is logical and rational.
Iron Weapons: No nation in the ancient Near East developed a full-fledged iron production technology until around 800-700 B.C. or even later. Yet the historians claim without a scintilla of proof that the Sea Peoples and the Pereset were able to do so before 1200 B.C. They further claim that the Philistines brought this technology to Palestine after they left Egypt. Yet there is a great dearth of archaeological evidence for iron in Philistia at these times. So the historians have to suggest that all the nations of the Near East were slow learners of iron technology and the Philistines, who supposedly had this knowledge from the start, when they came to Palestine failed to produce abundant evidence of this. And that these suggestions (unproven assumptions) regarding this matter are logical and rational. That is, the Philistines had iron, but hardly produced any of this metal, just as did all the other people.

Stratigraphy: The 20th Dynasty of Ramses III at Piramesse lies directly beneath that of the Ptolemies and to explain this fact M. Bietak has put the word “hiatus” there to suggest an 800- or 600-year settlement gap; he never describes all the detritus and other materials that should have filled in this many centuries-long gap. At Tanis 4th century Egyptian bricks at a building are always laid beneath 21st and 22nd Dynasty bricks of the 11th-10th century B.C. The historians claim that these 4th century bricks were miraculously placed beneath 11th-10th century bricks in the rebuilding without one scintilla of evidence to back up their claim, and that doing so is logical and rational.

Weapons, armor, helmets of the Pereset and Sea Peoples: The Pereset wear headdresses identical to those of the Persians. The Sea Peoples wear helmets, carry armor and weapons identical to those of the 4th century Greeks as depicted on the walls of Medinet Habu. The historians claim that all these are not Persian or Greek in spite of Henry Brugsch-Bey saying that their “Greek type is unmistakable.” Thus the historians claim that Persian headdresses are not Persian headdresses, that Greek helmets, armor, and weapons are not Greek helmets, armor, and weapons, and that this is also a logical and rational conclusion.

The Medinet Habu Temple: The architectural form and style of the pylon or portico of Ramses III’s temple is quite like those of the Ptolemies about 800 years later. It was not razed by Cambyses although he claimed to have destroyed it and the others save that of the Jews in Elephantine. The depictions on the walls of Medinet Habu of hunting scenes are clearly copies of works of the Neo-Assyrian/Persian epoch which occurs 600 to 800 years later. The names of the Sea Peoples also appear on the walls of Ptolemaic temples 800 to 900 years after Ramses III. The historians claim that the Ptolemies wrote about these Sea Peoples of almost a millennium earlier for some unknown reason, built almost identical
Charles Ginethal, *Pillars of the Past*, vol. III 109
temple pylons and created stylized Neo Assyrian/Persian hunting scenes from the 12th century B.C., and this, too, is logical and rational.

The historians want to hold on to their terribly dysfunctional “Sea Peoples” chronology no matter how much evidence is incongruent with it or is inexplicable and has no basis in supporting evidence. And like to many other problems outlined in these volumes, they cannot see or will not recognize that with their chronology none of these will be resolved, for years, decades, centuries, or ever.

**THE SEALAND/SEA COUNTRY PEOPLE AND CHRONOLOGY**

The Sea Peoples/Greeks not only show that 800 years must be removed from Egyptian history, but they play a similar role in Mesopotamian history. They ruled there by the name of the Sealand Dynasty that is directly related to the fall of the Old Babylonian/Hammurabi Dynasty, which, as was shown in volume II, did not exist between about 2000 and 1600 B.C. but during Persian times. That is, the Old Babylonians were the Persian rulers of Babylonia. In that volume, in the unit “Dark Ages,” pages 118-129, evidence was presented that showed the Old Babylonian dynasty not only begins with a “Dark Age” but ends with a lengthy one as well. By placing the Old Babylonians in the Persian epoch, this later “Dark Age” disappears. However, the fall of the Old Babylonians/Persians in this shortened chronology was not outlined in that volume. All that was shown was that this empire fell but the ancient documents related to that event have never been analyzed by the historians. All they suggest, without proof, is that the Hittites raided Babylon and the Old Babylonian Empire fell. Yet on pages 121-122 we cited Sidney Smith who admitted with regard to 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.” H. Gasche *et al.* concluded: “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.” Others were cited to the same effect.

Therefore, since the Old Babylonians are the Persian rulers of Babylonia, their empire had to have fallen to Alexander the Great and to have been eventually ruled by the Seleucid Greeks. Furthermore, what remains of that record should show that the Old Babylonians/Persians fell to a nation with a name that is Greek. That is the case, and the nation to whom it fell is named the Sealand or Seacountry People.45

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Just as the Sea Peoples warred in Egypt in the 4th century B.C., so too did the Sealand/Seacountry people bring down the Old Babylonian/Persian empire in the 4th century B.C. This, in fact, is well documented in the ancient records but the established chronology of the Sealand made a shambles of it and it was “elbowed out of the chronological sequence entirely,” as expressed by Henige earlier.

“The chronicles of [L.W.] King showed three early dynasties [in Mesopotamia]—then called Dynasties I, II, and III, and now known as the First [or Old] Dynasty of Babylon, the Sealand dynasty, and the Kassite dynasty— as (apparently) ruling in succession for 304, 368 and 576 years. Totaling these and projecting them back from the mid-twelfth century, when the Kassite rule [supposedly] ended, resulted in figures of ca. 2400 BCE for the First Dynasty of Babylon. [Because this was far too old to begin this chronological/historical structure, the Sealand dynasty was gradually elbowed out of the chronological sequence entirely…]”

The Sealand dynasty was simply discarded by most of the historians, even though there was documentary evidence that they were directly involved in the destruction of the Old Babylonian empire. That is, because the historical documents could not be accommodated in the established chronology, an entire people was eliminated as an active agent in the fall of that nation. In this regard, we can see not only how whole empires such as the Sumerians, Mitanni, etc., unknown to the ancient world, were invented by the historians to accommodate the established chronology, but in this case and clearly—as Heinsohn, Sweeney, and Velikovsky have shown—others were alter-egos of nations known to the ancients. So the Sealand people, well-documented and well-known, were eliminated for the same chronological reason. As Velikovsky correctly removed the historical blinders regarding the origin, history, and chronology of the Sea Peoples, the Greeks of the 4th century B.C., so here we must follow in his footsteps and remove the historical blinders regarding the origin, history, and chronology of the Sealand/Seacountry people—the Greeks of the 4th century B.C.

As is well known, the Persian empire of which Babylonia was a part experienced a series of wars with the Greeks and was finally overthrown by Alexander the Great. Since these events are documented in the chronicles and histories of that time, they should also be reflected in the documentary evidence regarding the Sealand dynasty. As Velikovsky has shown, the Sea Peoples fought wars in the same order in Egypt that directly reflect the wars of the 4th century Persians and Greeks with Ramses III; similarly, the Sealand people fight a series of wars with Babylonia/Persia that directly reflect the wars fought by the Greeks as noted in The Persian Wars by Herodotus, and others. Importantly, these wars are...

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46 Henige, op.cit., p. 138
fought in the same order, by kings in the same order, having battles in the same order with the same outcomes. The scientific and technological, as well as linguistic and other evidence which places the Old Babylonian dynasty in Persian times having been presented in volume II, we can get onto the historical materials that illustrate these events.

Because the Sealand dynasty was improperly placed in Mesopotamian chronology, its homeland like that of the Sea Peoples that fought Ramses III is unknown and was therefore placed in or near the Arabian peninsula, or along the edge of the Persian Gulf. But these locations are based mainly on conjecture and interpretations without direct proof or solid evidence for support. Raymond Philip Dougherty, a proponent of Arabia as the homeland of the Sealand Peoples, informs us:

“On account of the appellation applied to it, scholars have been disposed to identify the Sealand with the marshy area north of the Persian Gulf … The fact that certain texts are in harmony with such a conclusion has discouraged endeavor to advance a different view. A lack of concrete data prevents easy inferences. Geological treatises in cuneiform giving specific information concerning districts, provinces and countries known at the time are exceedingly rare. In fact, no such general document entirely satisfactory in character has been found … Furthermore the scribe whose stylus left its impress upon soft clay referred to lands in a very casual manner. There was little attempt to describe their real extent or to name their precise borders. Ancient cuneiform writers assumed that those for whom they wrote understood the geographical terms which they used.

“There is an additional difficulty in this field of research. So far as our present knowledge is concerned the history of the Sealand cannot be based upon records, which had their origin in that country … [The Sealand homeland has not been found; our] knowledge of the Sealand depends, therefore, upon texts which originated in other lands.”

Harriet W. Crawford also admits, “… the [Persian Gulf] marshes became the center of the so-called Sealand dynasty of which almost nothing is known. Even the Sealand capital has not been identified and few items of its material culture can be confidently identified. All that remains are a few textual references.” Of course, once the Sealand was improperly located geographically and chronologically, none of its remains could be found. But if the Sealand people were the Greeks, all their strata and relics are known and do exist. As with so many other nations, once the historians have misrepresented their place in time, their relics cannot be found, as has been demonstrated repeatedly in these volumes of

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47 Dougherty, op.cit., pp. 1-2
Pillars of the Past. According to the historians, the Sealand is either in Arabia or the marshlands of the Persian Gulf; the only problem is, they can’t find it.

As the homeland[s] of the Sea Peoples who invaded Egypt has/have never been found, so too there is no archaeological evidence that establishes the homeland of the Sealand people. In order to account for the fact that the Sealand “sank into oblivion,” Dougherty admits:

“With the fall of nations [in the second millennium B.C.] and destruction of cultured centers knowledge of the Sealand and remembrances of its intimate contacts with the Tigris-Euphrates valleys disappeared from the minds of men. This cannot be a cause for astonishment since the Sealand sank into oblivion as part of that past whose record became lost in the debris of ruined cities.”

There is no known home of the Sealand people nor any recollection of them, according to Dougherty. But as we will see when we discuss the Second Sealand Dynasty, the recollection of these people and their connection to the Greeks will be scientifically, i.e. astronomically established. Dougherty further explains:

“None of the references to the Sealand in the texts which have been discussed demands its complete identification with the primitive marsh country at the head of the Persian Gulf. It seems necessary to posit a larger area of activity and wider base of operation for the potency exhibited by the Sealand. If more room for the real Sealand of cuneiform inscriptions has to be sought on the map of the ancient world, the peninsula of Arabia offers the most suitable space for extension. In fact it is necessary to expand in that direction beyond the preëmpted territories of other nations, in order to secure a sphere sufficiently large for the role played by the Sealand … [The Sealand] … politically attained a broad arena in which its forces developed and found expression … [Thus] it seems necessary to posit a larger area of activity and wider base of operation for the potency [in the political life of Mesopotamia] exhibited by the Sealand.”

The Sealand was, accordingly, a major factor in the history of Mesopotamia, but at the same time it “sank into oblivion and became lost in the debris of ancient cities.” All of this is of course historical conjecture. If, on the other hand, the Sealand is Greece and the Aegean, there will be a clear-cut record of its activities, and these are to be found in the events of the Persian wars. Yet we must pursue the location of the Sealand further because the evidence does point to the Mediterranean Sea to the west of Babylon, not the south where Arabia and the Persian Gulf are situated.

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49 Dougherty, op.cit., p. 1
50 Ibid., p. 24
In this respect, Dougherty tells us that “Sargon of Agade made war on the Sealand.” If, as historians maintain, the Sealand was located in southern Mesopotamia, travel to it would be overland or by the Euphrates and Tigris rivers flowing south. It would not be separated from Babylonia by both land and sea. However, the translation of the chronicle of Sargon states that “‘booty of the Westland’ [contains the words] ina ma-a-ti tamtim’ ‘by way of the Sealand’ [does] match the [identical] expression ina ma-a-ti tamtim in the Neo Babylonian chronicle. Leonard W. King suggests that [this expression] may have been used by the cuneiform copyist to mean ‘by land and sea’."

If the marsh land at the head of the Persian Gulf which is contiguous with Babylonia was the home of the Sealand no booty would have had to be transported “by land and sea.” With respect to Arabia as the homeland of the Sealand, there is another problem with the direction. Arabia is located south and somewhat to the west of Babylonia. But, as Sargon’s chronicle states, booty came from “the Westland” and the literature is emphatic that Sargon traveled to “the Westland”:

“‘The sea of the setting of the sun he crossed and in the third year (the land of) the setting of the sun … his hand conquered.’

“… Sargon … crossed the sea of the setting of the sun, i.e. the Mediterranean Sea. Some take it to mean that he journeyed as far as the island of Cyprus. Others hold that it implies only a coastwise traversing of the Mediterranean Sea.”

Either interpretation of the chronicle leaves no doubt that booty came from the Sealand, from the Mediterranean Sea. This cannot be if the marshes of the Persian Gulf or Arabia were the location of the Sealand. The Sealand was located in the direction of the “setting of the sun,” it was somewhere in or by the Mediterranean Sea and, to be transported to Akkad ruled by Sargon, goods had to cross part of that Sea. The only place that fulfills all these geographical particulars is the Aegean and Greece. Thus, there is clear evidence that the Sealand was located in the Aegean and Greece.

As for the Persian wars with Greece and the Old Babylonian wars with the Sealand, the facts speak for themselves:

The relations of Persia with Ionia in western Anatolia/Turkey and the Greek cities should in certain respects be similar to the relations of the Old Babylonians with the Sealand. Persia did exercise its influence over Ionia, as is well-known, and imposed annual payments of tribute. It did not, however, exercise its influence over the whole Hellenic peninsula. According to Will Durant, “Only one important nation remained outside this vast system of government and trade [controlled by

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51 Ibid., p. 26
52 Ibid., p. 6
53 Ibid., pp. 5-6
Persia], and that was Greece. By 510 [B.C.] Darius had hardly heard of it outside Ionia. ‘The Athenians,’ he asked, ‘who are they?’”

The same condition also existed regarding the Old Babylonians’ relationship with the Sealand, and we are told as much, Leonard W. King reports. “The fringe of territory [at] the extreme [of] Babylonia always exhibited a tendency to detach itself from Babylonia proper.” He further reports: “The early kings of [Old] Babylonia had always been content to leave the [supposed] swamp dwellers [Sealand] to themselves and at most exact a nominal recognition of suzerainty.” Thus the early Persian kings held Ionia, Thrace, and Macedonia, but not all of mainland Greece; and in similar fashion the Old Babylonians left the fringes of the Sealand to themselves.

The cause of the Persian wars is well known. The Athenians encouraged Ionia to resist Persia by stopping their payments of tribute. The king of the Sealand, in the surviving documents, is known as Ilumailum; although his identity is clearly not given, we take Ilumailum to be an iconic figure whose identity was attached by the scribes to Alexander the Great and assume that the chronicler[s] who later wrote this material confused him with the Ionian leader who revolted against Babylonia. It is known that Alexander took over Ionia and exacted payments to support his troops prior to invading Mesopotamia and particularly to take the Persian army that came to oppose him. This is, in fact, implied by King who tells us it was “Ilumailum who PROBABLY … headed a revolt … and declared his independence of Babylon.” Dougherty suggests: “More likelihood may be attributed to the view that he [Ilumailum] actually invaded Babylonia.” The closeness of this connection between Alexander and Ilumailum, though not a reality but rather an iconic identity, I tentatively draw from Dougherty’s description of Ilumailum which clearly reflects the ambition and character of Alexander:

“The initial ruler of what is known as the First Sealand Dynasty must have been a genuine conqueror. At the same time it can hardly be doubted that he possessed corresponding organizing ability. The energy and skill which he evinced in his clashes with Samsu-iluna and Abi-eshu [kings of Old Babylonia] laid dynastic foundations which lasted for three full centuries after his reign. Such an achievement entitles Iluma-ilum to recognition as one of the notable monarchs of his time. History would have been different if the Sealand had not produced him.”

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56 King, *op.cit.*, vol. II, p. 201
58 Dougherty, *op.cit.*, p. 23
59 Ibid., p. 21
“Iluma-ilum, the founder of the dynasty, must have been an ambitious sovereign. His aims were of such a character as to engender the determined opposition of … the First [Old] Dynasty of Babylon. Although the text revealing these circumstances is extremely terse and somewhat fragmentary there can be no doubt that a definite but abortive effort was made to bring military disaster upon Iluma-ilum … It is probable that Iluma-ilum’s founding of a new dynasty resulted from his thrust against the empire centered in Babylon. That he was content with a passive threat is hardly believed. More likelihood may be attributed to the view that he actually invaded Babylonia and, because he remained undefeated, added a portion of its territory to that of his own, thus establishing a domain which was a suitable arena for the activities of a long succession of capable rulers.”\textsuperscript{60}

Hammurabi, who was astronomically equated to the time of Darius the Great, carried out the first great war against the Sealand, as did Darius the Great against Greece. Like Darius, Hammurabi brought “the end of [the kingdom of] Eshnunna [which] was recorded in the thirty-eighth year,” and “In its now fragmentary lines, the king proclaimed his victory over Elam, Gutium, Subartum …”\textsuperscript{61}

As with Darius I whose campaign against Greece ended in ignominious defeat, King remarks that it is possible that “the end of Hammurabi’s reign was clouded by disaster.”\textsuperscript{62} This disaster is thought to have been caused by the recovery from defeat of Hammurabi’s Mesopotamian enemy, Kim Sin. Yet the disaster may also have been caused by the defeats he sustained in Greece.

Darius was followed by his son Xerxes who, to avenge his father, fought a second war with the Greeks. In the same vein, Hammurabi’s son Samsuiluna fought a second war against the Sealand. The Persian Xerxes launched a massive invasion of Greece by both land and sea. There was a major naval battle at a bay or small gulf at Salamis in Greece, where he suffered a great defeat. Further defeats were inflicted upon his army a year later at the battle of Plataea with an additional defeat at a naval engagement at Mycale. The outcome of these disasters, though Xerxes had carefully planned the campaign, ended with a loss of territories in that region. “Xerxes, Darius’ successor, lost Macedonia … and the Persians were driven out of Thrace and the coastal cities of western Asia Minor [Ionia].”\textsuperscript{63} Thus, Xerxes lost all his holding in Greece and Ionia. Just like Xerxes, “Samsuiluna’s answer [to the defeats of his father Hammurabi] was to raise further levies and lead them against his new foe [the Sealand].”\textsuperscript{64}

\textsuperscript{60} Ibid., p. 20
\textsuperscript{62} Leonard W. King, Studies in Eastern History vol. II (London 1907), p. 170
\textsuperscript{63} The Columbia History of the World (NY 1986), p. 167
\textsuperscript{64} King, A History of Babylonia, vol. II, op.cit., p. 199
We are further informed that “Samsuiluna twice marched against [Ilumailum of the Sealand], the first time fighting a costly but indecisive battle, the second time suffering a defeat.”\textsuperscript{65} It is thus rather interesting to interpret this generalized description of the two battles which seems to reflect the Spartan stand at Thermopylae where Xerxes the Persian and Samsuiluna the Old Babylonian were for a time halted. As noted above, Xerxes experienced a major defeat at the naval engagement at Salamis. In this respect Samsuiluna’s sea battle appears to be eerily like Xerxes’. A thumbnail sketch of Xerxes’ naval debacle shows:

“the Persians managed to extricate a large portion, at least half of their ships from [the Greek admiral] Themistocles’ trap. They abandoned the scene of the battle strewn with wreck[s of their vessels] and floating men, whom the [Greek] victors clubbed with oars or spitted like tunny [fish]. The slaughter … went on till nightfall.”\textsuperscript{66}

It is fascinating to learn that Samsuiluna’s naval engagement with the Sealand was also fought at a body of water referred to as a gulf and that his men slain there littered the waters. King says that the

“… battle was fiercely contested on the very shore of the gulf, for a later chronicle records that the bodies of the slain were carried off by the sea; yet it was either indecisive or resulted in the discomfiture of the Babylonians.”\textsuperscript{67}

Clearly, what is described is the Battle of Salamis, fought in a gulf, which is what the bay of Salamis would appear to be to a land-based people. The reason why the slain were carried out to sea was not because they were killed on the shore but rather at sea. Hence, the battles of Darius the Great and his son Xerxes against Greece are reflected in the battles of Hammurabi and his son Samsuiluna against the Sealand.

It is well known that Xerxes retreated after the Battle of Salamis and as for Samsuiluna, he too did just that: “If the [Old] Babylonian army succeeded in retreating in comparatively good order, it would have formed a sufficient justification for Samsuiluna’s boast that he had given the rebellious [Sea-]land a lesson.”\textsuperscript{68} And just as Xerxes lost his northern possessions, so did Samsuiluna. Dougherty states: “There is indirect evidence that this reconstruction is not imaginary. Cuneiform records seem to demonstrate that Samsuiluna’s suzerainty over certain Babylonian cities ceased long before the time when he was followed by Abeishu.”\textsuperscript{69} It is thought that these cities lay to the south of Babylon which would therefore place the Sealand on the edge of the Persian Gulf. We are,

\textsuperscript{65} CAH, op.cit., p. 222
\textsuperscript{66} CAH (Cambridge UK 1932) p. 312
\textsuperscript{67} King, A History of Babylonia, vol. II, op.cit., p. 197
\textsuperscript{68} Ibid., p. 20
\textsuperscript{69} Dougherty, op.cit., p. 21
however, specifically told by Georges Roux: “At the end of [Samsuiluna’s] disastrous reign, Babylonia was safe and the kingdom [was] amputated of its northern and southern provinces.”

After Xerxes we come back for a moment to the Sea Peoples, discussed by Velikovsky, who fought in Egypt at first on the side of the Egyptians but later joined the Persians. The Persian Xerxes is followed by the Old Babylonian Artaxerxes I while Samsuiluna is followed by Abeishu, and both engage the Greeks and the Sealand in a second unique military combat. In both cases waters of a river or canal are diverted which traps their opponent who manages to escape. The events of Artaxerxes I are outlined by Velikovsky as follows:

“When [Xerxes] was assassinated, his son Artaxerxes I … followed on the throne. A few years later, a revolt took place in Egypt, headed by Inaros, a local chief. The Athenian fleet of two hundred triremes sailed up the Nile to help Egypt against the Great King. At first the Persian garrison was routed and took refuge in the citadel of Memphis, but after a few years a new Persian army freed the beleaguered garrison and defeated the Athenian fleet, leaving it high and dry by diverting the flow of a canal. The Athenians [trapped on land] burned their fleet and retreated to Cyrene.”

Thus the Greeks escaped Artaxerxes I’s trap. The Old Babylonian account of Abeishu recounts quite a similar contest but not in the same country. It describes:

“A third expedition against the southern rebel [the Sealand] was sent by Abeishu, son and successor of Samsuiluna, whose long reign of twenty-eight years was marked by no known external event other than this spectacular failure, which is related only in the chronicle; he attempted to trap Ilumailum by damming the Tigris, but although the earthwork was successful the rival leader escaped.”

In this respect the battles were precisely alike. It is here suggested that Abeishu did fight this battle in Egypt but that the location was misunderstood and misinterpreted by the scribe who assumed that it was the Tigris that was diverted rather than a Nile canal.

At this point we arrive at the last king of the Persians, Darius III and the last Old Babylonian king, Samsuditana. As with the earlier kings the events that led up to and culminated in the end of their dynasties should be similar for Darius III and Samsuditana. The final defeat of Darius III by Alexander and Darius’s subsequent death after that last great battle between the two is known quite well. Durant explains:

“Alexander received the submission of Babylon, partook of its wealth, distributed some of it to his soldiers, but charmed the city by making obeisance to

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71 Velikovsky, *op.cit.*, p. 21
72 *CAH* (1973), *op.cit.*, p. 223
its god and decreeing the restoration of its sacred shrines. By the end of the year he had reached Susa [in Elam] whose population, still remembering the ancient glory of Elam, welcomed him as a deliverer. He protected the city from pillage, but comforted his troops by dividing among them some of the fifty thousand talents ($300,000,000) that he found in Darius’s vaults … To the Greek cities of Asia [Ionia] he appears to have remitted the ‘donations’ [forced payments for his army] that he had elicited from them at the outset of the campaign.”

The fall of the Old Babylonian empire is taken to have been caused, as discussed in volume II, by a Hittite raid; while in the short chronology an attack came through Anatolia, beyond which the Ionian cities are located. The document of this attack reads “men of Khatti [Hittites/Lydians] marched against the land of Akkad; in other words … Hittites [Lydians] from Anatolia marched down the Euphrates and invaded Babylonia from the north-west.” That is the direction from which Alexander descended toward Babylon. Importantly, in the footnote to this ancient passage, King makes the following statement: “We may confidently regard the phrase as referring to the Anatolian Hittites, whose capital at Boghazköi must have been founded earlier than the end of the fifteenth century.” King assumes this capital was built early enough so that its king could raid Babylon; for this there is no scientific nor technological proof. In volume I we gave proof that the Hittites were the Lydians and thus the raid had to have taken place around Lydian times or thereafter. It is suggested that the scribe identified the invaders as Hittites/Lydians because they came through this region as did Alexander. King makes it clear that “The chronicle does not record the result of the invasion. But we may PROBABLY connect it with the fact that the Kassite king Agum ka krime brought back to Babylon from Khami … on the middle Euphrates, the cult image of Marduk and Serpanitum and [like Alexander] installed them once more with pomp and ceremony within the shrine in E-Sagila.”

In addition, as did Alexander, the head of this invading army sent booty to Hana or Ionia. Roux admits “for some obscure reason [they] left [booty] behind at Hana.” And similarly, just as with Alexander who traveled south to Elam, it is suggested “Southern Babylonia may also have suffered in the raid.” What is most striking is that Samsuditana, the last Old Babylonian king, somewhat like Darius

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73 Durant, op.cit., p. 545
74 King, A History of Babylonia, op.cit. p. 210
75 Ibid.
76 Ibid.
77 Roux, op.cit., p. 201
78 King, A History of Babylonia, op.cit., p. 211
III, “may have fallen in defense of his own capital,”\textsuperscript{79} which can be interpreted that at the end of the war Samsuditana, like Darius III, was killed.

Although certain aspects of these two histories appear to be different, the history of the Greeks’ relations and wars against the Persians are strikingly in most respects just like the relations and wars of the Sealand with the Old Babylonian dynasty. The same kings of Persia, in the same order, have relations with and wars with the Greeks as the kings of the Sealand, in the same order, have relations with and wars with the Old Babylonians. In a few instances there are unique events, such as the sea battle of Salamis and the battle by a gulf where the bodies of the defeated army are carried out to sea. Then there is the diversion of a river or canal that entraps a foreign force but in both cases the trap fails and the enemy escapes. And then there is the probable death of the last two kings at the end of their empire. Certainly not every element is identical but given the very fragmentary remains of these events the correlations are rather strong.

With the fall of Persia, the Greeks transformed that part of the world and introduced many new, far-reaching changes into it with the establishment of the Greek Seleucid kings who came after Alexander, as Greek culture, in large measure, was transplanted to Mesopotamia. Durant shows: “This imposition of Greek civilization upon the Near East is one of the startling phenomena of ancient history. No change so swift and far-reaching has ever been seen in Asia.”\textsuperscript{80} Therefore, employing Heinsohn, Sweeney and Rose’s reconstruction of chronology, we must expect to find not only a vast change in the artistic, cultural tradition, along with the imposition of a stable government, but most importantly, technological developments that could only have existed in the first millennium B.C. and could not have arisen in Old Babylonian times. On this point of government, King states:

“Doubtless other local kingdoms arose during the period following Babylon’s temporary disappearance as a political force, but we have recorded no trace of them, and the only fact of which we are certain is the continued succession of the Sea Country kings. To one of these rulers, Gulkisar, reference is made on a boundary stone … drawn up in the reign of Enlil-nadin-apli, an early king of the Fourth Dynasty [supposedly of the Kassites]. On it is given the title ‘King of the Sea Country,’ which is also the late chronicler’s designation for E-Gamil, the last member of the [Sealand] dynasty … Such evidence seems to show that the administrative centre of [the Sealand’s] rule was established at those periods in the south; but the inclusion of the [Sealand] dynasty in the King’s List is best explained on the assumption that at least some of its later members imposed their suzerainty over a wide area … They were evidently the only stable line of rulers in

\textsuperscript{79} Ibid.
\textsuperscript{80} Durant, \textit{op.cit.}, pp. 576-577
a period after the most powerful administration [the Old Babylonians] that the country had yet known had been shattered.”

And finally, we arrive at the technological advances the Sea Peoples brought to Mesopotamia that could not have existed in the second millennium B.C. The Sealand craftsmen were making advanced forms of glass that Bronze Age furnaces could not produce. Historians simply assume that because the established chronology is correct, then whatever anachronistic technology they find in the ancient world had to have been produced at that time, in spite of the evidence against it. The chronology determines the technology, according to their reasoning, whereas we have consistently argued and shown that the technology determines the chronology. With regard to glass production, which the Sealand people brought to Mesopotamia, we learn:

“One singular piece of evidence attests, however, a flourishing art, which seems to have arisen rapidly at this time [after the fall of the Old Babylonian empire]. In the first year of Gulkisar, sixth king of the Sealand Dynasty, is dated a very curious tablet inscribed with secret recipes for making various kinds of glass, each bearing a trade name. So advanced was the technique of this art that it had already become a cherished mystery among the craftsmen, and consequently this tablet wherein the secrets are enshrined was written in a style of scribal ingenuity, probably meant to be intelligible only to those in possession of certain vocabularies restricted to adepts of the trade. Whether the ascription [which is original and explicit] of these recipes to the time of Gulkisar is accepted or disbelieved must depend on the date which is assigned to his reign, but in general it would seem to be about a century earlier than the earliest glass vessels in Egypt.”

That is, the Sealand had the most advanced forms of glass according to the established chronology, before Egypt, which supposedly had the first glassmakers, produced this material. Here then we recapitulate our position regarding history and chronology with first things first—science and technology first, all else secondary. Once we have established the chronology on these foundations—that is, giving the “when” for historical events—then, and only then, do we have the right and the obligation to explore and present the history, that is, showing how the “events” correlate with the “when.”

Yet I may be accused of doing precisely what I have condemned, namely interpreting documents to uphold the short chronology and this will be seen as having a double standard of inference. Nevertheless, at some point, after the various forms of scientific and technological evidence have established a chronological sequence, one is forced to show how history correlates with that

81 King, A History of Babylonia, op.cit., p. 212
82 CAH (1973), op.cit., p. 226
chronology. Before any critic raises questions and criticisms and claims this historical analysis of the Greek-Persian and Sealand-Old Babylonian events is invalid, they must prove that the science, technology, linguistics etc., which give us this chronological place, allow for some other historical interpretations or that another history fits these scientific, technological foundations. I suspect that the only recourse left to those who uphold the established chronology or those of Peter James, *et al.*, or of David Rohl, etc., will be to ignore the forensic chronological foundations and argue that other documents contradict what has been put forth, as though these are valid without forensic historical underpinning. Thus I must remind these critics of what Roger Matthews wrote, as cited in volume II:

“I contend that every ‘frank and distinctive’ statement made … about the Mesopotamian past based on [the] 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.”

Certain critics of Heinsohn, Sweeney, Velikovsky, and even Rose have chosen to speak of “the historical record.” Their names I will not, for now, mention. They suggest that the “historical record” is known almost to a certainty. That is not and never was the case. With respect to that “uncertain historical record,” the analysis of the correlations of the Greeks and Persians with those of the Sealand and Old Babylonians as well as the Peoples of the Sea with Ramses III’s 20th Dynasty, will show that the forensic historical evidence meshes with the historical evidence. It does not do so for the established chronology nor for the revisionists such as Peter James *et al.* and David Rohl. Since the forensic historical foundations are either non-existent or rarely correlate with their historical analyses, they cannot turn around and argue on the basis of their non-existent forensic evidence that what has been presented here must be in error. Since their chronologies have no scientific or technological foundations, they can have no standing as history and therefore no standing for criticism.

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83 Roger Matthews, *The Archaeology of Mesopotamia, Themes and Approaches* (London/NY 2003), p. 60
THE SECOND SEALAND DYNASTY AND CHRONOLOGY

The chronology of the Second Sealand Dynasty is also tied by the established chronology to that of the First, just discussed above. Dougherty reports “An interval of about five hundred years elapsed between the end of the First Sealand Dynasty and the beginning of the Second Sealand Dynasty... Is it reasonable to suppose that a thousand years of Sealand nationalism was followed by five hundred years of Sealand ineptitude...?”

Dougherty adds: “Very sparse indeed is the information provided by cuneiform documents concerning the Second Sealand Dynasty and the five centuries that preceded it.” In essence we have a half millennium-long “Dark Age” between the First and Second Sealand Dynasties.

That is, 500 years of “ineptitude” by the Sealand is needed to have it disappear from power and then arise out of this “Dark Age” to eminence soon after half a millennium. This is the same problem that confronted historians with the Nubian Egyptian 25th Dynasty discussed above. It is also the nature of the problem of the Hittites who were supposedly destroyed by the Sea Peoples around 1200 B.C. but who then created a flourishing nation in Syria after their empire was annihilated. These are the sorts of problems that the established chronology creates because it is basically unscientific. Empires arise and are destroyed but after centuries of a Dark Age arise like the phoenix from the ashes to new prominence. It is suggested that such episodic developments: growth, then major destruction followed by a “Dark Age” of centuries of quiescence, do not lead or rarely lead to a renaissance of empire and/or nationhood. Furthermore, there is no scientific or technological evidence to support this conclusion as it relates to the Second Sealand Dynasty, existing around 1000 B.C. Since the Old Babylonians are the Persian rulers of Babylonia based on Rose’s astronomical evidence, if the Second Sealand Dynasty were to follow them 500 years later, it would date this dynasty in Roman times, a thoroughly untenable and impossible chronological concept!

Since the Peoples of the Sea were the Greeks of the 4th century B.C., involved with Egypt and Persia, and the First Sealand Dynasty were the Greeks prior to and following Alexander the Great during the 4th century B.C., the Second Sealand Dynasty also had to be somehow intimately connected to the Greeks of the 4th century B.C. Dougherty further tells us: “The Second Sealand Dynasty, although it covered slightly more than a score of years, was unusual in certain respects. Its rulers are described as follows: ...

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84 Dougherty, *op.cit.*, p. 28
‘[There are t]hree kings of the dynasty of the Sealand.’ The geographical
designation … distinguishes this dynasty from all others. No extant cuneiform
statement defines the First Sealand Dynasty so definitively.”

We are informed that the Second Sealand Dynasty had three major kings ruling in
Babylonia for a short span of years. But the only Babylonian dynasty for which there is
astronomical evidence, linking it to the Greeks in Seleucid times, which had three major
kings, and dated to the 4th through 3rd centuries, and which in a sense was
geographically unique—that is, living in Babylonia at the same time as the Greeks—is the
Neo-Babylonian Dynasty. Although it had three major kings, it also had three short-
lived, ephemeral ones. It was not a powerful entity under the Persians nor after the
Greek domination of Babylonia. It was very clearly a vassal nation that the Seleucid
Greeks permitted to rule in its own domain but subject to control by these foreign
overlords. The reason for this evident conviction on my part that this is indeed the case
is that the first king of this dynasty, Nabopolassar, is acclaimed in a cuneiform
document as the founder of the Second Sealand Dynasty and that the following two
kings in other documents clearly indicate that they were subordinate to the Sealand.

Dougherty presents the following proof of this direct tie of Nabopolassar to the Sealand:

“The Neo-Babylonian empire … sprang from this vigor and persistence, for
there is clear evidence that Nabopolassar, the founder of the dynasty, was
connected with the Sealand. The authority for this statement is the following
cuneiform passage [which translates] ‘According to the tablets which
Nabopolassar, THE KING OF THE SEALAND, plundered from Erech.’ Scholars
are agreed as to the meaning of these words. Nabopolassar is revealed as a Sealand
king and the conclusion that he served in this capacity before he gained control of
the throne of Babylon is generally accepted. His act of despoliation at Erech could
hardly have been performed after he began to rule Babylonia. It is best explained
as the result of an invasion from the Sealand.”

Dougherty is emphatic on this point and restates it four times:

1. “That Nabopolassar won his way to an independent kingdom in Babylonia
with the backing of the Sealand is an inference subject to little doubt.”

2. “…without question … the reign of Nabopolassar [and his] rise to power in
Babylonia was based upon Sealand kingship.”

3. “It is suggested with considerable certainty that a Sealand king, i.e.,
Nabopolassar, founded the Neo-Babylonian Dynasty.”

86 Ibid., p. 31
87 Ibid., p. 111 (capitalization added)
88 Ibid., p. 112
89 Ibid., p. 118
90 Ibid.
4. “Nabopolassar, the founder of the Neo-Babylonian Empire which had its center in Babylon, began his political career as a king of the Sealand.”\(^{91}\)

It must be pointed out that in volume II, pages 202-208 and pages 625-632, Lynn E. Rose astronomically demonstrated that Nabopolassar reigned from -341 to -320, based on three levels of astronomical data. Both the astronomical data and the cuneiform tablet which named Nabopolassar as “the king of the Sealand” tie him directly to the 4th century B.C. and to the Greeks as their vassal ruler in Babylon.

This being the case, the ruler who followed him, Nebuchadnezzar I, who we have shown was confused with Nebuchadnezzar II, should also exhibit clear evidence that he was a vassal of the Sealand/Greeks. In the following citation which is ascribed to Nebuchadnezzar II but must be attributed to Nebuchadnezzar I, we find that this, too, is the case. Here we will omit the Roman numerical suffix to Nebuchadnezzar’s name, that is “II.” On this relationship Dougherty reports:

“A document recorded in Babylon in the seventeenth regnal year of Nebuchadrezzar [suffix omitted] deals with a judicial action concerning the ownership of a house… Evidently the procedure was one of considerable importance so far as the Sealand was concerned for two Sealand officials acted as judges … Whether these two Sealand officials were in Babylonia because of weightier concerns or whether they were called from the Sealand cannot be determined. This uncertainty does not make it less significant that Sealand officials of such high position functioned in … the Babylonian government in the reign of Nebuchadrezzar [suffix omitted]. Professor T.J. Meeks has granted permission to publish the contents of a tablet in the twenty-first regnal year of Nebuchadrezzar … the translation … follows:

‘Six shekels of silver … the son of Mushézib-Marduk, and … the son of Nannâ, who [went] to the Sealand for the prefect [or governor] took for their provisions.’

“It is evident that this record refers to the dispatch of two men to the Sealand with revenue for a high official. The provenance of the tablet is not indicated, but it appears to be of the type found at Erech. The … ‘prefect,’ or ‘governor’ was presumably a representative of the Babylonian court’s interests in the Sealand. That the Sealand in the time of Nebuchadrezzar was intimately connected with Babylonia is definitely proven by this text. In a [further] document dated in the fourth regnal year of Nebuchadrezzar … there is a reference to … ‘Batatsu, the Sealander’ …”\(^{92}\)

Independent countries do not have foreign judges adjudicate civil cases as did the one in Nebuchadnezzar’s 17th year. They do not dispatch emissaries to send payments to foreign governors or prefects in their courts. They do not make governors of foreign lands “high” court officials who must be paid. Although they may hire foreigners these don’t generally become influential paid members of their

\(^{91}\) Ibid., p. 149

\(^{92}\) Ibid., p. 113-114
courts. All this strongly suggests that Nebuchadnezzar was under control by the Sealand and a vassal of it, as was Nabopolassar.

Also, the first form of evidence that ties this evidence to Nebuchadnezzar I and clearly not to Nebuchadnezzar II is that none of the documents go beyond the regnal year 21. Nebuchadnezzar I reigned between 20 and 22 years while Nebuchadnezzar II ruled for 37 years.

In the short chronology of the Neo-Babylonian Dynasty outlined in volume II, Nebuchadnezzar had to be sandwiched between Nabopolassar and Nabonidus who are separated by 25 years as Rose has demonstrated. This leaves 21 years for Nebuchadnezzar and four years for Amel-Marduk, Neriglissar and Labashi-Marduk. Since Nebuchadnezzar I reigned about 21 years and the cuneiform documents that connect him to the Sealand only cover 21 years, this is strong presumptive evidence that the Nebuchadnezzar in these documents is Nebuchadnezzar I. Daniel T. Potts actually identifies him as “Nebuchadnezzar I fourth king of the Second Sealand.”

All this evidence confirms that Nebuchadnezzar I was the second king of the Second Sealand Dynasty. These documents that refer to the Sealand do not date beyond Nebuchadnezzar I’s last reigning year.

The second form of evidence is that Nebuchadnezzar II reigned ca. 595-568/67 B.C. Although Greece existed at this time, there is no known evidence that it had a strong influence in Babylonia. But during the time of Nebuchadnezzar I, dated from -320 to -299, Greeks undoubtedly were in control of Babylonia and thus as the Sealand Dynasty would have enormous influence on its official business.

As for the last major king of the Neo-Babylonian Dynasty, Nabonidus, who is astronomically dated by Rose from 296 to 279 B.C., that is, he reigned for 17 years, there is a document from his fourteenth year that also connects him to the Sealand. According to Dougherty:

“In the fourteenth year of Nabonidus’ reign a document was recorded at Erech providing for the return of a man to the temple. The main attestor in this legal arrangement was … ‘Ahusnunu, the chief Bowman of the Sealand.’ These references to the Sealand in Neo-Babylonian texts representing occurrences in the everyday life of the people furnish evidence of a highly credible character. It is suggested with considerable certainty that the Sealand was an integral part of the Neo-Babylonian empire, and this accords with cuneiform testimony that the Sealand king., i.e. Nabopolassar, founded the Neo-Babylonian Dynasty.”

As for the three short-lived ephemeral kings at the end of this dynasty, Dougherty concludes:

93 Daniel T. Potts, The Archaeology of Elam (Cambridge UK 1999), p. 236
94 Dougherty, op.cit., pp. 114-115
“No available documents depict the status of the Sealand during the brief reigns of Amēl-Marduk, Neriglissar and Lābāshi-Marduk.”

But because of the legal documents of Nabonidus, Nebuchadnezzar I and that of Nabonidus’s 14th year, Dougherty asserts that “It appears, then, that the Sealand [Greeks] was not separate politically from Babylonia at any time during the Neo-Babylonian era.”

It is thus evident that wherever there are documents that refer to the First and Second Sealand Dynasties, these must refer to the time of Greek influence or interaction in Mesopotamia. Let us now briefly sum up the Sea Peoples, the First Sealand and Second Sealand Dynasties’ importance to chronology and history. The Greeks comprise these first two entities, the Neo-Babylonians under Greek Seleucid dominance the last. In Egyptian history and chronology, the Sea Peoples who oppose Ramses III are no longer an unknown people. They are the Greeks. The reasons for their migration to Egypt are no longer speculative or merely historical guesswork. Their place in history is fixed in detail while their place in history in the established chronology is nebulous at best. And their place in the chronology of the ancient Near East shows that the dates of Egyptian history into which historians have placed them are false. We know “who” they are, “why” they came to Egypt, and “when.”

The same condition pertains to the First Sealand Dynasty; in Mesopotamian history their homeland is no longer based on speculation and historical guesswork and lacking any archaeological evidence of its existence on the edge of the Persian Gulf or in Arabia. They are no longer a shadowy people, but the Greeks. The reasons for their invasion of Mesopotamia are also no longer conjecture and speculation. Their place in the chronology of the ancient Near East is also fixed while their place in that chronology shows that the second millennium B.C. period in Mesopotamia, in which historians have placed them, is false. We know “who” they are and “why” they came to Mesopotamia, and “when.”

As for the Second Sealand Dynasty, their place in Mesopotamian history is also no longer unknown or based on conjecture. They are just what historians say—they are the Neo-Babylonians. Their place in the history of Mesopotamia is fixed in some detail on this conclusion. Their place in that chronology shows that they do not come 500 years after the First Sealand Dynasty around 1200 B.C. or somewhat later, but they are contemporary with the First Sealand Dynasty. Their place in the second millennium, where the historians have placed them, is also false. We know “who” they are and “why” they reigned in Babylonia, and “when.” This is the greatness of the short chronology of Heinsohn, Rose, Sweeney and in large part Velikovsky.

95 Ibid., p. 118
96 Ibid.
Problems and historical issues that have defied solution for almost two centuries are resolved and the interconnections between the various empires of the Near East are fully integrated. Above and beyond all this, this history and chronology of the ancient world is supported by scientific and technological evidence, something almost totally lacking with that of the established chronology or that of James et al. and David Rohl. In order to place the Second Sealand Dynasty in the second millennium B.C. there must be scientific or technological evidence that corroborates the placement. The interpretation we have presented here does fit in with the forensic historical evidence, and therefore has a valid chronological foundation behind it. The concept that the Second Sealand Dynasty can be fit in the second millennium B.C. has no such support; therefore, it cannot be placed in that time.
CHAPTER 4

THE HYKSOS

Heinsohn and Sweeney have pointed out that much of the history of the ancient Near East has been triplicated. Civilizations that only existed in the first millennium B.C. had to be duplicated in order to fill the second millennium B.C. and then triplicated to fill the third. All this was done because of the historians’ belief that civilization began around 3000 B.C. In the present chapter we have a perfect example of this triplication. The great Assyrian empire of the first millennium B.C. (not the Neo-Assyrians/Persians) was duplicated and placed in the second millennium B.C. as the Hyksos who conquered Egypt. Then it was triplicated as the Akkadians of the third millennium. When we re-place the Hyksos and Akkadians with the Assyrians in the proper chronology of Heinsohn and Sweeney, all the numerous problems associated with the Hyksos and Akkadians are resolved as we found to be the case with the Sea Peoples. The Assyrians/Akkadians/Hyksos all conquer Egypt, employ the same weapons of war made of bronze, bring innovative technologies to Egypt. In so doing, they introduce the ancient Industrial Revolution to Egypt so that it trades with the rest of the civilizations clustered around the eastern Mediterranean. As part of this trade, new domesticated animals spread from the east into Egypt and Palestine which via trade move to the Aegean, to Greece, to Italy and the rest of Europe.

In this respect, when we speak of the Hyksos we also mean the Akkadians and Assyrians; we disagree with Velikovsky’s identification of the Hyksos as the Amalekites who he claimed fought against the Hebrews on their Exodus journey and invaded Egypt. While Velikovsky lowered Egyptian history by 800 years, that still required that Egyptian history/chronology had to begin around 2300 B.C. Heinsohn and Sweeney, however, suggest that Egyptian civilization began around ca. 1200-1100 B.C. Therefore, the Hyksos of the second millennium and the Akkadians of the third millennium B.C. could not have existed. Since this author, while following much of Velikovsky’s historical revision, supports Heinsohn and Sweeney because the forensic historical evidence on so many levels supports their revision, it will be shown here that this evidence explains the nature and history of the Hyksos in Egypt.

As with the Sea Peoples whom Velikovsky presented as the Persians and Greeks of the first millennium B.C., the very same condition pertains to the Hyksos, in that they, too, must be a nation that existed in the first millennium. There were only two great nations that built empires in that first millennium, that conquered and ruled Egypt before Alexander, namely, the Persians and the Assyrians. Therefore, based on these
considerations, the Hyksos, who came prior to the Sea Peoples, have to be the Assyrians. In fact, Israel Finkelstein and Neil Asher Silberman unwittingly suggest that this is what Manetho might have had in mind when he wrote of the Hyksos:

“The fact that Manetho, writing [of the Hyksos] fifteen hundred years later describes a brutal invasion rather than a gradual peaceful immigration should probably be understood in the background of his own times when … the invasions of Egypt by Assyrians, Babylonians and Persians in the seventh and sixth centuries BCE were still fresh in the Egyptian consciousness.”

Similarly, Cyril Aldred tells us: “Manetho’s account of the appearance of the Hyksos on the Egyptian scene as the irruption of a conquering horde spreading fire and destruction was colored by memories of more recent Assyrian and Persian invasions in his own time, and has to be discounted.” It is suggested that this conclusion should not be “discounted” but rather examined for the historical truth it contains. Here, too, we again encounter the very same problem we met with the Sea Peoples and Pereset, that is, they are a totally unknown horde that bursts upon history and then disappears into a black hole. Richard Garnett et al., over a century ago, described the situation thus:

“Some people of an unknown race coming from the mountains of the North, or from the East, established themselves in Mesopotamia … It may have been the same people which overwhelmed Syria and Palestine and entered Egypt as the Hyksos … On the Egyptian monuments the Hyksos are designated as foreigners; which may indicate that the dominant element of the invasion was the unknown race from the North, which had gathered to itself or taken into its service the Semitic population into whose former homes it had penetrated.”

Some historians have suggested that the Hyksos came from Syria or Canaan, or Arabia, or even Nubia. But since, according to the established chronology, Egypt was in contact with these people and countries, they would have designated them by their proper Egyptian name and the people that invaded and ruled Egypt would not be classed as some unknown foreign horde. The question of who the Hyksos were, where they came from and where they disappeared to, is like déjà vu with the history of the Sea Peoples. The literature on these questions is repeatedly negative. James Breasted writes: “If we ask ourselves the nationality, origin and character of this mysterious Hyksos empire we can hazard little in reply.”

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In terms of archaeology, André Parrot tells us: “In archaeology it is difficult now to unravel at once and with equal felicity the Hyksos enigma.”\(^5\) William Hugh Plommer calls “the Hyksos mysterious conquerors from Asia. Who these were and why they left so little behind in Egypt [is unknown].”\(^6\) E.A. Wallis Budge adds: “Of Salates the first Hyksos king nothing is known historically, and there are no monuments known which can correctly be asserted to be the work of the kings of the first Hyksos dynasty. The country from which the Hyksos came, also is unknown. Some Egyptologists consider the Hyksos to be Cushites, and some think they are to be identified with the Accadians; others again believe them to be Phoenicians or Semites.”\(^7\)

Not only do the historians not know who the Hyksos were or where they came from, or why they came, they don’t know how long they stayed, though they have reached a consensus about how long these enigmatic people ruled over Egypt. Henry Smith Williams explains:

“The length of the rule of the Hyksos is as unknown to us as the number of kings. Manetho makes two dynasties (Dynasties XV and XVI) rule, which, according to Josephus [who cites him] reigned 511 years altogether over the whole of Egypt, whilst the tables of Africanus gave 284 [years] to the XVth [Dynasty] (an evident misquotation of Josephus’ 260) and 518 [years] to the XVIth [Dynasty]. For the XVIIth Dynasty according to Africanus 43 [years] … and 43 Theban kings ruled 151 years … It is impossible for these figures to be correct, but there is no means of getting at the historical truth, even approximately.”\(^8\)

In this same respect, Kurinsky reminds us:

“The period of rule over Egypt by the [Hyksos] ‘hill-country kings’ has been designated by the historians as the ‘Second Intermediate Period’; … John Baines and Jerome Malek attempted to put Manetho’s perplexing rendition of that period into reasonable order. …

“The sincere attempt of Baines and Malek, and of other reviewers of conventional chronology, to squeeze the Manetho breakdown of a fragment of Egyptian history into rational segments is doomed to produce only a morass of contradictions. Examination of the mishmash of Egyptian history results from Manetho’s dynastic construction demonstrates that its acceptance as given is utterly naïve … hundreds of kings are purported to have ruled Egypt within a period of less than two hundred years.”\(^9\)

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\(^5\) André Parrot, *Discovering Buried Worlds* (Ann Arbor MI 1965), p. 59


\(^8\) Henry Smith Williams, *The Historians’ History of the World* (NY 1907), p. 120

\(^9\) Kurinsky, *op.cit.*, pp. 65-66
And how do we know with certainty that the Hyksos ruled for over 200 years? This is all assumed by historians to be the case, and this conjecture is passed along as historical reality. Budge illustrates the conjectural nature of this historical assumption: “The exact length of the Hyksos is unknown, but probably did not exceed 200 years.” On the other hand, Barbara Watterson suggests: “The Hyksos rule in Egypt lasted just over 100 years.”

What, then, of the Hyksos language? This, too, is unknown. According to Andrew Bruce Davidson et al.: “From the language [of the Hyksos] we can draw no arguments, for we know nothing of it save a few Greek translations of the royal names.” Sheila O’Connell-Roussell and Brian Singer-Towns speak of “the lost Hyksos language” while Ann Rosalie David reports, “the Hyksos do not appear to have had a common language…” Norman K. Gottwald adds “the Hyksos adopted the Egyptian language once they became its rulers.” As for literary evidence of their stay in Egypt, Legrand Clegg II tells us: “The Hyksos left no literary evidence of their occupation of Egypt. Indeed, they left practically no large monuments at all. What we do know of them has been painfully gleaned from a host of scarabs … cylinder seals, and a few other isolated objects …”

Regarding the religion of the Hyksos, almost nothing is known except that they worshipped a god equivalent to the Egyptian deity Seth. Thus Herman te Velde states:

“The religion of the Hyksos is only known to us from indirect sources. The Egyptians state that the Hyksos were worshippers of Seth:

“‘Then king Apophis made him Seth as lord, and he did not serve any god who was in the land except Seth. And he built him a temple as a perfect and eternal house beside the palace of king Apophis. …’

“Monuments of the Hyksos period, made by Egyptians for the Hyksos rulers and provided with hieroglyphic inscriptions, also afford evidence that the Egyptian artists represented the Hyksos as worshippers of Seth.”

Te Velde provocatively points out “After the Assyrian period there are hardly any indications of Seth-worship.” That is, te Velde admits that while ruling Egypt

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10 Budge, op.cit., p. 48
11 Barbara Watterson, The Egyptians (Oxford UK 1977), p. 60
12 Andrew Bruce Davidson et al., A Dictionary of the Bible (NY 1908), p. 660
14 Ann Rosalie David, The Egyptian Kingdom (Leiden, the Netherlands 1975), p. 20
15 Norman K. Gottwald, The Tribes of Yahweh (Sheffield UK 1999), p. 392
17 Herman te Velde, Seth, God of Confusion: A Study of His Role in Egyptian Mythology and Religion (Mrs. G.E. van Baaren, transl.; Leiden, The Netherlands 1977), p. 121
the Hyksos and the Assyrians had the worship of Seth central to their religions. The Assyrians worshipped Seth as Bel-Marduk or Baal-Zebub. Henceforth this connection and distinction will not be employed.

Breasted’s statement respecting our knowledge of the Hyksos is pregnant with great truth, “the late tradition regarding the Hyksos … is but the substance of a folk-tale like … many other such tales from which their knowledge of Egypt’s past was chiefly drawn by the Greeks.” Kurinsky finished this parade of ignorance about the Hyksos calling the historical account a “myth of a mysterious invading horde of an unknown race who came from an unknown area and disappear just as mysteriously.”

All in all, because of the exceeding length of the established chronology, practically nothing of historical substance is known of the Hyksos and their allies, just as we saw with the Sea Peoples. They are called a “people of an unknown race” who “established themselves in Mesopotamia.” They were “Asiatics for they are called so by the Egyptians,” “the Hyksos sojourn in and withdrawal from Egypt are all that happens in terms of historical fact.” Archaeologically “it is difficult now to unravel … the Hyksos enigma.” They “are mysterious conquerors” who “left little behind.” To accept Manetho’s account we must accept “a morass of contradictions” in “the mishmash of Egyptian history” which “is utterly naïve” or accept that “hundreds of kings are purported to have ruled Egypt within a period of less than two hundred years.”

The Hyksos subdued Egypt by “force” or “without the exercise of force.” Of their language “we know nothing of it save a few Greek transcriptions of royal names.” The Hyksos spoke a “lost language,” yet “left no literary evidence of their occupation of Egypt.” With them “we face a baffling phenomenon: the absence of contemporary records.” Of their religion, like that of the Assyrians, “The Egyptians state that the Hyksos were worshippers of Seth.” Their entire history reflects “but the substance of a folk-tale … from which their knowledge of Egypt’s past was chiefly drawn by the Greeks.” In essence Hyksos history represents a “myth of a mysterious invading horde … who came from an unknown area and disappear just as mysteriously.”

On the basis of this pseudo-evidence, historians have erected a chronological/historical structure which has been handed down to generations of students for over a century as a logical and rational explanation of reality. Therefore, our first concern is to determine on the basis of science and technology when the Hyksos existed. Proponents of the established chronology maintain that they arrived in Egypt around the early to mid-second millennium B.C. and departed sometime in the 17th century B.C. or later.

18 Ibid., p. 141
20 Kurinsky, op.cit., p. 63
TIN, WEAPONS, AND CHARIOTS

As with the Sea Peoples, whom we equate with the Greeks of the 4th century B.C., who had iron weapons because at that time they were well into the Iron Age and employed Greek weapons and armor of the 4th century B.C., because they were in fact the Greeks, we run into an almost identical situation with the Hyksos. The Hyksos, whom we equate with the Assyrians and Akkadians, had tin bronze because they lived in the first millennium B.C. when tin sources became known (as outlined in volumes I and II) and employed Akkadian and Assyrian weaponry of that time because they were in fact the Assyrians/Akkadians. We have repeatedly shown that tin to make bronze only came into the Near East, based on the established chronology, after 1100 B.C. and even closer to the present based on the short chronology. While the Sea Peoples had iron, the Hyksos had tin bronze which indicates that the Hyksos came prior to the Sea Peoples/Greeks. Kurinsky sums up the importance of the metallurgical contribution of the Hyksos to Egyptian civilization:

“Not least among the many marvelous materials to which the Egyptians were made acquainted during the time of [the Hyksos] rule was a diversity of metals …

“The quality of [Egyptian] copper was first improved by adding arsenic and then alloying it with metal, processes the Egyptians had never known … Egyptians were taught [by the Hyksos] to alloy copper with this magic metal [tin], to transform it into a new, harder, and more durable material, bronze. [Tin b]ronze tools could be honed sharper because bronze is harder [than copper]; they were longer lasting because bronze resisted decay better …”

Not only do the Hyksos bring tin bronzes to the Egyptians, but they bring weapons that originated during the Akkadian period centuries before the Hyksos and from the Assyrian period centuries after the Hyksos. That is, the Akkadians of supposedly 2300 B.C. and the Assyrians of supposedly the 1300’s B.C. have the same weapons as the Hyksos in Egypt of supposedly ca. 1800-1600 B.C. As the Sea Peoples employ weapons unique to the Greeks of the 4th century B.C., let us examine the weapons of the Akkadians/Hyksos/Assyrians that are also unique in their way.

The type of sword employed by the Akkadians, Hyksos, and Assyrians is known as the “sickle” sword or “scimitar” sword because it closely resembles a sickle. Heinsohn here speaks of these swords:

“The close relation between Hyksos and Old-Akkadian [weaponry is] recognizable from … the earliest sickle swords …”

21 Kurinsky, op.cit., pp. 114-115
“In the second half of the third millennium [B.C.], the earliest sickleswords make their début. These were the curved swords used for striking. They are clearly depicted on relics from the Akkadian period’ [writes Y. Yadin, The Art of Warfare in Biblical Lands in the Light of Archaeological Discovery (London 1963), p. 45]. Archaeologists assume that, beginning in Mesopotamia, ‘scimitars certainly very quickly spread to neighbouring nations as a weapon as well as a symbol’ [according to H. Bonnet, Die Waffen der Völker des Alten Orients (Leipzig Germany 1926), p. 92]. Yet [Bonnet adds on page 94], ‘there is no hint whatsoever that the scimitar came to Egypt before the time of the Hyksos’ … Thus, with a date of 2500/2400 [B.C.] for the earliest Mesopotamian scimitars, this powerful weapon supposedly spent at least 750 years on its journey to Egypt. Even more of a mystery was caused by the conservatism of the Hyksos. When they decided to adopt the scimitar they chose the Old-Akkadian model, which was ‘state of the art’ more than seven centuries earlier.”

There were various forms of the sickle or scimitar sword in Akkadian/Assyrian culture but that of the king was more ornamental, though its design as a sickle sword was unmistakable. One of these Assyrian scimitars was discovered in Palestine. According to Percy Stuart Peache Handcock, “the most remarkable bronze weapon recovered is the well known scimitar from Gezer … this fine scimitar is 1 foot 11 inches [about 600 millimeters] long … this weapon vividly recalls the bronze scimitar of the Assyrian king Adad-nirari I …”

In figure 2 are depicted sickle scimitars from the Akkadians, the Hyksos, and the Assyrian king.

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22 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 (Torino, Italy 1993), p. 212

At this point we ask the question: Since historians maintain that the Egyptians were trading with the Levant, Syria, etc. from Old Kingdom times and these scimitars were quickly spread around the ancient world, why did the Egyptians, who traded with this region, take 700 years or more to learn about and use this weapon? Were they just slow learners?

A second weapon invented by the Akkadians is the composite bow also employed by the Hyksos and Assyrians. According to Robert Drews, “Essentially, the chariot became militarily significant when it was combined with another intricate artifact [of war], the composite bow, which was also known for a long time.”

To the Egyptians, Asiatics were known as bowmen or bowman. David P. Silverman explains: “The attitude to the Asiatics among the Egyptian population was that they were ‘vile,’ ‘wild,’ ‘doomed,’ ‘the abomination of Re.’ The following text … paints a vivid picture: ‘speak now of the Bowman! Lo, the vile Asiatic…”

The same conundrum exists with regard to the bow as with the scimitar, as Richard Gabriel comments:

“The first appearance of the composite bow is found in Akkad on a relief picturing the warrior-king Naram-Sin. The relief dates from 2200 B.C., and it may well have been the composite bow that allowed the early Akkadians to triumph over enemies armed only with the simple bow.

“The spread of the composite bow to other areas of the Middle East was comparatively slow, perhaps because of the complexity and cost of manufacture, as well as a desire to conceal the technology as a military secret. The composite bow does not appear in Egypt for at least a thousand years after its appearance in Mesopotamia and, like the chariot, was introduced by the Hyksos.”

A further problem with the composite bow as it relates to the Hyksos is that it is made of materials not found in the Near East. William Hayes explains this aspect of the composite bow in that it “is bound the whole of its length with birch bark.

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26 Richard Gabriel, *The Culture of War: Invention and Early Development* (Westport CT 1990), p. 43
The tree from which the bark was obtained, the European white birch, is not found in the Near East south of Cappadocia [Turkey], and was imported from some country well to the north of Egypt.”

In order for the Hyksos to have obtained this birch bark, they had to have access to and trade with the region north of Cappadocia. This is also a significant problem for the Akkadians of 2300-2200 B.C. However, when we equate the Akkadians and Hyksos to the Assyrians, the problem vanishes. As was pointed out in volume II, the Assyrians were carrying on extensive trade in metals and textiles with Anatolia [Turkey]. Trade with Assyria, the great empire that dominated the ancient Near East, would bring European products into Akkadian/Assyrian/Hyksos Egypt.

A third weapon of war that connects the Akkadians and Assyrians to the Hyksos is the chariot. It is generally held that the Hyksos had war chariots, but war chariots did not become prevalent until around 1800 B.C. or somewhat closer to the present. These chariots, too, were made largely of tin bronze. Yet again, tin was not available in Mesopotamia to build large battalions of these vehicles. In fact, according to Drews, even in the 1700s B.C. the chariot was seen rarely because so few of them existed:

“In short, early in the eighteenth century B.C. one could occasionally see, in the northern tier of the Fertile Crescent, horses trotting along a road from one city to another, drawing a ‘chariot’ (that is a two-wheeled cart, the wheels being spoked, and the entire vehicle being of relatively light construction). But the scene was exceptional and it is important to appreciate the fact that at least in the Fertile Crescent the horse and chariot did not have any military value …”

In fact, Drews elsewhere informs us that it wasn’t until the 1600s (17th century B.C.) that the war chariot comes into its own.

“The recent scholarship on technical aspects of the chariot permits us to establish approximately when chariots became militarily significant. The era of the war chariot … began in the seventeenth century B.C. Before that time chariots seem to have been of little importance on the battlefield.”

The great problem with this chronological interpretation is that, as shown in volume I, the scientific stratigraphical dig at Tall Munbaqa proved that the period between 2300-2200 B.C. (the time of the Akkadians) and around 1650 B.C. (the time of Mitanni/Medes) simply did not exist. Therefore, the dating of the use of chariots for warfare, in Mesopotamia and elsewhere, must be lowered. That is, the war chariot would have come into being hundreds of years later. And as will be illustrated in the unit on “stratigraphy,” the military chariot had to have its origin in the first

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29 Drews, *The End of the Bronze Age, op.cit.*, p. 105
millennium B.C. If one keeps the Hyksos in the time frame of the established chronology of Egypt, which itself is scientifically and technologically untenable, they could not have had chariots. They supposedly came to Egypt around 1700-1600 B.C., before the chariot, as a military weapon, was employed in Mesopotamia.

Since Heinsohn and Sweeney equate the Hyksos with the Assyrian empire, the evidence regarding the chariot should show that only a powerful, well-funded, and rather highly advanced nation could manufacture it in large numbers. The Hyksos do not fit that description. Again we turn to Drews who outlines the fact that keeping a chariot army in the field entails costs that only a highly advanced empire could possibly afford:

“The limitations on the size of a chariotry [army] were imposed most of all by the enormous expense of maintaining one …

“Defensive armor for the chariot crew (and sometimes even for the horses) was a major expense.”

The problems only multiply when it comes to the expense and maintenance of the chariots themselves. The years I spent in the U.S. Army in a maintenance battalion showed that there is much more than building various machines for warfare entailed; namely well trained and efficient repair battalions to service these machines. Of this aspect of chariotry Drews further explains:

“Apart from the expense of purchasing all these items, and of hiring all the necessary specialists (charioteers, chariot warriors, grooms, veterinarians, carpenters) there was the matter of [providing] food. Stuart Piggott has estimated that eight to ten acres of good grain-land would have been required to feed one team of chariot horses …

“Keeping track of the chariots and charioteers required a small bureaucracy of clerks and quartermasters.”

Small barbaric states do not raise armies with thousands of chariots. Gabriel puts the entire nature of this expense into clear perspective:

“Manufacturing and maintaining a chariot corps was a very expensive business somewhat similar to the problem of sustaining a modern armored corps. In the Egyptian case, almost all parts needed to make the machine strong yet flexible, had to be imported from outside Egypt. Construction required skilled craftsmen in significant numbers who worked in permanent factories. Special repair shops had to be maintained, and when on the march, mobile repair and parts battalions were needed to keep the machines combat-ready. Like modern armored forces, only the wealthiest of states could afford to produce and maintain … chariotry in substantial numbers.”

30 Ibid., p. 110
31 Ibid., pp. 111-112
32 Gabriel, op.cit., p. 42
It should be pointed out that Drews tells us:

“Composite bows were also notoriously expensive. Such a bow was a very effective weapon, having double or triple the range of the self bow, but its manufacture was costly and difficult (the layering and laminating of wood, horn, and sinew was done at long intervals, and a properly aged bow would leave a bowyer’s shop five or ten years after he had bought in the raw materials from which it was made).” 33

Above and beyond these weapons, Heinsohn cited R.M. Emberg’s The Hyksos Reconsidered (Chicago IL 1939), pages 43-44, regarding other weapons: “It is certain that the earliest dated specimens of forms like, or comparable to, some metal implements [of war] regarded as Hyksos have come from Mesopotamia. Specifically … crescent shaped dagger pommels and socketed axeheads.” But as we pointed out in volume I, page 230, the socketed axe, spear, or other tool in Egypt “remained unsocketed for nearly 2000 years.” Why, after the Hyksos introduced the socketed tools, didn’t the Egyptians make and employ them for hundreds of years after the Hyksos left Egypt? Like the sickle sword, composite bow, and the chariot, the anachronistic nature of the socketed weapon and tool points to the first millennium.

STRATIGRAPHY

According to the established chronology the Akkadians came around 2300-2200 B.C. and the Old-Assyrians around 1300 B.C. Yet, as one and the same civilization, if they lived in the first millennium B.C., their materials should always be found in contexts at archaeological digs at the identified stratigraphic level beneath the Greeks, and beneath the Persians (that is the Old Babylonians/Persians and the Neo-Assyrians/Persians), and finally directly beneath the Mitanni/Medes. They should always be located in the third strata group beneath the Mitanni. That we have shown in volume II, pages 289-291. This fact was emphasized by Heinsohn:

“What nation settled the strata … immediately below the Mitanni… layers in Northern Mesopotamia? When the most careful excavations of the 1920’s and 1930’s tried to answer this question, the archaeologists found themselves in confusion , … [for the sites of] Tell Billa …, Nuzi …, Tepe Gawra …, and Chagar Bazar … contained … Old-Akkadian … remains [directly beneath the Mitanni remains]”. 34

Interestingly, there was supposedly about a 750-year settlement gap placed in the archaeological charts, presented by the archaeologists although no windblown materials were ever shown to exist between the Akkadian stratum and that of the Mitanni, which is the situation at Tall Munbaqa. That being the case in Mesopotamia,

33 Drews, The End of the Bronze Age, op.cit., p. 110
34 Heinsohn, op.cit., p. 209
we would expect to find that the Hyksos stratum also lies directly beneath the Mitanni in Syria and Palestine. On this point, Heinsohn adds: “In this respect, they strikingly resembled the Middle Bronze [Age] Hyksos immediately preceding [lying directly beneath] Mitanni in Syro-Palestine …”35 Heinsohn earlier reported: “the Hyksos of Syro-Palestine’s Middle Bronze Age … stratigraphically … also precede [lie directly beneath] the Mitanni …”36 Sweeney expresses it this way: “The archaeologists working in Syria/Palestine had to designate the Hyksos strata [found there beneath the Mitanni] as Middle Bronze 2 because they had already a priori placed the Hyksos almost a thousand years after the Akkadians …”37

The Hyksos, dated from ca. 1700-1650 B.C., are found directly beneath the Mitanni in Syria and Palestine. The Akkadians dated to 2300-2200 B.C. are found directly beneath the Mitanni. The Old Assyrians dated to around 1960-1400 B.C. are found directly beneath the Mitanni. And all without windblown materials: “… none of the hiatuses [settlement gaps] was ever proven [to exist] by windblown layers …”38 Here then the massive problem of why this exists must be explained if the established chronology is to be sustained. The only answer is that some form of erosion, denudation, excavation or other process was at work to allow three civilizations dated to three different times—the Akkadians ca. 2300-2200 B.C., the Old Assyrians dated to ca. 1960-1400 B.C., and the Hyksos dated to ca. 1700-1650 B.C.—to all lie directly beneath the Mitanni strata dated to ca. 1500-1360 B.C., based on the established chronology. In order for this to have happened, these processes for each always needed to have operated at uniquely different rates. For the Akkadians it worked with vigor and removed about 750 years of various materials, relics, etc. For the Old Assyrians it didn’t work at all and therefore removed nothing of these various materials, relics, etc. In the case of the Hyksos, these processes operated weakly. And miraculously, in each case it stopped so that each of these civilizations’ strata would lie directly beneath that of the Mitanni. Those who accept that such a state of affairs can occur evidently subscribe to Mark Twain’s dictum “Apparently there is nothing that cannot happen.”39 On the other hand, those who subscribe to the canons of forensic history outlined in these volumes, will inevitably subscribe to philosopher “David Hume’s insistence that belief in miracles [must] yield to canons of evidence.”40 Oscar Handlin goes on to say:

35 Ibid.
36 Ibid., p. 208
37 Emmet Sweeney, The Pyramid Age (Corby UK 1999), p. 35
38 Heinsohn, op.cit., p. 210
39 Mark Twain, Mark Twain’s Autobiography. The Chapters from the North American Review (Madison WI 1990), p. 9
40 Oscar Handlin, Truth in History (Cambridge MA/London 1979), p. 85
“The miraculous and providential ways of understanding the past disappeared, not so much through brutal attacks, as in Thomas Paine’s *Age of Reason*, but more important in a slow, subtle alteration which made the extraordinary manifestations of God’s [miraculous] hand less relevant than the continuing processes of the world of nature. The events of the past transpired within a regular order. Not that every occurrence was entirely rational, but even irrational … phenomena were open to understanding by the human mind because natural laws governed all.”

The laws of geology and logic do not permit such a unique miraculous situation to exist wherein the Akkadians, the Old Assyrians, and the Hyksos all living in the Near East at different times left their materials directly beneath the Mitanni living at one time.

What does this indicate? Since all these civilizations are stratigraphically located directly below the Mitanni without windblown materials between them, and as shown in volume II, chapter 3, the Mitanni are the Medes of the first millennium B.C., then all these civilizations are in fact the Assyrians of the first millennium who were conquered by the Medes. It is the Assyrians whom historians have misidentified as the Hyksos that invaded Egypt, and this relates directly to the view that the Hyksos could possess the bronze, sickle swords, composite bows, and war chariots. As the Assyrian empire, these invader-conquerors had access to materials from all across their empire and beyond, through trade. The birch bark that covered their composite bows could be shipped from southern Europe via Anatolia where the Assyrians had trading colonies. From this region they also obtained their tin bronze, as outlined in volume II, chapter 4. As for the ability to obtain large funds to buy materials to build composite bows, manufacture chariots, obtain horses, pay for charioteers, chariot warriors, grooms, veterinarians, carpenters, food for the horses, a bureaucracy of clerks and quartermasters, and keep repair battalions in the field, the Assyrian empire’s wealth was enormous. How could the Hyksos, being an unknown, semi-barbaric people, have an army with many hundreds or a few thousand chariots without an empire to tax in order to support all that having such weapons of war entails?

It may be argued that there is no evidence that the Hyksos possessed chariots as summed up by Ahmed Osman:

“There is nothing in this evidence to support the view that the Hyksos used swift chariots in their conflicts with the Egyptians, either at the time of their invasion of the country or their expulsion. It would appear that another quarter of a century elapsed before chariots became an instrument of warfare …

“The fact of the matter is that the whole question of the use of chariots has entered the debate as an assumption on the part of some scholars because they found the Egyptian kings of the Eighteenth Dynasty [who came after the Hyksos]...”

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using chariots. Where did they come from? They concluded that the chariot must have been introduced at the time of the Hyksos invasion.”\textsuperscript{42}

Osman also turned to John van Seters’s work on the chariot in Hyksos times, who because of the lack of evidence regarding this matter suggested: “The question of chariots, therefore, ought to be dropped from discussion of Hyksos origin.”\textsuperscript{43}

Nevertheless, Osman has also cited a limestone stele of Pharaoh Kamose, who began to drive the Hyksos out of Egypt, which was found in 1954 by Mohammed Hammed and Labib Habachi at Karnak, and in which Kamose describes his intent to wreak havoc on these foreigners:

“I shall drink the wine of your vineyard …, I shall lay waste your dwelling; I shall cut down your trees; I shall drag your women to the ship’s hold and I SHALL SEIZE YOUR CHARIOTRY.”\textsuperscript{44}

To escape the obvious fact that Kamose claimed that the Hyksos had chariots prior to their expulsion from Egypt, Osman argues “There is no mention … of this chariotry having taken part in any fighting and Kamose himself refers to having sent his ‘strong troops … on foot’ when he set about the task of quelling the remaining rebels behind his lines.”\textsuperscript{45} The problem Osman never addresses is: Why would Kamose claim that the Hyksos had chariots if they had no such weapons? That the Hyksos/Assyrians/Akkadians withdrew these weapons from Egypt after taking the country seems rather obvious. Once they had subdued the land, rather than keep in Egypt large numbers of very important weapons that were extraordinarily expensive, they pulled the weapons back to Assyria to be ready to attack new enemies and defend their homeland. The assault on their base in Egypt was evidently swift and thus they could not move reinforcements to Egypt in time to halt the rebellion. They may also have had problems much closer home which occupied them. Nevertheless, Kamose declares that the Hyksos had chariots even though certain historians deny this obvious fact. If they never had war chariots, how could Kamose know they did? The Hyksos did have chariots and as van Seters said, “The question of chariots … ought to be dropped from discussion of Hyksos origin” because the only way Kamose could say the Hyksos had them is that he was completely aware of this fact and said so. After all, the Egyptians destroyed all mention of these foreigners who dominated them for about a century, so one could hardly expect to find evidence of them. Was Kamose clairvoyant and did he know that chariots would be employed in the future by his Egyptian heirs?

\textsuperscript{42} Ahmed Osman, \textit{The Hebrew Pharaohs of Egypt} (Rochester VT 2003), p. 69
\textsuperscript{43} \textit{Ibid.}, p. 70
\textsuperscript{44} \textit{Ibid.}, p. 66 (capitalization added)
\textsuperscript{45} \textit{Ibid.}, p. 69
THE HYKSOS LANGUAGE

Since we equate the Hyksos with the Akkadians and Old-Assyrians, it is only to be expected that the language they spoke and wrote was Akkadian. Because the Egyptians evidently removed everything of the Hyksos reign after expelling them, it is suggested that their language is unknown. Nevertheless Yigal Yadin et al. found in an excavation at Hazor, Palestine three jugs dated to the Hyksos period with writing on them which was undoubtedly Akkadian. As Yadin et al. stated that “an inscription was found incised in cuneiform, the earliest in this script to be discovered in Palestine.” The inscription was published in Yadin’s book and is reproduced in Figure 3.

Figure 3

2. Old Akkadian (Early Bronze V) inscription on jug C339/1 from Hyksos Middle Bronze II (strata 4-3) at Hazor

Instead of this language being some variant of Western Semitic which was spoken in Palestine in the time of the Hyksos, it was clearly Akkadian, the language of the Akkadians spoken 750 years earlier. Yadin immediately understood that these Hyksos jugs proved “the fact that the grammatical form of the name is Akkadian and not Western Semitic,” as was expected. A. Malamat further explained:

“It is instructive that the first element of the name [on the cuneiform inscription at Hazor shown by Yadin et al.] has the Akkadian form is me (i.e., ‘he has heard’) and no the West Semitic form one would expect is amah as in the name Isamah Adad known from Mari.”

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46 Yigal Yadin et al., *Hazor II* (Jerusalem 1960), p. 117
This material was taken from Heinsohn and Sweeney without attribution. After the Hyksos expulsion by the 18th Dynasty, that dynasty employed Akkadian to communicate with their foreign neighbors. This suggests that the Egyptians were closely integrated in Hyksos times with Mesopotamia and corresponded with its foreign centers via the international language of that time, Akkadian.

Thus the Hyksos had tin bronze which could not be produced in the second millennium B.C. because there were no sources for tin at that time. This places them in the first millennium B.C., in the time of the Assyrians. They employed weapons, the sickle sword or scimitar, composite bow, and the war chariot, the very weapons of the Assyrians. Their relics are found directly beneath the first millennium Mitanni/Medes as are those of the Akkadians and Old-Assyrians, which requires that they all be placed in Assyrian times. The language they employed is Akkadian, the same language used by the Assyrians. They worshipped the Assyrian equivalent to the Egyptian god Seth. Even before they invaded Egypt, they were a wealthy empire that could tax its subject states to finance its massive military machine. While the historians have been toiling for a century, to no avail, to discover who the Hyksos were, where they came from, why they invaded Egypt, and by what means they did so, as well as when these historical events occurred, the short chronology, as it does with the Sea Peoples, shows “who” they were: the Assyrians; “where” they came from: Assyria; “why” they invaded Egypt: to extend their empire; the “means” by which they did so: a great military war machine with sickle swords, composite bows, and chariots; and “when”. The historians may continue to toil to resolve all these issues to no end for who knows how long before the realization dawns that it is the chronology to which they adhere that is the root of their problem.

But there is more: the burials of the Hyksos are distinctly Akkadian/Assyrian. The capital of the Hyksos in Egypt was a stronghold in the Nile Delta, Avaris, today called Tell el-Dab’a. Their burials as investigated by Manfred Bietak indicate that these conquerors were urbanized and sophisticated rather than, as some have suggested, barbarian nomads. As Bietak is cited by Heinsohn to show their Akkadian/Assyrian connection:

“‘There is abundant material for the study of the cultural appearance of the Middle Bronze Age in the Delta available at Tell el-Dab’a. The cultural background of [the] population was urban rather than nomadic. Indicators in this respect are the intramural burials, a typical urban custom not practiced by nomads, and distinct architectural traditions of this culture, e.g., the vaulting technique [for their tombs]. The majority of tombs was constructed according to a technique which had a tradition in [25th/24th century …] Mesopotamia [the time of the Akkadians] but not in Egypt. The Middle Bronze Age element [of these burials] in
the town site seems to have been increased [over time] by a new influx [of people] which most likely was responsible for the rise of the Hyksos rule in Egypt.\footnote{49}

Why would the Hyksos employ a form of burial that supposedly was used in Akkadian Mesopotamia 700 years earlier, unless they were Akkadians/Assyrians? What made them imitate the burials of a people long since lost to history and forgotten unless they themselves were the Akkadians/Assyrians? Not only did the Hyksos imitate these past forms of burial, but because they had become in a sense Egyptianized, they introduced an innovation in coffin forms, which displayed great skill in its use of wood. Kurinsky in this respect shows:

“The more sophisticated carpentry of the Asiatics and the availability of suitable woods quickly brought the anthropoid coffin into Egyptian popularity [this coffin had the shape of a person with a highly rounded mold, rather than oblong cornered edges]. The introduction of tenoning [projections that fit snugly into deep grooves to give the coffin strength] and other advanced woodworking techniques made the intricately constructed sarcophagus possible and cheap enough ‘so that even a moderately well-to-do citizen could afford a set of two nested one within the other’ [like nested Russian decorative eggs]. The decoration of the first of these coffins incorporated a Mesopotamian motif which harkens back to early Akkadía, [displaying] the enveloping wing of a raptor or of the fabled griffin...\footnote{50}

Again, why would the Hyksos harken back to the Akkadian period by using an Akkadian motif on their coffins unless they were Akkadians/Assyrians? What made them imitate the motifs of a people long since lost to history and forgotten unless they were the Akkadians/Assyrians? In these cases of burial, (1) the construction of the tombs and (2) the motifs on the coffins are Akkadian/Assyrian, which, like the sickle sword, composite bow, and war chariot copy Akkadian forms used 700 years earlier.

In terms of burials at Avaris, we further encounter an extraordinary anomaly. As pointed out in volumes I and II, there is clear-cut scientific, technological, and other evidence that the allies of the Assyrians—the Scythians—were buried in the Royal Cemetery of Ur dated to around 2300 B.C. Bietak, during the 1997 excavations at Tell el-Daba uncovered a mortuary precinct with several cemeteries directly dated to the Hyksos period. In one of the vaults there was uncovered the burial of a warrior with a sacrificial horse at the entrance to his tomb, and next to one wall was found a woman that had been sacrificed and placed there at the same time the warrior was entombed. In an Internet article, “Recent Find of A Warrior


\footnote{50}{Kurinsky, \textit{op.cit.}, pp. 118-119, emphasis added}
Tomb with A Servant Burial in Area A/II at Tell el-Dab’a in the Eastern Nile Delta,” we find:

“... when the tomb was opened by the excavators [a] single equid [horse] skeleton was found in the entrance area together with a round bottomed cup and a jar. Next to the northeastern wall a young female servant was buried in a slightly contracted position looking towards the tomb chamber. ... Because of the circumstances of this and other burials of the period there is a strong possibility that the girl was offered to her master as a human sacrifice. ... He was buried with his weapons and an assemblage of different pottery types. Bones of goats or sheep placed on a dish next to his head are [the] remains of a meat offering.

“The tomb [like Scythian tombs] is accompanied by several other[s] and seems to be at the centre of the group, possibly a hint at social implications.”  

Bietak discusses these, stating there are:

“at least two tombs of servants who seem to have been intended to accompany their master to the next world ... very likely ... this was a secondary burial.”

Unable to comprehend these as Scythian entombments, Kathryn A. Bard remarks: “Hyksos burial customs are unique, especially the burial of equids.”

All these aspects of the burials are Scythian but the historians cannot conceive that a 1700-1600 B.C. grave in Egypt could hold an armed Scythian warrior with sacrificed horse, and servant, and food for the afterlife. As with the Scythian burials in the Royal Cemetery at Ur, dated to Akkadian times, the same questions will arise: “who then were the people who received such rites?”, “who were the sacrificial victims,” this cemetery “presents historians with very difficult problems,” they are a “Great Enigma.” But since the Scythians were allies of the Assyrians, of course a contingent of these warriors would follow them to Egypt.

Herbert Butterfield in his analysis of certain historical problems that have defied solution points to the rigidity of historians thus:

“... it is a common defect of historians to poke the new evidence into the old [chronological] structure of story, instead of reducing the whole narrative to its primary [scientific and technological] materials, and then putting the pieces together again in a genuine work of reconstruction. A child of seven, fresh from the bosom of nature, would hardly fall into the error; but, as in the case of a rusty man-trap which I once saw on exhibition, it takes the languor and rigidity of a heavy adult to be caught in such a contraption. Because the old story has dug itself deep, and made many grooves in our minds, we must always ... churn [recalcitrant

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53 Kathryn A. Bard, Encyclopedia of the Archaeology of Ancient Egypt (London/NY 1999), p. 218 
54 citations from Charles Ginental, Pillars of the Past, volume I, op.cit., pp. 311-312
evidence] … somehow into our system, or alternatively we leave odd and unrelated facts—refractory snippets of story—hanging about. … Sometimes, we might feel, it would even be better if the historian could actually go to work with a mind unloaded of all hypotheses—could collect facts and amass his microscopic details, and place everything in [the correct] chronological order, until the moment comes when he can brood over the whole without any parti pris [preconceived idea]. It might be better if he could wait until the bubbles of mercury jump together and run into a single shape—wait until the pattern begins to stare at him from a multiplicity of facts …”

As with the time consumed by historians who have wrestled with the problems of the Sea Peoples and the Hyksos, Butterfield’s insights are à propos:

“We may note in conclusion that, so far as we can gather from the evidence on this particular subject, a hundred years of historical enquiry may carry students further from the truth than they were at the beginning. It may take one hundred and fifty years before the most critical problem is brought to the consciousness of historians or the most acute of the controversial issues is raised. Even if by this time sufficient clues are in the hands of scholars, further delay may arise because of the tendency to fit new evidence into a framework that has been allowed to become too rigid. … So there does not appear to be a point where the mind of the historian can safely rest. … we must not imagine that everything has been settled.

“… we have seen that it is possible for retrogression to take place in historical science if students lose touch [with the facts of science and technology].”

THE AKKADIAN, ASSYRIAN, HYKSOS
ANCIENT INDUSTRIAL REVOLUTION

In volume II, chapter 4, we described the trade in certain goods carried on by the “Old Assyrians and Akkadians” whom we dated to the first millennium B.C. In that chapter on the far-flung trade that spread from Europe, the Aegean, Anatolia, and other regions, we demonstrated the size and range of goods transported into Mesopotamia and the wealth that accompanied it. David Christian describes this trade and the nature of the Akkadian/Assyrian empire:

“During the reign of Sargon of Akkad (who ruled from ca. 2350 BCE for ca. 50 years), we have the first evidence for a new stage in state formation: the appearance of a state controlling several different city-states and their hinterlands. Sargon claimed to feed 5,400 men every day, a figure that may indicate the size of his retinue. Using what may have been the world’s first standing army [which his

55 Herbert Butterfield, Man on his Past. The Study of the History of Historical Scholarship (Boston 1955), pp. 159-160
56 Ibid., pp. 169-170
empire could afford to supply with food, military weapons, etc.), he defeated rival city-states. Then, instead of merely exacting tributes from them, he incorporated them into his own empire by demolishing their walls and appointing his own sons as ensis, or governors. He also supported trade networks that reached throughout Mesopotamia and as far as central Asia and the Indus valley, as well as through Egypt and into sub-Saharan Africa. Mesopotamia acted as a main hub for these networks, but the density of settlement and the scale of political power under Akkadian [Assyrian] rule probably also made it the first ever center of gravity of a regional network of exchange.

“What it meant to be at the center of these widespread networks of exchange, where wealth and information were pooled in huge quantities, is suggested by this description of the Akkadian capital, Agade, from early in the second millennium [B.C.]:

“In those days the dwellings of Agade were filled with gold,
its bright-shining houses were filled with silver,
into its granaries were brought copper, tin, slabs of lapis lazuli, its silos bulged (?) at the sides …
its quay where the boats docked were all bustle …
its walls reached skyward like a mountain …
the gates—like the Tigris emptying its water into the sea,
holy Inanna [the goddess Venus] opened its gates.”

Therefore, the Hyksos/Assyrians and their allies in Egypt were at this time also part of this great trade which enriched and changed Egyptian culture in many ways. These numerous contributions brought by the Hyksos/Assyrians indicate the greatness of their state and its development. With the conquest by the Hyksos/Assyrians, Egypt entered the ancient Industrial Revolution and imbibed of it multifaceted gifts which were transforming the entire ancient Near Eastern world from a largely agrarian economy to one which included numerous technological changes in almost every field of material development and culture. Because of this the Hyksos/Assyrians in Egypt came into commercial contact with distant regions to which their materials were sent or from where they imported other materials.

In terms of practical innovations brought to Egypt by the Hyksos, Michael Haag shows that

“Hyksos contact … brought many innovations to Egypt, among them … an improved potter’s wheel, new vegetable and fruit crops and also humped-backed cattle (zebu) …”

With respect to the zebu, which will be discussed below in depth, Kurinsky tells us:

“[A] great beast was brought into Egypt [by the Hyksos] …, the … zebu from India, which was bred to be well adapted to the climate and conditions of the area. These beasts did not require extensive range, for they made efficient use of

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available fodder. The cows supplied milk, cheese, and meat to the hungry population and were excellent for pulling plows, an onerous labor that had been performed by men. No longer would the fellahin, back bent straining against leather straps which leashed him to the plow, fall faint from exhaustion in his effort to grow grain. The zebu made excellent draft animals and performed useful work, such as lifting the waters of the Nile and its canals into the irrigation ditches by means of levers and turnstiles …

“The Egyptians were not altogether ignorant of the use of animals in place of men; they had learned from the Nubians that the long-horned Nubian cattle were of use in plowing, the scenes of crude plows leashed to horns of single cows are to be found in earlier Egyptian scenes. The introduction by the [Hyksos] of the two-handled plow, to teams of zebu oxen and of new methods of yoking made the process of plowing vastly more efficient. The largest and most explicit relict in the collection of the [New York] Metropolitan Museum of Art, which was taken from the forehall of a Theban [18th Dynasty] tomb chapel, reveals the radical changes in agricultural technology. Hayes pays particular attention to the change in harnessing. ‘The yoke of the interesting two-handled plow is not lashed to the horns of the beast as was the [earlier] Egyptian custom, but rests upon their necks forward of the upward projecting [zebu] humps. Even more extraordinary is the two-wheeled ox-cart appearing in the register.’

“Thus instead of carrying the wheat or barley [harvested] in huge hampers slung over the shoulders of fellahin, or at best in bulging bags slung over Asiatic donkeys’ backs, or piled on sledges and dragged, great loads were laden onto a cart with open latticed sides and driven to the threshing floor. The cart is provided with four spiked wheels similar to those of the chariots but is shown to be of sturdier construction.”

There were also “New fibers and new fast dyes [introduced which] made fabrics more durable and colorful and added another dimension to the quality of life.” Along with that, “The upright loom, long known in the lands to the east, revolutionized the Egyptian weaving craft. As productivity was increased, the cost of fabrics was reduced and they became universally available.”

As with all these innovations found in 18th Dynasty contexts, it is often argued that the Hyksos did not introduce these novelties. However, because the scientific and technological evidence places the 18th Dynasty well into the first millennium B.C., as outlined on volume I, the Hyksos had to come just prior to the 18th Dynasty, but that requires that they be placed in Assyrian times, and there is no doubt that the Assyrians had all these advances. Until the historians have a scientific

59 Kurinsky, op.cit., pp. 116-117
60 Ibid., p. 116
61 Ibid.
and technological foundation for the established chronology, their argument cannot be sustained. All these innovations existed in Mesopotamia prior to the 18th Dynasty, and the empire that preceded them, the Assyrian/Hyksos, also possessed these innovations. Why wouldn’t the Assyrians/Hyksos carry these to Egypt?

Along this line of textile innovation, Frederick Monderson cites Samkange (1971, page 64), who “credits the Hyksos with also making peaceful additions to the culture they found in the Nile Valley … such as improved methods of spinning and weaving, [and] using the upright loom.”62 With respect to new crops cultivated by the Hyksos, Pierre Montet informs us that: “The pomegranate, the olive and the apple tree were first introduced in the Hyksos period and were regularly and successfully cultivated thereafter.”63

Because the Hyksos were Mesopotamian Assyrians, they imposed their own system of weights and measures on their Egyptian subjects. To keep records that were understandable to them they shunted aside the Egyptian system and used their own. Kurinsky explains:

“The regulation and expansion of trade from [Hyksos] Egypt to its neighbors and the regionalization of commerce are unmistakably manifested by the fact that basic standard weights [previously] used in Egypt and other standards were replaced by those of Mesopotamia.”64

In order to carry on this international Mediterranean trade, improvements in Egyptian ships were needed and were added. Kurinsky shows:

“The Egyptians had long sailed the Nile in feluccas, simple boats that were handled adeptly. These boats could not be easily managed on the high seas, … for they lacked keels. They could ill withstand the buffeting [strains] of ocean waves or be kept on an even course, and so were restricted to plying placid river waters or to maneuvering along the shore of a peaceful sea in benign weather. The [Hyksos] had long since learned to affix a keel to the bottom of boats, a revolutionary device that stabilized them, made them more maneuverable, safe, and seaworthy.

“Seaworthy ships opened a new trading era for Egypt, for until then the activity of Egyptian merchants was largely limited to places accessible by land. Trade with the islands of the Mediterranean blossomed and flourished, and Egypt became a more important factor in the economy of the region.”65

Because we place the Hyksos/Assyrians in early mid-first millennium times, they would have been contemporary with the Minoans and Mycenaean. When the

64 Kurinsky, op.cit., pp. 125-126
65 Ibid., p. 118
traders of these Aegean regions and Greece came to Avaris, they brought their weapons with them and thus these would be found in Egypt as well as their homelands. Martin Bernal reports that “at Tell el-Dab’a, the Hyksos capital of Avaris … double-edged bronze weapons can be found… Furthermore, the double-edged blades found in the Shaft Graves of Mycenae depict strictly Egyptian scenes.”

Meres J. Weche too shows that “the same type of dagger found at Tell el Ajjuj (Sharuhen) levels associated with the Hyksos … are found in Crete.”

The connection of the Hyksos with the Aegean is well exemplified in mural art. Of this connection Christian J. Mamiya and Fred S. Kleiner report:

“The most impressive of the Avaris murals depicts bull-leapers seen against a maze background that many believe is a topological reference to the Minoan palace at Knossos. Not only is the subject of the painting Aegean but also the technique (primarily true fresco on lime plaster) and most aspects of style and iconography. Few doubt that Aegean rather than Egyptian artists decorated the palace, although there is little agreement as to why the Egyptians employed foreign painters or chose an Aegean subject.

“Whatever the final answer to this historical riddle, the excavations at Tell el-Daba have demonstrated that contacts between Egypt and the Aegean world were not confined to trade and politics. In fact, painted walls and floors of Aegean style, technique and subject also have been discovered in recent years in a Canaanite palace at Tell Kabri in northern Israel. Similar finds have been made … at Alalakh in Syria … Together these startling discoveries provide evidence for a rich international exchange of artists and ideas in the Mediterranean world … Art historians can no longer study the great civilizations of Egypt, the Near East and the Aegean in isolation.”

These artistic inspirations spilled over into the 18th Dynasty which followed on the heels of the Hyksos/Assyrians. According to Ralph Ellis:

“Akhenaton’s [18th Dynasty] artistic reforms have often been said to have been unique and revolutionary, but this is not at all correct. While Akhenaton’s artistic preference was unique to Middle and Upper Egypt, it has exact parallels and similarities with the Theran-Minoan artwork of Avaris.

“[Art historian, Vivian Davis remarks] ‘Much comment has been made on the Minoan character of the nature scenes at Amarna [in Egypt] which … has suggested to some … an Aegean influence in the art.’

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67 Meres J. Weche, *op.cit.*, p. 300
“In this quotation Vivian Davis is pointing out that although the artwork at Amarna looks distinctly Minoan, the Minoan empire had all but collapsed by this time. So how did the Minoan artwork suddenly appear at Amarna?”

It is suggested that Minoan art reached Egypt of the 18th Dynasty. Not only did it affect Egypt but also Palestine, to influence artistic tastes there, as William C. Hayes tells us about jugs that were discovered in three tombs at el Lisht in Egypt along with one “vase unique in Egypt but belonging to a typical Palestinian and Syrian type known at the end of the Middle Bronze IIA period…” [Helene J. Kantor reports]. This vase is not merely a normal import from Palestine, however, “it is outstanding for its decoration utterly unparalleled in both Egypt and Palestine … [On the vase are depictions of geese and dolphins like] Cretan art of the Middle Minoan III [period].” Here we can see the tentacles of artistic trade across the Mediterranean. The level of intellectual activity related to the Hyksos/Assyrians is seen in their program of making copies of Egyptian literary and technical texts, regarding which Hayes admits: “However we may evaluate [the Hyksos] they were evidently not the ruthless barbarians conjured up by the Theban [18th-19th Dynasties’] propagandist of the early New Kingdom and the Egyptian writers of later periods … and curiously enough some of our best surviving copies of Egyptian literary and technical work date from that time.”

Beyond all the above Hyksos innovations, there are those of musical instruments about which Hayes writes: “The lute, a musical instrument unknown in Egypt before the New Kingdom [18th-19th Dynasties], was like the horse, the composite bow[, the sickle-sword] and other innovations, probably imported from western Asia during the latter years of the Hyksos domination … it is the earliest Egyptian lute of which we have any record.” Joachim Braun states: “Enduring Hyksos innovations in Egypt included lute playing.” Laurence Picken claims: “Long lutes … first appear after the Hyksos invasion.” In Hyksos strata at Tell el-Ajjul in Palestine, Braun further shows a terra-cotta figurine that holds a lute. But what is most unusual is the fact that historians have to argue that it took about 800 years after its Akkadian ancestor existed for it to reach that part of the world. Braun admits:

“The lute, an instrument with completely new possibilities and new performance principles, had for at least eight hundred years already been a standard in the Babylonian Kingdom before finally appearing in ancient Israel/Palestine during the

69 Ralph Ellis, Cleopatra to Christ/Scota (Kempton IL 2006), p. 92
70 Hayes, op.cit., p. 12
71 Ibid., p. 219
72 Ibid., p. 24
73 Joachim Braun, Music in Ancient Israel/Palestine (Grand Rapids MI/Cambridge UK 2002), p. 81
74 Laurence Picken, Folk Musical Instruments (Oxford UK 1975), p. 144
75 Braun, op.cit., pp. 80-81
first half of the sixteenth century B.C. Interestingly, it appears concurrently, or perhaps one or two centuries later, in Egyptian iconography.”

With regard to the lyre, Hayes tells us “The lyre, another stringed instrument of unquestionable Asiatic origin, was adopted about the same time [as the lute].” He goes on to say of this type of lyre:

“For many years after its introduction into Egypt it continued to be regarded as a ‘foreign’ instrument, and is often represented in tomb paintings of the Eighteenth Dynasty being played by a Syrian.”

As for the harp, Hayes teaches us that:

“… the arched harp was the only one indigenous to Egypt from the Old Kingdom … it has been surmised that the Egyptians of the earlier periods had only the simple five-tone scale, without half tones … During the Hyksos period and the early New Kingdom … the music and musical instruments of western Asia brought about a great change in Egypt … the introduction of instruments like the multistringed harp, the multifretted lute, and the oboe with its numerous, closely spaced finger holes brought with it a much more elaborate system of tuning and far greater flexibility and scope of handling the tone.”

In terms of leisure pursuits “Board games became popular among the Egyptians of late Hyksos time”. These were games of position like pachisi or backgammon in which each player used five pieces or more and “the moves [were] determined by throwing knucklebones or sets of wands, the equivalents of dice.”

All these innovations transformed not only Egypt but the entire ancient Near East into one that was far more advanced than anything that had come before. The goods of the entire world of that time were carried into the lives of millions of people and were transformative. The description that will now be presented must be attributed not only to Hyksos/Assyrian Egypt but to nearly all the ancient Near East. Samuel Noah Kramer claims:

“Although Egyptian inscriptions picture the Hyksos as arrogant and impious, this was no barbarian horde. They seem to have been active merchants, introducing a new series of weights into Egypt. A recent discovery of a stela tells of ‘hundreds’ of Hyksos ships with rich cargo at a port in Egypt. Objects bearing the names of Hyksos kings have been found all over the Near East.”
Amélie Kuhrt shows that the Hyksos capital “Avaris … had a harbour which served as a staging port for river-borne traffic, particularly trade goods waiting to be shipped southward. In his attack on the city [Pharaoh] Kamose found 300 ships of Retenu moored in the harbour, filled with ‘lapis lazuli, turquoise, bronze axes, ben oil, incense, fat, honey and various types of timber …’”83 The Hyksos/Akkadians/Assyrians were a great, sophisticated, commercial and politically advanced empire. Kurinsky cites various major historians to this effect:

“The progressive developments wrought under Asiatic [Hyksos] influence during this period have been widely acknowledged, although the implications … have not been fully appreciated by those who refuse to acknowledge them. ‘Archaeological investigations have shown that in the Hyksos period … civilization in the country [Canaan] attained a high state of development,’ summarizes Aharoni in laying the background of the period. John Van Seters notes the effect of the Asiatic … on Egypt, ‘With the settlement … new sedentary culture came into the land and brought with it a high level of urban life …’ … G.E. Wright is frankly awed by the extent of the progress made during this period ‘… of the greatest prosperity that … had [been ] seen to that time …’ Cyril Aldred insists that not only was Egyptian civilization inseminated with a far superior culture but would have descended into severe decline without it. Hayes expands on the permanence of the effects of the Eastern influence sailing over the Hyksos bridge.

“There flowed into the Nile valley in unprecedented quantity new blood strains, new religions and philosophical concepts, and new artistic styles and media, as well as epoch making innovations of a more practical nature …”84

To this Kurinsky adds: “The fact [is] that the Second Intermediate Period was the time in which Egypt experienced its greatest leap forward in cultural, technological and economic affairs …”85 Because historians cannot envision an ancient Industrial Revolution that reverberated across the ancient world in the 8th and 7th centuries B.C., they cannot accept the importance of this episode of epic proportions that changed the ancient world forever. Society made a great leap forward to the benefit of humanity.

However, with the coming of great wealth, ease, pleasure, and the blessings of all this, there was an immense negative side to this development. Because of the growth of trade and wealth and the demand for goods, the institution of slavery which had existed since earliest times in civilization grew. All historians on this subject admit that there was a change that occurred during the 7th-6th century which ruined the lives of millions of human beings who were either sold as

83 Kuhrt, op.cit., p. 180
84 Kurinsky, op.cit., pp. 71-72
85 Ibid., p. 72
household slaves to the new growing wealthy classes that benefited from this revolution, or, worse, were enslaved to do the tedious and dangerous work of producing the raw materials necessary for this great leap forward. Most of these were captured prisoners of war. Isaac Mendelsohn reports: “that captives of war spared on the battlefield were reduced to slavery is amply attested in the annals of the long history of the Ancient Near East.”86 “The great projects of military fortifications, of road, irrigation, and temple constructions, accomplished by the state would have been almost impossible without the help of war prisoners, many of whom were skilled craftsmen.”87 “Indeed, captive craftsmen were deemed so valuable an asset that they were placed on the list of booty, second only to princes and high state officials. Even those countries that had voluntarily submitted to the Assyrian yoke had to include groups of skilled workmen as part of their tribute to the court.”88 In fact, “Traffic in foreign slaves was an integral part of the merchants’ activities in Ancient Babylonia. The supply of war captives and native-born slaves was at times not sufficient to satisfy the demand for menial help in agriculture, industry, and the households of the wealthy, and hence there was a need for importing slaves from the neighboring countries.”89

Although there were other sources of slaves, foreign captives who did not speak the language of their conquerors, once brought back from the battlefield, did many important tasks: they

“… became the property of the king, i.e. state slaves … who, with the assistance of corvée gangs and hired free labor, constructed roads, dug canals, erected fortresses, tilled the crown lands, and worked in the royal factories connected with the palace [or temple holdings] … Among the tasks assigned to these inmates were activities in the weaving, brewing, and general work department of the palace.”90

These goods were not only sold or used in the homeland of the empire, but were traded to other parts of the empire. The larger the empire, the farther its reach, and at the height of imperial rule, slaves were easily available and cheap. When these empires stopped expanding or were themselves under attack and losing territory, that long reach stopped, the number of slaves employed fell, and the cost of them grew more and more expensive and eventually prohibitive. James W. Russell outlines this development:

“Max Weber (1896), in his early and brilliant analysis of Roman slavery, concluded that once the empire stopped expanding and ceased to be able to supply

87 Ibid., p. 2
88 Ibid.
89 Ibid., p. 3
90 Ibid., p. 92
the slave market continually with large numbers of war captives, slave prices went up, increasing [the] costs of production [of goods and services] substantially. In time the slave system became too expensive to maintain for many large landowners, forcing its abandonment. In its place landowners expanded tenant farming arrangements which did not require as much supervision. The reins loosened as slaves were increasingly turned into serfs who had their own houses, families, and crop shares. Marc Bloc … also points to the increase in slave costs as motivating the shift from plantation to land-tenure [share-cropping] systems.91

One of the major factors in the growth of slavery was the development of great wealth and particularly the capacity to coin money. This created a banking and business class that developed with it, especially in Greece and later in Rome, who could organize this slave resource for profit, even going so far as to buy and lease large numbers of slaves to carry out various large-scale industrial operations, especially mining, but only at the peak of their imperial reach. Russell in this respect reports:

“Ancient slavery lasted from the rise of Greece in the sixth century BC until the fall of Rome in the fifth century AD. During the ten centuries of its existence, however, slavery was the predominant labor system only during the classical periods of Greece (fifth and fourth centuries BC) and Rome (second century BC until the second century AD). During periods of rise and decline in each of these areas other labor systems predominated.

“The first Greek villages grew up as simple walled enclaves within which farmers slept at night. During the day they left to tend their fields. By the seventh century BC, after land-ownership differences had developed, the richest families formed into governing aristocracies. By the sixth century B.C. trade with outside areas had sufficiently developed to add a new mercantile dimension to the Greek economy. Alongside old wealth based on ownership of land … grew new wealth based on ownership of workshops, ships and other lands, which was used for foreign trade…

“… spread[ing] slavery developed as mercantilism fueled the Athenian economy after the sixth century BC. Slaves worked vineyards, provided manual labor for urban workshops, and rowed ships that transported commodities to foreign markets …

“In classic times Athenian economy and society clearly rested on slavery. Most free families owned slaves … and compared to other slave societies their use was relatively evenly spread across the economy in small to medium-size groups. Only in mining areas were there large concentrations [of slaves]. But slaves may not have been the majority or even the largest sector of the laboring population.”92

Sidney Homer and Richard Sylla describe this mercantile system thus:

“The Greeks of the seventh century B.C. developed an economic system that was commercial, urban, and monetary. Credit facilitated trade. There was

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92 Ibid., pp. 64-65
extensive borrowing at interest, especially on ship loans. ‘In next to no time the commercial genius of the Greeks rises to the notion of speculation … capital accumulation is only an investment with the view to accumulating more.’ The poets of the day bitterly compared the new standards with the old ideals. The power of the old kings, once based on soil, cattle, and descent from the gods was gone. Oligarchy had supplanted hereditary monarchy. Even the landed nobility, except in Sparta, sought after movable wealth; some used their surplus wealth to become merchants and shipowners and compete with the lower class tradesmen.”

The same process occurred in Mesopotamia but the level and size of this commercial development, though large, was not as significant as it was in Greece. In the Akkadian/Hyksos/Assyrian epoch this development around the 8th-7th centuries largely paralleled Greece. Peter M.M.G. Akkermans and Glenn M. Schwartz describe this:

“The era of urbanization in … Syria was punctuated by the first unambiguous emergence of empire in the ancient Near East, the Akkadian state [in] Mesopotamia, which subjugated or raided large parts of Syria … We define empire as large-scale expansionist, multi-regional and administratively complex states. In the Akkadian case, which incorporated all former city-states of southern Mesopotamia as well as large segments of neighboring regions, a new level of imperial administration was installed above the traditional city-state authorities.”

Three books which cover this historical development, for those interested, are (1) Isaac Mendelsohn, *Slavery in the Ancient Near East*, (2) *Slavery in Classical Antiquity* edited by M.I. Finley, and (3) William L. Westermann, *The Slave Systems of Greek and Roman Antiquity*.

The Greeks and, I believe, nearly all other peoples of the ancient world, accepted the institution of slavery as the natural make up of humanity and justified this human bondage in terms of their natural superiority just as American black slavery was justified. Compare Aristotle’s words with those of owners of black slaves:

“Aristotle made explicit the parallel between the slave and the domesticated beast:

“Tame animals are naturally better than wild animals, yet for all tame animals, there is an advantage in being under human control, as this secures their survival … By analogy, the same must necessarily apply to mankind as a whole. Therefore all men who differ from one another by as much as the soul differs from the body, or man from the wild beast (and that is the state of those who work using their bodies, and for whom that is the best they can do)—these people are slaves by nature, and it is better for them to be subject to this kind of control, as it is better for the other creatures I’ve mentioned … [A]ssistance regarding the necessities of life is provided by both groups, by slaves and by domestic animals. Nature must

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therefore have intended to make the bodies of free men and slaves different also; slaves’ bodies strong for the services they have to do, those of free men upright and not much use for that kind of work, but instead useful for community life.”

This type of justification was prevalent in the ancient world. Slaves were actually branded with the same mark as livestock and had their hair cut short to display their station in life; this too was widespread in the ancient world. That is, all the leisure pursuits that the Greeks and Romans enjoyed were largely built on the backs and labor of slaves. “As a noted British classicist puts it:”

“[The Greeks] could imagine no alternative [to slave labor]; the life of the citizens in the polis, the only form of civilized organization they knew or could imagine, would have been impossible without that leisure they prized so highly, leisure to haunt the gymnasium, the roofed porches where men congregated for conversation and dispute, the theater, the assembly, the courts, and all the varied time-consuming duties and pleasures of the free male citizen.”

Moses I. Finley, in a highly annotated research paper, “Was Greek Civilization Based on Slave Labour?”, concluded:

“Without question the Greek city-states of the fifth and fourth centuries B.C., and among them notably Athens, used slave labour upon a fairly large scale. But the slaves were employed at the same work as the free, usually side-by-side with them, and apparently without prejudice or friction. In any sense which implies either that the enslaved population predominated over the free or that the Greek city-state displayed the mentality of a slave-ridden society, Greek culture was not founded upon slavery.”

Finley, a classicist and Hellenophile, I suggest, is in error. What would have happened to the Greeks and all the other cultures if over night all slaves vanished? Would Greece have remained so rich that its male citizens would have all that leisure to spend their time in one gymnasium or other? Who would work the mines, row their ships, tend their animals, clean their houses, etc.? Undoubtedly life would have drastically changed for these craftsmen and gentlemen without the slaves who made their leisure possible. And what of the women who were sexually abused by their masters alongside the wives of these masters? Did they feel prejudice and friction? And how does Finley know how these other slaves felt working side by side with freemen who went home to their families and not to slave quarters? I see

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96 Ibid., pp. 34-35
97 Ibid., p. 38
98 Ibid., p. 41
Finley’s objectivity as lacking a humane sense of what these slaves felt. As a case in point, where slaves did not work side-by-side with freemen, Finley reports:

“In mining and quarrying the situation was decisively one-sided. There were free men in Athens for example, who leased such small mining concessions that they were able to work them alone. The moment, however, additional labour was [required and] introduced (and that was the more common case), it seems normally to have been slaves. The largest individual holdings of slaves in Athens were workers in the mines, topped by the one thousand reported to have been leased out for this purpose by the fifth-century general Nicias. It has been suggested, indeed, that at one point there may have been as many as thirty thousand slaves at work in the Athenian silver mines [at Laurium] and processing mills.”

Whatever the number—30,000–20,000–10,000—that was a sizable percentage of the population that did not work as free men; the work was brutal and the slaves were treated brutally. This Finley and others fail to grasp and present. Arthur Wilson describes the life and endless toil these slaves were forced to endure. With the death of the tyrant Pisistratus, the Laurium mines were run by the state:

“As a direct result … Laurium came under renewed scrutiny. A programme of exploration leading to better understanding of the formation and potential richness of the deposits encouraged deeper penetration of the silver-bearing ore. … For the first time shafts were sunk to follow the irregular veins of silver which wound their way from the surface to the bigger ore bodies beneath. Present day archaeologists have identified the remains of more than two thousand shafts, one of which was nearly 400 feet [130 meters or the height of a 40-storey building] deep. These main vertical shafts were seldom more than six feet by four feet in section and show footholds carved out of the rock to assist climbing. Often shafts were inclined [less vertically] to provide stairways, the steps of many of which can still be seen. [The u]nderground passages were only two to three feet in height and width—just large enough to admit a miner, but too small for effective work and most of the bigger masses of ore were removed by digging downwards, and sometimes upwards from one level to another.

“Within a few years [because of the value of silver for coinage and trade] the lonely promontory of Cape Sounion [Laurium] was transformed into the site of one of the biggest undertakings ever carried out in the ancient world. Thousands of slaves worked in conditions no better than those that had existed in the Egyptian gold mines [of the Ptolemies]—and were so deplored [for cruelty] by Diodorus and other Greek classical writers. This is not to imply that the critics remained silent when writing of [the same or similar conditions of] their own country, though it is evident that they were generally more restrained. Plutarch, for example, in his appraisal of the Athenian statesman Nicias, reproaches him for employing

100 Ibid., p. 57
‘ordinary’ slaves and not criminals [to work the Laurium mines]. ‘One cannot,’ he adds, ‘much approve [even] of gaining riches by working mines, the greatest part of which is done by malefactors and barbarians, some of them, too, bound and perishing in those close and unwholesome places.’ Criminals or innocents, the slaves of Laurium did not have a happy time. As [Thomas] Rickards has recorded:

“Each slave was provided with a lamp of terracotta, or baked clay, having capacity for enough oil to burn ten hours, which was the length of his shift. Niches in the rock show where the lamps rested while the slaves, in cramped posture, pecked at the rock. In groups of five, the miners, followed by about twenty carriers, stumbled and crawled one after the other to the place of work, where there was room for only one man at a time, and even he had to dig while on his knees, on his stomach, or on his back. He broke about 25 tons per month. His tools comprised a hammer, a gad or chisel, a pick and a shovel, the metallic parts of which were made of hammered iron that had been tempered by quenching in water [to make it hard and strong enough to do this work]. The broken ore and waste were removed in panniers made of esparto grass or bags of oxhide, these were handed from man to man because there was no room for the carriers to pass with their load, except along main levels.’

“On surface, the ore was sorted, crushed with iron pestles and reduced to the size of grain [by other slaves] before going to the washing tables, where it was further sifted and crushed to leave a residue which could be taken to the furnace for smelting and casting into ingots.”

Wilson goes on to say:

“Such then was the position in the closing years of the 6th century BC. By now the mines of Laurium were providing the Athenian exchequer [or state treasury] with so great a part of the total revenue [of the state] that every citizen received an annual cash bonus from the profits…”

The gold mines run by the Greek Ptolemies in Egypt were especially brutal, as described in 113 B.C.:

“These men, thus, with candles bound to their foreheads, cut the rock, the white[ness of the] rock showing the direction for their labours. Placing their bodies in every conceivable position they throw the fragments [cut from the vein] on the ground—not each one according to his strength, but under the eye of the overseer, who never ceases from [whip] blows. Then boys, creeping with great labour, [remove] the stones which have been broken off and carry them out of the mines.

“Next, from these a crowd of old sickly men take the stone and lay it before the pounders and they strenuously pound the rock with an iron pestle in mortars cut out of stone, and reduce it until the largest piece is no bigger than a pea…

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102 Ibid., p. 83
“The next task is performed by women, who along with their husbands or relations are placed in enclosures. [Where several mills are placed together at one handle, filthy and almost naked, the women lay to at the mills until the measure handed them is completely reduced. And to every one of those who bear this lot, death is preferable to life.”103

Diodorus Siculus described these mines thus:

“No leniency or respect of any kind is given to any man who is sick or maimed, or aged, or in the case of a woman for her weakness, but all without exception are compelled by blows to persevere at their labors, until through ill-treatment they die in the midst of their tortures. Consequently, the poor unfortunates believe, because their punishment is so excessively severe, that the future will always be more terrible than the present and they therefore look forward to death as more to be desired than life.”104

David Brion Davis adds:

“In modern times the eminent classicist Bernard Knox climbed into the mine shaft at Laurion, in Attica, the main source of revenue and coinage for Athens … Knox found that the shafts down which one descended by ladder 130 meters into the earth measured 2 meters [6.5 feet] by 1.3 meters [4 feet]. At the bottom miners were forced to crawl into dark galleries … Knox badly scratched his knees and hands, frayed his shirt and trousers, and then got stuck in a dark bend as he tried to crawl out backward. Yet thousands of slave miners worked in such a hellish environment, with crude oil lamps, ten hours on and ten hours off.”105

It is clear that much of the mining and other jobs in the ancient world were carried out under hellish conditions. And these conditions became exacerbated after the seventh century B.C., when iron existed plentifully to make shovels, pestles, picks, etc., when coinage allowed for greater trade, where business men could buy and sell goods and slaves all across the ancient world, where empires through their conquests brought thousands of slaves home to run their states, where roads allowed for such trade with large sea-going ships. And much of these advances which enhanced the lives of free people everywhere were built on slave labor. The best words which expose these deplorable conditions were written by a Hebrew slave in the mines of Egypt, as discussed by Simcha Jacobovici in his 2006 documentary The Exodus Decoded aired on the History Channel:

“O El save me from the mines”
or: “O God save me from the mines”

To answer Finley best, think of Spartacus’s slave rebellion in Roman Italy. Slaves by the thousand flocked to him no matter what their station because slavery

103 Ibid., p. 40
104 Davis, op.cit., p. 42
105 Ibid.
is basically a degraded state in which to live. During the Peloponnesian War, Thucydides reported that at the first opportunity 20,000 slaves fled from their masters in Athens. Something is dreadfully missing in those historians who fail to condemn this in their writings. Perhaps they need to be objective as historians but this author is disturbed by that objectivity.

**THE HYKSOS, DOMESTICATED ANIMALS, AND CHRONOLOGY**

The movement, by trade and other means, of domesticated animals from various regions of the ancient Near East to and from Egypt does give evidence about chronology. For example, the movement of the camel or goat from Arabia or Mesopotamia to the Sahara via Egypt can be explained in terms of the short chronology. As with the great length of time it took, say, for the sickle sword, composite bow, chariot, and the lute to come to Egypt in Hyksos times, these same long spans exist for the bringing to and/or sending from Egypt to other regions a variety of domesticated animals. If the established chronology is correct, there will be a clear movement of these animals showing close chronological connections as they were moved from one region to another across the breadth of Mesopotamia into Palestine and thence into Egypt and beyond. If the short chronology is correct, the same conditions and relationships of these movements should exist. However, if, say, a domesticated animal is shown to have been raised in Palestine for a long time but came to Egypt, which supposedly was trading with this land, only after a very long period—several hundred years—that would clearly indicate that the chronology that allows for such long spans is incorrect. There are a number of such cases that show that the transport of domesticated animals constantly meshes with the short chronology and clashes with the established one. Below, the movement of the camel will show just this.

**THE CAT AND ASSYRIAN/HYKSOS CHRONOLOGY**

Jaromir Málek in his discussion of the cat in ancient Egypt describes what historians often neglect in their quest for an orderly and valid chronology:

“There are innumerable ways of approaching ancient civilizations. I suspect that those who suggest that we can never understand [these approaches] are deluding themselves. The traditional well-tried method concentrates, in a broad sweep, on the fluctuating fortunes of the kings and their courts, the rise and fall of empires, and the monuments of the mighty. But a large canvas and a thick brush dilute rather than focus attention, and tend to stress the unusual, the strange and the heroic, making communication between now and then [in terms of understanding chronology] difficult. The unexceptional, the normal, and the ordinary—and this [evidence], like
it or not, applies [but] usually remains unspoken. Yet recognition and familiarity are the elder sisters of understanding [chronology].”

An understanding of the domestication and transportation of the cat from Egypt to the rest of the world is an excellent, ordinary, unexceptional, and normal phenomenon that will give clear understanding of Hyksos chronology. The incorporation of the domesticated cat into ancient Egypt and its distribution thereafter to the rest of the world, especially into Greece, the Aegean, and then to the Greek colonies in southern Italy, gives clear evidence that the Hyksos/Assyrians/Akkadians ruled in Mesopotamia and Egypt just where Heinsohn and Sweeney have placed them, that is, in the mid- to early first millennium B.C.

In the following pages it will be demonstrated that the cat ties the Hyksos/Assyrians, the Minoans and Mycenaeans to the 8th-7th centuries and earlier, the Dark Age of Greece. The cat was clearly part of these cultures and then was incorporated into the Greek colonies in southern Italy in the 8th century (the 700s) B.C. Here we encounter a gap in the established chronology between cats living in Minoan and Mycenaean times which they date to the 13th century B.C., but then taking 500 to 600 years to arrive in southern Italy. The same 500- to 600-year gap also exists for the cat from the time that Minoan and Mycenaean society became non-existent until it flourished again in Hellenistic times in Greece itself. This will all be outlined below.

Donald W. Engels, in his excellent and intriguing book, Classical cats: The Rise and Fall of the Sacred Cat delineates this history:

“The Felis sylvestris libyca, the direct ancestor of all domestic cats, is a feline opportunist that has not only survived but flourished in the drastically changing natural and human environments of North Africa for the last five million years … Interbreeding between it and catus [later house cats] … has led to a gradual reduction in its modern body size and other wild characteristics. This may be why examination of many mummified ancient Egyptian cats shows that they are larger than modern libyca… The ancient cats were closely related to the larger ancestral libyca itself, and the modern libyca itself has declined in size through interbreeding.

“There are several significant references to the libyca. Diodorus Siculus … noted that in a region of what is now called central Libya, the wild cats (aliouroi) have driven out … many birds…

“The natural historian Claudius Aelian writing in the second century AD made many shrewd observations on cats. One passage on the taming of the Egyptian libyca deserves to be repeated in full.

“‘In Egypt, the cats … show that animal nature is not entirely intractable, but that when well treated they are good at remembering kindness. They are caught by pandering to their appetites, and when this has rendered them tame, they remain

perfectly gentle. They would never set upon their benefactor once they have been freed from their genetic and natural temper. Man, however, a creature endowed with reason, credited with understanding, gifted with a sense of honor, supposedly capable of blushing, can become the bitter enemy of a friend for some trifling and casual reason and give out confidences to betray the very man who trusted him.”

When, then, did the cat become a grain storage building mouser, a household pet and deity that was mummified in ancient Egypt? On this, Engels reports:

“The earliest remains of cats [based on the established chronology] in domestic contexts from Egypt date from about 4000 to 3000 BC, but were probably the tame wildcats rather than domesticated cats. Wildcats of various species were first represented in Egyptian art from 1900 BC, about the time *libyca* was domesticated. This is also the time that the first representations appear of what are probably domesticated cats …

“The name given to the domestic cat by the Egyptians was onomatopoeic ‘miu’ or feminine ‘miut.’ So we find names such as Pa-mui, ‘the tom-cat,’ and Ta-muit, ‘the cat.’”

The reason for the Egyptians adopting the cat as part of their domestic stock is clearly based on its value to them:

“Among the factors that undermined the serenity and security of Nilotic [Nile River Valley] life, the most significant were deadly snakes, such as cobras and vipers, and rodents, both mice and rats. Since there was little men could do to protect themselves from such dangers, the appearance of an animal that could destroy such vermin would have been a welcome event. Indeed, since snakes can inflict fatal bites to humans, it would have been literally a life-saving event. Since granaries and silos attracted rodents, they represented a reliable source of food for cats, who would leave the grain alone. Feeding scraps to the cats would assure their presence near their food supplies and homes. As territorial creatures, they would soon strike up associations (if not exactly friendships) with the humans and come to regard the area around their homes [and granaries] as their own. Thus it was just as much a factor of the cats adopting the humans in their territory as the humans adopting cats [in theirs].

“Before long the people began to recognize the benefits of having cats in the house. Households with cats had more food, less sickness [from vermin] and fewer deaths. Its personality and behavior compared well to the other pets that they had in their homes, such as dogs and monkeys. Its cleanliness no doubt attracted the Egyptians, while its ‘house training’—the burial of its excrement outdoors in the sand more preferable to them than the fertile earth of the fields—and its general rejection of grain-based foods, the staple of the Egyptian diet… must also have

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recommended it to their service. In exchange for comfort and safety the cats were willing to give up some of their freedom. Selective breeding would ensure that only the tamest and best behaved individuals would survive in human company.

“...It must also be noted that the cat was a new type of domestic animal. Other animals were exploited for their hides, meat, milk, or hair. Some were used for transport like the horse, donkey, mule, and later the camel… the dog was used for hunting, herding, but not for killing rats in antiquity. The cat, however, was used solely as a predator of small animals [harmful or dangerous to man] and later as a human companion.”

In terms of dating the cat in Egypt, Jaromir Málek points out the difficulties involved:

“The excavations of the late predynastic settlement at Abydos in upper Egypt yielded skeletal evidence for the cat dating to just before 3000 BC, but it is not possible to establish whether it was wild, tame or domesticated. Such uncertainty also plagues material of somewhat later dates, e.g. from Elephantine, near Aswan, [conventionally] dated between c. 2500 and 1900 BC. More often than not, an animal that became a pet would have been buried regardless of whether it had started life wild or domesticated, and conclusions based on [these] isolated instances of such discoveries are unsafe. The finds of mummified cats do not predate the first millennium BC. There is little doubt that by then the animal had been fully domesticated…

“Textual evidence concerning cats before the beginning of the New Kingdom (1540 BC) is very restricted. It is perhaps appropriate that the oldest images of cats in ancient Egypt occur as hieroglyphs. The earliest appear on a small fragment of a [12th Dynasty] temple wall found in the vicinity of the pyramid of King Amenemhet I (1980-1957 BC) at el-Lisht … some 50 km [31 miles] south of Cairo.”

In terms of the Hyksos, there is clear-cut evidence that they had cats because, as T.G.H. James points out, there are unambiguous representations of these animals inscribed with a Hyksos king and queen’s names. “Two gold spacer bars [are] decorated with recumbent cats, part of an elaborate piece of jewelry inscribed for the [Hyksos] Seventeenth Dynasty king Nubkheperre Inyotef and his wife Sobkemsa.”

It is highly probable, though not certain, that the cat came to Egypt with migrants from the Libyan Desert–part of the Sahara–when the great drying up occurred around 800-700 B.C. Since cats do live in deserts of endless sand, but not domestic ones or *F. sylvestris*, as Engels remarks, “A desert basin like the Sahara would have been inhospitable for *F. sylvestris*, as well as for its prey animals, the birds and rodents.” Based on this climate consideration, we would place the Hyksos just around and after the great desiccation began, that is, in the times of the

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109 Ibid.
110 Málek, *op.cit.*, pp. 45-46
112 Engels, *op.cit.*, p. 49
Assyrians. Here, then, is the way in which the chronology of the Hyksos can be understood, based on the cat in Egypt and its being carried to the Aegean and Greece, or to the Minoan and Mycenaean civilizations. There is clear evidence of close relations of Egypt with these northern Mediterranean societies. The Egyptian cat, of course, could not have come to these regions other than by being transported on merchant or other ships. With respect to the Minoans on Crete and the Aegean, Engels gives the following evidence:

“Remarkably, there are several depictions of the cat on frescoes and other objects of art that survive from the Late Bronze Age of the Aegean and Greece, 1700-1200 BC. Most notable is a probable Minoan representation of a cat from a fresco found at Akrotiri on the island of Thera (Santorini) dating to before 1628 BC. … The animal is shown in a Nilotic [Nile River Valley] environment … chasing ducks. Although the animal is [painted] blue in color and has elongated spots on its coat, it has a flatter feline face with jaws agape, and a feline body. The characteristic ringed tail of small Egyptian wildcats is portrayed in a typical upright position.

“The animal in question has sometimes been identified with the serval cat [of sub-Saharan Africa] and not with libyca or catus, but this is unlikely. In second-millennium BC Egypt, the serval was an exotic import from sub-Saharan Africa. When it was depicted in Egyptian art, it was frequently a gift or trade object from Nubia.

“One of the earliest representations of a cat from Crete occurs on a seal stone dated stylistically to about 1800-1700 BC… There is also a stunning fresco from the palace at Ayia Triada of a cat hunting birds amidst Cretan flora, and another from Knossos… Three Cretan seal stones depict cats chasing ducks… A final representation of a Bronze Age cat comes from the Minoan site of Palaikastro, in Eastern Crete. This is a terracotta head [of a cat] dating to about 1400 BC. One of the symbols in the Linear A syllabic script used on Minoan Crete is a cat.”

The same forms of evidence are also found on the Greek mainland of Mycenaean times.

“There are also several portrayals of cats from mainland Greece during the Late Mycenaean era (1700-1200 BC). Most striking is a [tin] bronze dagger inlaid with silver and niello from Grave Circle V at Mycenae. This object dates to approximately 1600 BC and depicts four cats hunting ducks in a[n Egyptian] papyrus marsh… The felines have sometimes been described as leopards (Panthera Pardus) but it is obvious that the animals in question are small cats (libyca or perhaps catus). This can be seen in the flattened profile of the cat … compared with the longer snouts on the lions from the same set of daggers, which are characteristic of larger pantherines such as leopards. … The bodies of both cats are only slightly larger than the duck they pursue [which is clearly too small for

113 Ibid., pp. 49-50
them to be panthers]. Surely the artist is skilled enough to depict the leopard at the correct scale [compared to ducks] if that was his intention.

“Finally, the markings on the animal’s body also resemble those on the libyca and not the spotted tail of the leopard.”\textsuperscript{114}

In addition to all this evidence of close relations between Egypt—that is, the Hyksos—and the Minoans, there is more:

“There were close connections between Egypt and the Aegean world especially during the time when Egypt was under Hyksos control … Although the Hyksos were not Minoan Cretans, there was certainly a close economic and cultural association between the two cultures. Massive quantities of Minoan pottery have been found at the Hyksos capital of Avaris (Tell el-Daba) as well as Minoan-style frescoes. Hyksos artifacts have also been found from the palace of Knossos [on Crete].”\textsuperscript{115}

In terms of the cat coming to the Aegean and Greece, Engels explains:

“This is a likely time for the cat to have spread into the Aegean and southern Greece. Central control over Egypt was lost [to the Hyksos], and the cats would lose little opportunity to accompany Cretan traders around the eastern Mediterranean on the fish- and mouse-laden vessels. Moreover … cats accompanied Egyptian ocean-going vessels during the New Kingdom (1570-1070 BC).”\textsuperscript{116}

Engels further comments:

“Some earlier studies of cats maintained that since the miu was a [sacred and] protected animal in Egypt, it would never have been given the opportunity to leave the country. This notion was no doubt influenced by the story of Diodorus … that when on military campaigns, the Egyptians would ransom captive cats … and bring them back to the[ir] country. However, as is widely known, cats [themselves] frequently fail to obey orders; rather they can invariably be expected to do so. This characteristic would make them difficult to constrain; especially futile would have been the task of keeping them from merchant ships with their cargoes of grain and, most likely, rodents and fish.”\textsuperscript{117}

From Greece merchant vessels carried the cat to other regions, so it spread across the Eurasian land mass.

“After Egypt, the original home of the domestic cat, Greece became the most important location for the distribution and spread of the animal. Beginning in the eighth century BC, the Greeks sent out colonies to southern Italy, France, Spain, the Balkans and the Black Sea. Available evidence shows that cats accompanied the colonists on their ships, as we might expect of opportunists who enjoyed dining on fish, shipboard rats and mice.”\textsuperscript{118}

\textsuperscript{114} \textit{Ibid.}, pp. 50-51
\textsuperscript{115} \textit{Ibid.}, p. 54
\textsuperscript{116} \textit{Ibid.}
\textsuperscript{117} \textit{Ibid.}, p. 46
\textsuperscript{118} \textit{Ibid.}, p. 48
What is this evidence? According to Engels:

“Important representations of cats occur in the coinage of the Greek colonies of Rhegion (Rhegium, Reggio di Calabria) and Taras (Tarentum, Tarento) in southern Italy, dating to the mid-fifth century BC… The coinage of Rhegion founded around 720 BC depicts a seated male figure identified with the city-founder Iokastos. On the floor beside the man, an obviously domesticated cat characteristically stands on its hind legs and plays with something … in the man’s hand. In the coinage of Taras, founded about 710 BC, the founder Phalanthos sits on a chair with a cat beside him …

“This marvelous series of coins indicates that the Greeks may well have taken their domestic cats with them on their journeys to found colonies … The cats were clearly domestic rather than Italian wildcats or feral cats, or else they would not be depicted in a domestic context. The most likely place for the cat to have come from is mainland Greece. In other coins from Rheidon, a duck or a dog sits under Iokastos’s chair, indicating that these may have been brought over as well …

“Although the coins date from the mid-fifth century [B.C.], the city founders Iokastos and Phalanthos are from the eighth …

“There is also a series of small bronze cats from the island of Samos that dates to the eighth century BC. It is not possible to tell whether these represent domestic cats but once again their presence on the island indicates that they were.”

This dating is disputed by certain authors who claim cats were not common but extremely rare in Greece and the Aegean in the 8th century B.C. For example Mel and Fiona Sunquist suggest:

“The earliest record of a domesticated cat in Greece is a 500 B.C. bas-relief scene of a cat on a leash confronting a dog. This must have been an unusual event because at that time cats were almost unknown in Greece and Rome [Italy]. Cats remained rare until the fourth century A.D.”

The reason writers offer that the cat was an exceedingly rare animal to the Greeks before Hellenistic times, after 330 B.C., is that they used polecats, that is ferrets, to rid their homes of mice. In this respect Olivia Temple and Robert K.G. Temple in their book on Aesop’s fables tell us: “Precision in terminology also reveals facts such as that household pets in ancient Greece were not cats but domesticated polecats or house-ferrets … Cats came to Greece from Egypt, but, until the Hellenistic period after Alexander the Great, they were rare or absent from Greek households.”

Yet if, as Mel and Fiona Sunquist state, the “cat remained rare,” or, as Olivia and Robert Temple conclude, “until the Hellenistic period … they were rare or absent from Greek households,” how can we possibly have cats well represented in the 5th or

\[119\] Ibid., pp. 55-56
\[120\] Mel Sunquist, Fiona Sunquist, *Wild Cats of the World* (Chicago IL 2002), p. 102
8th century B.C. colonies at Rhegion and Taras, or have small bronze cats found in 8th century B.C. contexts on the Aegean island of Samos? The Greek colonies in Italy were sent there from Greece. If the cat was inordinately rare or non-existent there, how did these Greek colonists have them as household pets? These colonies were trading with their city-state homelands all through this period and therefore the cat would have come to Italy from Greece well before Hellenistic times. The chronology for the transportation of the cat from the Egyptian to the Minoan and Mycenaean cultures and thence to Greece and thereafter to Italy is riddled with obvious chronological contradictions. Of the time of the cat in Italy, Engels shows:

“The earliest representation of a cat from an Italian context may be seen in the Etruscan Bucchero ware raised bowl or compote . . . dating from the sixth century BC and now in the Museum of Fine Arts in Boston. . . . Along the lip of the bowl are the heads of four cats . . .

“That cats were well established in peninsular Italy by the fifth century BC can also be seen in the superb fresco from the Etruscan tomb of the Triclinium at Tarquinia dating to about 470 BC. This scene portrays [a] large obviously domesticated cat, with the libyca-type coat coloration and markings, striding deliberately . . . among the couches . . .

“That three Apulian and Campanian vases dating to the late-fifth to the mid-fourth century [B.C.] also depict the cat. These are in the Greek colonial artistic tradition of southern Italy.”122

In these instances one can see that there were cats in the Greek colonies in Italy dated variously from the 8th to the 5th century B.C. These cats were taken north as far as the land of the Etruscans in the 6th century B.C. and at other points along the Italian peninsula by the 5th century B.C. Yet in Greece itself the cat was not common or was unknown until after Alexander the Great, after 331 B.C. The gaps in this chronology make no sense. The chronological evidence shows that the cat came to the Aegean, Minoan and Mycenaean civilizations in the 8th-7th centuries B.C., was well established in the Greek colonies, sent to Italy in the 8th-7th centuries B.C. and from there moved northward during the 6th, 5th, and 4th centuries B.C. The entire chronology of the movement of the cat out of Hyksos Egypt to these regions is orderly, logical, and without gaps. The gaps in this scheme in the established chronology were invented by proponents of the established chronology to protect it; in so doing one is left with a chronology of the cat’s movement that is not orderly because of these gaps.

The argument that the destruction of the Minoan and Mycenaean civilizations caused the cat to disappear or die without human presence for its support is without substance, as Engels explains:

122 Engels, *op.cit.*, pp. 88-89
“Between 1300 and 1200 B.C., the Minoan and Mycenaean culture of Greece came to a destructive end … The following era, that of the Greek Dark Ages, from about 1200 to 800 B.C., marked a nadir for Greek culture. There are no certain representations and few remains of cats from this [supposed] time. Still the cats of the Bronze Age probably survived the massive destruction on the mainland of Greece, Crete, and the [Aegean] islands. Considering the cat’s ability to adapt and take advantage of changed environments and circumstances, it is certain if it was present during the [Minoan and Mycenaean] Bronze Age, it could survive on its own during the Dark Age as a feral cat, barn cat, or perhaps village cat, as it has elsewhere throughout the ages.

“Cats, unlike warlike and destructive humans, are survivors … cat colonies have managed to survive for over a century on hostile, remote, sub-Antarctic islands with a complete lack of human intervention. They could surely have survived on the hospitable Aegean islands and the Greek mainland through the Dark Ages of Greece. Certainly by the Archaic era, 800-500 BC, the cat was a well-established element of the Greek natural environment and has remained so ever since.”

Another possibility that may be offered to get around this problem is that the cat population crashed so badly from some disease or event that it died off completely in all these regions. Nevertheless, the regions are so distant from one another that whatever event extirpated them in one would not have done so in the others. And even this one possibility is clearly without merit, as the Sunquists show: “… cats sometimes go through major population crashes, but … relatively large litter sizes and the potential of two litters per year allow the cat numbers to recover quite fast.”

Once cats were introduced to Minoan Crete, the Aegean, and Greece they would not disappear. They would survive as feral cats and remain in this region throughout its history. The established chronology makes a mockery of these biological/ecological facts. The chronology of Heinsohn and Sweeney is confirmed and corroborated by the biological/ecological facts.

THE ZEBU AND HYKSOS/AKKADIAN/ASSYRIAN CHRONOLOGY

In the previous chapter on the Hyksos, we learned from Samuel Kurinsky’s *The Eighth Day* that the Hyksos introduced the hump-backed zebu into Egypt. These zebu-type cattle were originally domesticated in India prior to their diffusion into Mesopotamia, Anatolia, Palestine, Syria, and then Egypt. As Mark Healy writes: “It was due to the Hyksos that hump-backed zebu cattle made their appearance in

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124 M. Sunquist, F. Sunquist, *op.cit.*, p. 72
Egypt.” Or, as Donald R. Prothero and Robert M. Schoch also state: “In ancient times zebu cattle were introduced to Africa from India or from the Middle East, and they were particularly popular in Egypt during New Kingdom [18th-19th Dynasty] times.”

The route out of India via Mesopotamia, Anatolia, Syria, and Palestine to Egypt is generally considered to be the manner by which this animal made its way there. Interestingly this animal appeared in Mesopotamia on objects during Akkadian times, 2300-2200 B.C. Allison Karmel Thomason shows: “images of animals such as zebu and water buffalo that were native to the Indus River valley appear on objects from the Akkadian period.” If the established chronology is correct, the remains of zebu will be found in sites from Hyksos times; and since the zebu was popular during the 18th and 19th Dynasties, just after the Hyksos, remains of these animals will also be found in sites from these epochs as well. On the other hand, Heinsohn and Sweeney’s Hyksos/Akkadian/Assyrian equation requires that remains of zebu cattle be found in ca. 8th century B.C. contexts.

In this respect there is an obvious physical difference that can be observed between the zebu and other cattle. Oliver Goldsmith in 1856 wrote that the only ways “in … which the two animals differ is the fatty hump on the shoulder of the zebu and in the somewhat more slender and delicate make of its legs.” Nevertheless, African cattle not of the zebu variety can exhibit such high shoulders that they may give a slight appearance of such a hump. R. Blench explains that because images of rock art in the Sahara depict cattle that do have a slight back hump, this creates a “… discussion [of the zebu] that is complicated by the many representations of cattle in the ancient Middle East, Egypt and in the Saharan rock art that show cattle with some sort of hump. Traditionally, zebu have been identified on the basis of Theban [18th, 19th Dynasty] paintings of c. 1500 BC. However [Alfred] Muzzolini … has undertaken an extremely detailed investigation of the representations of cattle in Saharan rock art. He concludes that there are some apparently early images of humped cattle that do not fit with the [chronology of the] zebu and therefore advanced the hypothesis of an independent evolution [in which African hump-backed cattle evolved separately and independently from the zebu] … In fact, the identification of humps is by no means self-evident … The

127 Allison Karmel Thomason, Luxury and Legitimation: Royal Collecting in Ancient Mesopotamia (Aldershot UK/Burlington VT 2005), p. 77
Damietta bulls of the Nile delta have pronounced withers which could hardly be
distinguished from [a] hump in most forms of representation.”\textsuperscript{129}

For example, one of the Egyptian sacred types of bull also exhibits a hump
distinctly like that of the zebu, on which Margaret Alice Murray comments that
this bull known as “Mnevis was entirely black and of a different breed from Apis
[bulls]; he was a heavily built animal with such high shoulders as to be almost
humped like the zebu.”\textsuperscript{130}

The fact that other Egyptian cattle can have a humped back would seem to
seriously call into question the evidence that the Hyksos actually introduced the
zebu into Egypt. But these Egyptian types were not employed in Mesopotamia
where clear evidence of them exists.

According to Gösta Werner Ahlström, Gary Orin Rollefson and Diana Vikander
Edelman:

\begin{quote}
\textquote{[In Israel] a soldier found a bull figurine … The bull statuette … represents
the Indian hump backed bull \textit{bos indicus}. One more example has been found in
Galilee in the Late Bronze temple … at Hazor. This bull is known from
Mesopotamia, Syria, Anatolia, starting in the fourth millennium BCE [3000 B.C.]
Figures of \textit{bos indicus} have been found, for instance, in Syria and Lebanon. Seals
have been found in Anatolia with representations of the zebu bull … A seal of the
Hittite king Mutawallis (c. 1310-1275 BCE) also shows a zebu bull. To these
examples can be added the reliefs of the battles of the Sea Peoples at the Medinet
Habu temple [of Ramses III, ca. 1080 B.C.] in Egypt.”}\textsuperscript{131}
\end{quote}

Zebu cattle were in Mesopotamia right up to Hyksos and Middle Kingdom
times where this animal is depicted in Egypt. The reason for the popularity of this
ox is that it is far superior as a domesticated animal to the various cattle it largely
replaced. Richard Meadow, an authority on the zebu, explains why:

\begin{quote}
\textquote{The evident popularity and success of cattle-keeping in the Greater Indus
Valley up to the present day is probably due, at least in part, to the nature of the
zebu breeds developed in the area. These humped animals are well known for their
heat tolerance and their ability to browse marginal range vegetation.”}\textsuperscript{132}
\end{quote}

Shepherd Krech \textit{et al.} show that the “Infusion of such stock must have
improved African herds. Zebu cattle generally need less water and forage than

\begin{flushright}
\textsuperscript{129} R. Blench, “Ethnographic and linguistic evidence for the pre-history of African ruminant
livestock, horses and ponies,” \textit{The Archaeology of Africa: Food, Metals and Towns}, Thurstan
Shaw, ed. (London/NY 1995), p. 77
\textsuperscript{130} Margaret Alice Murray, \textit{The Splendor that was Egypt}, revised (Mineola NY 2004), p. 99
\textsuperscript{131} Gösta Werner Ahlström, Gary O. Rollefson, Diana Vikander Edelman, \textit{The History of Ancient
Palestine: From the Palaeolithic Period to Alexander} (Melksham UK 1993), p. 364
\textsuperscript{132} Richard Meadow in Daniel T. Potts, \textit{Mesopotamian Civilization: The Material Foundations}
(London 1997), p. 256-257
\end{flushright}
taurines [humpless cattle] and gain back weight more quickly after droughts.”

Therefore, during the Hyksos period and into the New Kingdom (ca. 1500-1350), hump-backed zebu replaced or dominated over other cattle breeds all around Egypt. And this is taken to be the case within Egypt as well. Egypt during the Hyksos and Middle Kingdom periods was well known to be trading with or in contact with the regions where zebu existed. Hence it is certain that the Egyptians also raised and employed them and pictured them. But how can one determine at just what time they entered Egypt? Here, then, is where chronology on a scientific level comes into play. Though outwardly certain Egyptian cattle of the Sahara and Egypt with smaller humps over the shoulders and not as far back on the spine can be misinterpreted as zebu, osteologically or in terms of some of their other skeletal features the distinction can be identified and can fundamentally determine whether a bovid is a zebu or not. As Blench shows, “The distinctive feature of the zebu is the presence of a fatty bump, a morphological feature that leaves no archaeological trace. Zebu can sometimes be detected from skeletal features if the right bones are present.”

Juliet Clutton Brock tells us just what osteological features determine whether a specimen is a zebu or taurine form of cattle: “Humped cattle can be distinguished in the archaeological record by the shape of the skull or by posterior thoracic vertebrae [chest vertebrae] which in the zebu have bifurcated [two-pronged] nural spines.” Thus it is self-evident that, since one can distinguish the archaeological remains of zebu humped cattle from taurines or cattle without humps, or with humps on their shoulders as opposed to humps further back on the spine, which is the visible biological marker of zebu, where and when these are found will specifically date the zebu’s entrance into Egypt. For example, Clutton Brock elsewhere shows that this was done: “Fragments of bifurcated spines, the oldest of which dates to 1400 BC, have been described from the Tell of Deir ’Alla in Jordan and it can be reasonably assumed that these came from zebu cattle.”

The authorities nevertheless suggest that zebu cattle actually entered Egypt not around 1600-1350 B.C. as expected, in line with the established chronology, but around the very time to which Heinsohn and Sweeney date the Hyksos, the 8th-7th century B.C. Krech et al. explicitly state: “humped zebu cattle (Bos indicus) … from the Indian subcontinent appear in eastern Africa some time after 1000

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134 Blench, op.cit., p. 77
136 Juliet Clutton Brock, A Natural History of Domesticated Mammals (Cambridge UK 1999), p. 90
Likewise, Blench states “The zebu originates in India and was probably brought to Africa at least 2500 years ago [or prior to 500 B.C.].”\textsuperscript{138} Herbert H.T. Prins et al. state: “Zebu cattle from India spread into Africa around 2,500 bp, [ca. 500 B.C.] and replaced the earlier-introduced humpless cattle in many areas.”\textsuperscript{139}

While there is so far no osteological evidence of zebu in ancient Egypt, the very best evidence dates these bovids to the time suggested by Heinsohn and Sweeney.

Here, then, is the problem. Zebu cattle supposedly ranged all across Mesopotamia into Anatolia, Syria, Palestine, and Jordan from 3000 B.C. down to the late Bronze Age around Hyksos and Middle Kingdom times. Yet while these Egyptian rulers were trading with or having direct relations with these regions, it took almost 1000 years at least for the zebu finally to come to Egypt, say from Jordan where its bones have been found dating to about 1400 B.C. Why did every culture from India to Jordan adopt the superior zebu as they came into contact with it except the Egyptians? Why did they, of all peoples, trading with lands that had the zebu, wait for almost 1000 years to adopt it? While there are direct close connections between the movements of zebu from India, to Mesopotamia, to Anatolia, to Syria, to Palestine, and to Jordan, the links stop at the Egyptian border for almost 1000 years. This gap, as with so many others in Hyksos-Mesopotamian chronology (the sickle sword, composite bow, chariot, lute, which took 700-800 years to arrive from Mesopotamia) are all indicative of the disorderly and illogical connections created by the established chronology. In terms of the short chronology, the movement of the zebu from India to southern Mesopotamia around 1000 B.C. or somewhat later follows an orderly and logical series of connections. From southern Mesopotamia the zebu is adopted by people along the Tigris and Euphrates rivers to the north arriving in Anatolia, and eastward and southward into Syria, Lebanon, Palestine, and Jordan, and then directly into Egypt around 800 B.C. By moving all these nations, as Heinsohn and Sweeney have done, into the late second millennium and down into the first millennium B.C., there is no 800-900-year gap between the timing of the arrival of the zebu in lands adjacent to Egypt and then into Egypt itself. To get around this gap it is suggested that although “There is so far no osteological evidence for the presence of zebu in ancient Egypt, but the possibility that true zebu cattle were occasionally imported into Egypt cannot be entirely ruled out … As [Helmut] Epstein … pointed out,

\begin{itemize}
  \item \textsuperscript{137} Krench et al., \textit{op.cit.}, p. 337
  \item \textsuperscript{138} Blench, \textit{op.cit.}, p. 77
  \item \textsuperscript{139} Herbert H.T. Prins, \textit{Competition Between Wildlife and Livestock in Africa}, Herbert H.T. Prins et al., eds. (Boston/Dordrecht/London 2000), p. 53
\end{itemize}
they can have had little or nothing to do with the much later [1000-500 B.C.] appearance of zebu cattle.\textsuperscript{140}

In other words, here and there during the Late Bronze Age (1600-1350 B.C.) the Egyptians imported zebu cattle to Egypt that were depicted on walls and artifacts, but they failed to breed them in spite of their many superior features compared with their own domestic cattle for some obscure reason and waited 800-900 years before coming to this realization. This is sheer assumption. Once the Egyptians had zebu cattle and saw they needed less water, less food, could tolerate greater heat than their own, they, like all the peoples of Mesopotamia and in-between, would have adopted them and used them to replace or hybridize them with their own small humped cattle, or even cattle without humps. The established chronology makes a mess of this development. The chronology of Heinsohn and Sweeney is well supported by this development. Because the Hyksos/Akkadians/Assyrians dominate Egypt around 800-700 B.C., the zebu appears in Egypt around 800-700 B.C. The connection is direct and speaks for itself.

**THE CHICKEN AND HYKSOS CHRONOLOGY**

Like the problem we encountered with the cat’s transmission from Egypt to the Minoans, Mycenaeans and thence to Greece, where there was a gap of about 1000 years before it reappears in Greece, a similar problem exists with the transmission of the chicken on its journey from India, and similarly the 1000-year gap for the zebu’s arrival in Egypt, pertains to the chicken. Kurinsky points out “the barnyard fowl which like the zebu had been … bred in India had been well-known in Akkadia for a thousand years, and almost as long in Canaan.”\textsuperscript{141} This chronological scheme is far from accurate in terms of the short chronology, but it introduces us to the period, based on the established chronology, when the domesticated chicken reached the various regions of the Middle East, Africa, and Europe. In terms of the short chronology, these transmissions will have occurred in the first millennium B.C. What one would expect to find is that the domesticated chicken came to the Akkadians and thence to the rest of Mesopotamia in the first millennium B.C. and to the rest of the ancient Near East along the route of travel also in that same millennium, and thereafter in later times of the first millennium to Syria, Palestine, Egypt, the Minoans, Mycenaeans, etc. Placing the Akkadians in Hyksos and

\textsuperscript{140} Caroline Grigson, \textit{Bos Africanus} (Brehm)? Notes on the archaeozoology of the native cattle of Africa, \textit{The Origins and Development of African Livestock: Archaeology, Genetics, Linguistics and Ethnography}, Roger M. Blench, Kevin C. McDonald eds. (NY 2000), p. 44

\textsuperscript{141} Kurinsky, \textit{op.cit.}, p. 117
Assyrian times as one and the same people requires that the introduction of the domesticated chicken to the Akkadians is in reality introducing it to the Hyksos and Assyrians. Thus the route it took should closely follow that of the zebu.

The *Gallus gallus* or wild fowl as opposed to the *Gallus domesticus*, the domesticated bird, does appear in Egypt in Hyksos time. Joan P. Alcock affirms: “The … fowl has been found in Egypt illustrated on an ostracon [potsherd] of the Nineteenth Dynasty ca. 1350 B.C. Chicken bones and eggs were found in a Seventeenth Dynasty [Hyksos] tomb.”142 According to Oded Borowski,

“… two illustrations suggest that the chicken was known in Israelite circles, at least, toward the end of the Iron Age II [period, well into the first millennium B.C.]. Another seal, identified as Phoenician and possibly dated to the eighth century B.C.E. depicts two well-formed roosters facing each other ready for fighting. This also places the domestic chicken in the ancient Levantine barnyard at the second half of Iron Age II.

“When did the domestic chicken appear in this region? Brothwell and Brothwell suggest [without proof] that the red junglefowl (*Gallus gallus*) … ancestor of the domestic fowl (*G. domestica*) … was known in Egypt by the time of the Old Kingdom. … Early evidence of chickens in Egypt [however] appears as a drawing of a well-formed rooster on a sherd from the tomb of Tut-ankh-Amen (ca. 1350 B.C.E.). Von Soden [on the other hand] suggests that the chicken was introduced to Mesopotamia from India in the first millennium [B.C.] and is [thereafter] depicted in later periods, but there was no term in Akkadian for the domestic chicken. …

“As for Palestine, the earliest chicken bones are present in Iron Age I strata in Lachish and Tell Hesban.”143

Wolfram von Soden explains: “An Akkadian word for the domestic chicken is unknown. Important studies on the keeping of fowl are found in A. Salonen, *Vögel und Vogelfang im alten Mesopotamien* [Birds and Fowling in Ancient Mesopotamia, in which one finds the word] Tarlugallu ‘cock’ (pp. 154-5), [which] probably refers to the domestic rooster only in isolated cases.”144

The obvious question is: How could there have been roosters, even in isolated cases, among the Akkadians, without hens? There were other words from other ancient cultures, for example the Sumerian/Chaldean that mean chicken. Here, Page Smith and Charles Daniels show “There is evidence that chickens were known in Sumer … and the Sumero-Babylonian word for cock was ‘the king’s

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142 Joan P. Alcock, *Food in the Ancient World* (Westport CT 2005), p. 70
143 Oded Borowski, *Every Living Thing: Daily Use of Animals in Ancient Israel* (Walnut Creek CA 1998), pp. 157-158
bird’. The Sumerians/Chaldeans came prior to the Akkadians. They, too, knew the rooster or “cock” and obviously had hens to reproduce this bird. Morris Silver succinctly reports: “With respect to animal stock and diet, the wanderings of the chicken may be cited. Sumer spoke of it as the ‘bird from Meluhha,’ identified with the Indus civilization; in Syria the natives spoke of it as the ‘Akkadian (Babylonian) bird’…” Thus we have evidence that shows that the domesticated chicken was known in Mesopotamia in Akkadian times in spite of the fact that no word has been found for it.

Yet we were informed above that “Chicken bones and eggs were found in a Seventeenth Dynasty [Hyksos] tomb” and on “an ostracon of the Nineteenth dynasty ca. 1350 B.C.” That is, it took the chicken about 700 years to make its way into Egypt. Silver further shows “an Egyptian text of the time of Thutmose III (1490-1436 [B.C.] marveled at the ‘bird that gives birth every day’.” Like the sickle sword, the composite bow, the chariot, the lute, which took 700-800 years to reach Egypt from Mesopotamia, or the zebu that took 800-900 years to reach Egypt from Jordan, the chicken took several hundred years to come to Egypt. That for such a long period the chicken traveled to various places in Mesopotamia and beyond, but then stopped at the Egyptian border for 700-800 years, is again disorderly and illogical chronology. How did the domesticated chicken come to Egypt if not through Mesopotamia, or the zebu? After all, these countries were all in one way or another carrying on trade and had diplomatic relations. Therefore, transmission of goods, materials as well as domesticated animals would have spread from India through Mesopotamia to Anatolia, Syria, Lebanon, and Palestine to Egypt. Unless one denies this trade, there is no way to avoid the route taken for the distribution of these goods.

The second problem related to the chronology of the chicken is “when” did it travel from Egypt of Hyksos and Middle Kingdom times (1600-1350 B.C.) across the Aegean to mainland Greece? As with the cat there is a large gap of time before chickens arrive in Greece. Since the Hyksos/Akkadians/Assyrians were trading with the Minoans it follows that the domesticated chicken would have been depicted in Minoan art. Sinclair Hood observes that a “Vase in the shape of a bird, perhaps a chicken, from an early circular tomb at Ayia Triadha [was found on Crete]. Probably Middle Minoan … or later.” On this form Rodney Castleden comments: “The chicken may have been introduced to Crete at about this time: a clay vase from Agia Triadha looks rather like a

146 Morris Silver, _Taking Ancient Mythology Economically_ (Leiden, the Netherlands 1992), p. 20
147 Ibid.
caricature of a chicken.”\textsuperscript{149} However, Hood finalizes the identification of the chicken living on Minoan Crete by pointing to a term that can “only refer to a chicken, but implies it was something strange and new.”\textsuperscript{150} That being the case, and trade ongoing at that Late Bronze Age period (1600-1200 B.C.), the chicken should have arrived on Mainland Greece soon after, but as was the case similarly with the cat, it arrives 500 to 700 years later. Thomas R. Martin specifically relates that “By the seventh century B.C. the domestic chicken had been introduced into Greece from the Near East.”\textsuperscript{151} Harold McGee outlines the size of the gap involved: “By 1500 BCE chickens had found their way to Sumer and Egypt and they arrived around 800 BCE in Greece…”\textsuperscript{152} On top of this John K. Papadopoulos explains: “…the chicken—known as \textgreek{περσικός ορνίς} or ‘Persian Bird’—in Greek art belong[s] to the years of the late 8th century B.C., but it was only during the [later] Archaic period that the chicken became common in Greece.”\textsuperscript{153}

Again, just as in all other cases cited above, there is a long gap between the movement of the domesticated chicken from Egypt and the Minoan civilization to Greece. And, of course, when we move the Hyksos/Akkadians/Assyrians well into the first millennium B.C. the movement of the chicken from India to Mesopotamia, to Egypt, to the Minoans, and thence to Greece, follows an ordered and rational chronological sequence with no centuries-long gaps.

THE HORSE AND HYKSOS/AKKADIAN/ASSYRIAN CHRONOLOGY

As was pointed out above, Kamose claimed on a stela that when he came to drive the Hyksos out of his country “I shall seize your chariotry.” Chariots, to put it bluntly, are pulled by horses, and thus it is evident that the Hyksos had chariots and horses when they came to Egypt. Further evidence that the Hyksos employed horses is presented by Charlotte Booth:

“Evidence shows that horses were present at Tell el Dab’a as horse teeth have been found here from the Hyksos period … Emery also discovered a horse burial at Buhen in Nubia, which was initially believed to be dated to the Middle Kingdom, but may have dated to the Second Intermediate [Hyksos] Period.”\textsuperscript{154}

\textsuperscript{149} Rodney Castleden, \textit{Minoans: Life in Bronze Age Crete} (London NY 1993), p. 50
\textsuperscript{150} Sinclair Hood, \textit{The Minoans: The Story of Bronze Age Crete} (NY 1971), p.91
\textsuperscript{151} Thomas R. Martin, \textit{Ancient Greece: From Prehistoric to Hellenistic Times} (New Haven CT 1996), p. 2
\textsuperscript{152} Harold McGee, \textit{On Food and Cooking: The Science and Lore of the Kitchen} (NY 2004), p. 70
\textsuperscript{154} Charlotte Booth, \textit{The Hyksos Period in Egypt} (Buckingham UK 2005), p. 36
It was also shown above that there were horse burials at Tell el-Dab’a that were identical in several ways to Scythian burials. So it is evident that the horse was in Egypt in Hyksos times but not before. But there were horses in Mesopotamia according to nearly all proponents of established chronology after 2000 B.C., and these were used to pull chariots. Gabriel states “the horse [in] Mesopotamia … had been known from at least the Sumerian period. The Sumerians called the horse *anshe-kur-ra* or the ‘ass from foreign countries.’ During the Akkadian period it was known as *sisu* in Akkadian.”\(^{155}\) As shown by Pam J. Crabtree, Douglas V. Campana and Kathleen Ryan, based on the established chronology:

“The earliest actual horse remains in the Near East appear to date to about 2200-2000 B.C. Horse bones have been recovered from the Old Elamite levels of Tepe Farukhabad … dated to about 2200-2000 B.C., [and] from Early Bronze Age levels of Arad in the northern [Israel] Negev dated to about 2200-2000 B.C. … and from the Kaftare phase of Malyin Fars province, Iran, dated about 2000 B.C.”\(^{156}\)

Austen Henry Layard, the excavator of Nimrud, also shows that “Assyria, and particularly that part of the empire which was watered by the Tigris and Euphrates, was celebrated at the earliest period for its horses …”\(^{157}\) Thus, the horse was well established in Mesopotamian Akkad 2200 B.C., in Old Assyria and in Hyksos Egypt ca. 1700 B.C., and so once again we find a 500-700 year gap wherein the horse is known and employed in the width and breadth of Mesopotamia etc., but even as Egypt was trading with the Levant, supposedly from the Old Kingdom onward they failed to adopt the horse for their own use. They appear to be the only people of that vast region that failed to understand the value of the horse and waited 500-700 years to incorporate it into their society.

There is yet a further problem with the spread of the horse from Hyksos times to the time the horse appears in the western Sahara. Here the same gap exists. The horse arrives there several centuries after it appeared in Egypt. Christopher Ehret writes:

“Horses first appear in Africa as far as we know during the Hyksos rule in Egypt in the eighteenth century BCE. The animal then spread westwards among the Berber peoples during the next several hundred years. Finally during the first millennium BCE, as two kinds of evidence show, someone took the horse across the western desert. One bit of evidence consists of numerous rock drawings of horses and even chariots in the Hoggar Mountains; these date broadly to the first millennium BCE. The second indication comes from the word ‘horse’ used in later times south of the Sahara, in the language of the western Sudan region. All through

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\(^{157}\) Austen Henry Layard, quoted in C.W. Ceram, *Hands on the Past* (NY 1966), pp. 243-244
the basin of the upper Niger River and into the Senegal basin the words such as seso (Mossal language) and so (Malinke language) mean ‘horse.’ These words can be derived from the Carthaginian word transliterated like ss w and probably pronounced something like sisu. This information shows us that knowledge of the horse first crossed the [Sahara] desert during the period of Carthaginian hegemony over North African trade between the fifth and second centuries BCE.”  

The horse and chariot came to Egypt in Hyksos times (1700-1600 B.C.) but then took around 1000 years to spread across North Africa and reach the Garamantes and Carthaginian civilizations. That, like so much else regarding the movements of domesticated animals, is thoroughly illogical. Yet when we place the Hyksos in the 8th-7th centuries B.C., the horse arrives in the western Sahara within about 100 years. This is an orderly, logical route and chronology for the transmission of the horse.

Repeatedly the Egyptians were extraordinarily late in adopting an entire range of domesticated animals from Mesopotamia and transferring them to Greece. When we look back at all the various domesticated animals discussed in these volumes of Pillars of the Past, the problems and contradictions inherent in the established chronology are inordinately numerous and glaring. Yet when we look back at all the various domesticated animals in terms of the short chronology, the problems and contradictions not only vanish but fit an orderly, logical development. And here I indulge my bile in reaction to the historians’ failure to see these obvious problems as they relate to their dysfunctional chronology and predict that these historians and archaeologists riding their hobby horses will continue to tilt at windmills even while understanding all the evidence presented in these volumes that contradicts their chronological circular-reasoned merry-go-round and will not deal with these facts. In terms of yielding an inch, they will act as either sheep or swine, toeing the line they learned and teach, and hurling invective at this thesis that fits the evidence. Some like horses attacked by wolves will mentally gallop away in order to get as far away as possible from these many contradictions to the established chronology. Some like the Cheshire cat in Lewis Carroll’s Alice in Wonderland will smile and ignore what is so difficult to face. Some like zebu bulls in a China shop will thrash about hoping to smash to pieces any vestige of these factual contradictions. Some will retreat into the arid desert of isolation like camels, and some like chickens will cluck, and cluck, and cluck the mantra that there is little or nothing wrong with their unscientific and technologically unfounded chronology. They have produced nothing but a barnyard cacophony of meaningless noise.

158 Christopher Ehret, The Civilizations of Africa (Charlottesville VA 2002), pp. 222-223
THE HORSE, CATASTROPHEISM, AND CHRONOLOGY

According to historians, archaeologists, etc., the horse was domesticated or at least running wild in herds in the Ukraine and Kazakhstan about 4000-3000 B.C. Yet if this is so, there is a problem: how could there have been a Velikovskian catastrophe 1500-1400 B.C. that must have greatly diminished the horse population? Clearly the established chronology of the horse and Velikovsky’s catastrophic thesis appear to be antithetical. This also applies to the horse being in Mesopotamia, Anatolia, Syria, Iran, and the Levant prior to 1500-1400 B.C. If Velikovsky is correct, the short chronology should also explain this problem, but more importantly, it should explain the chronology of the horse in Eurasia and across to Africa without any major gaps in the chronology of its migrations. The connections between the places from which they migrated to new regions should exhibit none of the anomalies that, as discussed above, are commonplace in the established chronology. There should therefore be a near-extinction of the horse in some regions followed by a few hundred year population explosion and subsequent movement of this animal across Eurasia and Africa in the following few hundred years without 1000-year gaps. This is exactly what the evidence shows. The horse can “average fifty miles a day for ten days and forty miles a day indefinitely”¹⁵⁹ and thus their migrations should fully support the short chronology. The fact of the matter is that there was a clear bottleneck or near-extinction of the horse in these regions conventionally dated to after the end of the Ice Age, 8000 B.C. James Warren Evans reports:

“Neither today, nor during most of the historical past, have there been any wild horses in the valley of the Tigris and Euphrates, in central Europe, in Britain, in southern Sweden, in the Netherlands, northwestern Italy, northwestern India, Libya or Kazakhstan. This seems anomalous because the Pleistocene fossil record of these localities indicates that this is precisely where the other subspecies of wild horses once lived.”¹⁶⁰

Here then is the first gap with the first contradiction to the catastrophist chronology of the horse. After the mass extinction of the megafauna throughout the world around 8000 B.C., it supposedly took 4000 to 5000 years for the horse populations that survived in some parts of central Asia to reestablish themselves across Eurasia. In the Americas, a few Spanish horses brought to the southwestern part of the United States escaped. It did not take them 4000 to 5000 years to grow in numbers and spread across the North American continent. To the contrary, it took only a few hundred years to do

¹⁵⁹ “Horse on Wheels”, Popular Mechanics (March 1941), p. 114A
¹⁶⁰ James Warren Evans, Horse Breeding and Management (Amsterdam, the Netherlands 1992), p. 54
so. Yet historians, archaeologists, anthropologists, and zoologists maintain that in prehistoric times the horse in Eurasia could not, and did not, do what the horse in north America did—namely spread across a continent of deserts, mountains, plains, forests, etc. and grow in numbers to fill all the regions. In fact, the contradiction seems never to have occurred to them. Although Evans sees this phenomenon as “seem[ing] anomalous because the Pleistocene fossil record … indicates that this is precisely where … other … wild horses once lived” the contradiction is not, so far as I have read, ever discussed. It is assumed that either hunting or the climate kept the horse population small, but in the Americas the native population that ate horse meat at first did not halt its population growth. The disproof of these hypotheses can be found in The Extinction of the Mammoth, chapters 1 and 2. Along these same lines Drews further reports:

“In Europe, the … once-plentiful wild horses were gone by the sixth millennium BC [8000 years ago], and domestic horses did not appear [there] in significant numbers until late in the fourth [3200 B.C.]. West of the Rhine, during this interval there is no sign of horses at all. In eastern, central and northern Europe excavations of sites dating from the sixth, fifth and early fourth millennia [B.C.] have from time to time produced horse bones, but in exiguous amounts. Of all the bones thus far recovered at early neolithic sites in central Europe less than one third of 1 percent came from horses, whether domestic or—more likely—wild. Even in the later neolithic settlements of the eastern Hungarian plain, a region exceptionally well suited for horses, only one out of every two thousand bones seems to come from a horse … And one must conclude that not only in western Europe but even in central and eastern Europe the horse was near extinction by the beginning of the fourth millennium BC. Toward the end of the fourth millennium … Europeans again began eating horse meat, this time perhaps from domestic animals.”

Simon Davis adds:

“Among more than 100,000 animal bones from several Neolithic sites in the Carpathian basin Bökönyi failed to find a single horse bone.”

Ann Bowling and Anatoly Ruvinsky show:

“The horse appeared in southeastern Europe (Greece) at about … 1900 BC … Much later, in the time of Philip of Macedon, the father of Alexander the Great … horses still were imported extensively into Greece from the kingdom of Scythia, [from] modern Romania, Ukraine and south Russia … From this time, expansion of the horse into the major western civilizations spread rapidly, with the recognition of its military usefulness.”

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161 Robert Drews, Early Riders: The Beginning of Mounted Warfare in Asia and Europe (London/NY 2004), p. 11 (emphasis added)
Juliet Clutton Brock attributes the extinction of the horse in Europe not only to hunting but to climate change: “natural changes in climate and environment may have contributed to the extinction of horses in temperate Europe.”\(^{164}\) She wonders if “the dwindling numbers [of horses] 10,000 years ago were due to natural or human agency. The European wild horse was saved [from final extinction] by reintroduction into its former habitat as a tamed animal.”\(^{165}\) The climate extinction hypothesis was also analyzed in *The Extinction of the Mammoth*, pages 60-74.

After its near extinction at the end of the Pleistocene between 10,000 and 8000 years ago it took between 3000 and 4000 years for the horse to recover and spread all across the steppe regions of the Eurasian landmass, but they did so quite suddenly. Here Drews explains:

“Late in the fourth millennium [B.C.] … significant numbers of horse bones began to be discarded in hearths, refuse pits, and garbage dumps in settlements where previously horses had not been in evidence. In Turkey’s … plain, on both sides of the upper Euphrates, horses make what seems to be a *sudden appearance* late in the fourth millennium [B.C.]. Further to the east, in the Kura-Araxes valleys, horse bones show up somewhat later, in Early Bronze levels, but *at many sites*. South of the Tauros and the Balkan mountains tastes differed, and the horse never became a significant food animal in the Fertile Crescent, Egypt and Greece. But in eastern Europe north of Greece, and particularly the Carpathian basin, villagers seem to have begun eating horsemeat shortly before 3000 BC, after millennia in which horse bones had been conspicuously absent from their kitchen middens. A few centuries later horse bones began to be *dumped in significant quantities* in Hungary, *accounting for almost 20 percent of the ungulate bones* found there in the third millennium [B.C.] levels. More generally, throughout eastern and central Europe the percentage of horse bones at third-millennium [B.C.] sites is *several times greater than the percentage had been* in the fourth millennium … The appearance of the horse as a food-animal in all of these locations in Europe and also in Anatolia, outside the area in which wild horses were still common, is a valid argument that by 3000 BC the breeding of domestic horses was widespread.”\(^{166}\)

The very same condition exists in the heart of the horse steppes of Asia where suddenly the archaeologists have discovered a great horse culture that consumed enormous amounts of horse meat and left great heaps of their bones. Sandra L. Olsen in this respect shows:

“The Botai culture of northern Kazakhstan is now seen as one of the most crucial sources of information for documenting this [horse domestication] landmark in human history. This is not, however, because the Botai region necessarily

\(^{164}\) Juliet Clutton Brock, *A Natural History ..., op.cit.*, p. 103
\(^{165}\) *Ibid.*, p. 102
\(^{166}\) Drews, *Early Riders..., op.cit.*, p. 12 (emphasis added)
represents the first or only site of horse domestication. Instead, the Botai culture provides the optimal case study for this difficult task because Botai sites are located in the heart of the native geographic range of the wild horse and date to the fourth millennium [B.C.], sometime soon after it is thought horse domestication began. Moreover, the Botai based their whole economy on the horse, and their large, permanent settlements have yielded enormous collections of horse remains.”

She points out that all of this is dated via calibrated C14 to 3700-3100 B.C. It is assumed that either human hunting of the horse or a change in climate and the environment drove the horse in these regions of the world to extinction or near extinction. Yet the ironic problem is that man kept hunting the horse in Europe and elsewhere before it was domesticated for either 4000 to 2000 more years but failed to drive the horse to extinction.

Instead of becoming better hunters, Europeans who had first driven the horse to extinction there, became incompetent hunters. While the climate contributed to the extinction of the horse during the Ice Age, the warmer, more hospitable climate that followed the Ice Age instead contributed to the horse’s inability to repopulate the region.

The established chronology presents four major gaps in the record of the horse that contradict that established timeline. There is a 3000- to 4000-year gap between the near extinction of the horse in Eurasia and the spreading of the horse across these continents into a large population. Abruptly and seemingly out of nowhere the horse is almost everywhere in large numbers. (1) There is no gradual increase in population but an almost overnight jump in numbers. (2) There is then, as pointed out in volume I, pages 357-9, a second gap of about 1000 to 2000 years for the horse to migrate out of central Asia into Anatolia, Mesopotamia, India, Syria, and Palestine. (3) There is an 800-year gap for the horse to enter Egypt from Mesopotamia. (4) It takes another 1000 years for the horse to move along the shores of the Mediterranean Sea to reach the Garamantes and Carthaginians in Libya and west Africa. All in all we have the horse taking about 6000 years to migrate out of central Asia to Europe, the Near East, to Egypt and then to west Africa.

In terms of Velikovsky’s catastrophic hypothesis the near horse extinction happens 1500-1400 B.C. and in line with the short chronology, it takes less than 700-800 years to do the same thing, with no gaps. This lack of major gaps in the record clearly supports Velikovsky’s catastrophic thesis and the short chronology, without hunting and climate excuses to paper over these gaps.

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168 Ibid.
AKKADIANS/HYKSOS/ASSYRIANS AND MAGAN, MELUHHA, DILMUN, AND CHRONOLOGY

In each case above we have used forensic historical and other evidence to locate various Egyptian dynasties much closer to the present. The Nubian 25th Dynasty was about 300 years closer; the 20th Dynasty of Ramses III is about 800 years closer; and the same condition pertains to the 16th and 17th Dynasties of the Hyksos which must be placed in first millennium times between around the 9th-7th centuries B.C. In addition, the locations of the various peoples directly linked to some of these dynasties are also unknown and disputed. The Sea Peoples, Pereset, Sealand/Sea Country People and Hyksos have been an enigma for historians because, as has been shown, the chronology into which these Egyptian dynasties and people associated with them have been forced, is overly lengthy.

The very same conditions pertain to the peoples and locations of Magan, Meluhha, and Dilmun. For about a century historians have been arguing who they are and where their homelands are located. There is also a rather fascinating similarity related to these problems. Just as we know from documents of the Ptolemaic period that the word Pereset–P-r-s-tt– means Persia or Persians and the word haunebu means Greeks or Greece, historians maintain that when these same words are written on the walls of Ramses III’s temple at Medinet Habu, supposedly 800 years earlier, they mean Philistines, not Persians, and Sea Peoples, not Greeks. So, too, with the people of Magan, Meluhha, and Dilmun: the names and locations of these entities in the first millennium are definitely known, as will be presented below, to have the following identifications: Magan means Egypt; Meluhha means Ethiopia, and Dilmun means the Indus civilization. Yet historians, when they encounter these same designations in their chronology dated to the second or third millennium B.C., argue that these names refer to totally different peoples in totally different locations and that these locations are really unknown.

The peoples of Mesopotamia supposedly of the second and third millennium B.C., carrying on trade with the various nations of the Middle East, clearly mention Magan, Meluhha, and Dilmun in their cuneiform tablets. But, once again, because their overly lengthy established chronology has extended the history of these three regions back into those early times, as with so many other peoples and places, neither the historians nor the archaeologists have been able to deduce with any solid degree of certainty where these lands actually were. And so an inordinate amount of ink and fruitless research has been spent over the last century and beyond on seeking them.
Let us begin: Allison Karmel Thomason in 2005, discussing the enigma of the location of Dilmun, Magan, and Meluhha writes:

“In the … Akkadian [Hyksos/Assyrian] period we learn from the royal inscriptions of Sargon of Agade, who ostensibly created the first empire in the ancient Near East [which supposedly did not include Egypt or Ethiopia], that the opening of new avenues of exchange and acquisition of exotic imports topped his list of important achievements … His royal inscription proudly proclaimed that ‘He moored the ships of Meluhha, Magan and Dilmun in the quay of [his capital] Agade …’”

More germane to our thesis she goes on to show that

“The precise geographic identifications of Magan and Meluhha [compared to Dilmun] remain even more clouded in controversy and mystery, and, again, it appears that the locations of these two areas must have shifted in the Mesopotamian world view over time. While Dilmun is mentioned in some of the earliest texts in the world, from Uruk of the early third millennium B.C.E., Magan is not mentioned until the Akkadian [Hyksos/Assyrian] period, or ca. 2300 B.C.E. In the third and second millennia B.C.E., with which we are concerned here, Magan must have been identified with the southern part of the Persian Gulf, and possibly the coast of Oman [because the Akkadians are not believed to have conquered Egypt]. … However in Neo-Assyrian[/Persian] times [in the first millennium B.C.E.], the location of Magan shifted in the Mesopotamian mindset, as Magan was certainly located within or synonymous with Egypt and points further south in Africa.

“Meluhha, which seems to have been the most eastern of our exotic trio of places, has been identified finally, with some trepidation, with the Harappan civilization in the Indus River valley in modern Pakistan. … By the Neo-Assyrian[/Persian] period, Meluhha must have referred to another locale, as references to it in Neo-Assyrian[/Persian] royal inscriptions and other texts generally indicate that it was located in Africa, and associated with either Nubia (modern Sudan) or lands further to the south.”

She calls these accepted designated locations of Magan and Meluhha as Oman and the Indus valley “tentative equations.” To the best of historians’ knowledge, after over a century of research, that is all they remain. “There is little doubt that Dilmun, Magan, and Meluhha were far-away places to the Mesopotamians, yet the precise location in antiquity of the lands bearing these names has been hotly debated.”

One of the ways archaeologists and historians have typically identified connections between different nations is by finding pottery of a type common in one of these lands in the strata of the other. In particular regard here is what C.J.

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169 Thomason, *op.cit.*, p. 75
171 *Ibid.*, p. 79
Gadd had to say about pottery unearthed in Mesopotamian Akkadian strata which we have identified with Hyksos/Assyria that definitely come from Magan which we identify with Egypt:

“To seal his mastery of the ‘four regions’ Naram-Sin celebrated a triumph in the south over Manium, king of Magan. This is attested by the unimpeachable consent of his own inscriptions, of later omens and chronicles, and of existing alabaster vases inscribed with his name and the words ‘booty of Magan’. These vases, combined with the names of Magan and Manium, have given a singular interest to this episode, for Magan was a name undoubtedly applied to Egypt in a later period of Babylonian history, and the VASES HAVE A DISTINCT LIKENESS TO EGYPTIAN ALABASTER VASES, WHICH MORE COMMONLY BEAR INSCRIPTIONS IN THE LATE FIFTH AND SIXTH DYNASTIES, THE DATES OF WHICH ACCORD WELL ENOUGH WITH THAT OF NARAM-SIN. [Therefore it is suggested that] THE RESEMBLANCE OF THE ALABASTER VASES MUST BE ASCRIBED TO NO MORE THAN ARTISTIC INFLUENCE AND PRODUCTS EMANATING FROM FIFTH DYNASTY EGYPT [to Magan to the south of Mesopotamia and not to Egypt] OVER TRADE ROUTES to the east as they did to the north. There is no sufficient [acceptable chronological] reason to believe that Naram-Sin can have been a foreign invader who helped end the sixth Dynasty in Egypt.”

Naram-Sin, dated to Akkadian times via the established chronology and therefore dated closely to the 5th and 6th Egyptian dynasties, has alabaster vases with his name on them and inscribed as being “booty of Magan.” These vases are fairly typical of 5th and 6th Dynasty alabaster vases, but Gadd could not, based on his allegiance to established historiography, argue, without any evidence, that the pottery dating, which historians utilize for such connections, does not mean what it clearly implies. So he claimed, without any evidence to the contrary, that these vases were foreign copies from Oman. While he does say that “… Magan [is] a land which can now with some confidence be located on the shores of the Gulf of Oman”¹⁷⁴, he does not, and cannot, prove that such Egyptian alabaster vases were ever made in Oman or shipped from some other location to Oman. Instead of pursuing accepted pottery identification as the methodology for establishing such connections between the Akkadians/Hyksos/Assyrians and Egypt, Gadd offered us a rationale, and it was only a rationale, to set aside the very evidence upon which historians and archaeologists claim pottery forms and other materials should be used to make these connections. When a giant of ancient history in one of the premier sources of

historiography—*The Cambridge Ancient History*—ignores even the possibility of such a connection, few historians will contradict this dictum. John Bright makes it clear: “Naramsin conquered Magan (in later texts a name for Egypt) and traded with Meluhha (a later name for Nubia) … But Magan is probably to be located in SE Arabia (Oman), while Meluhha is probably the Indus valley.”\(^\text{175}\)

Is there any archaeological evidence that ties Magan and Meluhha to either Oman or the Arabian peninsula? If these early Mesopotamian societies were trading with these regions regularly, one would surely expect to find many different relics from Mesopotamia unearthed in the strata of these lands and buildings that indicate thriving trading centers existed there. Harriet Crawford reports:

“Magan is thought to be the name used to describe the Arabian mainland south of Bahrain, which became important for a whole variety of products … It may also have been applied to part of the Iranian coast to the north. Meluhha was the furthest away from Sumer [in Mesopotamia] and there is no evidence for direct contacts between the two countries in the ED [Early Dynastic] period.”\(^\text{176}\)

The very same condition applies to Dilmun which historians equate with the island of Bahrain and not the Indus Valley: “Oddly, the archaeological evidence between Mesopotamia and Dilmun [Bahrain] is very slight. Little pottery from Mesopotamia has been found and the often quoted iconographic parallels drawn from the seal stamps are of a rather generic sort, many equally at home in Syria.”\(^\text{177}\)

John Keay admits that “The Mesopotamians claim to have once conquered ‘Meluhha’, for which there is no archaeological evidence.”\(^\text{178}\) Samuel Noah Kramer, the late dean of Sumerian studies, states:

“And so we come to the perplexing and tantalizing question of the location of Dilmun, a problem which has troubled students for over half a century. The majority of scholars identify Dilmun with the island of Bahrein in the Persian Gulf, and for well over two decades a Danish expedition has been carrying on excavations on the island in the hope of uncovering the remains of a great [trading] civilization there, but so far in vain.”\(^\text{179}\)

Crawford vividly describes the clear absence of such evidence:

“In spite of the upsurge in archaeological activity … it will be obvious that the evidence relating to the most important [trading] years in the Early Dilmun period is still very fragmentary. There are many questions we cannot begin to answer. We have next to no evidence on settlement patterns for instance, nor is there a realistic

\(^{175}\) John Bright, *A History of Israel* (Louisville KY 2000), p. 36, fn 25
\(^{177}\) Harriet E.W. Crawford, *Dilmun…, op.cit.*, p. 152
hope of attaining it due to the physical conditions in the area. … On Bahrain, for example, a survey has located a proportion of the sites and the approximate position of others can be tentatively deduced from the positions of the surviving [burial] mound fields, but the sizes of the sites, their functions and their relationship to each other are lost. The loss of this information makes any attempt at central place analysis impossible and deprives us of one crucial tool in the attempt to recreate and understand the way man was using his environment.

“… at Saar [on Bahrain] … no horizontal exposure of a site of the period has been extensive enough to allow analysis of the use of space within a settlement. No critical analysis of domestic housing has been attempted either because of the lack of information … There is no evidence for administrative buildings and very little for manufacturing areas. We are still fumbling in the dark. This has led to the evidence from Saar being treated in this book as representative of all settlement in Dilmun, simply because there is nothing else … The evidence from the Oman peninsula suffers from the same limitations…”

These supposedly great trading centers have nothing to indicate that historical trade reality. As with the Sea Peoples, Pereset and the Hyksos, what we know—or rather, don’t know—about these people contradicts the historical analysis of them. Michael Rice gives us what we have continually found to be the situation for various unknown peoples assigned importance in the established chronology:

“Over 100 years of archaeology in the [Persian] Gulf and Bahrain have revealed much about the material culture which [historians assume] flourished [there]. But two fundamental questions remain as impenetrably unanswered as ever they were:

“Who were the Dilmunites and where did they come from?

“We are reasonably certain that the Dilmunites were not Phoenicians … There is no evidence that the Dilmunites were Sumerians, though the destinies of the two peoples were closely linked. We do not know what was the language of Dilmun. That inscriptions in most of the historic languages associated with the region have been found … prove[s] only that Dilmun had contact with the peoples of many lands …

“We do not know what a Dilmunite looked like. We do not know what his customs of dress were, other than the odd fragments of jewellery which have survived.”

As Crawford said, these problems of evidence also pertain to Oman. There is almost nothing archaeologically to indicate that Bahrain, Oman or southern Arabia were great centers of trade. Therefore let us examine this question on the basis of scientific and technological evidence. In the case of Magan being located in Oman and not Egypt, historians have suggested that the copper mined in Oman exhibits a distinctive alloy, namely nickel, which thus connects the copper products found in Mesopotamia to the Omani region. Daniel T. Potts claims that “… it seems clear that the copper imported

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180 Crawford, Dilmun..., op.cit., p. 143
by [the Old Babylonians/Persians] around 1800 BC and said to be ‘Dilmun copper’ must have originally come from Magan [Oman].”\textsuperscript{182} He adds: “… Dilmun’s copper was in fact that of Magan by another name. Yet, by 1800 BC, Dilmun was so adept at its role of middleman in the copper trade between Magan and Mesopotamia that the scribes of Ur no longer identified incoming shipments of copper as that ‘of Magan’.”\textsuperscript{183}

Thus the interpretation seemed to make metallurgical and scientific sense. Oman, supposedly Magan, did have copper mines which could ship this metal via Dilmun [Bahrain] to Mesopotamia. Furthermore, the copper found in early Mesopotamia had a tiny amount of nickel and the historians were quick to claim Omani copper did indeed contain nickel. Sir Leonard Woolley made just that claim as have many others: “Sumer imported [copper] from Oman … Oman copper ores contain both nickel and tin, so that the smelted metal is a natural bronze; objects from the royal cemetery at Ur show … as much as 14 percent tin and … up to 2 percent nickel…”\textsuperscript{184}

Vere Gordon Childe also stated in the same year as Woolley, 1963, that “the high nickel content of early Sumerian and Indus copper suggests that both civilizations were drawing on the ores from Oman which show a high nickel content.”\textsuperscript{185} But the fact of the matter is that Omani copper does not contain nickel. In \textit{Reallexikon der Assyriologie} this fact is made clear:

“The textual references to ‘copper from Magan’ have long been taken as references to Oman as the main source of Sumerian copper … This conclusion was, at the time, based on the very dubious argument that the presence of the trace element nickel in copper objects from Ur and Kīš indicated that the copper came from Oman … recent analytical work has indicated that the copper sulfide ores of Oman have no nickel … This seemed to rule out Oman as a possible source of Sumerian copper.”\textsuperscript{186}

The fact of the matter is that Omani copper is of two types, neither of which contain “nickel” as John Dayton had shown as far back as 1978, when he cited the evidence regarding this fact and stated: “Oman could not have been the source of nickel-rich Sumerian copper.”\textsuperscript{187}

Therefore, not only does Oman lack archaeological evidence that it was a major trading center as pointed out by Crawford, but the copper it contained does not have trace amounts of nickel. The science and archaeology are totally negative to the historians’ proposition that Magan is Oman, as is the metallurgy.

\textsuperscript{182} Daniel T. Potts, \textit{Ancient Magan: The Secret of Tell Abraq} (London 2000), p. 120
\textsuperscript{183} \textit{Ibid.}, p. 121
\textsuperscript{185} Vere Gordon Childe, \textit{The Bronze Age} (Cambridge UK 1963), p. 8
\textsuperscript{186} Bruno Meissner \textit{et al.}, \textit{Reallexikon der Assyriologie} (Berlin/NY 1999), p. 367
On the other hand, if Magan is Egypt and not Oman, as Heinsohn and Sweeney maintain, then the copper ore mined there will exhibit nickel in it. And that is just what it does. Flinders Petrie tells us “The earliest source of copper was Sinai where there yet remain thousands of tons of copper.”\textsuperscript{188} A. Lucas reports:

“Copper ores occur within the geographical limits of modern Egypt in two widely separated localities, namely, in Sinai and in the eastern desert. … “The evidence for ancient copper mining and smelting [at these sites] is twofold, first, the existence of mines with ruins of mining settlements and ancient slag heaps, and second, inscriptions in the neighbourhood of the mines left by [Egyptian] mining expeditions.”\textsuperscript{189}

Again, the fact of the matter is that the ores from the copper mines in the Eastern Desert of Egypt do contain nickel, as pointed out by Jack Ogden. “In Egypt itself, copper ores occur along most of the entire length of the Eastern Desert into Nubia … with various lead, zinc and nickel associations.”\textsuperscript{190} Dayton agrees with this and on a map points out that the Eastern Desert copper mines of Egypt have nickel in their ores.\textsuperscript{191} Bridget Allchin and Frank Raymond Allchin fully admit that the “copper [at Ur was] of a different quality from that of Oman.”\textsuperscript{192} And Gadd himself went so far as to suggest that the Egyptian Sinai mines were the source of copper at Ur. “The origins of the metals … used so lavishly at Ur and in other deposits of the third Early Dynastic period, have been investigated with much care. Most probable source of the copper, which betrays a slight mixture of the rare and then unisolated metal nickel, is thought for this reason to have been Oman or possibly Sinai.”\textsuperscript{193}

If the copper came from Egypt during the period of the 5th and 6th Dynasties, just as the alabaster vases with Naram-Sin’s name and the words “booty from Magan” which exemplify an Egyptian origin for these objects, then copper found for these dynasties should also be an alloy of copper and nickel. It should be noted that a great deal of ancient copper and other metal objects were remelted to form items for other uses. However, one major bronze statue from the 6th Dynasty has survived and it exhibits direct evidence of nickel. R.F. Tylecote reports that an Early Bronze Age “copper … statue of Pepi I (Sixth Dynasty…) … provid[es] …

\textsuperscript{188} W.M. Flinders Petrie, \textit{Arts and Crafts of Ancient Egypt} (Edinburgh/London 1909), p. 100 fn
\textsuperscript{189} A. Lucas, \textit{Ancient Egyptian Materials and Industries} (Dover ed., Mineola NY 1962), p. 202
\textsuperscript{191} Dayton, \textit{op.cit.}, p. 180, map
\textsuperscript{192} Bridget Allchin, Frank Raymond Allchin, \textit{The Rise of Civilization in India and Pakistan} (Cambridge UK 1982), p. 188
\textsuperscript{193} Gadd, \textit{op.cit.}, p. 132
two examples of copper with high nickel content (1-1.35 % Ni).” Ogden also shows “… the over-life-size Sixth Dynasty statue of Pepi I is, according to what is probably the most reliable analysis to date … almost pure copper, with 0.7 percent iron … and 1.1 percent nickel.”

Thus not only does Oman lack archaeological remains of a great trading capital, and lacks copper with nickel; Egypt’s 6th Dynasty had the same type of alabaster vases found in Mesopotamia, and significantly was mining copper with trace amounts of nickel.

In fact, as long ago as 1895, Archibald Henry Sayce actually suggested that Naram-Sin in his conquest could have taken command of the Sinai peninsula, and thus would have controlled the Eastern Desert with its copper mines:

“Sargon’s son and successor was Naram-Sin … who continued the conquests of his father. His second campaign was against the land of Magan, the name under which Midian and the Sinaitic peninsula were known … The result of it was the addition of Magan to his empire…

“The copper mines of Magan, which are noticed in an early Babylonian geographical list, made its acquisition coveted alike by Babylonians and Egyptians.”

Therefore, Heinsohn and Sweeney’s linkage between the Akkadians/Hyksos/Assyrians controlling Egypt/Magan to obtain copper is once again supported by scientific metallurgical evidence. The Akkadians/Assyrians would have had copper with nickel because as the Hyksos in Egypt they would have controlled the mines in the Eastern Desert that produced this ore which clearly was employed to make the over-life-sized statue of the 6th Dynasty pharaoh Pepi I. All the pieces of this puzzle fit Heinsohn and Sweeney’s chronological revisions and contradict the established chronological model yet once again.

With regard to Dilmun and Meluhha there exists an unusual aspect to their locations. While Heinsohn and Sweeney identify Dilmun with the Indus civilization, historians in general locate Meluhha with the Indus culture and Dilmun with the island of Bahrain in the Arabian Gulf. If it can be shown that Bahrain is Dilmun, Heinsohn and Sweeney’s thesis will be dealt a major blow. However, if it can be shown that the Harappan civilization is Dilmun, then Heinsohn and Sweeney’s identification will be validated.

The first evidence we will deal with is the direction of Dilmun with respect to Ur in Mesopotamia. Bahrain lies due south of Ur while the Indus cities lie east of Ur. And

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195 Ogden, *op.cit.*, p. 152
here the statements in the literature from Mesopotamia are rather decisive. Samuel
Noah Kramer, a proponent of the thesis that Dilmun is the Indus civilization, states:

“Dilmun is not ... just a literary fiction ... It has a long history ... Long after
the Sumerians had ceased to exist, ... Dilmun is repeatedly mentioned in
cuneiform documents: there are Dilmun messengers and caravans ... it seems to
me then that the description of Dilmun in the Flood story [of Gilgamesh] as ‘the
place where the sun rises’ indicates clearly that it should be sought not only east of
Sumer but also not south as far as the island of Bahrain lies. However in more
recent years, new inscriptive material has become available which indicates that
whatever its western boundary, Dilmun extended much further to the east and
included those parts of Iran, Pakistan, and India on which flourished the
remarkable Indus civilization.”\(^\text{197}\)

Crawford writes, “The land of Dilmun is variously said to have lain ‘in the middle
of the sea,’ ‘in the land of the rising sun,’ and ‘at the mouth of the great rivers’.”\(^\text{198}\)
Theresa Howard-Carter also reports, “Dilmun is specifically defined in the texts as:
The land of crossing; the Land of Sun rising ... at the mouth of rivers.”\(^\text{199}\)

This represents a major problem for historians who locate Dilmun at Bahrain
due south of Mesopotamia where the sun could never rise. Michael Rice offers the
following explanation to place Dilmun due south of Mesopotamia and in the east
where the sun also rises (to give Ernest Hemingway’s book a tiny mention):

“One of the most frequently applied epithets of the Gulf itself was the Sea of the
Rising Sun. This demonstrates the danger of taking the ancients seriously in the
matter of geological descriptions. No one could believe the Sumerians to be so obtuse
or so unobservant as to imagine that the sun rose to the due south of their land, which
could be the only conclusion if ‘sea of the rising sun’ is taken literally.”\(^\text{200}\)

In other words, accepting that this “most frequently employed epithet,” that
Dilmun is located in “the Sea of the Rising Sun,” should not be accepted because
“This demonstrates the danger of taking the ancients seriously.” On the one hand,
Rice accepts that “No one could believe the Sumerians to be so obtuse or so
unobservant, ...” yet we must not take them seriously. Why? Obviously because
Bahrain “due south” of Ur has to be Dilmun, that’s why!

However, the citations also note that Dilmun is located “at the mouth of the
great rivers.” And this refrain runs through the ancient Mesopotamian literature.
Jane Garry and Hasan El-Shamy tell us “It [Dilmun] is an island at the confluence

\(^\text{197}\) Kramer, \textit{In the World of Sumer}, op.cit., pp. 194-195
\(^\text{198}\) Crawford, \textit{Dilmun}, ..., op.cit., pp. 1-2
\(^\text{199}\) Theresa Howard-Carter, “The Tangible Evidence for the Earliest Dilmun,” \textit{Journal of
Cuneiform Studies}, vol. 33, no. 3/4 (July-October 1981), pp. 210 ff
\(^\text{200}\) Rice, op.cit., p. 11
of two rivers. The Sumerian tale of Enki and [his wife] Ninhursag calls Dilmun a land or island east of Sumer.”\textsuperscript{201} Nancy K. Sandars states: “For the Sumerians, ocean was somewhere out beyond the Persian Gulf, and there, too, was Dilmun, where the rivers ran into the sea…”\textsuperscript{202}

In this instance, it is quite clear that the land of Dilmun lay to the east where the “sun rises” and is a land of “great rivers” “at the confluence of two rivers” “where the rivers ran into the sea.” Now neither Bahrain nor any of the islands associated with it have “great rivers” that “ran into the sea.” The land that completely correlates with these two requirements is the Indus civilization which had many rivers at that ancient time and two great ones—the Indus and the Sarasvati. Georg Feuerstein, Subhash Kak and David Frawley explain “the [Indus] plain which is now mostly desert was formerly watered by several rivers, including one [the Sarasvati] that was larger than the Indus …”\textsuperscript{203} Nevertheless, by completely ignoring these two direct descriptions of Dilmun, historians claim they know that Dilmun is Bahrain which is south of Ur and has no rivers.

The astute reader may recall in volume II, chapter 3, “Medes and Mitanni” that I argued that the Medes were the Mitanni and that although it is claimed the Medes lived to the east of Baghdad and the Mitanni were centered to the west of Baghdad, they occupied the same homeland. The reader may then claim I am employing a double standard of inference, because I accept that Dilmun lies to the east of Ur where the sun rises. In one case I accept the direction of Dilmun as presented in the literature, in the other case, that of the Medes, I don’t accept the direction as presented in the literature. But as I have repeatedly maintained, the statements in the literature are only valid when they are supported by scientific, technological or other “solid,” “actually tangible” evidence. In the case of the Medes’ homeland east of Baghdad, James Muhly said in his review of the evidence of a conference held to find it, there was a “Black Hole.”\textsuperscript{204} There is no “solid,” “tangible” evidence for this location whatsoever. The Mitanni, on the other hand, provide enormous amounts of “solid,” “tangible” evidence for their location, and thus I equate both the Medes and the Mitanni with the same homeland, along with the other scientific and technological evidence presented in that volume.\textsuperscript{205} Thus, because Bahrain lacks substantial archaeological “solid,” “tangible” evidence that

\textsuperscript{201} Jane Garry, Hasan El-Shamy, eds., \textit{Archetypes and Motifs in Folklore and Literature: A Handbook} (Armonk NY 2005), p. 198
\textsuperscript{202} Nancy K. Sandars, \textit{The Epic of Gilgamesh} (London/NY \textit{et al.} 1972), p. 39
\textsuperscript{203} Georg Feuerstein, Subhash Kak, David Frawley, \textit{In Search of the Cradle of Civilization} (New Delhi, India 1995), p. 62; see page 88 for a map of these rivers
\textsuperscript{204} Ginenthal, \textit{Pillars of the Past}, vol. II, \textit{op.cit.}, p. 330
\textsuperscript{205} \textit{Ibid.}, pp. 340-347
it was a great trading center and because it has no rivers as opposed to the Indus Valley civilization, I accept the latter as the location of Dilmun. Yet there is more evidence of a “solid,” “tangible” nature that supports the Indus location for Dilmun.

There was trade in ivory from Dilmun, as the literature indicates. Peter Roger Stuart Moorey specifically shows that “in the body of the texts from Ur … ivory is attributed to Dilmun (Bahrain) whither it had presumably been shipped up the Gulf from the Indus where ivory was plentiful on sites of the Harappan period, both as tusks and as objects.” Notice that the Ur texts say the ivory came from Dilmun and that Moorey claimed, without proof, but on the assumption that Bahrain was Dilmun, this ivory was “PRESUMABLY … shipped up the Gulf from India” to Bahrain. Thomason adds to this “Many of the goods that are associated with Dilmun, including ivory … as well as elephants, could have had their source in India.” That is, Thomason tells us Dilmun was “associated with elephants.” But again, to dismiss this direct connection between elephants and Dilmun, these were PRESUMABLY first shipped to Bahrain before being reshipped to Mesopotamia, again without one scintilla of proof for this except that PRESUMPTION. Richard L. Smith presents the following case regarding elephants and apes from Dilmun:

“A poem from the time of Naram-Sin speaks of elephants and apes, ‘beasts from distant lands jostling in the great square.’ Elephants and apes could not have originated in Dilmun … but reference to … Melukha [Meluhha] provides the key for understanding the full extent of this early trade system. Melukha was India, or more specifically the Indus River Valley culture …”

Whilst it is admitted ivory “is attributed to Dilmun” “as well as elephants” by the historians, at the same time they claim that “elephants could not have originated in Dilmun” which they identify with Bahrain; they then attribute them to Melukha, or India. However, the ancient literature upon which they depend for these identifications does not make this “ivory–elephant” connection to Melukha. The only reference to ivory and Melukha, according to Jane McIntosh, is “curiously although the Mesopotamians used ivory [artifacts], their surviving texts record Meluhha as the source only of ivory birds.” Moorey also admits “ivory from Meluhha is only mentioned in connection with ivory bird figurines … Otherwise in the body of the texts from Ur … ivory is attributed to Dilmun.”

In fact, the mention of the “ivory birds of Meluhha” may not refer to figurines

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207 Thomason, *op.cit.*, p. 77 (capitalization added)
210 Moorey, *loc.cit.*
because Potts elsewhere shows only one ivory figurine of a bird. “An ivory bird of Meluhha mentioned in a Mesopotamian text from Ur may have looked not unlike the small ... duck of ivory found in 1998 ... at Tell Abraq [United Arab Emirates].”\(^{211}\) This one artifact is not known for certain to be that of the ivory bird of Meluhha mentioned in the literature. On this point Potts claims “it is intriguing to think that Mesopotamian references to imported ivory birds from Meluhha MAY INDEED REFER TO JUST SUCH AN OBJECT AS THIS.”\(^{212}\)

Furthermore, the ivory birds of Meluhha were not ivory in color but multicolored. Geoffrey Bibby calls these birds “multicolored ivory birds of Meluhha.”\(^{213}\) I very tentatively suggest that the “ivory” referred to regarding these ivory birds of Meluhha, may be the iridescent color of birds that were brought to Mesopotamia from Meluhha which Heinsohn and Sweeney identify with Ethiopia. And as a matter of fact, even today, Ethiopia still is the habitat of luminous, multicolored birds as reported by Frances Linzee Gordon and Jean-Bernard Carillet in their discussion of “Birds of Ethiopia”:

“The birds of Ethiopia are so diverse and so colorful that even the die-hard non birder will find it difficult, given time, not to become hooked. To date 862 species have been recorded ... and although South Africa claims most species, Ethiopia boasts the special distinction of possessing a high number of endemics (birds found only in that country).”\(^{214}\)

After all, Sargon boasted of all the different animals in the square of his capital so it is reasonable to suggest that the multicolored ivory birds of Meluhha were in fact the multicolored iridescent birds of Ethiopia.

If Meluhha was India, as historians suggest, why aren’t ivory and elephants directly connected with it in the literature, except this possible bird? Since Mesopotamia was trading with Meluhha over centuries, the people there would surely have known that elephants lived there, and not have associated ivory and elephants with Dilmun. Potts further offers that “D. Collon has concluded that ivory used in Mesopotamia always came from the Indian Elephant (Elephas maximus) and that the animals were imported intermittently as well.”\(^{215}\) The elephant is the 10,000 pound problem in the room defining the location of Dilmun. On this Kramer is adamant: “The fact that ivory artifacts were imported from Dilmun is fundamental and crucial for the location of Dilmun and its identification

\(^{211}\) Potts, \textit{op.cit.}, p. 130
\(^{212}\) \textit{Ibid.}, p. 131 (capitalization added)
\(^{213}\) Geoffrey Bibby, \textit{Looking for Dilmun: An Account of the Search for the Lost Civilization} (NY 1970), p. 221); see also Feuerstein \textit{et al.}, \textit{op.cit.}, p. 120, and Allchin, Allchin, \textit{loc.cit.}
\(^{214}\) Frances Linzee Gordon, Jean-Bernard Carillet, \textit{Ethiopia & Eritrea} (Melbourne/London 2003), p. 48
with the ancient Indus land. For this [as opposed to Bahrain] is the only large and wealthy maritime land to the east of Sumer.”

Another product from Dilmun is timber. Romila Thapar shows that “Gudea of Lagash refers to ships bringing him timber from Dilmun.” Thomason writes of “The ruler of the city-state Lagash, Ur-Nanshe …, mentions in a partially restored inscription that ‘He had ships of Dilmun transport timber to Lagash…” Potts explicitly states there is “an indication that Dilmun boats were built of wood from Dilmun.” That is, the ships from Dilmun were built of wood from Dilmun. It is therefore assumed the ships were from Bahrain and traveled to the Indus to bring wood to Mesopotamia. But there is no physical evidence that Bahrain was a great maritime nation while there is blatant, obvious evidence that the Indus people were. Kramer reports:

“During the past few years, there have been uncovered in Pakistan several sites of ancient Indus towns which were originally on the coast of the Arabian Sea, although as a result of coastal uplift, these are now some distance away from the edge of the sea. The existence of these settlements, taken in conjunction with the numerous long-known sites strung all along the Indus River, indicates clearly that the Indus civilization depended largely on water-borne trade, coastal and riverine. This is now corroborated by the excavations conducted over the past five years in Lothar, a site in India not far from the Gulf of Cambay, where what seems to be a well-planned rectangular dockyard built of baked bricks has been uncovered, complete with spillways, water-locks, and loading platforms… Now this type of maritime civilization must have been characteristic of Dilmun, to judge from the Sumerian inscriptions in which ‘ships of Dilmun’ are mentioned repeatedly.”

He cites a poem that states “Dilmun became the dock-yard-house of the (inhabited) land.”

There are no known ancient dockyards on Bahrain, and yet it is assumed by historians that Bahrain was a major maritime nation, without a shred of evidence of a shipyard there. And undoubtedly the Indus region was heavily forested. Feuerstein, Kak and Frawley state “in fact India … had immense forests.” Bahrain, on the other hand, is described by Marshall Cavendish thus: “The landscape of Bahrain is

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216 Kramer, op.cit., p. 193
218 Thomason, op.cit., p. 79
220 Samuel Noah Kramer, “The INDUS Civilization and DILMUN; The Sumerian Paradise Land,” *Expedition* vol. 6, no. 3 (Spring 1964), pp. 48-49
221 Ibid., p. 48
222 Feuerstein et al., op.cit., p. 118
mostly desert with barren rock and thin, dry, stony soil. There are sand dunes and salt pans around the coast. … Bahrain has a desert climate. No rain falls between May and October and it is rare during the rest of the year.”

One of the major factors historians cite as proof that Bahrain is Dilmun and not the Indus civilization is that it contained a great deal of “sweet water.” Crawford argues thus that “The literary texts also tell us that the island was notable for its sweet water springs. Bahrain is the largest island in the Arabian Gulf north of Oman … and is famous for the artesian springs of fresh water which well up on land and even in the sea.”

Gregory L. Possehl claims that this sweet-water evidence clinches the location of Dilmun to Bahrain:

“… Dilmun was an island in the Arabian Gulf that could be reached … from the northern end of the Gulf of Bahrain. The ancient texts also tell us that one could draw fresh water from the sea near Dilmun, and indeed there is a place off the coast of Bahrain where one can do just that, an observation that clinches the case.”

But the same existed for the Indus-Sarasvati complex of rivers which were emptying the entire Indus basin prior to the climate change that dried up most of its sources. Just as the Amazon River at its mouth sends fresh water miles into the Atlantic Ocean, so too did these great ancient Indus valley rivers. According to A.D. Taylor who served with the British military in the 1800’s:

“The Munneja Bank extends 6 to 7 miles off shore, from the principal mouth of the Indus, and occupies a sea face of 20 miles in extent … at the junction of the salt and fresh water the sea is whitish and sometimes at night [of] a luminous appearance. During the inundation [when the Indus River is at its highest elevation] the water is frequently fresh at the mouths of the river, and a vessel at anchor will often have good fresh water, though muddy on one side, and clear salt water on the other, the boundary line of the two waters is observable several miles out to sea.”

Consider then, when the Indus Valley had several major rivers, with the Sarasvati larger than the Indus River, and all flowing with a great deal more fresh water into the ocean, how much further out to sea that sweet or fresh water would have extended then than it did around 1838. Today, of course, the Indus is dammed and has major canals leading water to irrigate the Indus plain, but not in ancient times. So the argument that fresh/sweet water “clinches the case” for Bahrain in reality does just this for the Indus civilization as the location of Dilmun. Kramer includes this further note for locating Dilmun in the Indus Valley and coast:

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224 Crawford, *Dilmun.*, *op.cit.*, p. 2
“Enki includes [in] the land of Dilmun:

‘May the wide sea bring you its [Dilmun’s] abundance
the city–its dwellings are good dwellings
Dilmun–its dwellings are good dwellings
Its barley is very small barley,
Its dates are very large dates…’

“Thus, the Mesopotamians thought of Dilmun as a blessed, prosperous, maritime land to which were brought the world’s goods by boat. …

“One of the most striking and impressive features of the Indus cities and towns is the important role which water and cleanliness seem to have played in the life of the people, as is evident from the extraordinary private buildings, as well as the carefully planned networks of covered drains built of kiln-baked bricks … All of which seems to fit surprisingly well with the Dilmun-Indus hypothesis.”

Kramer cites “Stephen Langdon as early as 1915 under the title ‘Sumerian Epic of Paradise, the Flood and the Fall of Man’ … describes Dilmun as a land noted for purity, cleanliness [etc.]. ‘The land Dilmun is pure, the land Dilmun is clean.’”

The last piece of evidence regarding Dilmun being located in the Indus plain is that the Dilmunites used as their standard of weights only those employed by the Indus civilization. Geoffrey Bibby on this matter asserts:

“A puzzle remains. Why had Dilmun used the standard weights of the Indus Valley? The Babylonians and Sumerians used a completely different system… Either the first commercial impulses to have reached Dilmun [Bahrain] must not have come from Mesopotamia but from India, or else India was a far more important commercial connection…”

The Dilmunites used Indus standard weights because they were the Indus people. None of this will register on the minds of historians infused with the notion that Bahrain is Dilmun, in spite of all the evidence against it. Kenneth Kitchen, the great standard bearer and proponent of established thinking, as well as of the established chronology, has outlined the evidence, as he sees it, that proves Dilmun is Bahrain. In citing him I will repeatedly capitalize his arguments where they are based not on evidence but upon assumption and presumption:

“Identification of Dilmun with the island of Bahrain goes back to 1880 when both Oppert and Rawlinson made the proposal within months of each other. The question has been debated ad nauseam since, especially in recent decades following the remarkable Danish excavations on Bahrain and Failaka islands and beyond. In the present state of our knowledge, there is a strong case for the identification of Dilmun with the Bahrain archipelago and including part of

227 Kramer, *In the World of Sumer ...*, op.cit., pp. 194-197
228 Ibid., p. 194
229 Bibby in Feuerstein *et al.*, op.cit., p. 118
adjacent Eastern Arabia. The essential points made in favour of such an identification will be listed summarily for convenience.

1. In the Neo-Assyrian epoch Assurbanipal ... claimed rule over ‘Dilmun which is in the midst of the lower sea.’ The latter expression is universally admitted to refer to the Gulf and THE IMPLICATION IS that Dilmun was an island in the Gulf.

2. A generation earlier Sargon II claimed receipt of gifts in submission of ‘Uperi, king of Dilmun whose hideout is located—like a fish—30 beru distant, amidst the Sea of the Rising Sun.’ As a phrase ‘Sea of the Rising Sun’ IS TAKEN MERELY AS A SYNONYM (like ‘Bitter Sea’) for the Lower Sea.

3. The ‘distance of 30 beru MAY PROVISIONALLY BE TAKEN AS reckoned from sailing clear of Mesopotamia’s coastline ... IT MAY READILY BE TAKEN as the sailing-time between there and Bahrain, as several [though not all] writers have clearly pointed out. The figure agrees well with the 300 miles or so from the mouth of the Euphrates to Bahrain.

4. The name Dilmun or Tilmun is AN ACCEPTABLE origin for Greek Tylons, which is Bahrain...

5. Natural phenomena support the identification. The renowned fresh-water springs of Bahrain that well up from the sea, not just on land, find their reflection in Sumerian mythological references to Dilmun.

6. The inscription of Rimun ... found ... in Bahrain mentions the god Inzak who (according to Mesopotamian sources) was the form of Nabu worshipped in Dilmun. Inzak as a Dilmunite deity attested in Bahrain COULD AGREE WELL with the identification of (insular) Dilmun and Bahrain.

7. The term Bahrain is not exclusive to Bahrain, but also included part of the adjoining Arabian peninsula. [Kitchen furnishes no evidence for this]

8. At a much earlier date ... [but not during the critical period of trade with Mesopotamia] the archaeological evidence shows close links between Hasa and Bahrain, as both share the use of burial tumuli [mounds] of the [same] kind.

To his discredit, Kitchen has completely omitted from his list the following:

1. That Kramer, an expert in Sumerian, did not ever suggest that the Sea of the Rising Sun was “merely a synonym” for the Lower Sea but on the basis of his knowledge of Sumerian linguistics firmly held that it means Dilmun lay to the east where the sun rises. What Kitchen suggests is that one of the greatest authorities on Sumerian linguistics doesn’t know what he is talking about. But this evidence fits the Indus civilization.

2. Kitchen never mentions the fact that the Danish excavators on Bahrain found almost nothing that clearly suggests it was a great maritime nation and that the same problem pertains to Oman on the southern Arabian peninsula. What Kitchen suggests is that this supposedly great maritime nation which traded for centuries

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230 Kenneth Anderson Kitchen, *Documentation for Ancient Arabia* (Liverpool UK 1994), pp. 139-141
with Mesopotamia left no shipyards, great storage facilities or accounting buildings while being this mighty trading center. But the evidence for these sites fully fits the Indus civilization.

3. Kitchen never mentions the fact that the literature in Mesopotamia clearly and repeatedly claims that Dilmun was a land in which rivers flowed down to the sea which are totally lacking for Bahrain and Oman; this means that neither Bahrain nor Oman could have been the location of Dilmun. But this particular evidence is in complete conformity with the Indus civilization.

4. Kitchen never mentions the fact that Dilmun is directly linked with having ivory and elephants (as per Crawford, above) and that neither of these could have come from Bahrain or Oman. But again, ivory and elephants surely came from the Indus.

5. Kitchen and the other proponents of Bahrain as the location of Dilmun have never offered any “solid” “physical” proof that Bahrain was the middleman in trade relations between the Indus and Mesopotamia, just the supposition that since they believe this to be the case, it must be the case, a completely circular argument.

6. Kitchen never mentions the fact that Dilmunites only used the Indus standard of weights which Bibby claimed as a “puzzle.”

7. Kitchen, like all the proponents of Bahrain as Dilmun, never sought to see if any other place had sweet water on its land and out to sea. Given the evidence above respecting the great river systems of the Indus that argument no longer excludes but fully includes the Indus as the location of Dilmun.

All the evidence proposed by Kitchen or omitted by him and his colleagues indicates that these historians are trapped in a great circular chronological argument. The physical evidence points to the Indus as Dilmun; the literary evidence points to the Indus as Dilmun. There is no physical evidence that Bahrain is Dilmun except its sweet water which sours in the face of the fact that the Indus-Sarasvati rivers and their many tributaries did inundate the sea for many miles from their mouths with sweet water. The evidence that shows Dilmun is the Indus civilization therefore precludes any possibility that Meluhha was located in the Indus region. The copper with nickel evidence makes Magan Egypt. All the above evidence makes Dilmun the Indus civilization. The only place left for Meluhha is Ethiopia. Saggs has given us the clue that explains why historians have tied Ethiopia/Meluhha to the Indus. Since Ethiopia in the first millennium was known to be the location of Meluhha, Saggs suggests “The shift of the name [Meluhha from Ethiopia to India] was probably encouraged by the fact that Africa was a source of the kind typical of the original Meluhha, notably ivory and ebony.”

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is the historians who have shifted the name of Meluhha from Ethiopia to the Indus, not the ancient peoples of the Near East.

And, given this penchant to force these lands into their chronological scheme, the debate over the locations of Magan, Meluhha and Dilmun just as with the locations of the homelands of the Sea Peoples, Pereset and Hyksos will continue to be debated *ad nauseam* and sought for years, decades, and perhaps even centuries from now to no avail. Historians have created Never-Never lands and have with might and main tried to make these a reality. Their quest for all these places and instances is a search for a mythical Eldorado that will elude them as long as they persist in their rigid belief system. So I cite to these brave scholars Edgar Allan Poe’s “Eldorado”:

“Gaily bedight,
A gallant knight,
In sunshine and in shadow,
Had journeyed long,
Singing a song,
In search of Eldorado.

“But he grew old
This knight so bold
And o'er his heart a shadow
Fell as he found
No spot of ground
That looked like Eldorado.

“And, as his strength Failed him at length,
He met a pilgrim shadow
‘Shadow,’ said he,
‘Where can it be
This land of Eldorado?’

“‘Over the Mountains Of the Moon,
Down the Valley of the Shadow,
Ride, boldly ride,’
The shade replied
‘If you seek for Eldorado!’”

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CHAPTER 5

THE 12TH DYNASTY, SOTHIC DATING,
SKEPTICS, REVISIONISTS, AND EVIDENCE

“Sturt Manning records the reign of Amenemhat II of the XII Dynasty of Egypt as ‘c. 1935 … 1877 … 1903 … 1895 …’¹ This might seem like a typographer run amok, but those familiar with Egyptian chronology know better, for it reflects the range of modern opinion on the dating of this reign. The competing views of the chronology of the ancient Near East epitomize ways in which knowledge about the past is gained–and lost … At no time has there been consensus, and seldom even an undisputed majority opinion [regarding chronology].”

David Henige
*Historical Evidence and Argument*
(Madison WI 2006), p. 135

The dating of the 12th Egyptian Dynasty by Lynn E. Rose indicates that the above citation is no longer valid. Although his work has not been generally accepted, no one has presented evidence–astronomical evidence–to refute it [see volume I, chapter 3]. Nevertheless, there are various skeptics and disbelievers who maintain–again without astronomical evidence–that Sothic dating is invalid. Among these are revisionists such as Gunnar Heinsohn and Emmet Sweeney, whose theses in many respects have been strongly buttressed by Rose’s astronomical dating. So, too, Clark Whelton, Frank Wallace, and others have indicated that astronomical dating is a pseudo-scientific method and is unacceptable as a gauge for the chronology of the ancient Near East. Sweeney and Heinsohn have cited Wolfgang Helck to support just this position. Here I cite Sweeney:

“Thus in 1983 Wolfgang Helck, the editor of the *Lexikon der Aegyptologie*, announced that ‘work on chronology has clearly arrived at a crisis. The reason for this is in part the adoption of dogmatic scientific [Sothic dating] facts without testing their applicability to Egyptian material and the serviceability of this material.’ Whilst a rearguard attempt has since been made to save Sothic chronology, it is apparent that the chronology of the entire system, even amongst top ranking members of the Egyptian establishment, is now in question.”²

Peter James and his colleagues likewise have taken a negative stand on astronomical dating, especially in the areas that undermine their own revision:

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¹ Sturt Manning, *Absolute Chronology of the Aegean Early Bronze Age, Radiocarbon and History* (Sheffield UK 2006), p. 117
² Sweeney, *Pyramid Age, op.cit.*, p. 75
“The case for accepting the astronomical dates for Egyptian history is now so riddled with doubts that the whole structure can be seen as creaking at the seams. As it totters, it is no longer acceptable merely to prop it up with generous ‘lengths for obscure periods’. The gaps in the evidence were, and remain, on the whole, quite real. The ‘royal lists’ [of kings] of the ancient world did indeed exaggerate, and modern scholars are no longer obliged to accept the inflated estimates [of that chronology]. The cumulative results of 100 years of archaeological research give them the lie.”

Notice that James et al. indicate that “100 years of archaeological research” disprove the established chronology, and not that astronomical dating does so. In spite of these statements, the proponents of the established chronology have stuck to their guns and held steadfast to this methodology and to the view that it still supports that established chronology. Pascal Vernus and Jean Yoyotte in 2003 claim: “The Twelfth Dynasty is solidly established at the beginning of the second millennium [B.C.], thanks to Sothic dating.” John M. Steele states: “There is no denying that … Sothic dates … are the basic organizing principle for dating Egyptian history …” Robert Morkot describes the quandary that Egyptologists find themselves in when they reject Sothic dating: “Some eminent Egyptologists have recently rejected the validity of Sothic dating, and many more would say that we no longer use it. However, the dates we [still] use are actually calculated using this method as a starting point!” In other words, even those who reject Sothic dating maintain that the dynasties dated by this method must still be kept where that method placed them and that we must analyze Egyptian chronology using this supposedly discredited technique as the starting point.

The lack of logic that is expressed by this view, wherein the method is rejected but its results are the starting points for chronological analysis, could not be more irrational nor more irrelevant. Good science and history require that both the methodology and the results of that methodology be either valid or invalid. And in a certain sense Sothic dating as good or bad science and history/chronology must be proven valid and its results accepted, or proven invalid and its results rejected. As a supporter of Rose’s Sothic dating, I accept the methodology and its results because it is both good science and good history/chronology, which will be yet again analyzed below. As will be seen, some of those who reject it in whole or in part nevertheless cannot extricate themselves from employing some form of astronomical Sothic dating to uphold their own revisions of ancient history.

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3 James et al., op.cit., p. 310
4 Pascal Vernus, Jean Yoyotte, The Book of the Pharaohs (Utica NY 2003), p. 44
Why, in fact, did Helck and the other Egyptologists claim that Sothic dating can no longer reign as a chronometer? That is what has led to this crisis. James et al. explain: “Wolfgang Helck has recently stressed a long-standing problem concerning the reliability of the Sothic reference in the Ebers papyrus. While the ‘emergence of [the star] Sothis’ is referred to in this text, no day [for its rising] is specified. It is now increasingly considered to be ‘highly doubtful and should not be used any more in the chronological calculations.’” Helck rejects the Sothic evidence in the Ebers Papyrus (which specifically refers only to the 18th Dynasty) that “should not be used any more in the chronological calculations.” Rose’s work, however, pertains to the El-Lahun Papyri for dating the 12th Dynasty. James et al. continue: “This rejection of the Ebers fixed point leaves the absolute chronology ultimately dependent on the single Sothic reference from El-Lahun. Yet this sole Sothic date from the Middle Kingdom [12th Dynasty] can hardly be used to support the conventional chronology for the New Kingdom, since between the two periods lies the Second Intermediate [Hyksos] Period, the length of which is still completely uncertain.”

Since, in the established chronology, the 12th Dynasty comes prior to the 18th Dynasty and between them lies the Hyksos period whose length “is still completely uncertain”, the chronology between the two cannot be properly known. But this argument has nothing that invalidates Rose’s placement of the 12th Dynasty. The El-Lahun data he employed move that dynasty forward in time by 1477 years so that it ends precisely with the coming to Egypt of Alexander the Great. What Helck was arguing against is the placement of the 18th Dynasty. He never rejected the data given in the El-Lahun Papyri as inadequate for dating the 12th Dynasty. When Rose re-dated the 12th Dynasty to the first millennium B.C., he employed the full panoply of El-Lahun evidence.

Those who reject Rose’s placement cannot rely on Helck, because his analysis only dealt with the Ebers Papyrus data as it related to the 18th Dynasty, which in no way invalidates the dating Rose achieved for the 12th Dynasty in the first millennium B.C. Thus the Helck pillar as an argument to refute Rose must be rejected. What those skeptics of Sothic dating as utilized by Rose have failed to take into consideration is the great number of data points which he has integrated in terms of astronomy, calendars, and history for dating the 12th Dynasty. The depth of these integrated data-points must be seen and understood in order to appreciate the immense difficulty that Rose’s critics face.

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7 James et al., op.cit., p. 228
8 Ibid.
The first data point is that he has a precise date for the heliacal rising of the star Sirius/Sothis from the El-Lahun Papyri in the 7th year of Sesostris III of the 12th Dynasty, which occurred on the 16th day of the 4th month of the second season (prt) as seen at Elephantine in Egypt.

The second set of data points cover a range of about 50 years in which the Egyptians celebrated lunar festivals. Rose was able to precisely place at first 34 of 36 festival dates, with two off by only one day which could be explained on the basis that the onsets of those two months were cloudy or incorrectly interpreted. Rose achieved a score of 0.944, a non-random level of agreement. And these months were not all consecutive.

The third set of data points is that the individual lunar festivals always occurred on an exact day after the new crescent in ancient Egypt. If a particular festival fell on the 8th day after the new crescent in one place, it fell on the 8th day after the new crescent where it is mentioned elsewhere. The lunar festivals always fell on 0, 2, 3, 8, 16, or 19 days after the New Crescent Moon.

The fourth set of data points is related to the seasonal rising of the Nile River which crests around the end of September or early to mid-October, for which the Egyptians celebrated the Nile crocodile god Sobek. That is, Rose’s data always had the Nile River cresting in the proper season of the year.

Although it is not in the papyri, the fifth data point is that the 12th Dynasty ends in the very year Alexander the Great conquers Egypt.

Now I submit that this number of precisely-dated data points in the El-Lahun Papyri could never correlate with the precise positions of the star Sirius, the orbit of the Moon around the Earth, and the orbit of the Earth around the Sun, and the precise lunar festival dates on particular days after the first crescents, and the cresting of the Nile in the proper season, all just by coincidence. That simply does not happen by chance. This only occurs when the literature in the El-Lahun Papyri and the astronomical phenomena are integrated in reality. This is good science and chronology and cannot be dismissed without proving how and why all these correlations are wrong. This, I further submit, cannot be done. It can be evaded, ignored, and vilified, but it cannot be scientifically disproved. That is the daunting task for those who wish to dispute Rose’s Sothic dating. The methodology and the results of that methodology are valid because of the breadth and depth of the many precise correlations. I don’t believe that those who dispute Rose have anything resembling astronomical evidence to maintain their negative stance, and I think they know that.
DAVID ROHL, PETER JAMES ET AL.,
AND ASTRONOMICAL DATING

As was shown earlier, David Rohl and Peter James et al. misdated the Nubian 25th Dynasty because they relied on Neo-Assyrian astronomical dates, which Rose had shown were off by nearly three centuries. In spite of their general rejection of Sothic dating, both Rohl and James et al. have nevertheless used the lunar data in the El-Lahun Papyri to support their own chronological revisions. While James et al. have shortened the chronology by about 200 years and Rohl by about 350 years, they therefore had to bring the 12th Dynasty closer to the present by 200 and/or 350 years in order for the dating of this dynasty to correlate with their chronological reductions. If, as they maintain, the 12th Dynasty reigned in the period where they located it, then the lunar data, etc., in the El-Lahun documents should correlate not only with their revisions but also with the Moon’s orbit around the Earth, and with the Egyptian calendar in all respects. But, as will be demonstrated below, their placement of 12th Dynasty on the basis of the astronomical evidence in both cases fails.

Let us begin this discussion with David Rohl’s analysis. Trevor Palmer has laid out the position of Rohl by citing David Lappin regarding this matter: “…David Lappin pointed out in 2002 that the lunar observations in the time of Senusret III [Sesostris III of the 12th Dynasty] are consistent with his reign commencing in 1698 BC, which fits the ‘New Chronology’ of David Rohl and his collaborators.”

Since Palmer, Lappin, Rohl et al. claim that the same astronomical data in the El-Lahun documents that Rose employed to move the 12th Dynasty 1477 years closer to the present can also be used to move it only a few hundred years closer, then Rose’s analysis would really be no better than theirs. That is what will engage us now: to see if that is indeed the case. Rose, however, has written a full rebuttal to Rohl, Lappin, Palmer and their collaborators on this very question:

“Lappin has claimed a very high score for his own version of David Rohl’s chronology: at one point (page 79), he even says it is 38 [hits] of 39 [dates]! (I should point out here that my 39 and Lappin’s 39 are not quite the same. I count 3 dates that he does not recognize… [while] he counts 3 others that I have long since rejected… [Sun, Moon, and Sothis (1999), pages 218, 244-245 and 247-249] The scores are not affected) …

“Lappin and Rohl simply discard the El-Lahun Sothic date in year 7 of Sesostris III, mainly because it fails to support Rohl’s chronology…”

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“The Lappin-Rohl chronology is also vulnerable in that it throws the seasons off by more than four months and is in blatant conflict with what we have reason to believe about the behavior [of the timing of the flooding] of the Nile River… [which] seems to have occurred at about the time of the autumnal equinox when the Nile was high. Lappin and Rohl are forced to have [the flood cresting] in May.”

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Lappin, in defending Rohl’s revisionist chronology which places the 12th Dynasty between September 1693 and September 1643 B.C., requires that first the heliacal rising of Sirius, as specifically stated in the El-Lahun Papyri, which fell in the 7th year, 4th month of the second season (prt), and 16th day of that month, simply never happened. Because there is no place for it in Rohl’s and Lappin’s chronology, they have ignored the fact that the El-Lahun Papyri unequivocally state there was a heliacal rising. Therefore, Lappin and Rohl have begun their foray into Sothic dating without Sothis.

But Sothis is an extremely important star as it relates to the dating of chronology in Egypt because it heralded the most important event in Egypt, namely the onset of the flooding of the Nile River which began in June and crested in late September to early October of the year. As Fred Schaaf explains: “One name for Sirius in Ancient Egypt was Sihor—‘the Nile Star’ (also ‘the Fair Star of the Waters’). But an even more important name for Sirius was Sopdet, the ‘preparer’ or ‘precedes’ of the Nile flood.”

11 That being the case, any interpretation of the lunar months related to the rising of Sirius should also be aligned to the rising of the Nile with an emergence of this star in June and the subsequent cresting of the Nile in late September to early October.

And that is precisely the problem, as we will see, with Rohl’s astronomical dating. Since his astronomical dating is based on ignoring altogether the rising of Sirius, he has the onset of the Nile flood occurring in the wrong season of the year and the cresting of the river also occurring in the wrong season. The interconnectedness of these events is crucial in determining whether one has achieved a correct chronological placement of the El-Lahun lunar month data. That is, because Rohl and his supporters have thrown out this vital piece of evidence, they have created an Egyptian calendar completely out of alignment with both astronomical and geological reality.

Second, Lappin and Rohl would nevertheless claim that their methodology fits the history properly where they have matched the months in the El-Lahun Papyri

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11 Fred Schaaf, The Brightest Stars (NY 2008), p. 89
with the orbit of the Moon. But if that was actually so, the seasons of the Egyptian year would correlate with and corroborate their 12th Dynasty placement. Rose has pointed out that the ancient Egyptians celebrated “the feast of Sobek, who was the crocodile-god of the Nile … at about the time of the autumnal equinox when the Nile was high.” And this evidence must correlate with and corroborate Lappin and Rohl’s chronology. Although they claim that the lunar festival dates fit the period to which they assign the 12th Dynasty, in order for this to be the case, they, at the same time, have the Nile River cresting around May instead of late September to early October, when, in fact, the Nile should have crested. In that respect the festival of Sobek must also be held in the wrong season of the year.

In order for this scenario to take place, the seasonal rains that cause the Nile to rise and crest, which occur in the highland of Somalia, arrive four to five months earlier than they should. And, in order for Lappin and Rohl’s chronology to be correct, the orbit or the tilt of the Earth’s axis had to be so different at that time as to allow the seasons to be out of alignment by four to five months. This further requires that the Egyptians completely revised their calendar to permit this disarrangement and then revised it again to bring the Nile’s cresting back to around the autumnal equinox. But this rearrangement of the seasons had to have happened to all the various nations surrounding Egypt!

Finally we arrive at the supposed 38 hits out of 39 for the lunar festival dates that Rohl, Lappin, Palmer and their collaborators claim as confirmation for their location of the 12th Dynasty in time. To analyze this claim, Rose examined 24 relevant New Crescent dates that the Egyptians used to see if these corroborated without any corrections to the Egyptian calendar with their 12th Dynasty placement. The fact of the matter is that to make Rohl, Lappin, Palmer and their collaborators’ dating seem to work required constant changes in the Egyptian calendar during the 12th Dynasty.

“Lappin claims to have hit a very high percentage of the lunar [festival] dates, so we are obliged to examine such claims.

“Lappin’s chronology can be tested by means of the following procedure: (1) from Schoch’s Tables, determine the date of each of the 24 New Crescents [because the particular lunar festival dates in the El-Lahun Papyri each come at a particular number of days after these New Crescent dates] that would be involved. (2) determine the equivalent Egyptian dates, assuming continuity [of the calendar since that time]. (3) add the characteristic or key numbers that are appropriate [to correlate the lunar festival dates with the New Crescent dates]. (4) [then] add whatever [additional] numbers of days would be needed to make the

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12 Rose, loc.cit.
El-Lahun dates fit. (5) tabulate such corrective numbers, letting the total for the most frequently occurring number serve as the number of hits [by Lappin] …

“When all 24 of the relevant New Crescents in the 17th century [B.C.] have been checked, it turns out that Lappin needs [one] correction of 87 days, 10 corrections of 88 days, and 13 corrections of 89 days. Thus, even if we allow him an otherwise-unknown 89-day shift in the Egyptian calendar, [at best] Lappin likewise hits only 13 of the 24 [lunar festival dates], which is indistinguishable from chance.”

Lappin, Rohl, Palmer and their collaborators have the lunar festival dates in the El-Lahun Papyri coming at the proper time only 13 times out of 24 dates, which is a chance occurrence that indicates nothing statistically. In order to accept Rohl’s chronology as outlined by Lappin we have to throw away the heliacal rising of Sothis/Sirius under Sesostris III, in spite of the fact that it is clearly there in the El-Lahun documents. Furthermore we must rearrange the orbit of the Earth or its axial tilt to allow the Nile to crest in May rather than around the autumnal equinox. Lastly, we have to ignore the fact that of the 24 relevant lunar festival dates accepted by Lappin and Rose, in Rohl’s chronology, only 13 of these are hits while 11 are misses.

While Rohl, Lappin, Palmer and their collaborators’ placement of the 12th Dynasty suffers from these gross errors, failing to meet the elementary requirements for handling this astronomical data, Rose’s work suffers from none of these. As we pointed out above, a good scientific methodology and the results of that methodology must correlate all the elements into a unifying whole. Werner Heisenberg understood and pointed out how important in science is the “unifying power of symmetry and simplicity,” as described by Mario Livio:

“Werner Heisenberg, one of the founding figures of quantum mechanics, the theory of the subatomic world, stated once as a criterion: ‘[Scientific b]eauty is the proper conformity of the parts to one another and to the whole … this is what symmetry and simplicity .. are all about.’”

The data from the El-Lahun Papyri pertain directly and unequivocally to the orbits of the Earth and the Moon, the position of the star Sothis/Sirius, and the timing of the seasonal flooding of the Nile. Each of these four elements must be in conformity with each of the others such that each creates with the others and with the whole a symmetry of integration. On all four levels of this evidence Lappin’s analysis does not meet this requirement. He has not correlated these forms of evidence, or made them corroborate one another. The analysis lacks symmetry and simplicity, and the parts are all disconnected from one another and from

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13 Ibid., pp. 112-3
astronomical reality, and therefore are disconnected from historical/chronological reality. On the other hand, Rose’s analysis attains “the proper conformity of the parts to one another and to the whole.” Lappin’s analysis is more like the Ptolemaic model of the motion of the planets. Each planet’s motion required a special set of epicycles and a separate deferent to give it the seeming appearance of symmetry. Like Ptolemy’s universe, Lappin and Rohl’s astronomy requires that the orbits of the Sun, Moon, and Earth had been conveniently rearranged or, more aptly, deranged, so that in tandem their astronomical positions are forced, against reason and reality, to give support to Rohl’s revised chronology. As with Rohl’s chronology of the Nubian 25th Dynasty based on Neo-Assyrian astronomical dating, his location of the 12th Dynasty in and around the 17th century B.C. is a failure.

Peter James and his co-authors fare no better in their location of the 12th Dynasty. They, too, disregard the date in the El-Lahun Papyri of the heliacal rising of Sothis/Sirius and instead concentrate on the many lunar festival dates as these relate to their 200- or so year shortening of Egyptian chronology.

“Because of these problems Egyptologists are turning increasingly to lunar records for an astronomical dating of the New Kingdom. While many of these can be made to fit with the dates originally calculated from the Sothic theory, they are based on only one observation and are of no value to absolute chronology since new moons will occur on the same civil [calendar] date at twenty-five-year intervals. Thus single lunar dates can really be used only to fine-tune an already established absolute chronology.

“Of far greater significance than isolated lunar references is the information in another papyrus from El-Lahun, giving sufficient data to determine the length of lunar months over an entire year. John Read [after excluding the date for the heliacal rising of Sothis/Sirius in that document] calculated that the [12 lunar-dated] observations recorded in the papyrus match perfectly with the pattern of lunar conditions in the year 1549 BC. Therefore, in Read’s opinion, this ‘placement of the Illahun [El-Lahun] calendar with an apparent 12 for 12 fit has to constitute one of the greatest chronological anchor points in ancient recorded history.’

What J.G. Read did was take the D document of the El-Lahun Papyri—with its 12 consecutive months of either 30 or 29 days—and found a perfect fit for these in the year 1549 B.C. Although his placing of these 12 consecutive months not only fit perfectly with the lunar festival dates and the seasonal flooding of the Nile at the proper time, there are other lunar-dated materials in other documents from El-Lahun that increase the number of lunar dates to 36. These additional dates, although not consecutive, do mention the reign year, the season and the month,
and therefore should correlate with the 12 consecutive month dates analyzed by Read, but these do not. The lowering of the chronology of the 12th Dynasty by about 250 years in terms of Read’s calculations, which reasonably corroborated James et al.’s 200-year revision, was unacceptable to the Egyptologists since it moved the 12th Dynasty of the Middle Kingdom directly into the 18th Dynasty New Kingdom epoch. James et al. explain the dilemma:

“Despite Read’s confidence in this absolute date, an immediate problem arose from the fact that it falls, in the conventional chronology, early in the [time of the] 18th Dynasty. Yet the El-Lahun papyrus has always been assigned a date in the Middle Kingdom some two and a half centuries earlier. Consequently Read had to argue for a redating of the text (in the relative sense) to the 18th Dynasty. [Richard] Parker demonstrated that this was impossible on historical grounds: the papyrus certainly dates to the late 12th Dynasty. He [therefore] rejected Read’s interpretation in favour of his own, which, even after emending one of the entries on the papyrus, still allows for a match of only ten of the twelve [consecutive lunar] dates with modern retrocalculations for the year 1813-1812 BC [where the established chronology places that 12th Dynasty year]. Parker’s method has already been resolutely dismissed by Read [since Parker’s fit was only 10 for 12 while his was 12 for 12]:

‘‘This type of chronology where one claims the historical record is wrong rather than [Parker’s] own analysis [of 10 hits for 12 dates], is no chronology at all.’

‘… For those who still believe that Egyptian chronology is firmly established by astronomical fixes, the El-Lahun lunar data remain a glaring anomaly. Taking these [12 consecutive lunar dates] at their face value would demand an automatic reduction of Egyptian chronology by some 250 years.’”¹⁶

The immense difference between James et al.’s lowering of the 12th Dynasty by about 250 years and that of Lynn Rose by 1477 years is that Rose not only has 10 hits for 12 consecutive lunar months in the El-Lahun document, but he also has a near-perfect score for all the other lunar festival dates in the same document which Read and James et al. do not have. Of 39 lunar festival dates that Rose has, as noted, 36 are hits and one of them can be attributed to bad seeing. Add to that the fact that Rose’s analysis also includes the heliacal rising of Sothis/Sirius in the 7th year, 4th month, 16th day of the second season of the pharaoh Sesostris III, and the depth and breadth of the evidence becomes conclusive for an “automatic reduction of Egyptian chronology by 1477 years.” Then add that all these lunar festival dates fall on particular dates of these lunar months on the correct day after the New Crescent, and the evidence in favor of Rose’s chronology is overwhelming. James et al. have none of these additional forms of evidence and thus, to paraphrase their

own words: “This type of chronology where one claims only 12 dates fit the record, but not all 36, nor the heliacal risings of Sothis/Sirius which works for Rose is no chronology at all.” And I add, “for those who still believe in James et al.’s chronology being firmly established by astronomical fixes, all the rest of the lunar and Sothis/Sirius data in the El-Lahun Papyri remains a glowing anomaly.”

Thus, the two major English revisionists, David Rohl and Peter James, and their colleagues, have failed to anchor their revisions based on astronomical Sothic/ Sirius dating, while Rose has met the criteria of all the scientific evidence/ astronomical data meshing with the documentary evidence in the El-Lahun Papyri.

**DAPHNE CHAPPLE’S CRITIQUE AND EVIDENCE**

Because Rose’s work is stunning in both senses of the word, and certain colleagues of Rohl and/or James understand the depth of his threat to undermine their chronologies, Daphne Chappell has come forth to criticize Rose. And she, too, has offered other evidence to date the 12th Dynasty to either the 15th or 16th century B.C. For the 16th century B.C., she too cites Read, but as we will see she has no solid astronomical basis for the dates which she proposes, and admits to this fact:

“Rose has found a fit in 353 BC for the 12th Dynasty phyle-priests. I [also] found a perfect fit in 1427 BC and Read found one in 1549 BC. Whether they belong there is another matter and one that will not, in my opinion, be proved with astronomical dating alone, nor by counting [lunar festival dates’] hits and misses.”

Again, note that what Chappell has not done—neither have James et al., Rohl, Lappin, Palmer and their collaborators—is attempt to fit the heliacal rising of Sothis/Sirius into her considerations. It is as if this data may be dispensed with without explaining why. They all do so because there is no possibility that the heliacal rising of Sirius could occur on the reported date in one of their analyses. So they defenestrate it–i.e. throw it out the windows of their minds, and Rose in response to Chappell directly pointed out “that she is paying no attention to the Sothic date.” Chappell, of course, has never explained the scientific basis for denying the Sothis/Sirius statement that this star rose on prt 16 in the reign of Sesostris III confronting her in the El-Lahun Papyri. When she accepts only one part of the evidence, part of the lunar data in that document, and dismisses the rest

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18 Ibid., p. 5
with the star date in that same document, it shows that she ignores inconvenient data.

Nevertheless, Chappell does claim that she has “a perfect fit in 1427 BC…” for this lunar date. A perfect fit for 1427 B.C., as with Read, would mean she has found 12 hits for 12 lunar months of that year. In fact, she doesn’t have anything of the kind and should be embarrassed to have made such a blunder. In his response, Rose has shown: “Chappell’s ‘perfect fit’ of 1427 BC turns out to be anything but … Chappell hits 8 of the 12 dates given in [Document] D. If we allow her ‘overtime’ explanation of the [date] II 3ht 20, she hits 9.”

What Chappell has instead of a “perfect fit,” is at best 9 hits out of 12 lunar dates or a score of 75 percent, not 100 percent. Again, Rose’s criticism on this point has been in the literature for over two years, and Chappell has neither owned up to the fact that she does not have a “perfect fit” nor shown Rose is in error regarding this matter. As with her dismissal of the Sothis/Sirius statement of the El-Lahun Papyri, Chappell has thus far failed to answer Rose. And the reader is asked to note that she has employed, as did Read, 12 lunar dates, and in her case completely ignored all the others without an explanation for doing so. In all these cases inconvenient evidence never enters into her discussion.

There is yet another problem with her criticism in these matters. In criticizing Rose, Chappell has turned to Richard A. Parker and his work with Papyrus Carlsberg 9. What she did was correlate the dates derived from Parker with dates in the El-Lahun Papyri to show that there was a clear “disparity of three days between Parker’s fit and that of Rose.” She adds:

“Parker considered that if a historical date is more than one day out from the schematic calendar for the calculated year there is no fit, so no need to proceed with the mathematically simple, but time-consuming, actual calculation [that Rose relied on] using Schoch’s tables. The lunar cycle [in the Carlsberg 9 Papyrus] is so constant that even a pattern of 12 consecutive months will be repeated every 25 years, with only one [sic] day of variation. There are, therefore, many possible fits that need to be tested [against Parker’s table] and a degree of accuracy required greater than 75% before one can even consider the placement might be correct, or so one would think. Parker claimed a fit beginning in 1813 BC with 10 out of 12 hits, including one restored date, one amended date and the two incorrect dates out by one day. Read claimed a fit beginning in 1549 BC, with 10 out of 12 hits, by assuming the lunar calendar began with the new crescent. Rose claims a fit in 353 BC, with 10 out of 12 hits…”

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20 Ibid.
21 Chappell, op. cit., p. 4-5
“… a fit of 10 out of 12, or 83%, is good enough to corroborate material dated from historical sources.”

Since Rose’s fit, according to Chappell in applying Parker’s table to the El-Lahun Papyri, is 10 hits out of 12, no better than Read’s 10 hits out of 12, or Parker’s 10 hits out of 12 lunar dates, she concludes that “to depend on it to the exclusion of other evidence seems unrealistic.” Chappell will then provide some other possible evidence that will refute Rose. This all sounds reasonable and astronomically accurate, but what Chappell has failed to inform her readers of is that Parker’s work with the Carlsberg 9 Papyrus and its application to the El-Lahun document to derive her percentages is not only “unrealistic,” but a “fraud” because the astronomical literature shows that Parker’s work has no merit in this regard. What Parker did was analyze the Carlsberg 9 Papyrus and interpret it to say just what he wished it to say. Marshall Clagett described the nature of the way Parker forced the Carlsberg 9 Papyrus to derive the results he wanted with a document from around 144 AD or somewhat earlier and retroject this information back to the second or even third millennium B.C.:

“Without detailing the other lines of reasoning that convinced Parker of the correctness of his description of the old lunar [second/third millennium B.C.] calendar but which do not produce certain conviction, I believe we can reasonably conclude that he has given us an [astronomical/calendrical] account that is only barely possible and is quite speculative in detail and not convincing in its over-all argument. His often used [instead of evidence] rhetorical expressions like such-and-such ‘cannot be other than,’ ‘can be nothing other than,’ ‘there is no other plausible explanation,’ ‘What can be more natural than,’ ‘It is a natural assumption,’ etc. when in most cases there could be alternative explanations, make this reader uneasy.

“In brief, it appears to me that Parker’s opinion [not proof] that the old lunar calendar was intercalary may be correct (though not certainly so) …

“Also, strictly speaking, the conclusion that the lunar month in the old lunar calendar commences on the first day of crescent invisibility is not certainly proved, although that seems to be the case for the later lunar calendar [after 500 B.C.] … I say ‘not certainly proved’ because the three main pieces of evidence on which the conclusion [of Parker’s] is based, namely the names of the days of the lunar month, the 25-year lunar cycles of Papyrus Carlsberg 9, and a series of double dates (the lunar month days in the civil calendar) all date from a period more than a millennium or two later than the date when the old lunar calendar flourished.”

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22 Ibid.
23 Ibid., p. 5
But Chappell has calculated her analysis as though there were possible intercalary months which Clagett claims is “Parker’s opinion” that “may be correct (though not certainly so).” She has also used Parker’s “conclusion that the lunar month in the old lunar calendar commences on the first day of crescent invisibility [which] is not certainly proved [in the early second millennium B.C.] although it seems to be the case for the later lunar calendar [where Rose places the 12th Dynasty].” That is, Chappell offers Parker’s evidence that the concepts related to the calendar accepted for the period no farther back in time than to around 500 B.C. can be used to analyze the calendar as far back as ca. 1500-2000 B.C., though Clagett on this point states this is “not certainly proved because of three main pieces of evidence on which this conclusion [of Parker’s] is based.

In one case Clagett informs us regarding the calendar “Unfortunately Parker has had to assume what he should have been proving. That is to say, he first assumes that the first three [lunar] feasts are chronological and relate to the old lunar year and then … in order to justify the chronological assumption he has to make a further assumption for which there is no hard evidence, namely that the festival of Thoth must indeed precede rather than follow the First Day of the year.”

Parker maintained that the lunar dates in the Carlsberg 9 Papyrus are noted for every other month over a 25-year period where typically 309 months is almost equal to 25 years and is off by only one hour. Yet Leo Depuydt claims “no satisfactory explanation has been proposed, I believe, as to why Carlsberg 9 would skip about every other month and begin with the second month [instead of the first, as Parker offers].” Elsewhere Depuydt states:

“Parker’s interpretation of the structure of the [25-year] cycle needs to be reinterpreted … But regardless of this reinterpretation … Papyrus Carlsberg 9 cannot be used as evidence to postulate yearly pairings for the civil-based lunar calendar for the reasons given below. Underlying the [25-year] cycle is the astronomical observation that 309 lunar months are about as long as 365 civil years … The degree of error in the cycle… is … a little over an hour in 25 civil years …

“Parker … did assume that Carlsberg 9 contains a system actually in use. But his theory is based on the assumption that Carlsberg 9 only provides a number for every other month, for a total of 150 (25 years x 6 columns). … However, it appears that Carlsberg 9 provides a number for 300 rather than 150 full civil months (25 x 12) in the 25-year cycle … The nine additional numbers up to 309 need to be inferred …

25 Ibid., p. 19
“Whether Carlsberg 9 does or does not reflect a system that was actually used at some point in the ancient Egyptian lunar calendar is not certain. Whatever may be of the matter, Carlsberg 9 cannot count as evidence for classifying the ancient Egyptian civil-based lunar calendar with either yearly pairing or monthly pairing. Carlsberg 9 may well only show, and I personally believe that that is all it does, a certain fascination with the numerical relation between lunar months and Egyptian civil years.”

In spite of these well-known statements in the literature, Chappell maintains:

“Papyrus Carlsberg 9 predicts the first day of the lunar month in the Egyptian civil calendar for this [25-year] cycle. It only provided dates for the second and fourth month of each season, but Parker tested its accuracy with double-dates that were known from other sources and was able to deduce the dates for the remaining months.”

In essence, Chappell used Parker’s highly questionable interpretation of the Carlsberg 9 Papyrus as evidence to evaluate Rose and support her dating when Depuydt specifically states: “Carlsberg 9 cannot count as evidence for classifying the ancient Egyptian civil-based lunar calendar,” and Clagett has told us that Parker “has given an account that is only barely possible and is quite speculative.” But Chappell used Carlsberg 9 that cannot count for classifying the ancient Egyptian civil-based lunar calendar to count against Rose and support her “perfect fit”, and expects her readers to swallow lock, stock, and barrel Parker’s “account that is only barely possible and quite speculative” to refute Rose. And she did this in the hope that no-one would expose the shallow and callow nature of her argument. In other words, according to Chappell, Clagett and Depuydt don’t know what they are talking about regarding Carlsberg 9!

It is here suggested that the competent reader get a copy of Marshall Clagett’s *Ancient Egyptian Science: Calendars, Clocks, and Astronomy* (Philadelphia PA 1995) and read on pages 7 to 28 his telling exposé of Parker’s manipulations of the evidence in order to make his analysis work. These pages will explode Chappell’s presentation. And it must be further noted that such thorough criticism by one of the great eminences of ancient astronomy, Clagett, is never discussed, mentioned, or

27 Leo Depuydt, *Civil Calendar and Lunar Calendar in Ancient Egypt* (Leuven, Belgium 1997), pp. 198-199, 202  
28 Chappell, *op.cit.*, p. 4  
29 Rose, *loc.cit.*
answered in full by Chappell, as if Claggett never wrote. She did the very same with Leo Depuydt, who also largely discounted Parker’s Carlsberg 9 Papyrus analysis. Why did she withhold this material from her readers as if it does not exist? For another brief example, Depuydt claims that “Brugsch, Borchardt and Parker have also expanded their discussion of the lunar calendar with hypothetical elements. Regardless of the plausibility or implausibility of these elements, several of them remain controversial: A striking example is … Parker’s original lunar calendar.”

Above and beyond all that, Chappell has utterly failed to deal with Rose’s analysis of Parker and Samuel’s work regarding Carlsberg 9. In Chapters three and four of his book Sun, Moon, and Sothis, Rose lists contradictions and problems with their work, and these are numerous. But Chappell, never having read this material which a serious scholar who wishes to criticize Rose should have read, omitted this evidence from her discussion as well. No-one has ever shown this evidence to be in error and this too speaks of the poor quality of Chappell’s critique of Rose.

Alan B. Lloyd presents the following critique of Richard Parker’s work on Carlsberg 9:

“For Parker’s thesis the crucial point is the rule of intercalation [of additional months to the year] which he deduced from the cycle viz. ‘Whenever the first day of [the] lunar [month] Thoth would fall before the first day of [the] civil Thoth, the month is intercalary’ … He [then] states (i.e.) that this rule ‘could have been easily applied before the 25-year cycle [inaugurated around 144 AD]’ in P. Carlsberg was evoked. He then however proceeds … to treat this ‘could have been easily’ as [if it was a] hard fact and states that the lunar calendar used in the [Carlsberg] papyrus had been dependent on the civil [calendar] and must, therefore, have been used after it … This is hardly compelling.

“(1) The antiquity of the introduction of Parker’s rule is quite unknown. It might have been invented at the same time as the 25 year cycle. There is certainly no way of telling which came first from internal evidence.

“(2) The relationship of the civil and lunar calendars in this papyrus proves nothing about relative antiquity [for its application back to the second millennium B.C. as Parker suggests]. …

“(3) The fact that Parker’s scheme would give us three calendars [in Egypt] working simultaneously … does not inspire confidence.

“(4) Why should the civil year be pegged to a lunar year at all [as Parker maintains]? To say [as Parker does] that the civil year ‘had no real being when separated from its natural [lunar] counterpart’ is absurd. Its [the civil year’s] ‘real being’ consisted of 365 days, divided into 3 seasons of 4 30-day months + 5 epagomenal days [i.e. requires no lunar months]. It was designed to provide

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30 Depuydt, Civil Calendar..., op.cit., pp. 141-142 (emphasis added)
convenient divisions for administrative functions. That it should have a fixed relationship to the astronomical year would be superfluous.”

David H. Kelley and E.F. Milone state that “Clagett is entirely justified in rejecting Parker’s use of the Ebers Calendar as evidence for this position.” These statements could be multiplied, but at this point it would be overkill for Chappell and we will proceed with other aspects of her criticism.

Typical of Chappell’s approach to evidence is to cite someone who says something that flies in the face of the evidence. She cites P.F. O’Mara writing in 2003 who “points out, we do not know whether the Egyptians measured the new day from dawn or sunrise.” Dawn comes about an hour before the Sun rises over the eastern horizon. However, Erik Hornung, Rolf Krauss, and David Warburton are authorities who rather contradict O’Mara and Chappell. They stated in 2006: “According to the Egyptian calendar day began during the observability of Mercury as morning star, i.e. at dawn before sunrise.” Clagett states: “It seems almost certain that the Egyptian day began at dawn, the hour before sunrise.” Lance Latham states: “The Egyptians, however, started their day at dawn.” Ross Hassig states: “For instance, day began at dawn for the ancient Egyptians.”

Were all these authorities wrong because Chappell and O’Mara suggest otherwise? Chappell adds: “Nor do we know whether the lunar month began at sunrise or sunset…” The difference in time from sunrise to sunset is roughly about half a day according to the season of the year in Egypt and this is a very large difference in time. But Chappell tells us “we” (the authoritative “we”) don’t “know whether the lunar month began at sunrise or sunset.” The fact of the matter is that other authorities [“we”] tell us otherwise. Eric H. Cline and David O’Connor, writing in 2006, tell us: “The Egyptian month lasted either twenty-nine or thirty days and was determined by observation in the early morning. When the moon’s crescent was no longer visible in the morning sky, the first day of the new month (psdntyw) was declared to begin at sunrise [not sunset].” N. Krauss specifically tells us in 2007: “The Egyptian month began with the waning crescent

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31 Alan B. Lloyd, *Herodotus Book I: Commentary I-99* (Leiden, the Netherlands 1976), pp. 24-25
33 Chappell, *op.cit.*, p. 5
34 Erik Hornung, Rolf Krauss, David Warburton, *Ancient Egyptian Chronology* (Leiden, the Netherlands/Boston MA 2006), p. 50
35 Clagett, *op.cit.*, p. 22
37 Ross Hassig, *Time, History, and Belief in Aztec and Colonial Mexico* (Austin TX 2001), p. 31
38 Chappell, *loc.cit.*
Moon before sunrise, not with the appearance of the new crescent just after sunset.” A.E. Samuel states: “It is also possible to determine the beginning of the [Egyptian] month by observation of the last visibility of the crescent in the morning [at sunrise, not sunset].”

One could go on and on regarding these times for the onset of the Egyptian day and month that contradict Chappell’s “we don’t know” when these occurred. But what she has done is use that faint possibility for a day or month not beginning at sunrise to prove Rose is in error and that her evidence and dating is correct when in fact she hasn’t proven either case one way or the other. The unwary reader, seeing her almost bombastic assertions, may well take Chappell at her word. And this is all she has offered as evidence: her own authority—“we.”

Rose was right to point this out to Chappell:

“The Egyptian day did begin in the morning and the civil month did begin on the morning of first invisibility. Although some, like O’Mara, worry rather pedantically about whether dawn means first light, sunrise, or what have you, virtually all Egyptologists are in agreement that it was in the morning, not in the evening. This is not some arbitrary pronouncement of my own.”

Typical of this approach of using her own authority as the main form of evidence, which is not really evidence, is this further unproven assertion: “Rose has crowded in lunar dates wherever they fit, but do they belong there? It is all too easy to find a fit for individual lunar dates and impossible to fail if the dates are not linked to a regnal year, or co-regencies are flexible.” Here she accuses Rose of placing lunar dates into the astronomical historical 12th Dynasty scheme without rhyme or reason, simply on his own to make the evidence fit. But Rose did nothing of the kind. These lunar dates are not “Rose’s dates,” they are the dates related to the reigns of 12th Dynasty pharaohs supplied by Ulrich Luft, one of the greatest authorities in this field of research. And in fact Chappell was told of this fact but omitted discussing that Rose told her that these “are from Luft.”

“Chappell says: ‘Rose has crowded in lunar dates wherever they fit [his chronology]’, but can she specify any occasions where I ever did that? EVERY PLACEMENT IS IN FACT BY LUFT, NOT BY ME – except for that one alternative psdnjw placement that I found for document A. When I introduced the list of feasts and New Crescents, I was of course speaking of each item on that

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40 N. Krauss, Star Map (Berlin/Heidelberg/NY 2007), p. 30
41 A.E. Samuel, Handbuch der Altertumswissenschaft (Munich, Germany 1972), p. 14
42 Rose, Reply to Chappell, op.cit., p. 8
43 Chappell, op.cit., p. 2
list when I stated that ‘THE PAPYRUS NUMBER, THE REGNAL YEAR, AND THE EGYPTIAN DATE … ARE FROM LUFT’.”

While Rose actually and directly informed Chappell that “every placement is in fact by Luft, not by me” and that “The papyrus number, the regnal year, and the Egyptian date … are from Luft,” Chappell insists Rose “crowded in” the lunar dates “wherever they fit.” Why she misrepresented the evident fact that he got his dates from Luft defies reason. What Chappell evidently did was in the words of Mark Twain, “Get your facts first, and then you can distort them as much as you please.”

Rose continues:

“However, Chappell seems to have missed that point and it seems that she finds me to blame for the good-faith findings by Luft, who is the world’s leading expert on these subjects. Clearly, she should have argued her case against Luft, not me! Luft has far more reasons for placing reports in one year rather than in another than Chappell could imagine. (Luft repeatedly examines prosopography, autography and various other clues that restrict our choices).”

What Chappell totally ignored is one of the greatest problems one faces when retrocalculating various quite short time dates such as first and last crescents of the moon back beyond 700 B.C. The immense problem lies in the uncertainty of the slowing of the Earth’s rotation over time. This is termed the “Earth’s acceleration” as one goes further back in time and is known as Delta T or ΔT. The Moon as it orbits the Earth creates a larger tidal bulge in the ocean and a smaller one on land directly underneath the Moon’s position above the Earth. The Moon thus gradually pulls on that oceanic bulge and slows the Earth’s rotation. As we go back in time, this slowing effect was greater, but not at a strictly regular rate.

An excellent example of a well-timed eclipse is presented by John M. Steele which relates to the slowing of the Earth’s rotation and indicates just how nearly exact it can be known prior to 700 B.C.:

“We have Roman solar eclipses. For example Theon of Alexandria … described a solar eclipse, which he observed on June 16, 364 AD [very precisely]. The time reckoned by civil days and equinoctial hours of the exact eclipse conjunction … took place … after midday on the 22nd of [the Egyptian month of] Payni … And moreover we observe with the greatest certainty the time of the beginning of contact reckoned by civil and apparent time as 2 5/6 equinoctial hours after midday, and the time of the middle of the eclipse as 3 ¾ hours, and the time of the complete restoration [of the solar disk] as 4 ½ hours approximately after said midday of the 22nd Payni …

44 Rose, op.cit., p. 7; capitalization and bold added
45 Mark Twain, cited in Leo Rosten..., op.cit., p. 7
46 Rose, loc.cit.
“Theon’s observed times [he probably used either a sun dial or sand or water clock] and those given by modern computation [make it] … immediately clear that all the contact timings are early by just under half an hour. However, despite this systematic error, these timings are very self-consistent.”

Steele goes on to explain that there were no accurate chronometers at that ancient period but nevertheless the evidence is quite clear that there was a nearly precisely-timed eclipse dated to a precise date—June 16, 364 A.D., that can be known from Delta T values and that there were others. But as we go further back in time, the number of eclipses known with some accuracy becomes smaller and less accurately measured so that by around 700 B.C. we have reached the point when solar eclipses can no longer be determined with any reasonable form of accuracy. Thus the position of the Moon used by the Egyptians for the onset of day at dawn or the end of the lunar month just after last crescent when the Moons crescent can no longer be seen becomes too vague to be of any value. And well beyond 700 B.C., this inaccuracy becomes exacerbated so that by 1500 B.C. and beyond one cannot accurately extrapolate to just such moments to determine when the Egyptian day or the Egyptian month began. Chappell, however, suggests that this can be done, and builds her chronological arguments on this ill-founded evidence. The 700 B.C. cut-off date for doing just that is commonplace in the astronomical literature.

Sacha Stern describes this acceleration thus:

“Since the rotation of the Earth … [slows] the time of ephemerides [position of the Sun, Moon, planets and other celestial bodies change] in earlier historical periods … for instance in the early 4th c[entury] CE Δt is thought to have been close to 2 hours … [or] two hours earlier. The rotation of the Earth … is known to be slowing down (partly as a result of tidal friction from the pull of the moon), but at an irregular and somewhat unpredictable pace. As a result, the value of Δt for ancient periods remains rather speculative.”

To determine as accurately as possible this acceleration of the Earth’s rotation in the past, observations of lunar and especially solar eclipses in great measure are used to help establish the rate. From recent times going back into the past the evidence for these eclipses was fairly accurate, at least as far back as clocks existed. But prior to this period, when no clocks existed, the accuracy of these eclipse times becomes less and less certain. Nevertheless, so long as there was enough eclipse data from ancient documents around the globe, the ability to determine ΔT was still

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47 John M. Steele, *Observations and Predictions of Eclipse Times by Early Astronomers* (Dordrecht, the Netherlands 2000), p. 103
fairly accurate. For a technical discussion of this material see F.R. Stephenson, *Historical Eclipses and Earth’s Rotation* (Cambridge UK 1997).

The problem is that there are fewer and fewer attested eclipse reports the further back in time one goes, and 700 B.C. seems to be the cut-off point, where the position of the Moon as related not only to solar eclipses but to the precise times of first and last crescent used for determining the beginning of the day and the onset of the month in Egypt and elsewhere can be significantly affected. Remember, the Egyptians began counting their days and months by looking for the short interval when the crescent Moon completely disappeared. That would be greatly affected by the slowing of the Earth’s rotation. This short interval can be off by hours and therefore should not be dated beyond what is generally and reasonably related to these eclipses. Robert R. Newton outlines this: “Within the period for which we have [eclipse] data, that is about -700 to the present, we know the parameter of D [Delta T] with an accuracy of about 2”/cy². When we go [back in time] to dates from -700, however, we are extrapolating beyond the data and the uncertainty in D [Delta T] necessarily increases …”

2”/cy² is equivalent to 2 seconds of arc of the Earth’s rotation over 100 years times the previous century squared. From 1900 to 2000 A.D. the Earth’s rotation on average accelerated by 2 seconds of arc, or 1² x 2 seconds of arc. From 1800 to 1900 A.D. the Earth’s rotation also accelerated on average 2 secs of arc, or 2² x 2 secs of arc = 2 x 2 x 2 = 8 secs of arc.

- 500 years prior = 5² x 2 = 50 secs of arc
- 1000 years prior = 10² x 2 = 200 secs of arc
- 2000 years prior = 20² x 2 = 800 secs of arc

In terms of calculating the time, the same formula applies except that instead of using 2 seconds of arc we replace this value number with the general length of time it takes the Earth to rotate 2 seconds of arc, which is about 29 seconds of time.

M.W. Markemson explains this time element:

“It is surprising to find that a change in the earth’s rotation rate of only 0.0016 seconds per century can produce a change in time of several hours in only 2000 years, but we must remember that such a change is an acceleration and must be multiplied by the square of the time interval to obtain the overall effect. Suppose, for example, that two clocks, A and B, start at exactly the same instant and are running at the same rate. Clock A runs continually at a perfectly uniform rate, whereas Clock B is “corrected” occasionally by observations of star transits so

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that its actual rate is determined by the earth’s rate of rotation. If the earth is
slowing down and each day is a little longer than the previous one, Clock A will
appear to gain on Clock B. If its rate per day appears to increase by 0.0016
seconds per century the average gain in rate per day is 0.0008 seconds. This
quantity, multiplied by 365.25 (the number of days in a century), yields 29
seconds as the amount by which Clock A is ahead of Clock B at the end of the
first century. Table 1 indicates how the apparent gain of A over B, or the actual
retardation of Clock B, increases proportionally to the square of the interval.

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This, however, is a general formula because the speeding up/acceleration back
in time is not uniform. This is corrected by using the numerous eclipse records to
fine tune the change in the Earth’s rotation by a fairly precise amount of time. The
actual time change over 2000 years may be many minutes different than the
formulae described above. Without this consideration, one cannot accurately
pinpoint astronomical data in the past.

Therefore, when Chappell extrapolates the position of the Moon for first or last
crescents as far back in time as 1427 B.C., which is 727 years distant from 700
B.C., she has no solid astronomical Delta T data basis from which to draw any
accurate conclusions. Neither Chappell, nor Rohl, nor Lappin, nor Palmer, nor
anyone else, for that matter, can rigorously date the 12th Dynasty into the mid-
second millennium B.C. or its conventional position in 2000-1800 B.C. because
they lack accurate data on Delta T to make such a claim or calculation. Yes, the
various computer programs do go back to this period and beyond, but they lack
the scientific factual bases upon which to do so. For example, Stephenson did just
that for a solar eclipse as reported by Newton who speaks of

“… the eclipse of -1374 May 3, which we studied … We remember that
Stephenson [1970] assumed that he could calculate the time of a [solar] eclipse at
this period of history with an accuracy of 5 minutes in time … Using this [Delta T]
assumption, he assigned the date of a solar eclipse from Ugarit as -1374 May 3, and
he found that the magnitude of the eclipse there was 0.99 [percent of the Sun
covered up]. M [Muller] [p. 13.2] finds that the eclipse was total there, and writes:

\textsuperscript{50} M.W. Markemson, “Ephemeris Time and Universal Time,” *Astronomical Society of Pacific
Leaflets*, vol. 9 (May 1963), p. 52
‘It can be said with high confidence that the -1374 date and event is correct, thereby establishing the event as the earliest independent verifiable Ugaritic date …”

“When we calculate the circumstances of the eclipse at Ugarit using the quadratic formula, we find that the magnitude was 1.0006, meaning a total eclipse as M says. However, when we use the linear formula, the maximum magnitude we find is –0.73 [percent of the Sun was covered by the Moon], meaning that the eclipse was not visible in Ugarit. Thus we do not know whether the eclipse was visible at Ugarit or not, and we have no idea of the magnitude if it was visible. There is no basis for taking -1374 to be the date of the record.’’

Kevin D. Pang et al. cite Peter Huber who in 1987 employed a Delta T value versus a time formula going back in time to specify lunar eclipse dates in the ancient world well before 700 B.C. They admitted “we are extrapolating the astronomical theories 2000 years beyond safe grounds.” They add, “The lack of clock error [Delta T] values before 700 B.C. … has left the time scale of the ephemerides unconstrained.” These researchers have been attempting to find new eclipse dates from Shang Dynasty times in order to calculate back prior to 700 B.C. But the evidence regarding Delta T is quite clear on this matter and this affects all that Chappell, Rohl, Lappin, Palmer, and James et al. have presented.

The first or last lunar crescent which Chappell has employed to determine the beginning of the lunar month occurs during a period of a few hours. Therefore, when one does not know and cannot accurately calculate the nearly precise position of the Moon during these precious few hours, one cannot know whether those crescents were visible at that time or not. Hence once can have no accurate fit, and no idea if it was seen by the Egyptians on a particular day to be used to start the lunar month. There is no astronomical basis for accepting Chappell’s 1427 B.C. date and day for the onset of a lunar month which she has postulated. The scientific Delta T door is shut for any such calculation.

What will be galling to Chappell, Lappin, Rohl, James et al., Palmer and their collaborators is that Rose has a fit which suffers from none of these Delta T problems. To put the case bluntly, Gerard Williams stated it thus, on which I will amplify:

51 Ibid., pp. 464-465
52 Kevin D. Pang et al., “Astronomical Dating and Statistical Analysis of Ancient Eclipse Data,” History of Oriental Astronomy (Dordrecht, the Netherlands 2002), p. 113
53 Ibid., pp. 95-96
“‘But you will admit, gentlemen, [Chappell et al.] said, a little desperately, ‘that my theory holds water, that is, in default of actual information’.”

Thomas H. Huxley sums up what Chappell and all the others that support Rohl and James et al. are actually up to when they employ astronomical data in the heedless way that we have observed:

“I do not presume to throw the slightest doubt upon the accuracy of any of the calculations … On the contrary, it is necessary to my argument to assume that they are all correct. But I desire to point out that [the case presented by Chappell] seems to be one of the many cases in which the admitted accuracy of mathematical process is allowed to throw a wholly inadmissible appearance of authority over the results obtained by them. Mathematics may be compared to a mill of exquisite workmanship, which grinds you stuff of any degree of fineness; but nevertheless, what you get out depends upon what you put in; and as the grandest mill in the world will not extract wheat flour from peascod, so pages of formulae will not get a definite result out of loose data.”

The loose data that Chappell relied on is Parker’s bogus data from Carlsberg 9 and the Delta T evidence she seems to be completely unaware of. But the fact remains that she cannot and does not account for all the data in the El-Lahun document but only those parts that confirm her position, whereas Rose used all the data. And furthermore, Rose has answered every point Chappell raised. To date she has failed to respond to his criticisms. I suspect that she cannot and will not do so. The very same applies to Rohl, Lappin and their supporters who have failed to answer Rose. I believe that if they had the wherewithal to respond, they would have done so long ago. This failure is a tacit acquiescence to the poverty of their evidence and the untenable nature of their chronology.

CHAPPELL AND THE 12TH AND 18TH DYNASTIES: TECHNOLOGY AND CHRONOLOGY

There are, however, other forms of forensic historical evidence related to chronology that clearly place the 12th Dynasty well into the first millennium discussed in volume I. Some of these are related to a point that Chappell has raised with respect to Rose’s chronology of the 12th Dynasty/Middle Kingdom and the 18th Dynasty/New Kingdom. Since the Sothic astronomical evidence ends the 12th Dynasty with the coming of Alexander the Great followed directly by the

54 Gerard Williams, Dr Mortimer and the Aldgate Mystery (London 2000), pp. 189-190, cited in Henige, op.cit., p. 173
Greek Ptolemies, no Egyptian dynasty could possibly come after the 12th unless they did so under the Ptolemies. Chappell asks “What are we to make of his [Rose’s] claim that the [12th Dynasty] Middle Kingdom was not known to the [18th Dynasty] New Kingdom?”  

In terms of the chronology, Chappell supports the idea that the 18th Dynasty had to come well after the 12th. Nevertheless, Chappell turns to documentary and archaeological evidence to refute the science.

Certain technologies, however, contradict Chappell’s critique on this point and support Rose’s position. If Rose is correct, the technologies of the 12th Dynasty as well as other forms of evidence should be more advanced than those of the 18th because the 12th Dynasty existed just prior to the Greek epoch. Since the 18th Dynasty preceded the 12th, it should have technologies that are inferior to those of the 12th. In this respect let us examine the technology of attaching wooden handles to tools and weapons in Egypt.

Hafting is the term used to describe the ways that this was done. There were four methods of attaching a wooden handle or hafting it to a stone or metal tool or weapon. One method was to lay the stone or metal part on a surface and place the handle on it in the correct position for it to be used, then lash these with leather to hold them together. This worked, but not very well, because the tool was lopsided and if one struck another hard material with it, such as a stone, the more one struck the more likely the lashings that held the tool or weapon together would shift or even open to separate the two parts. The second was to tang or split or cut one end of the handle apart to form a wedge, force the stone or metal element into that opening, then lash it with leather or rope so the two were held together. This made the tool balanced rather than lopsided, but the harder one struck it against an object, the more likely the handle would split and the stone or metal part would be driven out of the wedge. The third method was to cut holes in the handle in one or more places and have the metal part of the tool or weapon snugly slotted into these handle sockets and perhaps then lash the tools together. This was a great improvement, but an axe so hafted if it hit an object obliquely might again split the handle. The last method of hafting and the best was to cast the metal tool or weapon with a socket in it, into which the handle could be snugly pounded. This gave balance to the tool, kept the handle in place without lashings, and the fact that the wooden handle was neither partially split nor riven with holes allowed it to maintain its integrity when great force was exerted. Today’s tools are socketed affairs.

In this respect the hafting techniques found for the 12th and 18th Dynasties can be compared. In volume I, pages 229-30, G.A. Wainwright discussed a spearhead.

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56 Chappell, *op.cit.*, p. 2
found in a burial chamber clearly of the 12th Dynasty which exhibited “no sign of intrusion of a later date” with a “Nubian spearhead” of iron that contained “no nickel.” Wainwright stated that the spearhead “is not tanged [lashed] … but socketed.” Jane C. Waldbaum understood the dilemma that confronted historians: there should have been no such iron socketed weapon at that time based on the established chronology; she states:

“[The site of] Buhen: another piece whose antiquity is doubted is a spearhead from the 12th dynasty tomb at Buhen in Nubia. It is non-meteoritic and the blade is flat, leaf-shaped and socketed. The grave in which it was found was intact [undisturbed] and the spearhead was associated with the skeleton [there]. Nevertheless Wainwright casts doubt on its antiquity because of its shape—it seems to be more like the weapons of modern natives than like contemporary Egyptian weapon shape. Wainwright also questions its large size (30.50 cm [12 inches long]), the fact that it was socketed and not tanged, and that it is non-meteoritic. It should be noted, however, that none of these objections is conclusive and the circumstances of its discovery seem unimpeachable.”

In terms of the established chronology, these elements cannot exist at that early time, nor even where Rohl, James et al., Chappell and their supporters put the 12th Dynasty. But these elements—non-meteoritic iron, socketed spearhead—fit perfectly well into the first millennium chronology of Rose, Heinsohn, and Sweeney.

When did socketed hafting generally become fairly common in Egypt? Again, the answer contradicts the established chronology and supports the short one. Vere Gordon Childe shows “The expedient of providing a hole [socket] in the ax-head parallel to the blade … was never adopted in Egypt … till late in the Iron Age.” Flinders Petrie also shows:

“The use of a socket was very slowly adopted by the Egyptians; they never employed it for bronze or for hammers, and only a few socketed iron axes have been found which are probably Greek or Roman importations. Egypt therefore gives no light on the history of socketing elsewhere. We have seen how the socket appears to have been used in Syria as early as the XII dynasty and in Greece in the XVIII dynasty … Nevertheless socketing does not seem to have

59 Childe, op.cit., p. 71
been very prevalent in the bronze age ... It is curious how slowly the most effective handle came into use [in Egypt].”

The reader is urged to note that Petrie specifically states of the Egyptians “they never employed [the socketed haft] for bronze.” That is, in Egypt itself bronze tools and weapons did not have a socketed haft in the head. While it is argued that the “socketed javelin head appears at the time of the Hyksos” and Kurinsky states “the [Hyksos] Semites taught the people of Egypt how to set the helve [haft] or handle into a socket through the [bronze] head instead of tying [lashing] the head crudely into it. This change made such tools more enduring and made it possible to wield them with greater force because the handle would not split as readily nor would the head fall off.” Nevertheless, the few socketed bronze spearheads found in New Kingdom/18th Dynasty times are not found in Egypt but, as reported by Suzanne Richard, “socketed bronze spearheads have been found in Palestine and Syria.” Petrie’s comment that in Egypt in the Bronze Age socketed tools and weapons were “never employed ... for bronze” and the statements of Petrie and Gordon Childe that socketing did not come to Egypt “till late in the Iron Age” and “how slowly the most effective kind of handle came into [Egyptian] use,” show that the 18th Dynasty did not employ this technique or, if so, only outside Egypt, and not with tools. This 12th Dynasty spearhead is not the only socketed object of this dynasty found in Egypt. Ann Rosalie David, speaking of Kahun in Egypt during the 12th Dynasty, shows that a “socket-head of a drill [was found there].”

While there is clear evidence that the 12th Dynasty employed—here and there—in Egypt socketed weapons and tools, and thus knew of this hafting technology and used smelted iron, the 18th Dynasty, if it actually followed the 12th, should have used socketed hafting profusely, based on Chappell and those who maintain that the 12th Dynasty came before the 18th. Instead, because the 12th Dynasty reigned in the late first millennium B.C., that is well into the Iron Age, it knew of and used smelted iron and socketed hafting while the 18th Dynasty seems to have known or employed neither technology IN EGYPT. The technologies of manufacturing smelted iron and socketed haftings thoroughly contradict the concept that the 18th Dynasty came prior to the 12th that Chappell offers, and supports Rose that the 12th Dynasty came after the 18th.

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60 W.M. Flinders Petrie, *Tools and Weapons Illustrated by the Egyptian Collection in University College London* (London/Aylesbury UK 1917), p. 11
62 Kurinsky, *op.cit.*, p. 115
Beyond all this, the 12th Dynasty was carving hard stone into statues and inscribing them. Peter Clayton shows there is a

“… seated black granite statue of [12th Dynasty pharaoh] Senusret III immediately identifiable by his distinctive, world-weary features and confirmed by the cartouche that is carved on his belt” [as well as another]

“Red granite head of Senusret III, one of the most powerful portraits of the king, found at Karnak.”

According to Chappell, the 12th Dynasty reigned in the Bronze Age, but as we have repeatedly discussed, one does not carve and inscribe such hard stone as granite with bronze tools. This corroborates the evidence that the 12th Dynasty had carburized iron tools and existed in the Iron Age, after 600 B.C., when iron became known and common in Egypt, and not in the 2nd millennium B.C. when iron was unknown in Egypt to do this work.

There is yet another technology that indicates that the 12th Dynasty reigned after 600 B.C. and thus could not be followed by the 18th Dynasty. Neither Chappell nor any of the proponents of Rohl or James et al.’s revisions dispute the chronological arrangement wherein the 11th Dynasty preceded the 12th. However, if it can be demonstrated that the 11th Dynasty reigned about 600 B.C., then the 12th Dynasty following it would reign up to the time of Alexander the Great, and the 18th Dynasty could then never follow the 12th, again, because it would have to reign in Ptolemaic times. Above we discussed slavery in the Laurion mines of Attica near Athens in Greece. We further discussed the Laurion mines in volume II, pages 422-3 and 463-4. There we described that lead ore to produce silver was mined, according to Milton Meltzer, at “Laurion about 550 B.C.”

John Dayton, too, shows: “The mines of Laurion in Greece are first mentioned in the Athenian records of 500 B.C.” And R.F. Tylecote shows “silver was, as we know from the Greek workings at Laurion (600-25 BC), a product of lead mining.”

Thus, it is rather clear that any Egyptian dynasty that had Laurion lead products reigned no earlier than 650 B.C. at best. Here, then, is the problem: there are clear analyses of 11th Dynasty silver objects that prove they come from Laurion lead, as Jean-Claude Poursat reports: “Z. Stos-Gale [shows]:

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66 Milton Meltzer, *Slavery: A World History* (NY 1993), p. 71; the page number was inadvertently omitted in volume II: author’s apology
analyzed lead and silver objects of Egyptian provenience in the Ashmolean Museum. Two XIth Dynasty silver objects are made of Laurion silver …”

This means that the 11th Dynasty reigned no earlier than around 650 B.C. and the 12th following it had to reign from some point in the 500’s to 331 B.C., just where Rose placed it. Nevertheless, based on the established chronology, historians argue that these mines had to have been in operation as far back as the third millennium B.C.—around the time of the 11th Dynasty. This, of course, is based on historical rather than scientific or technological evidence. For example, Poursat, above, claims 11th Dynasty silver “offers some support to the hypothesis of contacts between Egypt and the Aegean in Middle Minoan times.” He is essentially arguing that since the established chronology is correct, the Greeks were mining lead to make silver as far back as 2000 B.C. But, as shown in volume II, pages 481-5, the technology to smelt silver from lead was only available around 600 B.C. Michail Yu. Treister admits as late as 1996 that “The view that the Laurion mines had never been exploited before the second half of the 6th century B.C. was defended by E. Ardaillon and is still prevalent today.” Yet he still argues that pottery dating, a non-scientific method, places these mines as running in the Bronze Age ca. 1200 B.C. or earlier and also in the 9th, 8th, and 7th centuries B.C. He then presents this astounding problem for all he is offering: “… lead isotope analysis has shown that there is no trace of Laurion silver in early Attic coins dating before ca. 530-520 B.C., and it is never found in substantial quantities in the metallic Attic coins, dating before ca. 500 B.C.”

We are being asked to accept that the Greeks were exploiting the Laurion mines during the third and second millennium B.C. so the 11th Dynasty could have Laurion lead for silver, that is, they knew how to smelt lead ore into silver, but when the Greeks decided to make coins 1500 to 1600 years later, they didn’t use Laurion lead to convert to silver. Why the Egyptians had silver from Laurion at 2000 B.C. but not the Greeks for coinage around 600 B.C. defies understanding. Jan G.P. Best et al. outline this obvious contradiction: “from the analysis of silver man had not learnt to obtain silver from galena [lead ore] before about 500 B.C.”

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70 Ibid.
71 Michail Yu. Treister, The Role of Metals in Ancient Greek History (Leiden/NY/Köln 1996), p. 23 (emphasis added)
72 Ibid., p. 24
73 Jan G.P. Best et al., Interaction and Acculturation in the Mediterranean (Amsterdam, the Netherlands 1980), p. 164
Nevertheless, it is possible that the 18th Dynasty might have come just prior to the 12th or overlapped it, because a “Dagger of Amenophis III c. 1400 [B.C.] from Beth Shan [Palestine] … is clearly made from the rich azurite deposit from Laurion.”\(^7^4\) This also places the 18th Dynasty in the first millennium. Yet there is no scientific or technological evidence to place the 18th Dynasty after the 12th nor place it in the second millennium B.C. where Chappell has it. (1) The Sothic astronomical evidence places the 12th Dynasty ca. 500 to 330 B.C. (2) The Laurion silver places the 11th Dynasty around 600-500 B.C. with the 12th following it, again 500 to 330 B.C. (3) The evidence of non-meteoritic iron places it after 600 B.C. when Egypt enters the Iron Age. (4) The evidence of socketed hafting places the 12th Dynasty in the “late Iron Age” according to Gordon Childe, again after 600 B.C. (5) The evidence of the 12th Dynasty carving and engraving of black and red granite—a very hard material—also places the 12th Dynasty in the Iron Age after 600 B.C.

None of this scientific and technological evidence supports Chappell and the proponents of the chronology she upholds in any way whatsoever. There is the possibility, though, that the 18th Dynasty did overlap the 12th, but it could never have come after it.

Add to all this the linguistic and other evidence offered in volume I with respect to the 18th Dynasty and also the 12th, and there is nothing to support Chappell’s contention that these dynasties reigned in the second millennium B.C.

The following anecdote presented by Henige explains the dilemma in which Chappell finds herself:

“The president of the James Cook Society was downcast. He had learned that an arrow reputedly made from one of Cook’s bones was actually made from an animal bone. Undaunted by the palling effects of technology, he expressed confidence: ‘[on] this occasion technology has “won” but I am sure that one of these days … one of the Cook legends will [prove to] be true—and it will happen, one day.’”\(^7^5\)

“[Cliff] Thornton’s lament underscores two points. The first is that hope continues to spring eternal. The second is that modern technology is a two-edged sword for the hopeful believer and has made a habit of throwing cold water on historical myths.”\(^7^5\)

In this case technology has thrown cold water on the chronology that Chappell and her allies have adopted. As a believer she may hope that the technology will place the 18th Dynasty after the 12th and that it “will be true—and it will happen, one [of these] day[s].”

\(^7^4\) Ibid.

\(^7^5\) Henige, op.cit., p. 57
There are other aspects of the 12th Dynasty which are pivotal in Egyptian history. In many respects the 12th Dynasty is the epitome of Egyptian development. For example in the construction of the town of El-Lahun, Sir Alan Gardiner explains that “The site of El-Lahun, excavated by Petrie, proves to be of exceptional interest, since it yields the remains of a town all of one period, revealing an exceptional degree of town-planning and a mass of furniture, implements and ornaments almost unique in the land of the Pharaohs.”

Barbara Watterson further points out:

“El-Lahun [is] the earliest known example of town planning… The streets, especially those in the western sector, were straight and crossed each other at right angles—an example of a grid system re-invented over 1000 years later by Hippodamus of Miletus. Down the center of every street ran a shallow channel, a little over half a meter [1.5 feet] wide which functioned as a gutter.”

The 12th Dynasty runs from about 2000 to 1750 B.C. based on the established chronology but over 1000 years later El-Lahun is well inside the first millennium around the time straight-street systems developed, roughly about 600 B.C. or later. Barry J. Kemp shows, however, that the 12th Dynasty seems to have built “geometric [laid-out grid] mud-brick towns and forts of the Middle Kingdom,” while the 18th Dynasty did not in its best preserved city “… the greatest city that has survived from ancient Egypt: El-Amarna of the New Kingdom [18th Dynasty] … Most of this city was built around a rejection of, or an indifference to, … a planned geometric [straight-streets that meet at right angles] aesthetic. … [I]nstead … [it] reflect[s] a mentality very different from [the grid system plan of] that behind Kahun [El-Lahun].”

Saggs further explains: “The city of El-Amarna … shows some degree of planning, in that it was based on three roads running roughly parallel to the [Nile] river and each other, but beyond that there is little evidence of overall town planning.”

The reason that the 12th Dynasty adopted layouts for towns and forts that were geometric was that it reigned closer to the time when such layouts were more common while the 18th Dynasty was further removed in time from this period and hence failed to use the grid plan. This again indicates that the 18th Dynasty came before the 12th.

Placing El-Lahun in the 6th century through to the time of Alexander the Great means that it would have had available to it materials from those preceding it. And

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77 Barbara Watterson, *The Egyptians* (Oxford UK/Malden MA 1997) p. 72
79 Ibid.
80 Saggs, *op.cit.*, p. 115-116
at El-Lahun, this seems again to be the case. The town, according to Petrie, had “two periods of occupation, the first from the 12th to 13th Dynasties and … [also] a brief … [one] of part of the site in the 18th Dynasty. However, this interpretation of the evidence is now in dispute: there may have been a continuous but dwindling occupation from the earlier to later periods.”\(^\text{81}\) This we reject since there is no scientific or technical evidence that places the 12th Dynasty prior to the others, especially the 18th Dynasty. Thus the 18th Dynasty’s occupation of El-Lahun came before the 12th. In this case we have evidence that indicates the 12th not only came after the 18th but the 22nd as well, as pointed out by Somers Clarke and Reginald Engelback who show “Among the wooden doors that have been preserved from dynastic times may be cited … one from El-Lahun … which, from an almost erased scene on it containing cartouches, appears to be of the time of King Osorkon I of the XXIIInd dynasty.”\(^\text{82}\) This door is now in the Cairo Museum.\(^\text{83}\) Osorkon I in the established chronology reigned from about 925-890 B.C. which implies that El-Lahun and the 12th Dynasty came after that time or after 890 B.C., which again would support the scientific and technological evidence that it existed around the 6th century B.C.

In terms of architecture we have evidence that places the 12th Dynasty in Greek times. The 12th Dynasty stone masons according to William Layton, writing in 1849, show “The works of the twelfth dynasty are remarkable … showing columnar architecture named Proto-Greek Doric. This seems to be fifteen hundred years older than the Greek Doric [order of column construction].”\(^\text{84}\) Georges Perrot and Charles Chipiez explain that

“The crucial form of the pillars at Beni-Hassan [of the 12th Dynasty], their want of a well-marked base, their sixteen flutes, the square abacus interposed between their shafts and the architrave [that were so like that of the Greek Doric order] when taken together, [had] a great impression upon the mind of Champollion. He thought that in them he had found the first sketch of the oldest Greek order [of column design] and that the type brought to perfection by the [Greek] builders of Corinth and Paestum had its origin in the tombs of Beni-Hassan; he accordingly proposed to call their columns proto-Doric.

“Here we shall not attempt to discuss Champollion’s theory. It would be impossible to do so with advantage without having previously studied the Doric


\(^{82}\) Somers Clarke, Reginald Engelback, *Ancient Egyptian Masonry* (reprint, Escondido CA 1997), p. 161

\(^{83}\) *Ibid.*, p. 162

\(^{84}\) William Layton, *The Civil Engineers and Architects Journal*, vol. 12 (London 1849), p. 75
column itself, and pointed out how little these resemblances amount to … The
general proportions of the Greek and Egyptian order are, however, almost
identical; the shafts are fluted in each instance, and they have the same air of
simplicity and imposing gravity.

“But it is futile [based on the established chronology] to insist upon any such
comparison. The polygonal column had long been disused [discontinued by the
Egyptians] when the Greeks first penetrated into the Nile Valley.

“The early Greeks also used an octagonal column like that also found at Beni-
Hassan. That is a column with eight sides.

“It was [also] in use in the time of the Middle Empire, during the eleventh and
twelfth dynasties.”

In other words, the 12th Dynasty stone masons were cutting with “general
proportions of the Greek [Doric] order [that] are almost identical; the shafts are
fluted in each instance and … have the same air of simplicity and imposing
gravity.” But more than that, these 12th Dynasty Egyptian stone masons also cut
unfluted eight-sided columns that were produced by the early Greeks, a style
which was discontinued by the Egyptians supposedly after the Middle Kingdom.
However, in terms of the short chronology there is no 1500-year gap separating
the Egyptian productions of these two types from the early Greek Doric columns.
They existed at the same time.

It is interesting to note that somewhat similar fluted pillars are found at 18th
Dynasty temples, but these are not nearly as similar to the columns of the Doric
order as are those of the 12th, which again indicates that the 12th Dynasty came
after the 18th, perhaps with some overlapping in time.

To review: The Sothic astronomical evidence places the 12th Dynasty after 600
B.C. The presence of Laurion silver in the 11th Dynasty places the 12th Dynasty
after 600 B.C. The presence of non-meteoritic iron spearheads places the 12th
Dynasty in the late Iron Age after 600 B.C. The presence of a socket on this
spearhead, and also on that of a drill at El-Lahun places the 12th Dynasty after 600
B.C. The straight-lined streets that intersect at right angles at El-Lahun and at towns
and forts places the 12th Dynasty after 600 B.C. And the wooden door with
cartouches from the time of Osorkon I of the 22nd Dynasty at El-Lahun, whose reign
conventionally ended about 890 B.C., indicate that the 12th Dynasty reigned some
time after this date, which would agree with other evidence to place it after 600 B.C.
That being the forensic historical case, other evidence of a non-scientific, non-

pp. 96-97
86 Ibid.
technological nature should correlate and be congruent with this date as well. And that, too, is the case.

A clear indication of the lateness of the chronological position of the 12th Dynasty can be seen in the highly advanced development of its jewelers. Ann Rosalie David specifically points out that “Middle Kingdom Jewellery was never surpassed in other periods, and demonstrates the standards of technical ability which had now been reached.”87 She adds “The treasures of the 12th Dynasty perhaps represent the acme of the jeweller’s craft in Egypt ... 12th Dynasty jewellery from Lahun has certain features peculiar [only] to that period and exhibits a specially high standard of craftmanship, it nevertheless follows the broad pattern of traditional Egyptian jewellery.”88 David P. Silverman describes this as “Among the most impressive and elegant of all small scale works of art of the pharaohs found in the tombs ... of the late Twelfth Dynasty ... No modern jeweler could surpass his or her ancient Egyptian counterpart in the delicacy, sensitivity to design and use of colour displayed in these exquisite examples of personal finery.”89 Mary Galway Houston discusses “the pendant or pectoral hanging over the breast from a chain round the neck and kept in position by a counterweight at the back is of most frequent occurrence [in Egypt].”90 She shows “This type of pectoral occurs frequently as an ornament subsequent to the 12th Dynasty but the execution of the later jewellery did not reach such a high standard.”91 The culmination and epitome of jewellery comes at the end of the development of a highly civilized society, not generally near its midpoint, which agrees with the 12th Dynasty’s placement after 600 B.C.

In terms of sculpture, the artists of the Middle Kingdom exhibit a profound sense of realism that departs markedly from Egyptian tradition. Horst Woldemar Janson and Anthony F. Janson describe this:

“The unquiet spirit of the Middle Kingdom is reflected in royal portraits [in stone] such as the one ... of Sesostris III. There is a real sense of shock on first seeing this strangely modern face. The serene assurance of the Old Kingdom has given way to a troubled expression that testifies to the difficulty of holding power ... At first glance the link with tradition seems to have been broken entirely. Lacking its royal trappings [the portrait of Sesostris III] displays a physical and

88 Ibid., p. 52
89 David P. Silverman, *op.cit.*, p. 227
90 Mary Galway Houston, *Ancient Egyptian, Mesopotamian + Persian Costume and Decoration* (reprint, Mineola NY 2002), p. 16
91 Ibid., p. 17
psychological realism that bespeaks a new level of self-awareness ..., one that was destined to live on in Roman portraiture.”

Realism, of course, comes into the artistry of the ancient world late in its development.

In this chapter we have confronted Chappell and the proponents of the modified chronologies of Rohl and of James et al. with a plethora of scientific, technological, and other evidence that dispels their chronological revision of Egyptian history. Rose and this author have answered Chappell’s and their critiques in full and more, by supplying additional evidence that refutes all of it. But neither Chappell, nor Lappin, Palmer, nor anyone else has fully answered our critiques, and that is indicative of a basic inability on their part to do so. Henige points to the dilemma: “Archibald Geikie, in his capacity as President of the British Association for the Advancement of Science noted ... when he expressed chagrin that ‘[i]t is difficult satisfactorily to carry on a discussion in which your opponent entirely ignores your arguments, while you have given the fullest attention to his [or hers]’.”

One final point, before moving on, which bears upon an understanding of the chronology of the 12th Dynasty. Based on either the established chronology or the British schools of Rohl and James et al., the fall of the 12th Dynasty as with so many other problems discussed in these volumes is unknown. According to John Albert Wilson:

“If the Middle Kingdom possessed such admirable qualities and if the Egyptian system had such flexibility, how did it happen that the state collapsed so suddenly after 1800 B.C.? We wish we knew the answer. A number of suggestions may be offered, which cumulatively may be of some significance, but the abrupt disintegration of the going system must still remain a mystery.”

But once again, this “mystery,” like so many others, is resolved by Rose’s work as it relates to the themes of Heinsohn and Sweeney. The 12th Dynasty, and the Middle Kingdom with it, fell with the conquest of Egypt by Alexander the Great in 332 B.C. As with all the other mysteries discussed in these volumes, the beginning of the 12th Dynasty is known from Sothic dating. It follows the 11th Dynasty that had silver from the Laurion mine which opened around 600 B.C. Its technological abilities—iron production, socketed hafting—, its geometrical town grid organization, its chronological relationship to the 18th and 22nd Dynasties, its mastery of jewelry, superior to all other dynasties, its natural realistic sculpture

92 Horst Woldemar Janson, Anthony F. Janson, History of Art: The Western Tradition (Upper Saddle River NJ 2003), p. 1034
93 Henige, op.cit., p. 207
94 John Albert Wilson, The Culture of Ancient Egypt (Chicago IL 1956), p. 154
like that of later times, its use of early Doric, Greek-like columns with 16 fluted lines and polygonal eight-sided columns, all speak with one coherent voice and say the same thing. The 12th Dynasty, perhaps along with others, was the final chapter in pharaonic Egyptian history. The integration and correlation of each piece of evidence into a congruent whole does not come about by coincidence. Such an orderly collaboration of multiple pieces of evidence only does so when what is being presented is a very close mirror of historical reality. In the reflection of that mirror the 12th Dynasty in terms of the established chronology or those of the British school—Rohl/James et al.—is without support. What we see is like the picture of Oscar Wilde’s Dorian Gray; they “found hanging on the wall a splendid portrait of their master as they had last seen him, in all the wonder of exquisite youth and beauty. Lying on the floor was a dead man, in evening dress … He was withered, wrinkled, and loathsome of visage. It was not till they had examined [the evidence] that they recognized who it was.”

Dorian Gray’s obsession with his own beauty is like the historians’ obsession with their overly lengthy chronology which for them is a thing of beauty. However, Gray’s life was changed by a book and he used that knowledge to avoid reality. One wonders at what point historians will stop avoiding the realities of forensic historical evidence and look at the profoundly loathsome distortion they have made of ancient world history. If historians truly believe that they love historical truth, I suggest they consider what Wilde also wrote in The Ballad of Reading Gaol, namely that “all men kill the thing they love.” That I believe is what historians have done to ancient chronological truth.

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95 Oscar Wilde, The Picture of Dorian Gray and other Writings (NY 2005), p. 239
ADDENDA

Herodotus in his Book II discusses an Egyptian pharaoh named Sesostris, identical to the three 12th Dynasty kings Sesostris I, II, and III. However, in terms of Rose’s chronological revision of the 12th Dynasty, Herodotus would have lived before two of the Sesostris pharaohs and could not have known of them unless they reigned prior to him. Some critic or other, either out of ignorance or for some other reason, may decide to inject this assumed contradiction into the debate to suggest this clearly proves Rose’s astronomical placement of the 12th Dynasty is in grave error. The problem with this criticism, as Rose explained to me in a telephone conversation on March 21, 2009, is that the “Sesostris” discussed by Herodotus is a “mythical pharaoh” from Egyptian lore. The literature abounds in references to the fact that Herodotus was presenting a “mythical Sesostris” and not a historical one. Clarence J. Glacken describes “The mythical Sesostris built canals, and the face of the countryside had been changed by forced labor …” He states that “Isocrates wrote of another mythical king, Busiris…” Donald A. Mackenzie speaks of the “confusion of historical events [that] may have given origin to the legends recorded by Greek writers regarding the mythical Pharaoh Sesostris, to whom were credited, with exaggeration … the achievements of Thothmes III and Ramesses II …” Madeleine de Scudéry states “Sesostris a mythical Egyptian king was reputed by Herodotus to have made great conquests in Africa and Asia.” Hopefully critics will not raise this issue but, given the depth of Rose, Heinsohn, and Sweeney’s challenge, one can’t be sure that the critics will refrain from this level of distortion to negate the real evidence that relates to the chronology of the 12th Dynasty.

AGRONOMY AND THE CHRONOLOGY OF THE 12TH DYNASTY

As Rose pointed out, the 12th Dynasty ends with the conquest of Alexander the Great, followed a few years later by the Macedonian pharaohs—the Ptolemies. If that is the case, there is practically no separation in time between the 12th Dynasty

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98 Ibid.
100 Madeleine de Scudéry, *Selected letters, orations, and rhetorical dialogues* (Jane Donawerth, Julie Strongson transls. and eds.) (Chicago 2004), p. 133 fn
and that of the Ptolemies. In terms of the established chronology these dynasties are separated from one another by 1300 years, and in the chronologies of Peter James et al. and David Rohl by about 1000 years. To prove that no such lengthy periods separate these dynasties but that they follow each other, separated by a very short period, we turn to the science of agronomy as it relates to irrigation and salinization of the soil in the Faiyum basin west of the Nile.

The Faiyum basin is somewhat flat but has a gentle slope. It is surrounded by low mountains and much of it is located well below sea level. There is no indication in the historical literature that its soil was destroyed in ancient times by irrigation salinization. Just as with the historical literature regarding the southern Mesopotamian plain south of Baghdad, no historian seems to have considered that it would have been impossible for the Faiyum basin to have been cultivated through irrigation networks for 1300 to 1000 years, which commenced with the 12th Dynasty and ended with that of the Ptolemies.

There was once a large lake that occupied much of this basin but it had no outlets so that the incoming water from the Nile along a canal opened in the 12th Dynasty, known as Joseph’s Canal, would bring salts to the lake. Without outlets, evaporation of the surface water of the lake will, over time, turn it from a fresh water reservoir to one that is highly salty. Therefore, any irrigation canals that carry water to the surrounding agricultural lands will carry this brackish water deep inland. Also, the higher one raises the level of the lake, the farther irrigation canals can carry this brackish water which will seep below the surface and raise the water table. As we have shown in volume I, chapter 14, “Agronomy and Climatology,” if the water table is raised about 20 inches/50 centimeters from the surface, capillary action and the Sun’s heat vaporizing the water will bring it to the surface where it will escape to leave its salts. Flinders Petrie describes the past and present conditions that persist in and around the Faiyum basin:

“This lake was not regulated artificially until the XIth Dynasty: and hence was a large sheet of water fluctuating with each rise and fall of the Nile and bordered by lagoons … where salt and natron would accumulate during the dry season of each year. At the present time the Lake of Faiyum is brackish, and the cliffs which border it contain so much salt that rain pools which collect on them are not drinkable.”

The question to be resolved in terms of the chronology of the 12th Dynasty is, did such an irrigation salinization episode occur in the Faiyum basin during and between the time of the 12th Dynasty and the Ptolemies, and is there evidence that the soil there had become poisoned by salt? With respect to the 12th Dynasty

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enlarging the lake and establishing a far-flung irrigation system around it, Kurinsky reports that:

“An American engineer, Francis Cope Whitehouse, was among a group of engineers retained by the British a century ago to resolve the problem of increasing the amount of arable land in the desert wastelands of Egypt, a country then under British [imperial] hegemony. Whitehouse astonished his employers by reporting that he had confirmed the existence of a vast lake artificially created … in the time [of the 12th Dynasty] and that the most practical method of irrigation the arid Egyptian desert was to reconstruct the system of irrigation … instituted 3,500 years ago!

“… In surveying the desert he found, to his amazement, that the problem of desert irrigation appeared already to have been addressed more than three millennia earlier… Tracing the remains of ancient irrigation by the existence of a small lake, the Birkut el-Qarum, in a deep basin of the desert known to the Egyptians as el-Fayoum some hundred kilometers [62 miles] southwest of Memphis. The Birkut el-Qarum, or Lake Karoum, was a freshwater lake in the midst of the vast Sahara desert, and yet it had no visible source. His interest piqued by this peculiar circumstance, Whitehouse began an investigation …

“Around the lake’s perimeter as well as at a considerable distance from its shores, Whitehouse came across the ruins of ancient dams, ditches, aqueducts, and a variety of structures that mutely testified to the existence of a vast and sophisticated irrigation system. Ancient fish bones, shells, and other signs scattered about the sands surrounding the Fayium’s oases unmistakably demonstrated that the lake had once been many times its current size … [and] hence much deeper …”

To gather what happened to this irrigation system, Whitehouse began a search of “archival documents in Cairo and discovered that medieval maps of the el-Fayoum region showed two lakes in the basin.” He further learned that these “medieval maps were mere copies of maps drawn in the time of Ptolemy of Alexandria … and found substantiation for an artificially created lake in the writings of … Herodotus, Pliny, Diodorus, Strabo, and Mutianus.” The lake indeed had been huge. The most striking aspect of this finding that Kurinsky reports is that

“… a vast network of canals flanking the Nile had existed long before the Ptolemaic era; they had been far more extensive, and further, a huge reservoir had been created consisting of two lakes which, if the canal system had not been debased by the [Ptolemaic] Greeks and other succeeding [Roman] rulers, would have continued to guarantee water to a vast area. The Greeks [assumed to have been] ignorant of the hydrology of the system, in attempting to increase acreage

102 Kurinsky, op. cit., pp. 87-88
103 Ibid., p. 88
104 Ibid.
by reducing the extent of the lake [so that its rich bottom soil could be farmed], had instead caused large areas of rich soil to return to dusty sand. Once fertile fields had relapsed into an arid landscape of sand, dust, and rock.”

Why the Ptolemies were ignorant of the fact that the Faiyum basin was a rich, irrigated, agriculturally productive region, we are not told. All this is assumed as fact with nothing of evidence as support. It is simply taken for granted that the Ptolemies did not realize that thousand upon thousands of acres of land that they controlled, containing a vast irrigation system and producing rich crops, existed. This is simply not believable because the Ptolemies had at their disposal a large bureaucracy that ran their kingdom and would clearly have known of the conditions in the Faiyum basin that allowed for rich crops; had that been the case, there would have been no reason to shut down the entire system and divert the waters elsewhere. There had to have been another reason which prompted the Ptolemies to abandon the lands in the Faiyum. The reason for doing so is exceedingly important to the chronology of the 12th Dynasty.

When the amount of water running from the Nile down the “Bahr Yousef” or Joseph’s River was reduced, all the lands that were watered along that stretch would have had less water for the canals coming from it to irrigate the land available for agriculture. To some extent, the supposed gain of land outside the Faiyum opened to agriculture by these waters would have been the loss of the lands on either side of Bahr Yousef as well as the lands in the Faiyum under irrigation cultivation. T.G.H. James gives us a hint as to why the Ptolemies, coming almost immediately after the 12th Dynasty in the short chronology, began to reduce the influx of water to the lakes and diverted it elsewhere:

“In the Faiyum … the visitor [today] is immediately struck by moving water, by the sinuous and impressive Bahr Yousef, ‘Joseph’s River,’ which brings Nile water into the region through the Hawara Channel, … [to] the Birket Qarun [lake], glassy and brooding, surrounded by beaches encrusted with salts, the recipient of all the drainage canals in the region, almost as saline as the sea (nearly 150 feet below sea level).”

That is, today the canals surrounding the lake are draining water into it, not carrying water off inland. The reason for this is obvious. The lake is so saline that its water would salt-poison the land.

The general view historians present is that about 1800 B.C. the 12th Dynasty cut or enlarged the original opening from the Nile to the Bahr Yousef to open the Faiyum basin up to be exploited via irrigation agriculture. Sir Alan Gardiner presented the historically accepted position thus:

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105 Ibid.
106 T.G.H. James, Ancient Egypt: The Land and Its Legacy (Austin TX 1988), p. 60
“The original lake sank below sea-level through the silting up of the channel until a king of Dynasty XII, by widening and deepening it, again, brought the lake into equilibrium with the river. Thus was formed the famous lake of Moeris [in the Faiyum] which by functioning as a combined flood-escape and reservoir, not only protected the lands of Lower Egypt from the excessive high floods, but also increased the supplies of water … According to the same theory, the level and consequent size of the lake were artificially reduced in early Ptolemaic times by construction of two barrages, a portion of the submerged [lake] area being thus reclaimed [for agriculture].”

Nevertheless, this lake or lakes had a long history of rising and falling water levels which affected the salt content of the soil. Whenever the lake dried up or was reduced in size, any salts in it would be left behind on the land. According to William C. Hayes:

“… by the beginning of the Paleolithic period … the [Faiyum] depression was occupied by a vast high-level lake, the surface of which lay 131-138 feet [39-42 meters] above modern sea-level. …

[Over time the lake level] fell … first to 92 feet [ca. 28 meters] and then 74 feet above sea-level. Toward the end of the Paleolithic period the river [that fed the lake] had sunk so low that its waters no longer flowed into the Faiyum and the lake dropped 18 feet [6 meters] below sea-level, and appeared indeed to have dried up altogether. Wind erosion [then] deepened [the basin to] its present maximum depth of 74 feet [22.5 meters] below sea-level.

“During the period of transition between the Old and New Stone Ages the Nile … began once more to send its flow through the Hawara channel into the Faiyum reestablishing the lake and raising its surface to 89 feet [27.12 meters] above sea-level. Soon, however, the Hawara channel itself began to be choked with silt, and this factor alone appears to have been sufficient to reduce the amount of water received by the Faiyum basin below that which is lost each year through seepage and evaporation. … the lake early in the Neolithic period had apparently fallen to 43 feet [13 meters] and this was followed by 33-foot [10 meters] above sea-level … [and then to] seven feet [2.13 meters] below sea-level, remaining at this general level throughout the balance of Egyptian prehistory well down into historic times.”

In this respect, John D. Cox describes one such episode wherein the Faiyum lake(s) dried up completely at the end of the Old Kingdom, which we date, as in the first chapter of this book, to around 800 B.C.

“Along the Nile … crop failure and famine that struck the Old Kingdom ended the … reign of Pepy II …

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“To [Fekri] Hassan there is no doubt that abrupt climate change–sudden drought–led to the devastatingly low flows of the Nile … ‘The Nile-fed Lake Faiyum, more than 200 feet deep [at that time], dried up entirely.’”

Thus this great evaporation lake laden with salt had to have charged the ground water with salts collected over millennia so that when the water table later rose with this burden of salts, it would rapidly damage the soil in far less time than the 300 to 400 years required for it to salt-poison the land, as happened in southern Mesopotamia.

In all these episodes the salts in the lake were being delivered to the soil whenever the lake level fell making the subsurface soils highly saline. In dry times rainfall would leach these salts down to the water table. As Fekri Hassan points out: “Successive droughts may … lead to salinization of the soils as salts accumulate…” Therefore, a rise of the water table could bring these subsurface salts close enough to the surface to salt-poison the land. Thus without an outlet the lake was supposedly an “evaporation lake between 1800 B.C. (12th dynasty, Amenemhet II, King Moeris) and the time of the Ptolemaic construction activities …”

What does all this mean, in terms of the chronology of the 12th Dynasty? It means that when the 12th Dynasty raised the lake level in the Faiyum, it would create a condition wherein the water table rose and the land around it would have been, as it was in southern Mesopotamia, salted and poisoned. T.G.H. James reports that the 12th Dynasty did raise the level of the lake.

“The lake remained at the high level of the [12th Dynasty] Middle Kingdom for well over 1,500 years and was then, as it seems, deliberately lowered … at the beginning of the Ptolemaic Period.”

Here then is the key point that James shows:

“In modern times, after centuries of neglect following the Islamic conquest of Egypt, the aim is to maintain the lake, the Birket Qarun, at [a] low level to ensure the efficient drainage [of salts out] of the cultivated area. This result is achieved by locks and barrages … And so the lake has become a vast sump, very saline, and … very unpleasant.”

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112 T.G.H. James, *Ancient Egypt, op.cit.*, p. 76
113 *Ibid.*, p. 79
Here the problem of soil salinization rears its scientific head. We are asked to believe that for 1500 years, from the 12th Dynasty down to Ptolemaic times, this higher level evaporation lake with its high salinity did not ever raise the water table to affect the soil. The reason that Ptolemy I and II etc. diverted the Nile’s waters away from the Faiyum basin was that the land there was becoming poisoned with salt from the irrigation canals and dams. This is proved by the fact that the Ptolemies immediately employed special methods in order to attempt to halt the salinization as outlined by Alan Wild who points out that: “A rotation of two year cereal crops followed by [one year] grass fallow or a leguminous crop appears to have been used in the Faiyum during the Ptolemaic period.”

Beyond that is the problem of just how the land that was under cultivation in Ptolemaic and Roman times was described. It was not described as a broad area as it is today but as a narrow strip of land, which evidently was the cultivated lake bottom surrounding the lake. John Tait wonders:

“The Faiyum as we see it today is a roughly triangular oasis … The irrigation has been modified again and again, but its basic shape as a broad region of cultivation even within changing limits and the changing area of the lake itself has never altered. To the modern visitor it is essentially seen as a wide flat area, surrounded by modest hills. Why does the Book of the Fayum [of Ptolemaic and Roman times] reduce it to a narrow strip?”

It had to be described as a narrow strip because it was a narrow strip cultivated along the lake bottom that was exposed when the lake level was dropped. It was only after the lake level had fallen substantially for centuries that rainfall leached the salts at the surface to the deeper water table and that cultivation could be recommenced. This again indicates the surrounding lands were not fit for cultivation because of salt-poisoning.

The fallowing–leaching process is a very well-known method of slowing salinization as discussed in volume I, pages 402-5. Here then are contradictions to the established chronology as well as the revisionist chronologies of James et al. or David Rohl, or anyone who places the 12th Dynasty in the mid- to early second millennium B.C. It is, namely, that salinization via irrigation is a very well understood branch of agronomy—a science! Salinization is a clear-cut, direct problem in the Faiyum basin today as J. Donald Hughes reports: “Egypt suffered less from salinization [along the Nile] than Mesopotamia because the regular [Nile] flood leached salt from the soil. Salinization did occur in irrigated areas

114 Alan Wild, *Soils, Land and Food: Managing the Land During the Twenty-First Century* (Cambridge UK 2003), p. 41
above the flood line, and was serious in the Faiyum, which is below sea level.”  

Shepherd Krech et al. also show:

“Salinization has been noted in many places around the Mediterranean, including the Fayum…”

R. Neil Hewison describes one aspect of the problem:

“As the Fayoum has no outlet either back to the Nile nor out to sea, drainage in the depression presents special problems. Until recently, all drainage water was taken by one of the two main drainage channels … or one of the other minor ones, down to Birkat Qarun in the north, to be lost by evaporation. The Birka, however, can take only a certain volume of drainage water to balance its rate of evaporation which is calculated at 370 million cubic meters [480 million cubic yards] per year. Any drainage water over that amount causes the lake level to rise and flood surrounding land, often doing irreparable damage because of the water’s high salt content.”

Yet we are expected to believe that with the lake at a much higher level, using its waters to irrigate the land, it somehow did not become highly saline. In its present condition the lake is highly saline. Fouad N. Ibrahim and Barbara Ibrahim report this fact. “Because of its high water requirement and its relative salt tolerance, rice is almost exclusively grown in the Nile delta and in Faiyum.”

We were told by T.G.H. James, above, that the beaches of the lake are “encrusted with salts.” The land away from the lake is also in danger as shown by Hughes who presents a photograph that demonstrates even today sites in the Faiyum where “Evaporation of water used in irrigation in this basin below sea level has left crystals of salt in the soil…”

Clearly, salinization is a problem in the Faiyum basin today if the water level in the lake, the Birket Qarun, is not kept, as T.G.H. James has told us, “at [a] low level to ensure the efficient drainage [of salts].” Why then, when the lake was much higher during the 12th Dynasty down to Ptolemaic times 1500 years later, in terms of the established chronology, wasn’t the land surrounding the lake irrigated by this water salt-poisoned by the irrigation water? Do the scientific processes of agronomy related to irrigation only operate in the present, but did they fail to operate in the past for 1500 years? Why do the processes that govern salinization work in the Faiyum Basin today but stopped working in the Faiyum basin in the

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117 Shepherd Krech et al., *op.cit.*, p. 823
118 R. Neil Hewison, *The Fayoum* (Cairo/NY 2008), p. 8
120 Hughes, *op.cit.*, p. 41, caption of photograph
past? How does one explain this monumental scientific contradiction to established chronology?

Whatever the solution, it is quite clear that a scientific process that operated in the present had to also have operated in the past. Therefore, the 12th Dynasty had to come directly before the Ptolemies, unless one wishes to suspend the laws of science. There is simply no escape from this conclusion. As the science of agronomy and the processes of irrigation salinization move the Old Babylonians of Mesopotamia into Persian times along with all the other scientific and technological evidence, the same science and same processes move the 12th Dynasty of Egypt into Persian times along with all the other scientific evidence. In this regard the science of agronomy correlates, corroborates, and is congruent with, all the other evidence in the short chronology. How do Chappell, or James et al., or Rohl, or any of their supporters, explain away this scientific evidence with scientific evidence of their own? What they must explain is: why is salinization a problem today in the Faiyum basin, which is undoubtedly a scientific fact, but why with a higher lake level and water table level for 1500 or even 1000 years, did these scientific processes fail to operate as they do today? They can’t and they won’t.

The reason why they can’t and won’t is based on the nature of scientific laws. All historians and scientists adhere to the position that the laws of science do not change. The laws of gravity were the same in the ancient past as they are today. So, too, the scientific processes that govern agronomy, as these pertain to irrigation agriculture. These were the same in the ancient world as they are today. In order to dispute the evidence of salinization of the Faiyum basin as outlined above, critics of this thesis must claim that the laws of science be suspended for 1500 years to maintain the established chronology. Since they cannot prove such an impossible case, and they cannot face the consequences of having to do so, they are left with ignoring or evading the science, which indicates again that they can’t do this and still maintain their rationality. Thus they won’t deal with it in order to hide from it outwardly on a social academic level and inwardly on a psychological level. There is simply no escape from this dilemma.
CHAPTER 6

THE FIRST INTERMEDIATE PERIOD,
A DARK AGE, AND CHRONOLOGY

Prior to the Middle Kingdom, which contains the 11th and 12th Dynasties, Egypt, according to the established chronology, was in the throes of a Dark Age. The dynasties generally assigned to fill this period are the 7th, 8th, 9th, and 10th. The period this encompasses runs from around 2300 to 2000 B.C. and is known as the First Intermediate Period. Barbara Bell, a noted Egyptologist, called this period in an article in the *American Journal of Archaeology*, vol. 75 (1971), pages 1-26, “The Dark Ages in History I: The First Dark Age in Egypt.”

This epoch has been studied archaeologically and the material evidence attributed to it is so assigned precisely in order to fill it. The length of the period is conjectured to have lasted a few centuries to several, but historians have reached a consensus, based on the established chronology, of about 200 to 300 years.

I.V. Vinogradov sums up the nature of the way this period exists. “Today, most Egyptologists confidently attribute the events for the First Intermediate Period—the time preceding the Middle Kingdom.”¹ Notice that in place of proof Vinogradov presents “confident attribution” that what occurred is shared by “most Egyptologists.” As we proceed, that is generally all that the Egyptologists can offer to support this epoch—confident attribution shared by most Egyptologists. I ask the reader to consider the concept of using “confidence” by any group of researchers as the basis for offering historical truth for any period with very little if any evidence for it. And then consider that this “confidence”, shared not by “all” the authorities but by “most”, is the basis for offering historical truth for any period. George Jean Nathan’s description of Jean-Paul Sartre, the philosopher, well details what the historians are up to when they employ “confidence” by “most” of their colleagues as proof of what they have actually claimed.

“He [Sartre] is his decade’s foremost theatrical confidence man which in view of strong competition is no mean achievement… His fecundity in such directions as philosophy, politics, sociology, the novel, the short story, the cinema, and the drama is that of a rabbit, yet he seems to operate under the delusion that his reproductions are not rabbits but lions and tigers, whereas even the rabbits he

factually delivers himself of are of the mechanical toy variety, stuffed with the sawdust of borrowed ideas.”

As we proceed, we will see that every lion and tiger of historical evidence for the First Intermediate Period is a rabbit of the mechanical toy variety, stuffed with sawdust and borrowed ideas. They have not produced historical proof but philosophy, politics, sociology, novels, short stories, cinema, and drama in its place; therefore let us examine this “confident” majority’s evidence.

Before doing so, what must be pointed out is that we have clearly established that the 12th Dynasty ruled mostly after 500 B.C. It is also well known and accepted that the 11th Dynasty reigned just prior to the 12th based on the established chronology and the evidence provided in the previous chapter. That means that the 11th Dynasty reigned from around 600 B.C. or a little earlier. Here then is the problem confronting the proponents who place the First Intermediate Period between about 2300-2000 B.C.: The First Intermediate Period ends with a pharaoh of the 11th Dynasty, and on the basis of the scientific and technological evidence, this puts the First Intermediate Period just before 600 B.C., somewhere from around 850 to 600 B.C. That places the First Intermediate Period directly in the Assyrian/Akkadian/Hyksos epoch. In other words, the First Intermediate Period cannot exist within the short chronology. If that is the case, the evidence to be presented will show that this is indeed so. What is clearly indicated is that the various reigns of individuals and/or groups assigned to the First Intermediate Period belong to either the Old Kingdom or the Assyrian/Akkadian/Hyksos period and have no standing on their own. Here is how Amélie Kuhrt describes the confident majority evidence for the First Intermediate Period. Her reservations will be noted in “Italics.”

“The chronology is beset by problems, but two things help to clarify somewhat, although not entirely … First the Turin Canon gives a total of kings at the end of dynasty VIII. The reign lengths of kings are fairly well known almost down to dynasty VI. Combining this information shows that there is a period of 22 ½ years for the whole of Manetho’s dynasties VII and VIII … Obviously, then, the rulers of these ‘dynasties’ were ephemeral figures. Despite this they did exercise control over the whole of Egypt. This is shown by the Koptos decrees … according to which these short-lived kings confirmed and disputed powers and exemptions to the nomarchs of Koptos … although the Koptos decrees may show devolution of central power, local governors still acknowledged the authority of the Memphite kings to legitimize their position. Secondly, the relatively well fixed date of the dynasty XII yields a date of 1991 [B.C.] for its inception. Since the reunification of Egypt was achieved by Mentuhotep XI of dynasty XI c. 2040 [B.C.], we can

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calculate the approximate length of dynasty XI and date its beginning c. 2133 [B.C.]. This has been the conventional chronology.”

The established chronology for the First Intermediate Period is “beset by problems” based on “fairly well known” “reign lengths” and the “Koptos decrees” which “may show devolution of central power” and also by the “relatively well fixed date of dynasty XII” with “the approximate length of dynasty XI.” “Fairly well known” is not “well known.” “May show” does not “show.” The “relatively well fixed date of dynasty XII” is not a “well fixed date.” And the “approximate length of dynasty XI” is not “the length of dynasty XI.” This is the “conventional chronology.” It is not supported by forensic historical evidence but by assumptions and estimates based on these assumptions.

According to George Rawlinson in 1881 the length of the First Intermediate Period was and, in terms of Kuhrt’s citation, still is a matter of conjecture, not fact:

“According to Africanus, Manetho assigned to the seventh dynasty 70 days [with 70 kings], to the eighth [dynasty] 146 years, to the ninth [dynasty] 409 years, to the tenth [dynasty] 185 years–total 740 years and 70 days. According to Eusebius, his numbers were: for the seventh dynasty 75 days; for the eighth and ninth 100 years each; for the tenth 185 years–total 385 years 75 days. By an arbitrary correction and combination of these two accounts, M. Lenormant produces for the period 436 years … which Dr Birch adopts… Bunsen following Eratosthenes and blending Manetho’s numbers into accordance, reckons the actual length 166 years…”

When one does not have a clear-cut scientific or technological basis upon which to build a chronology, of course, all sorts of divergent estimates for the length of the First Intermediate Period can be conjectured. If the chronology of Egypt is lengthened then the First Intermediate Period can be lengthened or shortened accordingly. But neither of these devices proves anything related to the chronology nor the reality of this period. Beyond this obvious dilemma, not only is the length of this period controversial, but the actual dynasties assigned to it are not in fact known in terms of documentation and not accepted by certain historians. B.G. Trigger and B.J. Kemp report “there being no evidence to support the existence of an intervening Seventh Dynasty.”

“Aside from any problems of interpreting the evidence, calculating the length of the First Intermediate Period has been a major problem for Egyptologists. Some sources such as the Turin Canon and Manetho record large numbers of rulers, but other sources (such as the Saqqara list) omit the period completely. The seventh

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3 Kuhrt, *op.cit.*, p. 155, emphasis added
5 B.G. Trigger, *et al.*, *op.cit.*, p. 112
and eighth Dynasties are said [but not proven] to have ruled from Memphis, which is why some Egyptologists prefer to regard them as [part of the] Old Kingdom. The ninth and tenth Dynasties ruled from … (Herakleopolis) and were acknowledged throughout the country by the nomarchs [local governors] …

“Egyptologists such as Flinders Petrie proposed a very long First Intermediate Period. The length of the period has more recently been calculated by archaeological means. Stephan Seidlmayer suggests that the Herakleopolitan pharaohs ruled for three or four generations before conflict with Thebes broke out. With the conflict lasting for between 90 and 110 years from the end of the eighth dynasty [through the 9th and 10th] to the reunification of Egypt by the [11th] Theban [Dynasty].”

Stephan Seidlmayer on this problem speaks of large-scale building that may be understood as evidence not only for the nature of the core institutions of the state but also for the fact that they were still functioning, even though “the glaring gap in the monumental [written] record during the First Intermediate Period suggests that the social system had become fragmented both in its political organization and cultural patterns.”

Thus Seidlmayer makes this period 105 years long. On the other hand, Mrs. A.A. Quibell suggests the period is about 500 years in length. In discussing these co-called dynasties she properly regards this period as a Dark Age and so titles it:


“What the catastrophe may have been which overtook Egypt at the end of the Sixth Dynasty is quite unknown, but it was most complete. Probably there were foreign invasions as well as internal disturbances…

“There is no certainty as to how long these dark ages lasted, and most various estimates of their duration have been given, ranging from less than three centuries to more than fifteen. The dating of the Old Empire [Kingdom] and all its monuments hangs on this, and it is naturally a matter of great importance for all the history.

“There is a date for the end of the period, as there is good evidence that the Eleventh Dynasty began about 2160 B.C., but it is not known how long the gap was between the end of Dynasty VI. and the beginning of Dynasty XI. Archaeological evidence, however, is clearly in favour of the time having been short, and although a great many names of kings who have reigned during these centuries are recorded, in such an epoch of turmoil and confusion kings probably followed each other in rapid succession, or may even have reigned at one and the same time in different parts of Egypt.”

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6 Robert G. Morkot, op.cit., pp. 104-105
8 A.A. Quibell, Egyptian History and Art, 2 ed. (NY/Toronto/London 1926), p. 50
James Henry Breasted along these same lines goes on to show:

“The data for determining the length of the dark period preceding the Middle Kingdom are scanty. Its beginning, in Manetho’s so-called Seventh Dynasty, is hopelessly obscure … Manetho divides the Heracleopolitans into two dynasties, the Ninth and the Tenth. The Turin Papyrus has a dynasty of eighteen kings immediately preceding the Eleventh … We have no means of determining how long these eighteen [kings ruled].”

What we have with this Dark Age, as with the Dark Ages of Greece, and those of Mesopotamia and Anatolia, is that for some completely unknown, obscure reason society broke down. In such cases the blame falls upon some unknown or obscure barbarian invaders or upon the unknown and obscure evidence that the internal organization of the state collapsed. None of these assumed causes has the slightest bit of evidence to support it. A Dark Age is needed to fill in the chronological gap, and the historians have diligently supplied interpretations of this almost non-existent evidence to fill the void. Yet it all amounts to assuming the evidence exists because of the assumed Dark Age.

Not only is the cause for this Egyptian Dark Age unknown, but as we saw, the lengths of reign of these so-called First Intermediate Period dynasties are also nothing but conjecture. No one knows why the 6th Dynasty fell or why this epoch was one of chaos and confusion. The archaeological evidence “is clearly in favour of the time [of the First Intermediate Period] having been short.” On the other hand “There is no certainty as to how long these dark ages lasted, and most various estimates of their duration have been given, ranging from less than three centuries to more than fifteen.” There is further a “glaring gap in the monumental record” for this period. Why? These kings supposedly failed to keep records of their achievements, taxes imposed, religious celebrations, etc. The reason, we are told, is that all this ensued because it was a Dark Age. No other evidence is given.

As with other Dark Ages, the cause of the fall of the 10th Dynasty is also not understood. Seidlmayer states:

“…the phase of Herakleopolitan [10th Dynasty, possibly 9th as well] rule before the advent of the 11th Dynasty— is rather less clear. There is a dearth of information of immediate chronological value because of the loss of most of the names of the Herakleopolitans and of all information concerning the lengths of their reigns in the Turin Canon, and because of the unsatisfactory state of archeological research in northern Middle Egypt and the [Nile] Delta, the heartlands of the Herakleopolitan kingdom. Because of the scarcity of data directly relating to the Herakleopolitans, it was even at one stage proposed that there must

have been no period during which Herakleopolitans were (at least nominally) the
sole rulers, and they must have been entirely coeval with the 11th Dynasty.\textsuperscript{10}

In essence the historians do not know for a fact the reasons for the rise of the 7th
Dynasty, after the 6th, nor do they know for a fact the reasons for the fall of the 10th
Dynasty. Further, both the 7th and the 10th Dynasties are, in the first case considered
non-existent as “the Saqqara list omits the [First Intermediate P]eriod completely” and
in the latter case the 10th Dynasty was supposedly “coeval with the 11th.” And all of
this is based not on evidence but on massive interpretive assumption.

Since this period is empty of materials the historians have sought to fill it with
whatever has come to hand. What then is the supposed evidence they have
gathered to claim this epoch existed? Seidlmayer argues:

“It is equally apparent, however, that the First Intermediate Period archaeo-
logical and epigraphic data indicate the existence of a thriving culture among the
poorer levels of society, as well as vigorous social development in the provincial
towns of Upper Egypt. Rather than being an outright collapse of Egyptian society
and culture as a whole, the First Intermediate Period was characterized by an
important, though temporary, shift in its centres of activity and dynamism. …

“Much of Egyptian history tends to concentrate on the royal residence, the
kings, and ‘court culture’, but in writing the history of the First Intermediate
Period it is necessary to focus instead on provincial towns and on the people
themselves, who make up the most basic elements of society.”\textsuperscript{11}

In this regard Seidlmayer shows the provincial evidence is most profoundly
observed in the burials of these commoners:

“This phenomenon seems particularly obvious in the funerary record, but it is
not restricted to this sphere. In fact, the most valuable objects … become most
abundant and widely represented in the graves of the early First Intermediate
Period–cosmetic stone vessels, ornaments and amulets of gemstones, and even
gold … It seems clear, therefore, that the provinces enjoyed favourable economic
conditions during the late Old Kingdom and the First Intermediate Period.”\textsuperscript{12}

What Seidlmayer and the other historians of this epoch expect one to accept and
believe is that only the outer Egyptian provinces with their villages and towns were
wealthy and prosperous enough during the First Intermediate Period to place
various luxury items in their tombs. But the very centers of these provinces, the
homes and capitals of the pharaohs and their courts were for some
incomprehensible reason impoverished and did not employ or enjoy these very
same luxuries in their burials. The reason for this, they claim, was that these

\textsuperscript{10} Seidlmayer, \textit{op.cit.}, p. 109
\textsuperscript{11} \textit{Ibid.}, p. 110
\textsuperscript{12} \textit{Ibid.}, p. 113
pharaohs were weak and incapable of obtaining these materials from the provinces. How do we know this to be true? It is assumed to be true and then explained as though it was a fact, that these First Intermediate Period kings could not muster up the wealth to give themselves lavish burials, while the common people could. All this is based solely on the historians’ interpretation of their assumptions of what actually took place. The best interpretation that can be given of the evidence is that this connection between the state capitals and the provinces exists only on paper and not in fact. Though the capitals exhibit no such luxuries, documents, monuments, etc., they nevertheless existed because the provinces, disconnected from these capitals, exhibit all the essentials of thriving communities. That is, in the heart of all this wealth, the rulers lived in an impoverished state. The poverty of such an historical explanation begs description. In the words of philosopher and mathematician Bertrand Russell,

“Man [historians also] is a credulous animal and must believe something. In the absence of good grounds for belief, he will be satisfied with bad ones.”

In essence, these capitals do not really exist. At this point, we suggest not only that the First Intermediate Period did not and does not exist, but all that has been assigned to this epoch belongs to either the 6th Dynasty, or the 11th and possibly 12th Dynasties, and that the evidence for these placements is biased chronological interpretation.

As we have seen, the evidence for the so-called 7th, 8th, 9th, and 10th Dynasties comes from provincial regions well outside the supposed centers of power. But since these regions were away from any centers of the dynasties of the First Intermediate Period, they could be and should be the provincial regions of dynasties on both ends of that period. They were provincial districts of the 6th, 11th, and possibly 12th Dynasties, which we will now undertake to prove is the case.

The study of pottery shape, design, and construction is one of the major methodologies by which historians and archaeologists relate the various regions to one another. In this respect, if indeed the First Intermediate Period was actually a unique epoch in Egyptian history, its pottery would reflect this by being in certain ways unique. If, however, as Heinsohn’s, Sweeney’s, and Rose’s theses suggest, and since Velikovsky wondered whether these dynasties existed at all, then the pottery of this period should be quite like the pottery forms found in the 6th, 11th, 13th, and possibly 12th Dynasties, or perhaps even others. Seidlmaier discusses the pottery types found for the First Intermediate Period:

“The period that followed the close of the Old Kingdom brought about fundamental changes in material culture. In fact during the First Intermediate

Bertrand Russell, quoted in Leo Rosten, op.cit., p. 59
Period, almost all artifacts took on a different appearance. We can review a few of the most significant aspects of this process.

“From an archaeologist’s point of view, pottery is clearly the most important type of artifact. Since the Early Dynastic Period and throughout the Old Kingdom, the repertoire of containers [clay pots] had been dominated morphologically by ovoid shapes: the point of maximum extension [width of the pottery] always lay slightly above the middle of the vessel. During the First Intermediate Period, this style was quickly abandoned. Now baglike or even droplike, sagging shapes were made. Clearly, the aim was [now] to adopt vessel shapes in order to take advantage of the capabilities of the potter’s wheel. In the case of ovoid containers, a large part of the outer surface had to be scraped into shape manually after throwing. In the case of bag-shaped vessels, the amount of work necessary could be reduced considerably. It seems significant, however, that this process took place only some 200 years after the first introduction of the potter’s wheel to Egyptian workshops during the 5th Dynasty. It was apparently only with the emergence of the First Intermediate Period that people were prepared to dispose of traditional models and give preference to more efficient modes of production [employing the potter’s wheel].”

The problem inherent in Seidlmayer’s analysis is that the baglike or bag-shaped pottery he assigns to the First Intermediate Period can also be dated to the 11th and early 12th Dynasties. Janet E. Richards asserts:

“An initial … survey … provided information on the ancient use of the North Cemetery in general, and on the occurrence of Middle Kingdom remains in particular. Of the thousands of ceramic sherds examined during the survey, none could be assigned … to the Old Kingdom. Similarly, none could be assigned to the First Intermediate Period: the bag-shaped jars found in the northern part of the cemetery could equally be ascribed to eleventh dynasty/early twelfth dynasty as to the Late First Intermediate Period.”

Therefore, the pottery dating method Seidlmayer employed to date these baglike or bag-shaped pots also places them in the 11th and early 12th dynasties. So, although he claimed that the ceramic evidence “is clearly the most important type of artefact” for determining the chronology for the First Intermediate Period, it does not support his nor any of his colleagues’ contention that this pottery is unique to that epoch. Seidlmayer, however, goes on to argue that there is another distinction to funerary artifacts related to chronology found in the provincial regions that supports the established chronology for dating the First Intermediate Period to the latter part of the third millennium B.C.:

14 Seidlmayer, op.cit., p. 113
15 Janet E. Richards, Society and Death in Ancient Egypt (Cambridge UK 2006), p. 156
Furthermore, a whole range of new objects became popular in provincial burials during the First Intermediate Period. In the Old Kingdom, the grave goods of poorer burials had been chosen entirely from among the types of objects used in daily life, but in the First Intermediate Period objects began to be made exclusively for funerary use. Crudely made wooden figures of offering bearers, boats, even whole workshop scenes, are good instances of this trend. Another example is the appearance and increasing use of coloured masks made from gypsum and linen (cartonnage) to cover the heads of mummified bodies. It also became increasingly common to use simple slab stelae as a means of marking the offering place in the superstructure of small mastaba-tombs or in chapels of simple rock tombs.\(^{16}\)

However, Seidlmayer shows that such renditions of models for tombs are also found in tomb decorations in the mastaba-tombs from the Old Kingdom:

“The appearance of these objects indicates that both the demands and the means available in the provincial towns were sufficient to support an area of craftsmanship specializing in ‘non-functional’ products. Even more important, however, is the fact that the prototypes of these types of objects have their origin in Old Kingdom elite culture. The model funerary figures of people engaged in fundamental tasks can be traced back directly to the repertoire of scenes from daily life depicted in Old Kingdom mastaba-tomb decoration. It appears that by the First Intermediate Period those factors that had previously inhibited cultural communication between social strata now ceased to operate.”\(^{17}\)

I ask the interested, attentive reader what we are being asked to accept as true by Seidlmayer. The poor people during the Old Kingdom were “inhibited” from learning about these scenes that decorated “elite” tombs and that only during the First Intermediate Period they somehow learned that these decorations existed, so they copied them by building crude wooden models to depict everyday life scenes. Yet, according to the chronology that Seidlmayer and his colleagues accept, the “craftsmen specializing in non-functional” funerary wooden models for some 200 to 300 years never learned how to improve their carving of figures, which remained “crudely made wooden figures”, throughout that same 200 to 300 years. This is highly unlikely to have been the case over so long a period. What is more likely is that certain poorer provincial people emulated the elites of the Old Kingdom by carving these crude models by themselves; not being professional wood workers their renditions remained crude through this time. And rather than being wealthy enough to engrave the stelae, they resorted to simple slabs of stone to mark their tombs.

It seems never to have occurred to establishment historians that people from the same nation living in different regions could have employed different models for

\(^{16}\) Seidlmayer, *op.cit.*, pp. 122-125

\(^{17}\) *ibid.*
burial pottery, etc. The real question is: how does one scientifically test the historians’ interpretations? What has been overlooked regarding the art of the 6th Dynasty and First Intermediate Period burials is that it is exceedingly similar in style, as William Stephenson Smith reports:

“Until well into the Sixth Dynasty the tombs of Upper Egypt had simply imitated in a provincial fashion the work done at court. The quality of the workmanship varies considerably from place to place... In Dynasty VI, one extreme is represented by the roughly cut reliefs of the Nomarchs of Elephantine at Aswan, which are scarcely to be distinguished in style from work of the First Intermediate Period.”

Another method for making a chronological determination of the First Intermediate Period is the use of written documents. However, we were told above that there is a “complete lack of royal monuments from the First Intermediate Period” from which one can read these inscriptions. Not only that, but Alexander J. Peden tells us that the “lack of textual graffiti from the Sinai peninsula ... evident during the First Intermediate Period appears to extend into the XIth Dynasty.”

Nevertheless, certain written materials have been assigned to this epoch because the historians assume this was a period of chaos and problems, and the literature they assign to it reflects these conditions. The Admonitions of Ipuwer discussed by Velikovsky as evidence of a celestial catastrophe is assigned to the First Intermediate Period. However, Miriam Lichtheim reports:

“Ever since Gardiner’s pioneering edition of this difficult text, his view of the Admonitions as the work of a Twelfth Dynasty author who laments the alleged calamities of the First Intermediate Period has held sway. It is, however, contradictory and untenable. Gardiner maintained on the one hand that ‘the pessimism of Ipuwer was intended to be understood as the direct and natural response to a real national calamity’ (Admonitions, p. 111), and on the other that ‘historical romance was always popular in Ancient Egypt, and there is no inherent reason why the Admonitions, even if referring to the conditions of the Tenth Dynasty, should not have been written under the Twelfth’ (ibid.). I submit that there is strong inherent reason why this cannot be so. If the Admonitions is the ‘direct response to a real calamity,’ then it cannot also be a ‘historical romance.’ The two are mutually exclusive. …

“... in short, as a work of the late Middle Kingdom [it is one] of purely literary inspiration.

“The unhistorical character of the whole genre was recognized by S. Luria in an article that did not receive the attention it deserved. [see S. Luria, “Die Ersten

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werden die Letzten sein” (The First Shall Be the Last), *Klio*, vol. 22 (1929), pp. 405-31] Adducing strikingly similar compositions from other cultures he pointed out the fictional, mythologic-messianic nature of these works and the fixed cliches through which the theme of ‘social chaos’ was expressed. From an Annamite song he quoted phrases that sound as if they came from the *Admonitions* …

“Luria also made the telling point that the description of chaos in the *Admonitions* is inherently contradictory, hence historically impossible: On the one hand the land is said to suffer from total want; on the other hand the poor are described as having become rich, of wearing fine clothes, and generally of disposing of all that once belonged to their masters.

“In sum, the *Admonitions of Ipuwer* has not only no bearing whatever on the long past First Intermediate Period, it also does not derive from any other historical situation.”

This clearly implies that the conditions for this lament by Ipuwer were not related to the history of Egypt but to something more horrendous—a past catastrophe remembered by humanity, just as Velikovsky suggested. In fact, Morkot shows:

“Trying to understand the First Intermediate Period, Egyptologists were deeply influenced by a number of documents of Middle Kingdom origin. The most important are known as the *Admonitions of Ipuwer* and the *Prophecy of Neferti*.

“We no longer read these as historical records, but as a literary genre …”

He adds “there is no evidence for any form of mass or popular uprising [during the First Intermediate Period].” Seidlmayer explains on the nature of these documents that:

“It should be noted that these texts do not actually claim to be set in the First Intermediate Period; nor do they mention any historical particulars. In the *Prophecy of Neferti*, the advent of Amenemhat I (1985-1956 BC) [of the Middle Kingdom] is foretold … which must be situated chronologically in the late 11th Dynasty and not the First Intermediate Period. Careful scrutiny is, therefore, required if we are to determine whether these texts bear any relation to the history of the First Intermediate Period, and even if they do, we need to investigate precisely how they relate to the actual historical events.

“Texts deriving from the First Intermediate Period itself are entirely lacking in that very note of despair that is the hallmark of Middle Kingdom ‘pessimistic’ literature… Certainly there are a number of … similarities between First Intermediate Period biographies and the Middle Kingdom pessimistic texts (such as Nile failure, famine, social unrest, war and a crisis affecting the foundations of

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20 Miriam Lichtheim, *Ancient Egyptian Literature* (Berkeley CA 1980), pp. 149-150
21 Morkot, *op. cit.*, p. 105
22 *ibid.*, p. 106
the state) but these similarities prove, in the first place, literary connections between the two.”

This proves nothing of the sort! There is no scientific or technological evidence for the existence of the First Intermediate Period. Seidlmayer assumes—based strictly on interpretations of archaeological evidence and documents attributed to that epoch—that this period comes between the Old Kingdom and the Middle Kingdom.

The historians agree that these various documents attributed to the First Intermediate Period are neither from it nor refer to it. But over and over we were told they were derived from either the 11th or 12th Dynasties. This brings us to the nature of the hieroglyphic writing in which these documents were expressed. This is well outlined by Thomas L. Thompson. Of the *Admonitions of the Sage Ipuwer* he explains:

“The traditional dating of this text places it in the First Intermediate Period (A.H. Gardiner, The Admonitions 1-5). This dating has been challenged by J. van Seters (JE A 50, 1964, 13-23; The Hyksos, 1966, 103-120) who offers as an alternative a dating in the late Thirteenth Dynasty. The orthography [or writing styles], language, and terminology resemble the Twelfth Dynasty literary texts. … His argument rests upon the philological and historical comparisons which he has made between the late Middle Kingdom on the one hand, and the Old Kingdom on the other. Nor should the Eleventh and Twelfth Dynasties be excluded from possible consideration for the setting of this text, for they too were not without their periods of disruption, however brief. The reference to the ‘Residence’, on the other hand, may restrict one to a choice between a dating at the end of the Sixth or at the end of the Thirteenth Dynasty (Van Seters JEW 50, 1964, 19f).”

William James Hamblin states “We have no specific information about rulers or military events of this period of coups, civil war, and anarchy, as the power of the Sixth Dynasty collapsed.”

“One of the most intriguing and potentially useful texts of the First Intermediate Period purports to be the political and military instructions of king Akhtoy III (c. 2090-2070 [B.C.]), of the Heracleopolitan ninth dynasty, to his son and successor Merekare (c. 2070-2050 B.C.), the last (or next to last) king of the line. Unfortunately, both the authorship and the date of this text is controversial. Wendy Raver, for example, argues that ‘the text may be pseudoepigraphical, but was probably composed in the court of Khety III during the Tenth Dynasty…’ R. Parkinson, on the other hand, believes the text ‘is not contemporaneous with the Heracleopolitan [Ninth-Tenth] Dynasty’ but should be ‘dated to late in the Middle Kingdom’… Given the paucity and ambiguity of the dates, it is impossible to

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23 Seidlmayer, *op. cit.*, p. 135
resolve the question with certainty. For this discussion I will assume the text originated in the court of the late Ninth-Tenth Dynasty, and that it reflects the historical realities of the period.”

In terms of documents from the First Intermediate Period we have the assumption used repeatedly that the texts come from this period and reflect the historical realities of it when in fact they do no such thing. The Koptos decrees of this period, too, exhibit the very same problems outlined above.

In essence, there is no evidence at all of either a scientific or technological nature that suggests in any way that the First Intermediate Period is anything other than a creation by Manetho accepted by the historians and held up to be reality by contrived interpretations of archeology and documents. Justification for this epoch is well outlined by Janet E. Richards:

“The First Intermediate Period, Dynasties 7 through the first half of Dynasty 11 (c. 2060-2040 BC), was traditionally classified as ‘disastrous,’ ‘tumultuous’ (Gardiner 1961), an ‘exceedingly dark time’ (W.S. Smith 1981:152) of confusion, social revolution, and general impoverishment. The art of the period was deemed ‘degenerate’ and ‘wretched,’ and the overall material culture descended to a ‘shockingly low level,’ ‘the deplorable state of Upper Egypt is clearly reflected in the clumsiness of its artistic efforts; evidently Egyptian civilization was at its lowest ebb’ (Gardiner 1964:111). These characterizations seem linked to two factors. The first lay in an early scholarly interpretation of the legitimizing, so-called ‘pessimistic literature,’ now dated to the Middle Kingdom, as true historical accounts from an earlier and utterly chaotic time. This traditional interpretation constituted in fact a naïve acceptance of ancient ideological assertions as valid contemporary descriptions … The second was the artistic dismay at the complete absence of significant royal monuments, and the virtual disappearance of what were understood as ‘high cultural’ forms [during the First Intermediate Period].

“In reality, however, the country did not plunge into a maelstrom of poverty and uncertainty. Central authority did break down, due perhaps in part to a decrease in agricultural prosperity caused by declining inundation levels. There continued to be kings of Egypt to whom nomarchs (or provincial governors) paid nominal allegiance; but order was maintained by these provincial elites who … assumed the functional place of the inner elite during the First Intermediate Period … and emulated high cultural forms in their tombs and biographies. High cultural ideology did not therefore disappear, though the integrity of the inner elite must

\[\text{Ibid., p. 377}\]
\[\text{see Nigel Strudwick, Ronald J. Leprohon, } \text{Texts from the Pyramid Age} \text{ (Leiden, the Netherlands 2005), p. 117 ff}\]
have eroded with decentralization: what did change was form, to a certain extent style, in part due to level of access to diminished court workshops."  
In terms of the short chronology the above citation is rendered thus:

The First Intermediate Period Dynasties 7 through the first half of Dynasty 11 was chronologically classified by historians based on little in the way of monuments, documents, tombs of these pharaohs, or evidence of a central authority. To cover up this exceedingly dark time, that has been traditionally classified as disastrous, the art of the period was deemed to belong to this epoch and not to the preceding 6th Dynasty nor to the following 11th, 12th or perhaps 13th Dynasties. The characterization of this period was presented because of a few factors, none of which had scientific nor technological bases, but were supported by making vast assumptions, none of which could be proved as factual. The first vastly overblown assumption is that Manetho’s invention, the kings from this period, existed. The second was that the artistic materials that existed had to be placed in that time slot. The fact that the artifacts were related to dynasties that came directly prior to and directly after the First Intermediate Period was ignored. The fact that the documents the historians ascribed to this period contained hieroglyphic forms and elements that came directly prior to and directly after this epoch was also ignored.

In reality there was no First Intermediate Period. The central authority did not break down because, as they suggest, there was no central authority nor have they even the slightest iota of clear evidence to support any such assertion. The local rulers did not assume authority of regions outside the capital because the provincial rulers belonged to provincial regions of the 6th, 11th, 12th, and perhaps 13th Dynasties.

THE HYKSOS SECOND INTERMEDIATE PERIOD IS THE FIRST INTERMEDIATE PERIOD

Having merged the First Intermediate Period with the 6th and with the early 11th Dynasties still does not tell us in any way the chronology, by which I mean the most exact place in time of this epoch. That must be addressed on the basis of scientific and technological evidence as far as possible, in terms of all that has been discussed in these volumes.

As has been shown, the 11th Dynasty, which preceded the 12th Dynasty, began around 600 B.C. Therefore the First Intermediate Period has to be located prior to that, but when? That is still the one overriding problem that must be addressed. But it is known that the First Intermediate Period followed the Old Kingdom, and this

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28 Janet E. Richards, “Modified order, responsive legitimacy, redistributed wealth: Egypt 2260-
epoch must not only follow that kingdom but must precede the 18th Dynasty. And all this must be set in the first millennium B.C.

According to Emmet Sweeney, the dynasties of the First Intermediate Period are in reality living in Hyksos times and therefore are the Asiatic Assyrians and their allies that ruled Egypt for over 100 years. He further claims that part of the 6th and the entire 13th Dynasties are included in the Hyksos domination of Egypt, and thus exhibit Asiatic elements. If, as Sweeney suggests, all these dynasties are contained in the Hyksos times, then the 6th, 7th, 8th, 9th, 10th, 13th, 14th, 15th, 16th, and 17th Dynasties—10 dynasties in all—belong to the Hyksos/Akkadian/Assyrian epoch of over 100 years that spans over 500 years in the established chronology. That is, the First Intermediate Period was the Hyksos/Akkadian/Assyrian Second Intermediate Period!

The question is: How does one scientifically prove that the 6th through the 10th Dynasties were ruled by the Asiatic Hyksos/Assyrians/Akkadians along with their Palestinian and Syrian allies? As long ago as the 1920s, Drusilla Dunjee Houston remarked that Archibald H. Sayce “found the shape of the skulls subsequent to the Sixth Dynasty different from those that preceded it.” What we would expect to find based on the short chronology is that the new peoples who came to Egypt with the Hyksos/Assyrians/Akkadians exhibited very different racial characteristics to those of Egyptian stock, and that they were quite numerous and appeared suddenly as they would if there was a massive invasion by an alien population. François Lenormant and E. Chevallier pointed out in 1871 that:

“It is also to be observed, that by comparing the skeletons of mummies from the tombs anterior to the sixth with those subsequent to the eleventh dynasty, we may observe, in the shape of the skulls, differences sufficient to lead us to the conclusion that the type of the population had been much modified in the interval by the introduction of a new element [into Egypt].”

That is, prior to the Sixth Dynasty the skeletons found in Egypt were mainly or almost entirely racial types that reflected the indigenous Egyptian population dating back to prehistoric times. But thereafter, a significantly large population had intruded and the racial characteristics observed in their skeletal remains that were found for the 6th, 7th, 8th, 9th, and 10th Dynasties were clearly not those of the original Egyptian population.

29 Emmet Sweeney, The Pyramid Age (NY 2007), pp. 97-113
30 Ibid., pp. 113-4
31 Drusilla Dunjee Houston, Wonderful Ethiopians of the Ancient Cushite Empire (reprint, Forgotten Books 2007), p. 59
Donovan A. Courville points out regarding these graves:

“During the last decade of the 19th century, a series of cemeteries were discovered widespread over Egypt which represented a people who were clearly not Egyptian. The bodies in the graves were not mummified. Sometimes the bodies had been dismembered before burial, but when entire, they were always resting on their left sides with their knees drawn up on a level with their chins and their hands raised to their faces. … These graves were found in many parts of Egypt, and the large number of graves did not permit the conclusion that they represented an isolated group of settlers or that they represented an unimportant section of the population. Nor could they represent the graves of an army, since there were as many graves of women as of men. The only evidence of Egyptian influence was the style of earthenware…

“Some of the graves were found to have intruded into Egyptian burials of the VIth Dynasty and earlier.”

The significant facts to note are that the graves were quite common, the people not mummified—therefore not Egyptians—and the graves intruded into burials of the VIth Dynasty. These facts indicate that this foreign population came to Egypt during or at the end of the VIth Dynasty. They were obviously dominant since they could dig up VIth Dynasty cemeteries to bury their dead, and the fact that these burials were widespread shows they dominated much of Egypt. With respect to the position of buried bodies lying on their left side, with knees bent and hands raised to their faces, being unmummified, William G. Dever shows the “Tomb … of a typical Asiatic (Canaanite) Middle Bronze Age from Tell el-Dab’a.” The typical burial, he claims, shows these features which indicate that the people buried throughout Egypt after the VIth Dynasty were the Hyksos or allies of them.

That is, the first large-scale change in the demographic make up of Egypt occurred in the 6th through 10th Dynasties, showing burial characteristics somewhat identical to the burials of the Hyksos. i.e. people lying on the left side with knees bent and hands up to the face. As is well understood, the very first large-scale invasion of Egypt that would exhibit such skeletal characteristics was the influx of the Hyksos/Akkadians/Assyrians.

G. Elliot Smith, after a long period of examination of these two populations, reported his findings in 1926:

“In looking through the notes that I began compiling … in 1901, I find comments relating to occasional specimens, even some from graves as remote as

34 William G. Dever, *Who were the early Israelites and where did they come from?* (Grand Rapids MI 2003), p. 12
the time of the Second Dynasty … in which the exact conformity of certain skeletons to the Proto-Egyptian types is questioned: such doubts [about the make up of the Egyptian population], however, are very rare until the Sixth to Twelfth [Dynastic] series is reached, when they become comparatively common.”

The germane point Smith makes is that up until the Sixth Dynasty non-Egyptian types were rarely found in the population. But from the 6th Dynasty forward in time, these non-Egyptian types were “comparatively common.” This does not reflect a gradual influx of a foreign population. If such was the case Smith would have found this foreign population gradually increasing in number over time. Instead he reports that from the 6th through the 10th Dynasties this foreign population becomes “comparatively common.” This means a large-scale foreign population influx into Egypt.

What were the traits of this new population that would give us a clearer understanding of where these people came from prior to entering Egypt? If they were of a variety of different groups, that too should be evident from their skeletal remains. And that, too, is what one would expect if the Hyksos/Akkadians/Assyrians came to Egypt with Syrians, Palestinians, and others.

“But in 1904 the problem [of who these newcomers to Egypt were] presented itself to me in a much more pronounced form. In the previous year the Hearst Expedition had begun excavating the Ancient Empire cemetery among the Giza Pyramids … For reasons which will appear in the course of this discussion, it would be difficult to exaggerate the importance of this material…

“A large proportion of the remains, and especially those of women, showed a close resemblance to the majority of Egyptian remains examined in Upper Egypt … but there were a few that I definitely labeled ‘alien’ … and a considerable number in which the head was bigger and especially broader, the features finer, and the skeleton generally more robust… It was not until the year 1908, when the remains of definitely alien populations found near the First Cataract were being submitted to critical examination and comparison with anthropological material from Egypt that the criteria were found for establishing in the remains of Ancient Egyptians the reality of certain physical traits distinctly foreign to Egypt…”

“The bones present a curious blend of features, such as we have grown accustomed to regard as distinctly Egyptian and others equally certainly alien, perhaps even representations of two fairly well-defined foreign populations.”

The first point is that early on the Egyptian population was largely represented by the skeletons of women. This makes perfect sense in terms of the Heinsohn and Sweeney equation that the Hyksos/Akkadians/Assyrians, when they first invaded

36 Ibid., pp. 115-116
37 Ibid., p. 117
Egypt, destroyed the entire Egyptian army and killed off much of the male population, or enslaved them, which was common practice. The vast majority that survived to be enslaved were women, many of them forced into concubinage, and therefore they are well represented in the early graves when the population changed. The second point Smith makes is that in general the new-comers were of two more or less distinct types with a mixture of these types. The Hyksos/Akkadians/Assyrians from northeastern Mesopotamia would be representative of one group, the Syrians and Palestinians of the other, with a wide mixture of these. Smith resumes:

“Although these aliens who begin to make their way into the Delta from Palestine and Syria [and/or elsewhere] … all conform to the same racial type, known as Alpine or Armenoid, they lack the uniformity of the Proto-Egyptian people. In fact there is a very wide range of variation amongst these as regards the form of the skull and face … Yet they all display the peculiarities of forehead, occiput, orbits, nose, and mandible which are distinctive of the Armenoid race.”

“That these contrasts between the crania from Upper and Lower Egypt, respectively, at the commencement of the Pyramid epoch are not confined to a few scattered individuals is shown by the means of the measurements of large series of skulls from the two territories.”

Not only do these newcomers appear “comparatively common” by the time of the 6th Dynasty, but they are found “by the means of large series of skulls from the two territories,” “from Upper and Lower Egypt.” The modern historian, however, who, based on the established chronology, could not envisage that Egypt was invaded by a large Asiatic contingent toward the end of the Old Kingdom, determined that this was a period of gradual immigration of Syrians and Palestinians into the Egyptian Delta and thence into Lower Egypt. Thomas L. Thompson well summarizes the difference of opinion of those early proponents of the invasion thesis as opposed to those later proponents of the gradual infiltration hypothesis, based on the texts ascribed to this period:

“With the publication of the texts and translations of the Instructions to Merikarê and the Prophecy of Neferty, the suggestion Gardiner first made in his publication of the Admonitions of Ipuwer that the First Intermediate Period witnessed major historical incursions of Asiatics in the Eastern [Nile] Delta became widely accepted. These three texts have continued to be the main sources around which this interpretation has been constructed; numerous other references in early Egyptian texts … have been seen as fully supporting this thesis.

“H. Frankfort … in 1926, brought together, in support of this interpretation, archaeological evidence for many Syrian related button seals which appear in Egypt …

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38 Ibid., p. 119-120
39 Ibid., p. 122
during the Sixth Dynasty … as well as the Inscription of Uni which mentions five expeditions against the land of the Asiatics during the reign of Pepi I. On the basis of the button seals, he concludes that the value of the Admonitions of Ipuwer (which was thought to refer to the time of Pepi II) as an ‘historical document’ was established. He concluded from this evidence that ‘we are dealing with a major disruption of the entire Near East by the Amorites’ and a ‘Syrian influx’ into Egypt. In the Inscription of Uni, he sees how already ‘under Pepi I’s reign, the first waves [of Amorites] come breaking on the eastern border (of Egypt), with anything but decreasing force.’ After such strong statements, it can hardly be surprising that Scharff would conclude from the Instructions to Merikarê that Egypt saw in the Ninth Dynasty an Asiatic [Amorite] occupation of the Delta; he suggests that this had already begun during the reign of Pepi I. Recently, this interpretation has been continued in the new edition of the Cambridge Ancient History where it has been related to materials from Mesopotamia and Palestine and placed into the context of widespread Amorite movements which were understood to involve the entire Near East during the period c. 2160-1780 B.C. [covering the First Intermediate Period]. Posener, who discusses the Egyptian material, bases his interpretation [for an Amorite invasion] upon the Admonitions of Ipuwer and the Instructions to Merikarê, using other early Egyptian records to support his interpretation. From the Admonitions, he sees evidence for a breakdown in trade between Palestine and Syria, and a weakening of defenses along the Egyptian frontier, caused by internal disorder which resulted in a movement of Asiatics [Amorites] ‘in force into the Eastern Delta.’ He speaks of these Asiatics as ‘invaders,’ and on the basis of the Instructions [of Merikarê], concludes that they are nomads who had come to Egypt from Palestine. From the Prophecy of Neferty, he concludes that there must have been a repetition of this invasion during the Eleventh Dynasty, though he sees in the Prophecy [of Neferti] also a reflection of the invasion of the First Intermediate Period. He points out the similarities of these movements to the Amorite movements in Mesopotamia; however, he does not feel that the evidence is adequate to show that we are dealing with the same people.”

Thompson continues:

“Kathleen Kenyon, on the other hand, writing in this same chapter of the Cambridge Ancient History on the archaeological material from Palestine for this period, does assert this identification, and sees a widespread movement of West Semitic nomads coming out of the Syrian desert into the settled regions around the Fertile Crescent, beginning in the region of Akkad [i.e. Assyria] about the time of Sargon [the great Assyrian king], but not reaching the Syrian coast until after the time of Pepi II [of the 6th Dynasty]. In Egypt, this period of nomadic disruption is seen to extend from the time of Pepi II to the time of Sesostris I [of the 12th Dynasty], and to some extent down to the time of Sesostris III. Its greatest effect is, however, on Palestine…

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40 Thompson, *The Historicity...*, op.cit., pp. 118-120 (emphasis in italics added)
“This picture of Egyptian history has been defended by many Palestinian archaeologists and Biblical historians … Kathleen Kenyon sums up this view of the First Intermediate Period very succinctly in her *Archaeology of the Holy Land*: ‘In 2294 B.C. (new dating following the *Cambridge Ancient History*: 2185) the Old Empire [Kingdom] of Egypt fell before the attacks of Asiatic invaders, and the period known as the First Intermediate began. Such a period ranks with the Dark Age of Europe…’ and: ‘Palestine … received a great invasion of nomadic groups in the last centuries of the Second (Third!) Millennium, which completely blotted out the preceding urban civilization of the Early Bronze Age. Egypt suffered the same fate … and Egypt was invaded by barbarians, some at least of them Asiatics. Peaceful conditions were not restored until Egypt was once more reunited under the Twelfth Dynasty …’

This explanation is still in dispute because the Egyptologists cannot find appropriate invaders for this period, and this suggests a gradual influx of Asiatics. The evidence for a swift invasion of Egypt was clear to these early researchers but the chronology into which the First Intermediate Period was fitted made it impossible for later historians to accept what appeared to be implausible or impossible in that time scheme.

Nevertheless, there is evidence from the Hyksos/Akkadian/Assyrian stronghold at Tell el-Dab’a, the city of Avaris, that Asiatics were living there. On this Janine Bourriau reports:

“There is evidence from Tell el Dab’a that a community of Asiatics, albeit very Egyptianized, existed there as early as the 13th Dynasty. So far, however, this is the only convincing archaeological evidence for a population of Asiatics within Egypt (but living differently from the Egyptians) during the Middle Kingdom. There are also references in contemporary texts to ‘camps of Asiatic workmen.’

“It is likely that the oldest settlement at Tell el Dab’a, which dates to the First Intermediate Period, was deliberately built as a component in a defense system constructed to protect the eastern boundary [against whom, since there supposedly was no military threat to Egypt by Asiatics?] … The non-Egyptian character of the community is evident from the layout of the houses (apparently following a Syrian model) and from the fact that tombs were integrated with the living areas rather than being placed in a cemetery outside the settlement. Not only are there differences in material culture, defined by pottery and weapon types, but the nature of the burials indicated a mixture of Egyptian and Palestinian traits. From a robber’s pit cut into a tomb chapel come fragments of an over-life-size limestone statue of a seated man holding a throwstick; the artistic style and the clothes are non-Egyptian …

“In the next stratum…, Middle Bronze Age culture becomes more pronounced, and tombs include burials with donkeys, sometimes in pairs. Other finds include

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41 *Ibid.*, p. 120
an impression of a cylinder seal of North Syrian style, fragments of Minoan Kamares ware pottery, and a gold pectoral..., also thought to be Minoan [which in vol. II of this series was scientifically and technologically dated to the first millennium]. Such objects, together with the ‘ordinary’ testament of Middle Bronze Age imported [Minoan] pottery and Egyptian imitations [of these], confirm the mixed character of the settlement. The origins of these Asiatics—if they had a single origin—are not easy to determine. The Asiatic culture was certainly heavily adulterated by the underlying Egyptian one, the bulk of the pottery was Egyptian (although dropping from 80 percent to 60 percent...) and the administration [of Tell el Dab’a], judging by the titles of officials on scarabs, was carried out on the Egyptian model. Parallels for the foreign traits have been found at southern Palestinian sites... and at Byblos (in modern Lebanon). In a study of the non-Egyptian pottery from Tell el-Dab’a, Patrick McGovern has postulated that most of it originated from the cities of Southern Palestine...

“The culture of the people of Tell el-Dab’a is not static but rapidly develops new traits and discards old ones. This makes the characterization of each stratum in terms of its architecture, burial customs, pottery, metal and other artefacts relatively clear, but leaves unanswered the question of why and how this cultural mixing and rapid development took place.”

What we have during the First Intermediate Period at Tell el-Dab’a is an Asiatic population, building the first city with Egyptian, Palestinian, and Syrian labor forces who bring their burial customs, architecture, etc. with them. These change rapidly as the site grows with new foreign groups working on the city. And this takes place supposedly during the First Intermediate Period. It does, however, make perfect sense that this is just what transpired when the Assyrians/Akkadians/Hyksos took Egypt and built their fortress at Tell el-Dab’a. There is no doubt that the skeletons of the Asiatics in these burials are Armenoid, just as the ones found at Giza and elsewhere.

Here is how Thompson (as do others) dismisses the invasion concept:

“The general picture which we have ... of Egyptian-[foreign] relationships appears to be borne out by an examination of the three major texts which have served as the basis of the theory that there was an Amorite invasion from Palestine during the First Intermediate Period: The Admonitions of Ipuwer, the Instructions of Merikarê, and the Prophecy of Neferty. In fact, as we shall see, no such invasion took place ...

“Certainly the Admonitions give no support to the invasion hypothesis; the usual terms for [foreigners and/or Amorites] are not even used in the text. The text does present, however, a graphic description of the Egyptian upper class’s view of social

upheavals. The complaints so eloquently expressed by Ipuwer are overwhelmingly complaints against the Egyptian officials, fellow Egyptians, and the situation which allows the poor and the lower classes to rise to positions of power.”

But the question that neither Thompson nor others deal with is: Why did the lower classes rise to positions of power? The implication is that they simply by force deposed those in power. What permitted this to happen? Answer: A break-down in the power of the state. Why this occurred is not known; it is merely assumed to be the case without any evidence that this breakdown occurred. The entire case is one piece of circular reasoning based on an unproven assumption that the wheels of government stopped.

In terms of the short chronology there is no such assumption; the government collapsed because of a foreign invasion and the lower classes for a time looted the homes of those in power who had no means of protection left to escape the wrath of the advancing Assyrian/Akkadian/Hyksos army. This is quite similar to the recent second Iraq war where the United States toppled that regime and the people looted the nation’s infrastructure, stores, markets, museums, etc. Without an intact state to protect these properties, the poor looted what they could. In ancient Egypt the very same conditions applied except the wealthy abandoned their property to save their lives. In northern Egypt the Assyrian/Akkadian/Hyksos army with its allies despoiled the cities they encountered and probably raped, enslaved, and took possession of what they could. In fact there is direct mention of foreigners in these texts.

“Until the very end of the text, the reference to foreigners is very superficial, referring to the people from the Egyptian desert whose actions are execrated, not because they are causing disruptions, but because they, like other[s] of the poor, have obtained some wealth: ‘the tribes of the desert … have become Egyptians everywhere’ ([Admonitions] 1:9) and ‘desert dwellers … are skilled in the crafts of the Delta’ (4:8). Only in the very last lines (14:10 ff) of the Admonitions are foreigners mentioned in a significant way, but it is specifically to say that the trouble with Egypt is not the foreigners: ‘Is it Nubians…? Then we will guard ourselves. Warriors are made many in order to ward off foreigners… Is it Libyans? Then we will turn away…” The real difficulty is that Egyptian troops [from different regions such as Nubia and Libya] have ‘turned into foreigners and have taken to ravaging’ (15:1).

“He ends by warning that the st.tyw and the desert peoples are aware of and disturbed by the disruption in Egypt; apparently this is a warning that the st.tyw may be inclined to take advantage of the settled regions if law and order are not reestablished.”

43 Thompson, op.cit., p. 138
44 Ibid., pp. 138-139
That is, various groups in Egypt that had been under Egyptian control, such as the Nubians and Libyans, revolted with the fall of the central power. They, too, could have been involved in pillage. But more importantly the people spoken of, the sꜣ.t.w who “may be inclined to take advantage of the settled regions if law and order are not reestablished” must be determined. Who are the sꜣ.t.w that Ipuwer fears? As Sweeney shows, they are the Hyksos/Akkadians/Assyrians who are referred to as “shepherd kings” or “foreign bowmen.”

“Manetho derived the word ‘Hyksos’ from two Egyptian words which translated as ‘Shepherd Kings’. Although the etymology is now rejected [for the interpretation of ‘Rulers of Foreign Lands’ or ‘Rulers of the Hill Country], there are good grounds for believing the Ptolemaic scribe [Manetho] had tapped into a genuine Egyptian tradition about the Hyksos, a tradition which somehow linked them with shepherds.

“Why link a nation of military conquerors with the humble occupation of the shepherds?

“The land most famous in the ancient Near East for its shepherds and sheep was Assyria. A bas relief on the stairway of the Apadana at Persepolis portrays the subject peoples of the Achaemenid [Persian] Empire delivering their tribute to King Xerxes. Each region brings the tribute upon which the economic strength of that nation is based. The Lydians, for example, with their long side locks [of hair], deliver measures of gold dust to the Great King. The Assyrians are there too. They bring fleeces and live sheep. The Assyrian kings of the Neo-Assyrian epoch were regularly portrayed wearing robes trimmed with woolen fringes and grasping in their right hands the Assyrian symbol of royal authority and power—the shepherd’s crook. The pharaohs of Egypt also used the shepherd’s crook as a symbol of kingly authority, but its use in this context appears to have been unknown before the Hyksos Age. Who then could have introduced such a royal symbol to Egypt but the sheep-rearing people of northern Mesopotamia, the Assyrian Shepherd Kings?”

Now a distinction must be made here between the very small shepherd’s crook scepter used prior to the Middle Kingdom and the long one introduced by the Assyrians/Hyksos thereafter. James Karl Hoffmeier explains:

“Throughout [much of] Pharaonic history, one of the regular symbols of kingship was a small shepherd’s crook … This same [small] crook is attested in Old Kingdom herding scenes, but from the Middle Kingdom [which followed the Assyrian/Hyksos epoch] onward, two types of crooks are regularly found in scenes where men tend cattle and various types of fowl. In the Middle Kingdom, the long crook is still found in the hand of monarchs …, but starting late in the Old

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Kingdom, this particular crook becomes reduced in size to that of a scepter carried by royalty and divinities.\footnote{46}

Ralph Ellis points out that we must

\footnote{46} “… bear in mind that the word hyk (king) is spelt using the shepherd’s crook as a glyph, and that the people that this term applied to were obviously known as aamu or shepherds. This is a sure sign that the prime meaning of the title ‘Hyksos’ involved sheep and shepherds.”\footnote{47}

Margaret Benson and Janet A. Gourlay pointed out over a century ago that:

\footnote{47} “Much controversy has centered round the name ‘Hyksos’ …

\footnote{48} “It is to be noted that the word Hyksos occurs only in Manetho. This people is always called Amu by the monuments. The scepter used by these princes was in the form of a shepherd’s crook, and this crook … has the hieroglyphic value heq (prince), [or king which] was retained by the native [Egyptian] kings after their restoration, as a symbol of sovereignty.”

This brings us back to the term $s\text{t}_{\text{tyw}}$ discussed above by Thompson. He shows

\footnote{49} “The term $s\text{t}_{\text{tyw}}$ … refers to the shepherds living to the east of the Delta, and also to the enemies of Retenu. Only the $s\text{t}_{\text{tyw}}$ along the Egyptian frontier are spoken of as ever having entered Egypt.”

What we observe is that the Hyksos used the long shepherd’s crook as a symbol of sovereignty, that their name in hieroglyphics employed the symbol of a shepherd’s crook, that the Assyrians/Hyksos kings in depictions of them at Persepolis are regularly portrayed grasping in their right hands the Assyrian symbol of royal authority and power—the long shepherd’s crook later used by the Middle Kingdom Egyptian kings and thereafter as a symbol of sovereignty, and lastly, only the name $s\text{t}_{\text{tyw}}$ refers to the shepherds living to the east of the Delta, the only people ever to have been spoken of as having entered Egypt. The problem for the historians is that they have no convincing evidence that explains why their kings, after the Assyrian/Hyksos epoch, used the hieroglyph (hyk) with the shepherd’s crook sign as the symbol for sovereignty. In terms of the short chronology all this evidence correlates with the fact that the Assyrians who invaded and ruled Egypt for over a century used the identical staff—a shepherd’s crook—as their symbol of kingship.

The first people in the first millennium B.C. to invade and dominate Egypt were the Assyrians. Since there is no scientific or chronological evidence for high

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\footnote{47} Ralph Ellis, \textit{Tempest & Exodus} (Cheshire UK/Kempton IL 2001), p. 197

\footnote{48} Margaret Benson, Janet A. Gourlay, \textit{The Temple of Mut in Asher: ...} (London 1899), p. 147

\footnote{49} Thompson, \textit{op.cit.}, p. 133
civilization in Egypt in the early to mid-second millennium B.C. when the Hyksos supposedly did the same, it seems rather clear that the Hyksos shepherd rulers of Egypt were Assyrian shepherd rulers. Even Thompson is forced to show

“The only reference [in the Admonitions] conducive to such an [invasion] interpretation (3:1b): ‘Barbarians from abroad have come to Egypt’ is preceded by: ‘the desert is throughout the land; the nomes are laid waste’ (3:1a), an apparent reference to the non-Egyptian desert people coming into the fertile regions. The ‘barbarians from abroad’ are from the desert lands along the Egyptian frontier.”

Egypt was, of course, isolated in the Nile valley with semi-desert on both sides of its frontier. An invasion would have therefore naturally been interpreted as an invasion from the desert and thus by desert people. Sweeney well explains the disruption of the Egyptian nation that followed on the heels of the Assyrian/Akkadian/Hyksos intervention:

“When a nation is subjugated by a foreign power there is generally a complete disruption of the established order. New local administrations are established throughout the conquered territory, whose purpose is to impose the will of the invading power. In short a feudal type situation evolves. Stranger then that one of the most outstanding features of the Sixth Dynasty was its feudalism. From the very beginning there appeared a new class of local rulers, ‘Great Lords’ of the districts or ‘Nomes’, who were completely unknown before, and whose power and authority vied with that of the pharaoh himself. ‘These nomarchs,’ according to [James] Breasted [A History of Egypt (1951 ed.), p. 132], ‘...are local adherents of the Pharaoh executing his commissions in distant regions, and displaying the greatest zeal in his cause; but they are no longer his officials merely; nor are they attached to the court and person of the monarch so as to build their tombs around his pyramid. They now have sufficient independence and local attachment to locate their tombs near their own homes.’

“But the nomarchs were not only local rulers of the epoch. The dislocation of Egyptian society caused by the Hyksos conquest led to political fragmentation and chaos throughout the Nile Valley. Independent and semi-independent states appeared in the various nomes. This fragmentation may have been exacerbated by periodic rebellions, incited by the vicious exploitation that normally accompanies conquest and colonization. Quite probably, high ranking members of the Hyksos ruling class would have been given lordship in the various regions to use and exploit as they saw fit; much in the manner of the Norman barons after the conquest of England. These ‘barons,’ we shall argue, incited rebellions in the land of the Nile throughout the Hyksos period, rebellions that gave rise to numerous ‘dynasties’ which, although placed in sequence in the textbook, actually reigned simultaneously. Among these

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50 Ibid., p. 139
The number of kings for these periods is well documented. W. Stevenson Smith points out: “Manetho’s statement [is that] the Seventh Dynasty consisted of seventy kings who ruled for seventy days[; this] has usually been interpreted as representing a brief period of strife…”

The second document that clearly delineates the feudal aspect of this period is the Instructions to Merikare. Here again Thompson explains:

“The Instructions to Merikare is the one major relevant text that can be clearly dated to the First Intermediate Period. It is composed in the form of last minute instructions to the pharaoh’s son … The Instructions give advice on the various difficult problems which face Merikare, and … speak about the peaceful relations which exist with the South, and Merikare is strongly advised to do nothing which would upset those relations. From lines 82 to 110, he speaks about the difficulties of the ‘Northland,’ i.e., the Delta [Hyksos] region, which has been very troublesome to him. He has succeeded in pacifying the West which now supplies Upper Egypt with wood from Syria. The situation is quite different however in the East which is ‘rich in bowmen.’ … He mentions the steps which he has taken to bring the situation under control, first changing the administrative structure of the region, and then establishing a frontier line … He settles the frontier with ‘picked men of the entire land’ to protect it. This enabled the people of the Eastern Delta to control the ‘bowmen’ … who now no longer offer a serious threat to Egypt. ‘He is only an ‘3m’ who cannot threaten the fortified cities.’ Merikare then receives further instruction in the methods of controlling these people. Mention is made of a garrison of 10,000 troops at … (Memphis) who have been encouraged to stay there by the grant of tax-free status. This section ends with a warning… that if the south should ever be threatened the ‘bowmen’ in the North will surely take advantage of the situation.”

Thompson sees this as merely “border raids and skirmishes … hardly a wholesale invasion.” But we were told that the Egyptians had established a city at Tell el-Dab’a during the First Intermediate Period, possibly, according to Janine Bourriau, above, “as a component in a defense system constructed to protect the eastern boundary.” That is, a city of mixed Egyptians and Asiatics existed in the Delta. But the garrison of “10,000 troops” is at “Memphis”, just south of the Delta, which means that the Delta had fallen into foreign hands. And just who were the “‘bowmen’ in the North,” (the Delta) who would invade Egypt if “the South

51 Sweeney, op.cit., pp. 99-100
53 Thompson, op.cit., pp. 139-140
54 Ibid., p. 140
should ever be threatened”? What people first brought the composite bow to Egypt? The answer is, of course, the Hyksos. This is the advice of a pharaoh to his son Merikarê not to engage the bowman invaders of the Delta and to garrison an army of 10,000 men at Memphis, just south of the Delta. Who sends 10,000 men to deal with skirmishes and raids and has to give them tax-free status to keep them in this dangerous position when only a few thousand would do? An army of 10,000 men is what one deploys when the country is threatened by foreign invaders. And they are deployed at the very place that is most vulnerable to invasion.

Hoffmeier reports who these “bowmen” are:

“The Admonitions of Ipuwer continues in the complaint genre like that of ‘Neferti,’ bemoaning the conditions of Egypt. Although the dating is debated, the work seems to reflect the kind of disruption of political and social life that characterizes parts of the First Intermediate Period. Ipuwer, while not devoting the same space to the problems of foreigners in the Delta as ‘Merikare,’ briefly describes the nature of the problem. Delta residents maintain defensive postures, carrying shields… The same is said of farmers plowing the fields… ‘The nomes are destroyed’ complains Ipuwer, ‘Foreign bowmen (pdtyw) have come to Egypt’ … In the lacunae-filled section of the work, several references are made to ‘Bowmen’ and ‘Asiatics (‘3mw).”

The picture we have from all the above is that of a largely defeated nation where the remaining powers are deeply concerned with the salvation of the rest of the country from foreign “bowmen,” “Asiatics,” to the north. Beyond that, even the names of certain 6th Dynasty kings are identical in meaning and almost the same in expression as those of the Hyksos. Here, too, Sweeney points out:

“Thus for example the two major pharaohs of the time [the 6th Dynasty], Pepi I and Pepi II, shared virtually the same names as the two major pharaohs of the Hyksos epoch, Apopi I and Apopi II. Indeed both names have identical meanings, i.e. ‘Apep’s man’ or ‘he of Apep’ (Apep of course being the dreaded Serpent of Chaos) [i.e. Seth, whom the Egyptians feared].

“Why, the reader might ask at this stage, should a monarch [of Egypt] name himself after the hated and feared dragon serpent [Seth] that had earlier apparently threatened to destroy mankind? The answer is no mystery. As a deity of death and destruction, Apop was a god of immense power. Ancient kings, in their wars, sought to emulate the destructiveness of the gods in their battles with the titans. Significantly enough, the kings of Mesopotamia were particularly noted for this. Thus Shalmaneser III, a later king of Assyria, recorded how he left the Hittite [Lydian] land covered in ruined heaps, ‘like spoil heaps left by the Flood.’ Even more pertinently, the kings of Assyria described themselves as ‘dragon.’ The same

55 Hoffmeier, op. cit., p. 59
king Shalmaneser III, for example, described himself repeatedly as ‘the great dragon.’”

As we reported above, the Assyrians worshipped the god Seth who was a dragon, so it was quite natural that some Assyrian/Hyksos kings would name themselves for him or attributes of him.

In this regard, Thompson, citing Ancient Near Eastern Texts (ANET), p. 276, tells us that there is “The nearly ubiquitous self-description of Assyrian kings as ‘great dragon…””

Sweeney goes on to describe the art of the First Intermediate Period that is like that of the Hyksos/Akkadians/Assyrians and is particularly like the art of the 6th Dynasty as well as others of that period, and cites Flinders Petrie’s The Making of Egypt (London 1939):

“Personal adornments and artwork of clearly Asiatic (specifically Mesopotamian) provenance makes its first appearance in Egypt at this time. Chief among the personal adornments are so-called ‘button badges,’ medallion-like talismans worn on a string around the neck. These amulets, usually inscribed with a cruciform design, were popular with a number of Asiatic peoples. During the Sixth Dynasty such amulets became common in Egypt, though most occur in the burials of the petty kings of Dynasties 7 and 8 [according to W.H. Hayes, CAH vol. 2, pt 1 (3 ed), p. 55] … button badges, although worn in northern Syria, were particularly associated with Mesopotamia. In the words of Petrie ‘… the eight-pointed star is figured on a button [badge] in Egypt, exactly like the pattern from Bismya in Sumer [in Mesopotamia].’ [Petrie, p. 122] Petrie suggested that some of these at least were worn by a bodyguard of foreign soldiers. [Petrie, ibid.] He regarded the appearance of the ornament as the material sign of an intrusive population.

“Other links with Asia are forthcoming: ‘On a jasper cylinder of Khandy, 2nd king of the VIIth … dynasty, he appears as a Syrian king giving life to Syria, while the Egyptian stands in the background holding a papyrus stem. The ibexes and guilloche [interlacing curved, twisted lines] mark this as a Syrian work.’ [Petrie, p. 123] The conclusion was inescapable [to Petrie]; ‘It is evident … that the Syrians had conquered and held Egypt as a joint kingdom with Syria.’ [Petrie, ibid.]”

Art historian Henri Frankfort shows that there is no difference between Hyksos art and that of the Hurrians and Kassites—possible allies of the Assyrians or subject peoples brought to Egypt from Mesopotamia as artisans: “In the history of art these newcomers remain indistinguishable. One cannot speak [of differences in] Hurrian, Hyksos, or Kassite style.” It is clear that Hyksos/Akkadian/Assyrian art in Egypt
is, as Frankfort stated, “indistinguishable” from that of Mesopotamia which means that the Hyksos were a nation that dominated Mesopotamia and could bring artisans to Egypt, or import items from this region. If they were Assyrians ruling in Egypt, this is a logical conclusion.

In summary, the scientific evidence shows that the skeletal features of the newcomers to Egypt during the First Intermediate Period, like those of the Hyksos/Akkadians/Assyrians, are Armenoid, that they appear rapidly in the 6th Dynasty and are “comparatively common” from that time onward to the 11th Dynasty. The most common Egyptian remains in Upper or southern Egypt are those of women which indicates that the males were either killed in battle or enslaved, which points to invasion by Hyksos/Akkadians/Assyrians. Although the newcomers often exhibit a mixture of racial traits, there are two main groups, suggesting one group—the Assyrians—were in certain respects different from their more easterly neighbors, the Assyrians having a greater preponderance of Nordic features.

The texts that are related to this period call the “foreigners” “Asiatics” and speak directly of these outsiders as “bowmen,” as rulers whose symbol of authority was the “long shepherd’s crook,” the royal symbol of the Assyrians. As for these outsiders being “bowmen,” it is well known that the Assyrians employed the composite bow which greatly outmatched the range of the Egyptians’ straight bow. These foreigners are spoken of as having their location in the Nile Delta, precisely where the Hyksos/Akkadians/Assyrians established their base at Tell el-Dab’a, the city of Avaris. The too numerous rulers of the First Intermediate Period indicate that the allies of the Hyksos/Akkadians/Assyrians were given control over different regions of Egypt and explains the many kings ruling these regions, which is inexplicable on other grounds. They, also, as vassals of the central authority, take their positions in the name of foreign pharaohs who rule Egypt. And these foreigners employ an art style “identical” to that of Mesopotamia, indicating that these newcomers admired Mesopotamian art. Their allies from Syria bring their “button badges,” etc.

We will return to the Hyksos/Akkadian/Assyrian period in Egypt below in the chapter on the Exodus, wherein we will follow Heinsohn’s thesis on this equation. Having covered the chronology of the Old Kingdom in Chapter 1, we have reached the end of Part One of this volume.
CHAPTER 7

PART II – PALESTINE

THE BIBLE AND CHRONOLOGY

The history of Palestine and its relationship with the Hebrew Bible is today fraught with great implications. Since the rebirth of the state of Israel in 1948, war and religious dissension between Hebrews and Muslims have created an atmosphere regarding the place of the Bible in ancient history that is deeply felt by both sides. Muslims maintain that they lived in Palestine before the Hebrews and thus the Israelites have no historical claim to that land. The Hebrews hold that God gave that land to them and thus their claim is historically valid. This work will not attempt to resolve this issue but rather examine the history and chronology of the Old Testament. The approach involved will be based on the same forms of scientific and technological evidence that have been employed in these three volumes to elucidate and evaluate the history and chronology of the ancient nations that surrounded, and interacted with each other and with, ancient Israel.

In this respect I am following Immanuel Velikovsky who directly and forthrightly claimed that the most valid and accurate methodology by which the history and chronology of the ancient Near East, including Israel, should be determined is through the use of science. In this regard he claimed:

“To my way of thinking, these books of the Old Testament are of human origin; though inspired, they are not infallible and must be handled in a scientific manner as other literary documents of great antiquity.”

As an example of Velikovsky’s reliance on science to explicate history and chronology, we turn to his discussion of “How Long Did … Ramses II Reign?” in his 1978 book, Ramses II And His Time. This question has been fully discussed in my article “Science, History, Ramesses II and Velikovsky” in The Velikovskian, vol. V, no. 4 (2003). There I enlarged on Velikovsky’s methodology. That is, the historians, based on the documents, claimed about 66 years for Ramses II. Velikovsky states:

“In modern textbooks on history, Ramses II is invariably invested with a sixty-six year reign. The issue on which certain authorities disagree … is whether Ramses II reigned from -1304 to -1238 or from -1290 to -1224. …

“For assigning to Ramses II the very long reign, several arguments carried weight. There are a great many monuments dating from the reign of Ramses II, some of them of colossal proportions. There exists a document dated in the sixty-

1 Immanuel Velikovsky, Stargazers and Gravediggers (NY 1983), p. 284
seventh year of Ramses; and there exists a written supplication by one of the later Ramessides to be granted by the divine powers a life double the years of Ramses II—which implies that in later generations Ramses’ life or reign was regarded as being of a prodigious length.

“This evidence does not stand unchallenged. Of the multitude of Ramses’ antiquities, most refer to the first three or four decades of his reign and, strangely, hardly any document is dated in the last two or three decades of his reign. In the beginning of his reign—as on the monuments commemorating his campaign toward the Euphrates (year 2, year 5), Ramses II marked the years from his accession. But late in his reign he may have reverted to dating from the beginning of the co-regency. Whether this is so or not, the fact is that Ramses II was not a very old man when he died and therefore did not reign sixty-six years—all that is necessary to establish.

“If he had reigned for sixty-six years as sole ruler after the death of his father, Ramses II must have reached his late eighties or nineties at his death.”

How did Velikovsky resolve this question? He did, in fact, turn to the very same type of evidence this author relies on, namely science and technology, to determine the age of this ancient monarch:

“Rudolph Virchow, the renowned anatomist of the second part of the nineteenth century, was known for his interest in archaeology. He investigated the skull of Ramses II’s mummy and wondered at the form of the jawbone; it could not be that of a very old man.

“G. Elliot Smith, the anatomist at the University of Cairo, who examined all available royal mummies of Egypt, wrote of Ramses II’s mummy: ‘The teeth are clean and in an excellent state of preservation; they were only slightly worn [in the front of the mouth, but deeply worn at the back, probably due to malocclusion or overbite].’ …

“He viewed the dental condition of Ramses II was more recently challenged by J.E. Harris and K.E. Weeks, who made an X-ray examination of Ramses’ body, oral cavity included. They found [the teeth at the back of the mouth had] ‘what must have been painful alveolar abscesses,’ yet they did not dispute Smith’s evaluation of Ramses’ age at his death. Dr. Wilton Krogman, working with the University of Michigan team that performed the X-rays, interprets the results as indicating that Ramses II was in all likelihood ‘between 50 and 55’ years old at the time of his death. The figure was obtained from a careful study of demineralization of the pelvis.

“The sternum (the breastbone) is a good indicator of the age of a person. Smith examined the sternum and wrote: ‘Part of the sternum had been broken off the upper part of the thorax. On raising this I was very much surprised to find that, in spite of the great age to which Ramses had attained, the manubrium sterni was not ankylosed [turned to bone from cartilage, a process that is indicative of age] to the gladiolus, and the ossified second costal [rib] cartilages still articulated by joints

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2 Immanuel Velikovsky, *Ramses II And His Time* (NY 1978), pp. 214-216
with the sternum.’ [In a very old man all this or nearly all this cartilage would have ankylosed or turned to bone].”\(^3\)

Here Velikovsky claims that scientific facts must override documentary evidence and specifically proposes that this is just how one must analyze historical chronology, in this specific case, but also in all other cases:

“Between a figure on a document and anatomical expertise, it is always the latter that carries greater weight. Would a Scotland Yard anatomist certify the age of a dead man—of a living one for that matter—on the basis of the state of ossification or on the basis of a date on a wedding certificate?”\(^4\)

In this case we have a comparable analysis to that employed in these three volumes; Velikovsky and I employed forensic anthropology as the methodology of determining the chronological age of Ramses II. However, I have gone further by employing forensic history—science and technology—to determine the chronology of the ancient Near East. The dead remains of Ramses II are thus no different from the dead remains of the ancient world. Both are subject to examination by scientific and technological methods to uncover and analyze the true nature of their ages. As Velikovsky has told us, “Between a figure on a document and anatomical expertise, it is always the latter that carries greater weight.” So, too, with all of ancient history, between the figures on the documents and the scientific and technological facts that can be applied to that time, it is always the latter that carry the greater weight. As a Scotland Yard forensic anthropologist would not and should not accept a certificate of the age of a dead man if it is contradicted by the evidence of ossification of his bones or dentition of his teeth, so, too, historians, archaeologists, and anthropologists should not accept the documents for the chronology of ancient dead history unless, and only unless, they are supported by forensic historical facts. When these forensic historical facts contradict the documents, these must be made to agree with the forensic evidence or be seen as fabrications, gossip, misreadings, misinterpretations, or fiction.

This author, following Velikovsky, will use the same approach to the chronology of the Bible. Velikovsky never held the Bible was “infallible,” as he has so often been accused by critics such as Stephen Jay Gould, among others. He claims it, like all other ancient documents, must be “handled in a scientific manner” and specifically holds that scientific findings must take precedence over documentary and clearly also archaeological evidence. And that, too, is precisely the methodology that will be employed to unravel the chronology of the Bible and explain the miracles of the Bible through scientific, technological evidence. In this respect, in a very great measure, we will follow Velikovsky to explicate the

\(^3\) Ibid., pp. 216-217
\(^4\) Ibid., p. 217
question of the Exodus. However, where science and technology contradict Velikovsky’s thesis, we must, perforce, still follow the forensic history. In this way, what science and technology uphold regarding these matters will be validation for Velikovsky; where these do not support Velikovsky, we will bow to the same evidence he said we must hold above all else.

As early as the time of Flavius Josephus, it was argued by him that the Pentateuch and the rest of the Bible rested on a long, solid, truthful tradition:

“For we have not an innumerable multitude of books among us, disagreeing from and contradicting one another [as the Greeks have], but only twenty-two books, which contain the records of all the past times; which are justly believed to be divine; and of them, five belong to Moses, which contain his laws, and the traditions of the origin of mankind till his death. This interval of time was little short of three thousand years…”

So sure was he of this that he could suggest: “I suppose that by my books of the Antiquity of the Jews, … I have made it evident … that our Jewish nation is of very great antiquity … Those Antiquities contain the history of five thousand years … I have demonstrated our antiquity, and confirmed the truth of what I have said…” The basis of his chronology came “from the writings of the Phoenicians, and the Chaldeans, and Egyptians. I have, moreover, produced many of the Grecian writers, as witnesses thereto.”

This evaluation, like that which defines the established chronology, was built on the interpretation of interlocking documents and not, at that time, on any foundation of science or technology. Nevertheless, this absolute assurance on the part of Josephus has not stood the test of time, as we will show below.

William G. Dever, one of the major Biblical historians of our time, has clearly elaborated the problem of Biblical historiography that exists today:

“For the past twenty years or so there has been increasing scholarly discussion of issues in Israelite historiography, yet there is a growing sense of uneasiness that has now reached crisis proportions. Given the problematic nature of our sources in the Hebrew Bible, is it possible to write a satisfactory history of Ancient Israel? Or, as Max Miller (1991) has put it recently, ‘Is it possible to write a history of Israel without the Hebrew Bible?’ …

“(1) The overall assumption [of those who deny this historiography] is that the texts of the Hebrew Bible are late postexilic, or even Hellenistic, not only in their present edited form, but in content as well, i.e., the literary tradition is largely ‘unhistorical’. (2) Much more attention must thus be paid to the role of ideology in

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6 Ibid., p. 763
7 Ibid.
history-writing—theirs and ours—including religion and cult. (3) A history of ‘Israel’
can begin only with the period of the Judges, or even the monarchy, since any
prehistory is unrecoverable … (4) Since the tradition is a literary one, not historical,
the most fecund approach may be through newer literary-critical methods (narrative
history; symbolic-structuralist and other forms of discourse analysis; folklore and
ethnohistory; etc.); and the most appropriate models may be those drawn from
anthropology. (5) The newer, more comprehensive rubric should be ‘social history’,
rather than religious or political history. (6) Archaeology is acknowledged as a
potential source of historical information (usually unexploited, however). (7) The
question of the ‘emergence’ of Israel is one of the most pressing yet controversial
issues. (8) Finally, several of the most recent works hint at the possibility of a new
‘secular history’ of ancient Israel or Palestine that will mark the next major
scholarly departure—indeed, I think, a new era in Israelite historiography."

What Dever has failed to acknowledge regarding the correct historiography of
Palestine is that which should be most essential—science and technology. I suggest
that “a new ‘secular history’ of ancient Israel or Palestine that will mark the next
scholarly departure—indeed, … a new era in Israelite historiography” will and must
be based on the short chronologies of Heinsohn, Rose, and Sweeney, but most
importantly, on scientific and technological evidence. And that is what we will
attempt to do with regard to that history and chronology.

Going deeper into the present difficulties of this history, Dever states:

“In many ways philology has been the basic tool of modern critical biblical
scholarship. The fundamental flaw in the philological approach, however, lies in
its unexamined presuppositions. It simply assumes (1) that the texts of the Hebrew
Bible as they have come down to us, despite the seemingly intractable nature of
some passages, taken together constitute an accurate witness of the actual
phenomenon of life in ancient Israel; and (2) that the rapid progress of
comparative Semitic philology would enable us to read these texts correctly and
comprehend their meaning. As Morton Smith put it in his 1968 Presidential
Address to the Society of Biblical Literature: ‘For a proper understanding of the
Israelites we must have the archaeological facts determined quite objectively and
independently by competent archaeologists, and the Biblical facts likewise
determined by competent philologians, and then we can begin to compare them.’

“In other words, philologians were confident that they could penetrate behind
the obfuscation of centuries of theological interpretation [of the Bible] to das Ding
an sich [the thing as it is]—if not to historical and religious truth then at least to an
‘exegetical truth,’ based not on correct belief but on a correct reading of the texts…

“Today this seems naïve, wistful, and rather sad …

8 William G. Dever, “Philology, Theology, and Archaeology: What Kind of History of Israel Do
We Want and What Is Possible?”, The Archaeology of Israel: Constructing the Past, Interpreting
the Present, Neil Asher Silberman et al., eds. (Sheffield UK, 1997), pp. 290-291
“The more intransigent problem with ‘mere philology’ is that such an excessively rationalistic, and ultimately literalistic, approach to history can never grasp its ‘inner reality’, if I may phrase it thus. Or to put it another, less impressionistic way: literature is not life [nor necessarily true], but rather the product of the intellectual and literary imagination of a creative few. Thus the study of elitist texts of the ‘Great [Biblical] Tradition’ alone can never enlighten us fully on many matters. In short, in this post-modern, post-positivist era, we ought to acknowledge at least that, even in possession of abundant textual data, we cannot really ‘reconstruct the past’. The past is gone irrevocably; and whatever our data, as Lewis Bedford (1983:31-32) reminds us, we can only draw inferences about ‘what it was like in the past’. ALL THESE INFERENCES ARE INDIRECT AND PARTIAL; MOST ARE UNTESTABLE, AND MANY ARE SIMPLY MODERN NOTIONS, NOT DERIVED AT ALL FROM THAT PAST BUT IMPOSED UPON IT. What we think we ‘know’ reveals more about us and our ignorance than about the past that we are proposing to investigate. To counter von Ranke’s familiar phrase, we can never really know ‘wie es eigentlich gewesen war’ [how it had actually been] historically or archaeologically.”

In this citation, Dever has summarized the very weakness this author has repeatedly presented respecting the usual methodologies for interpreting the past, namely reliance on texts and reliance on archaeology. These ineluctably lead to “inferences [that] are indirect and partial; most are untestable, and many are simply modern notions … imposed upon [the past].” Nevertheless, unable to rid himself of these two methodologies, Dever admits in his conclusion the morass into which these approaches have led:

“Baruch Halpern has recently observed of the current state of biblical studies: ‘Today the welter of competing claims, the cacophony of methods, betrays the culmination of the decades. The synthetic eschaton [or last word] promised in the apocalypse of philological positivism has not arrived’…

“The age of positivism in archaeology is similarly over in this post-Albrightian era [referring to William F. Albright whose archaeological work supported the validity of the Bible]. Perhaps it is time to liberate the writing of the history of ancient Israel, as well as its literature and religion, from all external dogmas. In particular, we must redefine the relation between our two best sources of information—texts and artifacts—not subsuming one under the dominant paradigm of the other, or leap-frogging back and forth between narrative and archaeology, which will only produce what Knauf aptly calls ‘a pseudohistory of nonevents’ … The point of departure must be a mutual, honest, critical dialogue between textual studies and the best that archaeology can offer—one that above all is humble, fully aware of what we do not know, and thus open to new insights about the past.

Ibid., pp. 291-292 (capitalization added)
Otherwise, as Santayana observed, we are doomed to repeat the mistakes of the past, as well as those of past scholarship.”\textsuperscript{10}

That, in effect, is all that has happened. What the historians are not fully aware of, what they do not know, is how the short chronology, based on scientific and technological evidence, will open Palestine’s history to new insights without which historians will be doomed to repeat the mistakes of the past as well as those of past scholarship. John Bright makes it quite clear that he, like Velikovsky, expects the Bible to be treated in a scientific manner, just as is expected for other ancient documents.

“The problem of describing the origins of Israel is one that inheres in the nature of the material at our disposal. If it is correct to say that history can be written with confidence only on the basis of contemporary records [primary sources], it is easy to see why this is so, for the patriarchal narratives are certainly not historical documents contemporaneous with the events of which they tell. Even though many feel that divine inspiration ensures their historical accuracy, to discuss the problem by appeal to [religious] dogma would be unwise. Surely the Bible need claim no immunity from rigorous historical method, but may be trusted to withstand the scrutiny to which other documents are submitted.”\textsuperscript{11}

The way to resolve this dilemma regarding when any sources were actually written had been worked out for Roman history by Mommsen and Burckhardt. The Romans employed documents and expressions in these that reflected the times these expressions were used. For example, if a text contained the words “thee” and “thou” instead of “you,” one could claim this document was probably not written in the 21st century. The same applies to specific linguistic expressions, idioms, and even the form of the script. If someone wrote that he had received an “email” one would know this was written during the era of the worldwide internet. This type of linguistic source analysis could be utilized with the philological expressions in the Bible. A clear example of this linguistic method is outlined by Halpern in his book on King David:

“There is … linguistic evidence [regarding the time Samuel wrote about King Solomon]. In the main, this shows conclusively that the text was written before the 6th century [B.C.], when Babylon exiled the population of Judah. Orthography, or spelling, is the key index. Before the Exile, for the most part, Israelite orthography was ‘defective’–with few vowels represented except at the ends of words. After the Exile, ‘plene’ spelling, which used consonants such as $h$, $w$ and $y$ to represent vowels in medial positions, inside words, was far more common. Later spellings–of the plene variety–dominate in our text because scribes ‘corrected’ spelling as they recopied scrolls, so that many Dead Sea Scroll manuscripts, for example, have very late orthography.

\textsuperscript{10} Ibid., pp. 306-307 (emphasis added)
\textsuperscript{11} John Bright, \textit{A History of Israel}, 4th ed. (Louisville KY 2000), p. 67-68
“This means that plene spellings, with internal vowel letters, are demonstrably late. In the case of biblical books, the spelling may have been updated by late copyists. But defective spellings [without consonants used as vowels inside words] indicate a preexilic date. The concentration of such defective spellings in the books of Samuel is extraordinary. The old spellings point to a starting point before the 6th century [B.C.] for the transmission of the [Samuel] text.”

This linguistic orientation toward analyzing the time when the texts of the Bible had actually been written was presented by Julius Wellhausen in his book Prolegomena To The History Of Israel. Thomas L. Thompson summarizes it thus:

“About a century ago, J. Wellhausen synthesized the results of more than two generations of Old Testament historical-critical scholarship on the sources of the pentateuch into the ‘documentary hypothesis.’ This synthesis concluded that the pentateuch and indeed the first six books of the bible had been formed from a composite of four originally independent documents [slightly different from one another, which explains the contradictions, since each set of writers or each writer told a slightly different version of the events] (commonly referred to by scholars as J, E, D and P: the Yahwist [J version], the Elohist [E version], the Deuteronomist [D version] and Priestly [P version] sources) dating successively from the early monarchic period to postexilic times. …

“The thrust of Wellhausen’s critical analysis of the pentateuch was essentially historical: to establish through an understanding of the history of the pentateuch’s composition and development as a composite text [by four sets of editors or writers] evidence for an evolutionary history of the religion of ancient Israel. In this, he sought to outline a stepped chronological development, away from primitive forms of religious beliefs through henotheism [where, as in other religions of the ancient world, one god is supreme over the others] to the mature understanding of prophetic monotheism and ending in what he understood as the narrow sectarianism of a priestly, cult-oriented legalism. Essential to this historical and evolutionary goal of Wellhausen and others was the isolation of discrete, independent sources and their chronological and ideological association with major epochal transitions in Israel’s history: J with the United Monarchy [under David and Solomon], Judah and the Davidic dynasty; E with the Divided Monarchy and the State of Israel [after Solomon]; D with the reforms of [the prophet] Josiah, the late preexilic period and the prophetic movement; and P with the exilic and postexilic periods and the priestly circles from Jerusalem.

“However, while the orientation of Wellhausen’s work was decidedly in the direction of a positive historical reconstruction of a history of Israel’s religion, the [chronological] implications of the documentary hypothesis largely eliminated any acceptance of the historicity of the referents of the pentateuchal narrative…

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12 Baruch Halpern, David’s Secret Demons: Messiah, Murderer, Traitor, King; I. Sutherland Black, Allan Menzies transls. (Grand Rapids MI 2004), pp. 59-60
“Essential to the history of scholarship expressed in Wellhausen’s synthesis was that these four discrete sources of the pentateuch were to be understood as literary documents created at the time of their written composition, and hence as compositions reflecting the understanding and knowledge of their authors and their world. This assumption contained the disturbing corollary that nothing historically dependable about earlier periods in Israel’s history could be gained from them. The usefulness of the pentateuch for reconstructing the early history of Israel prior to the time of composition was thereby decidedly curtailed. After two decades of intense and often personal attacks on his work, the ‘Graf-Wellhausen approach’ to the so-called historical books of the Old Testament had become the dominant critical interpretation by the end of the [19th] century.”

Thompson adds:

“Wellhausen’s contributions to the history of critical scholarship were immense. But none was as great or as lasting as this on the pentateuch. It is hardly an exaggeration to state that most of the next century [the 20th] of research on the pentateuch and the prehistory of Israel developed either from Wellhausen’s synthesis or was consciously in reaction against it.”

Norman Karol Gottwald rather nicely sums up the situation today which fully pertains to Wellhausen’s analysis:

“Today … we stand in the midst of grave reservations about the reliability of the biblical texts for reconstructing history in any assured form. These caveats arise from two intersecting lines of thought. One caution follows from the perceived lateness of the final compilation of the Hebrew Bible, which in the eyes of many scholars throws into severe doubt the tenability of the information related about earlier historical periods. The events reported (narrated time) and the setting of those reporting (narrators’ time) are perceived to be so far removed from one another, often by many centuries, that the very capacity of late traditionists to reconstruct earlier history is thrown into doubt. The second caution overlaps with and reinforces the first, namely, that the late compositors of the Hebrew Bible were so preoccupied with and limited by the horizons of their commitment to fashion a new ‘Israelite–Jewish’ community following the destruction of the states of Israel and Judah, that their ideological perspective altogether cancels out any interest on their part in reporting the actual history of Israel before the collapse of those states.

“On this view of our sources, when the factors of late composition of the traditions and their ideological alienation from Israel’s preceding history are taken together, it is concluded that we simply cannot rely on the Hebrew Bible to tell us anything substantial about the history of Israel prior to the fifth-fourth centuries [400’s through 300’s] B.C.E. In short, the determinative writers/editors of the Hebrew Bible had little

13 Thomas L. Thompson, Early History of the Israelite People from the written archaeological sources (Leiden, the Netherlands/Boston MA/Köln, Germany 2002), pp. 1-2
14 Ibid., p. 3
or no reliable information about earlier times and, in any case, were primarily interested to shape or invent images and accounts of the past that served their immediate interests in rebuilding Israelite/Jewish identity on a new foundation. If this approach is adopted our [historical] discourse about ancient Israel will proceed under grave suspicion about the biblical records and will seek some basis for assessing the [history] of ancient Israel that does not fundamentally rely on a late, jaundiced account. Whether there exist sufficient resources outside of the Hebrew Bible to compensate for the fragile biblical resources is itself highly problematic. As a consequence, our dilemma as historians is that without a large measure of reliance on the Hebrew Bible, the politics of ancient Israel may remain undecipherable.”

In Wellhausen’s own words:

“Of the Hagiographa, by far the larger portion [of the Hebrew canon] is demonstrably post-exilic, and no part demonstrably older than the exile. Daniel comes as far down as the Maccabaean wars, and Esther is perhaps even later. Of the prophetical literature a very appreciable fraction is later than the fall of the Hebrew kingdom; and the associated historical books … date, in the form in which we now possess them, from a period subsequent to … the year 560 B.C. …”

In essence, the Bible was written, in the draft we have today, during Persian and Hellenistic times! How could biblical writers know what had transpired some 4, 5, 6, or even 7 centuries or more earlier in detail or even correctly? In the “Preface” to Wellhausen’s book, Robertson Smith reports the “book produced a great impression throughout Europe, and … its main thesis … was felt to be so powerfully maintained that many of the leading Hebrew teachers of Germany, who till then stood aloof … declared themselves convinced by Wellhausen’s arguments.”

Historians who adhered to the Bible as a valid source of history in the 1800s and 1900s were able to respond to these criticisms because of the work of Heinrich Schliemann who unearthed the supposedly mythical city of Troy. In this respect, Edwin Yamauchi outlines the arguments that came to prevail which are largely in vogue today:

“In William Dever’s vigorous denunciation of the so-called biblical minimalists [who maintain the Hebrew Bible has no historical validity], he makes a very important observation:

“I cannot resist pointing out that here, once again, the revisionists [minimalists] reveal how scarcely innovative they are, indeed how out of touch with developments in many allied fields. For instance, ‘biblical’ archaeology’s attempts to deal with the question of the historicity of the Hebrew Bible have often been compared with Classical archaeology’s struggle with the ‘Homeric legends.’ A generation ago, even a decade ago, Classicists and ancient historians would

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16 Julius Wellhausen, Prolegomena To The History Of Israel (Whitefish MT reprint 2004), p. 11
17 Ibid., p. 1
have dismissed Homer as a mythical figure and would have argued that the tales of the Trojan Wars were mainly ‘invented’ by much later Greek writers. (Sound familiar?) … It is now thought that those stories of warfare do not simply reflect the situation of Greece in the 8th-7th centuries, but go much farther back to a genuine historical situation of the 13th-12th centuries [B.C.], that is, to the period of the movements of the various ‘Sea Peoples’ across the Mediterranean (including the biblical ‘Philistines’). Thus, it is now argued, a long oral tradition, preserving many authentic details of earlier Greek history, persisted down until about the 8th century [B.C.], at which time these traditions were finally reduced to writing.’ (William G. Dever, *What Did the Biblical Writers Know and When Did They Know It?* (Grand Rapids MI 2001), pp. 278-9)

Since the myths of Homer and the Trojan wars were apparently true because the cities of Troy and Mycenae were shown to be real, as Schliemann proved, and since these events of the 13th-12th centuries B.C. had evidently been told and retold for centuries down to the 8th century when Homer put them down on parchment, so too the same would also apply to the early history of the Bible. Archaeology could be applied to dig up the cities of the Bible to prove the descriptions of these, as presented in the biblical narratives, were also true, which implied these stories had been told and retold for centuries down to the 4th-3rd centuries B.C. when they were put down on parchment by the four sets of biblical editors. Yamauchi further explains and expands on this concept:

“Just over a century ago Heinrich Schliemann inaugurated Aegean archaeology … by his excavations at Troy in 1870, followed by his work at Mycenae in 1876. Before this most scholars began Greek history with the First Olympiad in 776 B.C. Homer’s story of a war with Troy in the Heroic Age was simply a tale without historical foundation. According to Carl W. Blegen:

“‘Professional Homeric scholarship in the nineteenth century was to a great extent marked by a deep skepticism; the poems were ripped to shreds, the subject matter was regarded as fiction or fancy inspired by minor episodes and raids, transferred by the poet or poets from various other places to Troy, and the possibility that there was any substratum of historical truth was brushed aside’… (Carl W. Blegen, “The Mycenaean Age,” *Lectures in Memory of Louise Taft Semple*, B.W. Braden *et al.*, eds. (Princeton NJ 1967), pp. 16-7).

“In the light of subsequent discoveries and developments, including the decipherment of Linear B [writing], scholars have come to recognize that Homer, who lived in the 8th century [B.C.], had preserved some memories of the Mycenaean epoch (14th-13th centuries [B.C.]). What is a matter of dispute is

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whether the *Iliad* has preserved a minimal number of garbled recollections and primarily reflects conditions of Homer’s Geometric [pottery] Age or whether it has conserved accurate and substantial traditions [of 500 or more years earlier].“19

This evidence was presented by classicist Emily T. Vermeule and consists of words found in Homer of the 8th century with Linear B script of the 14th-13th centuries B.C. and of objects excavated at Troy and Mycenae. What Vermeule reported in her *Greece and the Bronze Age* was:

“For example … linen is attested in all three sources, … the pear by both Homer and excavations, … the cypress [tree] by both Homer and Linear B … the coriander by both Linear B and excavations. … Given the fragmentary nature of inscripational and material evidences, there will always remain gaps in our evidences so that there will always be room for skepticism. …

“Despite a large residue of unconfirmed materials, the striking confirmation of some elements does raise the possibility that Late Bronze [Age] information could be accurately transmitted over the divide of centuries to later eras. This was the conviction of Vermeule: ‘We say in justification that large parts of the poems incorporate Mycenaean traditions, that the five hundred years separating the fall of Troy [level] VIIA from the Homeric version of its fall have wrought only minor innovations, a few misunderstandings of the past and adaptations to more modern experience’.”20

I ask the reader to note the depths of the interpretation involved. It is assumed, without proof, that ancient Greek traveling poets recited these Homeric sagas more or less accurately over the centuries. But, as we have shown in volume II, page 437, the oral Homeric tradition lasting 500 years exists “only in the mind’s eye of scholarly hypothesis and imagination.”21

Furthermore, the historians jump from the connections of these words and buried materials to the possible proof that there was a Trojan War. These are massive interpretations based on extraordinarily modest evidence. Nevertheless, this was likewise to become the approach of archaeologists as they dug into the ground in Palestine. Like the archaeologists of Greece, who interpreted what they unearthed at Mycenae and Troy with the shovel in one hand and the *Iliad* and the *Odyssey* in the other, the archaeologists of Palestine were to interpret what they unearthed there with the shovel in one hand and the *Bible* in the other.

Richard E. Averbeck explains the obstacles involved in obtaining historical evidence from such archaeological procedures:

“One scholar’s mythology may be another one’s history or theology, and both could be mixed up and confused with historiography. It seems to me that the discussion too often ends up going around in circles without getting to the heart of the

19 *Ibid.*, p. 70
problem of the relationship between biblical and ancient Near Eastern history and myth. We too easily end up in a tangle of unclear or unhelpful definitions, confused or misguided perspectives, and/or inadequate or even misleading methodologies.

“… [One should] make every effort to avoid such confusion … In order to do so, … it will be very important to clearly distinguish between the [archaeologically] verifiable data from the Bible and from the ancient Near East and the various scholarly theories about those data. In this area of study scholars sometimes confuse their own or someone else’s theory with the actually verifiable data that are available, and end up treating a theory as if it were confirmed data and/or ignoring data that do not fit the specific theory. Precisely because this is such a difficult topic to manage, it requires intellectual honesty and openness to the available data, whether biblical or extrabiblical, but we must be sure to distinguish between data and theories about the data.”

I further ask the reader to note that all the evidence that will be used and was to be used to evaluate the historical accuracy of the Bible was interpretable documents and interpretable archaeology. The historians who have turned to these forms of evidence have failed to subject any of this data to scientific fact or technological analysis. This, we maintain, is the fundamental problem.

THE FAILURE OF BIBLICAL ARCHAEOLOGY

Roland deVaux has written a telling critique of how archaeological evidence has been misapplied to support both the historical reality of the Iliad as well as the Bible. We cite his work in extenso because, as we will see, his warnings regarding archaeological evidence have gone largely unheeded by both the archaeologists investigating Troy as well as Israel. His paper is provocatively titled: “On Right and Wrong Uses of Archaeology.”

“My … example, that of the Trojan War, is … relevant to our purposes. Here is an event at the end of the thirteenth century BC, the historic memory of which was transmitted by oral tradition through the “Dark Age,” taken on by epic singers as their theme, and received its final expression in the two cycles of the Iliad and the Odyssey, probably around the eighth century BC. The two poems were then written down and received their official, definitive form under Pisistratus in the sixth century. The presumed city of Troy, Hissarlik, has been excavated many times with the avowed intention of recovering the city of Homer. The earliest excavator, Schliemann, had first identified it with the most ancient of the nine cities superimposed upon Hissarlik, Troy I, then with Troy II. His successor, Dörpfeld, preferred Troy VI, where Mycenaean pottery is abundant, and his opinion

is still defended by some. But Troy VI was destroyed by an earthquake, not by an armed attack, and the Mycenaean pottery is still extant in Troy VIIa. It is this level which the last excavator, Blegen, identifies with the city of Homer, and his conclusion has been accepted almost unanimously. Blegen thinks that ‘the tradition of the expedition against Troy must have a basis of historical fact’ and this historical fact appears to him to have been proved by archaeology. ‘It can no longer be doubted … that there really was an actual historical Trojan War in which a coalition of Achaeans, or Mycenaeans, under a king whose overlordship was recognised, fought against the people of Troy and their allies.’

“This has prompted M.I. Finley to remark, ‘Blegen and his colleagues may have settled, insofar as such matters can ever be determined with finality by archaeology, that Troy VIIa was destroyed by human violence. However, they have found nothing, not a scrap, which points to an Achaean coalition or to a “king whose overlordship was recognised” or to Trojan allies; nothing which hints at who destroyed Troy.’ He constructs the hypothesis that Troy VIIa was destroyed by, or in connection with, the invasions of marauders from the North called the ‘Sea Peoples’ and he concludes: ‘New Hittite or North Syrian texts may yet produce direct evidence. Until then, I believe the narrative we have of the Trojan War had best be removed in toto from the realm of history and returned to the realm of myth and poetry.’ In response to this, J.L. Caskey recognizes: ‘Material evidence from the site of Troy has indeed not proven that the place was captured by Mycenaean Greeks … We are left without a compelling reason even to go on calling it Troy … The archaeological evidence, like the literary and historical, is incomplete and inconclusive.’

“This does not, however, justify a rejection of tradition. For his part G.S. Kirk concedes ‘that epic tradition can distort historical events.’ But to what extent? The Homeric tradition ‘is not just a vague legendary one; it preserves a great deal of accurate information, not only about Bronze Age social institutions but also about Bronze Age armour, buildings, and people.’ In its transmission it has assumed almost as many traits associated with the beginning of the Iron Age, it is true, ‘but can we believe that the interruption of the tradition, whether poetical or non-poetical, caused by the upheavals at the end of the Bronze Age, can have been so severe as to destroy not merely the details but the very outlines and whole substance of events belonging to the last heroic period of the Achaean civilisation? The magnification of a heroic past is common enough; how common is the virtual creation of a great heroic enterprise?’ D.L. Page admits that ‘the evidence of Homer, that Greeks from the mainland sacked Troy (this I call the “basic narrative”), cannot be proved to the exclusion of other possibilities,’ but he accepts the fact as historical because it accords with the results of archaeology and with the evidence of the Hittite texts. ‘The Homeric account has been confirmed since 1879 to an extent unimaginable before that time. It is very likely the true account;
at least it is the only one which can claim the support of various and abundant evidence in both literary and archaeological records.’

“I have quoted at length because biblical scholars find here their own problems dealt with by means of the same arguments and resolved which are not certain. If Troy VIIa was destroyed after the great Mycenaean centers of the continent, it is evident that the historicity of the Trojan Wars, i.e., Page’s ‘basic narrative,’ cannot be maintained.

“… Archaeology does not prove that Troy VIIa was destroyed by the Achaeans. Our knowledge of the history of occidental Asia Minor in the thirteenth century is very limited, but we do know that it was an epoch of great troubles, the forerunner of the fall of the Hittite empire. Troy could have fallen under the assaults of the Hittites, or of other local enemies, or of invaders who crossed the Hellespont.

“… As for the relation of the Achaeans with the Mycenaeans and the presence of Mycenaeans in Asia Minor, archaeology reduces these to a minimum. In a recent publication, J. Mellaart pointed out that Mycenaean influence in Anatolia was ‘ineffective, peripheral, and exotic’ and he concludes his study thus: ‘I need hardly to point out that I find the traditional account of the Trojan War archaeologically and historically inacceptable.’

DeVaux’s discussion of biblical archaeology is no less telling:

“It is not my intention to take a position for or against the historicity of the Trojan War. It has been my desire only to show how the same problems have presented themselves in areas other than ours. If I have seemed to dwell upon this … example, it is because it furnishes a good parallel to a biblical question which, more than any other, puts archaeology in conflict with the texts: it is the question of the ‘conquest’ of Canaan. The stages which separate these events of the thirteenth century from the final composition of the book of Joshua are similar to those which separate the Trojan War from the ‘canonical’ recension of the Homeric poems of the sixth century BC. The principal cities mentioned in the narratives of the conquest, Jericho, Ai, Bethel, Gibeon, Hazor, Lachish, are identified in the field with at least as much certainty as that of Troy at Hissarlik. These sites have been excavated. Archaeology permits us to date the end of the Late Bronze Age and the beginning of the Iron Age in Palestine in a manner considerably more precise than the last phases of the Mycenaean Age in Greece and the Aegean. The history of Palestine in that epoch is better known to us than that of the Troad. These advantages should permit a firmer judgment and an easier accord concerning the ‘confirmation’ which archaeology brings to the narratives of the conquest. But, on the contrary, the same kinds of problems present themselves, and the same kinds of arguments are exchanged between opposing camps.

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“Most serious is the refusal to accept the historicity of the narratives with that expressed by the archaeologists themselves who have excavated the sites. In regard to Jericho the most recent judgment expressed by Miss K. Kenyon is: ‘It is impossible to associate the destruction of Jericho with such a date (the Exodus in the thirteenth century BC). The town may have been destroyed by one of the other Hebrew groups, the history of whose infiltration is, as generally recognized, complex. Alternatively, the placing at Jericho of a dramatic siege and capture may be an aetiological explanation of a ruined city. Archaeology cannot provide the answer.’ Concerning Ai, Judith Marquet-Krause concluded: ‘Chapters seven and eight of Joshua, which could be considered historical, are part of a legend.’ The most recent excavator of Ai, J. Callaway, thinks that the long biblical narrative is based only on the capture by the Israelites of a small village established by other immigrants on the ruins of the city of the Ancient Bronze Age. This leads to his more general conclusion that ‘we can no longer take for granted that the conquest of Canaan by invading Israelites accounts for the Late Bronze destructions of Bethel, Lachish, Tell Beit Mirsim, or Hazor.’ In so far as Gibeon is concerned, J.B. Pritchard declares, ‘There can be no doubt, on the basis of the best evidence available, that there was no city of any importance at the time of Joshua.’ He continues, ‘The apparent anomalies found in the archaeological results from three sites which figure prominently in the narratives in the first part of Joshua (Jericho, Ai, Gibeon) suggest that we have reached an impasse on the question of supporting the traditional view of the conquest with archaeological undergirding.’

‘The situation at Hazor looks more favorable to the director of the excavation, Y. Yadin: ‘The excavations have shown in a decisive manner that the great Canaanite city was destroyed by fire, and was rebuilt, in the second part of the thirteenth century B.C. … This destruction must be attributed to the one described so minutely in the book of Joshua.’ However, his assistant in the excavation, Y. Aharoni, discovered a number of settlements of the beginning of the Iron Age in Galilee and concludes: ‘This picture does not fit in with the theory that the Israelites penetrated Galilee in one big military campaign in which they defeated the Canaanites led by the king of Hazor. On the contrary, it fits with the views of those scholars who think that in Galilee, too, the penetration into unsettled regions preceded the decisive military encounter.’

‘I know, of course, that in each of these cases one could propose a solution which would put archaeology in accord with the biblical narratives: the remains of the Jericho captured by the Israelites may have been obliterated by erosion; tradition may have transferred to the neighboring city of Ai the historic memory of the capture of Bethel; several tombs and some sherds indicate that there was occupation of Gibeon during the Late Bronze Age which the too limited excavations did not bring to light; and the Israelite settlements of Galilee came after the fall of Hazor. My intention is not to discuss the validity of these explanations, but only to point out the single fact that when one is obliged to resort
to them it clearly indicates that the ‘confirmation’ brought by archaeology to the biblical narrative is rarely without ambiguity.

“In certain cases the value of the archaeological evidence and the significance of the text which it is supposed to clarify have both been exaggerated. Bethel was destroyed in the course of the thirteenth century, but this does not authorize J.L. Kelso to write, ‘The last Bronze Age city was destroyed by Joshua’s troops;’ because a) the date of the destruction has not been securely fixed; b) the capture of Bethel is not mentioned in the book of Joshua; c) its conquest by the house of Joseph was, according to the book of Judges, the result of treason and not of military attack; nor does the text say that the city was then destroyed. Lachish was destroyed at the end of the Late Bronze Age, but archaeology cannot be used to confirm either the date or the fact of an Israelite conquest. An hieratic inscription on a bowl dates from the fourth year of a Pharaoh; while it is possible that the Pharaoh was Merneptah this date provides only a terminus post quem, and one must also take into account the scarab of Ramses III found in the stratum of the destruction. Miss O. Tufnell, who participated in the excavation and published its results, accepts a later date, ‘some time during the first decades of the twelfth century B.C.’ If other authors prefer a date somewhere around 1220 BC, it is because it is in accordance with a certain chronology of the Israelite conquest. But the book of Joshua contains no account of the capture of Lachish; the city is mentioned only in the summaries of the Conquest which are secondary literary compositions. The agents of the destruction of Lachish may have been the Israelites, or the Egyptians, or the Sea Peoples, or other Canaanites.

“Similar choices are possible for other sites. This period is one of great turmoil in all the Near East: the collapse of the Hittite empire, the end of Egyptian domination in Asia, invasions from the north; the settlement of the Israelites is only one element in this whole movement. All the cities of the Late Bronze Age were destroyed, some several times, during this period; the phenomenon extends well beyond the limits of Palestine, and the Israelites are not the only ones responsible for it. In Palestine itself, the Bible prevents our attributing to the Israelites the destruction of the coastal cities or of cities such as Beth-Shan, Megiddo, Taanach and others, because it explicitly states that they were not conquered by Joshua.

“The much poorer settlements which replace them at the beginning of the Iron Age provide no better evidence of the arrival of the Israelites, simply because archaeology is not able to establish the specifically Israelite character of any of the changes which then appear. One can only subscribe to the recent declaration of H.J. Franken: ‘Archaeologists would be totally unaware of any important ethnic change at the end of the Late Bronze Age were it not for the biblical tradition.’

“But the biblical tradition exists. It may have overstressed the role of Joshua and the extent of the first conquests, but it cannot have completely invented the story that groups of Israelites entered into Palestine at that time and established
themselves at least partly by violence. One will always have to reconstruct biblical history by starting with the texts, and the texts must be interpreted by the methods of literary criticism, tradition criticism and historical criticism. *Archaeology does not confirm the text, which is what it is, it can only confirm the interpretation we give it.* … If the results of archaeology seem to be opposed to the conclusions of text criticism, the reason may perhaps be that not enough archaeological facts are known or that they have not been firmly established; the reason also may be that the text has been wrongly interpreted. Accord must finally be achieved between these two means of knowing historical reality, but it can not and must not be attained by a tendentious use of archaeological facts. *If biblical studies have suffered from an excess of textual criticism, the remarkable and beneficial growth and progress of archaeology must not be permitted to lead to an opposite excess.*”

A major part of deVaux’s paper deals with the objectives of biblical archaeology. It becomes clear that the researchers, although thinking of themselves as neutral and objective, were in fact neither. They did come to Palestine with the shovel in one hand and the Bible in the other:

“I turn now to another abuse which is more difficult to detect and evaluate. It is evident that the extraordinary development in archaeological research in Palestine during the past one hundred years has not been motivated, as in other areas of the Near East, by the desire to resurrect some great civilization, nor by the hope of discovering many written documents or objects of notable artistic value. From this standpoint excavations in Palestine have always been very unproductive compared to those in Egypt, Mesopotamia, and, more recently, Syria and Asia Minor. Palestine has been explored and excavated because it is the ‘Land of the Bible.’

“A little more than a century ago, in 1865, the *Palestine Exploration Fund* was established in London ‘for the purpose of investigating the Archaeology, Geography, Geology, and Natural History of Palestine,’ and its first president, who was the Archbishop of York, laid down as principles the following: 1. That whatever was undertaken should be carried out on scientific principles; 2. That the Society should, as a body, abstain from controversy; 3. That it should not be started, nor should it be conducted, as a religious society.’ Nevertheless, the *Prospectus* announcing this new foundation to the public began with these words: ‘No country should be of so much interest to us as that in which the documents of our Faith were written, and the momentous events they describe enacted,’ and on the cover of the *Quarterly Statement* which the *Fund* began to publish in 1869 it was stated: ‘A Society for the accurate and systematic investigation of the archaeology, topography, geology and physical geography, natural history, manners and customs of the Holy Land, for biblical illustration.’

“With the English society as a model, the *Palestine Exploration Society* was founded in New York in 1870, and its program added the ‘defense’ of the Bible to

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24 *Ibid.*, pp. 75-78 (emphasis added)
its ‘illustration.’ The appeal to the public said: ‘The work proposed by the *Palestine Exploration Society* appeals to the religious sentiment alike of the Christian and the Jew … Its supreme importance is for illustration and defense of the Bible. Modern skepticism assails the Bible at the point of reality, the question of fact. Hence whatever goes to verify the Bible history as real, in time, place and circumstances, is a refutation of unbelief … The Committee feels that they have in trust a sacred service for science and for religion.’ The same year 1870 saw the birth in London of a new association, the *Society of Biblical Archaeology*, ‘to investigate and systematize the antiquities of the ancient and mighty empires and primeval peoples whose records are centered around the venerable pages of the Bible’; it set as its purpose ‘the investigation of the Archaeology, Chronology, and History, of Ancient and Modern Assyria, Arabia, Egypt, Palestine, and other Biblical Lands’; its first president, Samuel Birch, declared: ‘Its scope is Archaeology, not Theology; but to Theology it will prove an important aid.’ The American society was short-lived … The *Society of Biblical Archaeology* did not survive the first world war. The *Palestine Exploration Fund*, however, is very much alive; it has remained faithful to its principle of scientific objectivity, though its new organ, the *Palestine Exploration Quarterly*, still bears the words ‘for biblical illustration,’ as the definition of its activities. Its undertakings, with those of younger institutions, have effectively contributed much ‘to illustrate’ the Bible.

“However, the biblical interest which has elicited and which continues to sustain explorations and excavations in Palestine may easily lead to an apologetic use of archaeology to ‘prove’ the Bible. We have seen this intention expressed in the founding of the short-lived *Palestine Exploration Society*. More recently, this tendency has inspired expeditions to Mount Ararat to recover the remains of Noah’s Ark, the interpretation of certain barren strata in Mesopotamian excavations as providing evidence of the Flood, an underwater exploration of the Dead Sea in search of Sodom and Gomorrah, and excavations in the neighborhood of Mount Nebo to locate the tomb of Moses. This apologetic preoccupation manifests itself in such books written for the wider public as Charles Marston’s *The Bible Is True: The Lessons of the 1925-1934 Excavations in Bible Lands Summarized and Explained*, in 1934, and the ‘best seller’ of W. Keller, *The Bible as History: Archaeology Confirms the Book of Books*, in 1956. Such abuses are easy to recognize and denounce. But some scholars of great competence and complete integrity, although condemning such excesses, look to archaeology to ‘confirm’ the Bible.”

As V.A. Harvey explicitly puts it, the historians and archaeologists, no matter how they deny it, make “judgements and inferences [historically and archaeologically, which] take place… against a background of beliefs. We bring to our perceptions and

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interpretations a world of knowledge, categories and judgements. Our inferences are but the visible part of the iceberg lying deep below the surface.”

Stephen Shennan goes even further in his condemnation of archaeology as a sound discipline:

“Collections of archaeological material do not speak for themselves; it is necessary for archaeologists to specify aspects of the material which interest them, and these will be determined by their aims (or, very often, by what has become traditional within the discipline). The process of going from aims to relevant aspects of one’s material is by no means straightforward. Some archaeologists would say that it has rarely been done successfully and that consequently many if not most archaeological (re)constructions of the past are little more than fictions.”

Ian Hodder, after outlining this interpretative aspect of archaeology, denies it is scientific:

“However much they would like to think otherwise, archaeologists rarely work as positivist natural scientists …

“Any discussion of ‘reading’ the past or of internal meanings has hints of empathy and a lack of science, so that the statement ‘it makes sense to me’ [instead of ‘it is proven’] appears to become the final arbiter of any [archaeological] debate. Archaeology thus becomes prey to special interests.”

Hodder further argues that even though “archaeologists may read material culture, we do not read it as if it were text.” The newest archaeology is still the old archaeology disguised with new terminology, but its basic underlying assumptions are not empirical. As Jonathan M. Hall explicitly states: “Most critically aware archaeologists have now come to recognize that interpretation of the material record is every bit as subjective as the historical interpretation of sources.”

Alberto Soggin concludes:

“Now we must accept one fact (and I want stress this, to avoid any misunderstanding and any a priori assertions to the contrary). In the historical period, traditions, whether oral or written, can still exist which go back to particular phases in this process and therefore allow us to reconstruct part of the development, even if only in outline and in particular areas. However, when we look at those traditions that we do possess, the facts that are there tend to be very

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27 Stephen Shennan, Quantifying Archaeology (Chippenham/Wilts UK 1997), p. 5
28 Ian Hodder, Reading the Past: Current Approaches to Interpretation in Archaeology, 2nd ed. (Cambridge UK/NY et al., 1991), p. 187
29 Ibid.
At the beginning of his career, the great American archaeologist W.F. Albright gave a warning: ‘The long memory possessed by semi-civilized peoples for historical fact is a pious fiction of over-zealous apologists…’ [this] view … however was soon to be replaced by other, much more optimistic ones.”

The same cautions presented by deVaux respecting archaeology were also to be replaced by much more optimistic ones, and it was upon these that the archaeologists between the years 1900 and 1970 were to construct a chronology and history of Palestine.

**BIBLICAL ARCHAEOLOGY ca. 1900-1970
THE RISE OF THE ALBRIGHT SCHOOL**

The major figure who carried out research in archaeology in Palestine and whose influence affected the thinking and direction of biblical studies in the United States and elsewhere was William Foxwell Albright. Throughout a major part of the 20th century his ideas dominated the way in which he and others would analyze and interpret biblical history. The dangers that deVaux pointed out were to be largely ignored by Albright and his followers and thus they were able to convince themselves and generations of scholars that the history and chronology of the Bible in most of that book was accurate and that the history of the Hebrew people was in the main accurate, too. It thus behooves us to examine Albright and his school to understand his influence. His ideas rose to great prominence and then collapsed when a new, younger group of scholars came to see that what he was suggesting was not science but interpretation.

Megan Bishop Moore has dealt extensively and in depth with Albright’s school and hence will be extensively cited. The reason we are going into all this is that we wish to show how a non-scientific, non-technological historical paradigm leads even the most well-meaning scholars to create history rather than historical reality. At the time Albright was working and writing, he believed that he was following the true scientific/empirical tradition of western scholarship and never for a moment considered that the tools he employed were flawed. In that sense, it is suggested that all modern historians imbued with the established chronology have followed in the same footsteps and created history rather than historical reality. Moore writes:

“Albright’s presuppositions about history and its goals stood simultaneously inside the scientific tradition and outside secular, particularist/historicist paradigms of history research and writing. He conceived of researching and writing history as a science, and maintained that historians should employ the empirical method objectively. … Furthermore, he argued that historians should

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elucidate the past by formulating laws that, like those of science, were universal. In other words, both empiricism and the substantive philosophy of history were, in Albright’s opinion, integral parts of the discipline.

“Albright’s most comprehensive statement of his philosophy appeared in his History, Archaeology, and Christian Humanism, published in 1964. Here, Albright describes himself as a positivist and defines positivism as ‘the expression of the modern rational-scientific approach to physical and historical reality.’ … The title to Albright’s only comprehensive historical work, From the Stone Age to Christianity, reveals his conception of the scope of history and his desire to connect the development of religious ideas to the course of past events. Indeed, Albright’s historical pursuits were undertaken as a means to understanding the Bible and especially the eternal truths that he believed were revealed in the experiences of ancient Israel. Consequently, Albright’s pupil David Noel Freedman has asserted that Albright was not primarily a historian … for Albright history could not be separated from other modes of human thought, especially religion. He believed that religion was the arbiter of all meaning, the way in which humanity organized and expressed its most profound thoughts. Albright thus held that historical research should shed light on religious experience and truths and should further strive to demonstrate the overall significance of these truths for humanity through the ages.”

In order to defend the Bible as history, Albright turned to what he conceived as the scientific method to uphold it. This is in many ways similar to creationist scientists today who are employing science to uphold the validity of the Bible and its description of the past history of the world. In both cases, the intention is to apply scientific evidence to support biblical history and physical reality. Furthermore:

“… Albright and [his pupil John] Bright accused some of their colleagues of unacceptable bias in history writing. Bright called [Martin] Noth’s ‘mistrust of the early traditions of Israel … little short of nihilism’ and Noth’s attitude toward archaeological evidence nihilistic, as well. In terms of the biases identified by McCullagh then, Albright and Bright suggested that Noth omitted significant facts about ancient Israel. Present-day observers of Albright, however, recognize that his own biases are evident. It would be difficult to argue that Albright was a ‘dispassionate foreign observer’ of ancient Israel, since he favored protestant Christian interpretations of the Bible … In retrospect, Albright too had a strong desire for a certain portrait of ancient Israel to be true.”

The problem underlying Albright and his followers was determining what was the relevant historical evidence on which he would employ the scientific approach to determine the validity of biblical history. Moore goes on to point out:

32 Megan Bishop Moore, Philosophy and Practice in Writing a History of Ancient Israel (NY/London 2006), pp. 47-48
33 Ibid., p. 49
“It has been established that Albright was an empiricist and that empiricism is grounded on the postulate that facts are external to the researcher and can be apprehended by scientific methods. Albright does not appear to have recognized the historian’s role in deciding what is a relevant piece of evidence and what is not … For Albrightians, facts could be found both in textual sources and in artifacts.”34

That is, documents and archaeological artifacts were the only facts to explicate historical reality. Albright was using the very same sources as did other historians while maintaining his method was empirical where facts are tested by stringent experimental tests. But, of course, documents and artifacts are not so tested; they are interpreted!

“In summary, the scholarship of William Foxwell Albright combined assumptions and goals that now seem dated and contrary to that to which scientific writing should aspire … the Albrightian approach to history and archaeology is now associated with a desire to describe Israel’s past in such a way that much of the Bible’s story is left essentially intact.”35

There was another school started in Europe by Albrecht Alt and enlarged by Martin Noth that held to a different approach to historical analysis but essentially reached the same conclusions as the Albrightian school. Moore sums these up as follows:

“Albright, Alt, Bright and Noth worked on the history of ancient Israel at the same time but saw themselves as representatives of two different currents in the discipline … In retrospect, however, some scholars find the assumptions and practices of the Albrightians and Altians in fact to have been quite similar with respect to their use of the Bible. For instance, [Thomas L.] Thompson has pointed out that Albright and Alt agreed that ‘the biblical tradition was generally historical in origin [and not legendary] and [thus] that the historical events [facts?] which lay behind any tradition could theoretically be discovered in the earliest forms of that tradition. In other words, both Albrightians and Altians believed that biblical texts reflected the reality they described in some way, and that biblical sources were more reliable the closer they were to the events they described.’ Thompson also argues that ‘Albright and Alt shared a common goal of constructing a history of early Israel on the basis of a critical appraisal and synthesis of biblical, archaeological and ancient Near Eastern studies.’ These shared assumptions about evidence led to the creation of what he calls ‘the Albright-Alt consensus,’ where:

“‘The long-standing debate between the schools of Albright and Alt, between the alternative interpretations of ‘conquest’ or ‘settlement’ [of Palestine by the Israelites] as an explanation of Israel’s origins, has not been as important as the common gains and the expanding basis of agreement that have been achieved by the two sides of the issues. Alt and Albright, and Noth and Bright, did not, after all, stand so very far apart.’

34 Ibid., p. 53
35 Ibid., p. 57
“Alt and Albright also helped to embed a number of other assumptions into the discipline. Both scholars firmly believed that objectivity in history was possible and that historical writing could represent the past, and drew on empiricism and scientific methodology to guide them. Both schools established the religious community of Israel as the subject of the history of ancient Israel, and thereby cemented religion’s role in explaining events in Israel’s past. Put in theoretical language, both Altians and Albrightians were more idealists than materialists, and both saw religious ideals as sources of rational explanations for past events.”

With this vision of Palestine, it was a foregone conclusion that the archaeological work of Albright, Bright etc. was going to prove that the Bible was basically a valid historical document. Thomas W. Davis, in his comments on G.E. Wright and Nelson Glueck, explains the conclusions their work reached:

“Wright, Albright’s student, explicitly combined theology and archaeology in his approach to biblical archaeology. William G. Dever has labeled Wright’s career ‘schizophrenic’ in the way Wright appeared to oscillate between the two fields. In reality, Wright’s theology and his archaeology interacted throughout his professional life, and the results in one field often had an effect in the other. Unlike Albright, Wright was an ordained clergyman who remained a churchman his entire life. Wright followed Albright’s lead and studied Palestinian ceramics.

“In a 1947 article, Wright first defined his view of biblical archaeology. He saw it as ‘a special “armchair” variety of general archaeology which studies the discoveries of excavators and gleans from them every fact which throws a direct, indirect or even diffused light upon the Bible.’ At the same time when Wright was formulating his views on biblical archaeology, he was making a reputation for himself in theological circles. Wright was recognized as a spokesman for the movement known as ‘biblical theology.’ The most important aspect of his theology from the perspective of his archaeology is his focus on revelation in history. This theme received its classical treatment in God Who Acts. … In the preface to the book, Wright says, ‘Biblical Theology is the confessional recital of the redemptive acts of God in a particular history, because history is the chief medium of revelation.’ This then is the justification for Wright’s archaeology—to better understand the ‘Mighty Acts’ of God.

“Wright approached archaeology as a historian, albeit a biblically-oriented one …

“Nelson Glueck was another major figure in biblical archaeology, and his views on the reliability of the Bible helped give the Albright/Wright school an aura of fundamentalism. Glueck was an ordained rabbi, and a biblical orientation is never far below the surface of his archaeology. He held a very positive view of the historicity of Scripture, and from that standpoint he issued a famous (or

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36 Ibid., pp. 67-68
infamous) statement: ‘It may be stated categorically that no archaeological
discovery has ever controverted a Biblical reference’.”37
Albright, no less, raised a very similar concept of the proven validity of the Bible
because of archaeological evidence. Here Joseph Free and Howard F. Vos cite Albright:

“‘Archeological and inscriptive data have established the historicity of
innumerable passages and statements of the Old Testament; the number of such
cases is many times greater than those where the reverse has been proved or has
been made possible.’ [To this Free added:]

“I believe that further archeological research will make it necessary to modify
even the last part of the statement. I do not know of any cases where the Bible has
been proved wrong’.”38
Joseph Free further stated: “Archaeology has confirmed countless [biblical]
passages that have been rejected by critics as unhistorical or contrary to known
facts.”39 Millar Burrows stated: ‘… archaeological work has unquestionably
strengthened confidence in the reliability of the scriptural record. More than one
archaeologist has found his respect for the Bible increased by the experience of
excavation in Palestine.”40 Bryant C. Woods claims:

“In every instance where the findings of archaeology pertain to the Biblical
record, the archaeological evidence confirms, sometimes in detailed fashion, the
historical accuracy of Scripture. In those instances where the archaeological
findings seem to be at variance with the Bible, the discrepancy lies with the
archaeological evidence, i.e., improper interpretation, lack of evidence, etc. – not
with the Bible.”41
J. David Pleins sums up Albright’s approach to archaeology:

“Albright’s Archaeology of Palestine begins deep in the prehistoric period and
moves in slices right down to the times of the Christian scriptures. References to
material artifacts, line drawings, site plans, and ancient writings outside of the
Bible litter the text. The expectation is that the Bible, like a rabbit in its cage, will
nestle down snugly in this richly articulate landscape. Albright talks confidently,
for example, of tying together destruction layers in Palestine around 1200 B.C.E.
with the ‘invasion of the Israelites’ found in the Books of Joshua and Judges. If
building activity can be connected with King David, the link is made despite the
lack of any inscriptions that would justify such a claim [of connection]. The same
goes for Solomon, of whose dominion Albright suggests: ‘Archaeology, after a

37 Thomas W. Davis, “Theory and Method in Biblical Archaeology, in Hoffmeier and Millard,
eds., The Future of Biblical Archaeology, op.cit., pp. 22-23
38 Joseph P. Free and Howard F. Vos, Archaeology and Bible History (revised and expanded ed.,
Grand Rapids MI 1992), p. 114
39 Joseph Free in Daniel Baer, The Unquenchable Fire (Xulon Press 2007), p. 44
40 Ibid.
41 Ibid., p. 45
long silence, has finally corroborated biblical tradition in no uncertain way.’ Likewise, the biblical record of the succeeding centuries of Israelite and Judean monarchic rule finds archaeological connections in the course of Albright’s extended discussion of those periods.

“The more distant and misty past of the patriarchs in Genesis is similarly illuminated while passing over the figure of Abraham. Albright is confident ‘we may date the generation to which Jacob belonged somewhere in the eighteenth or seventeenth century B.C.’ Linguistic facts, notes of population movements, and Egyptian tomb paintings round out the assessment of the Jacob traditions. Albright is not altogether silent about Abraham. In another publication he turns the camel-rich figure of Genesis into an archaeologically correct ‘donkey caravaneer.’ The plethora of supposed connections between archaeological artifacts and the patriarchal narratives of Genesis led Albright to conclude: ‘So many corroborations of details have been discovered in recent years that most competent scholars have given up the old critical theory according to which the stories of the Patriarchs are mostly retrojections from the time of the Dual Monarchy (9th-8th centuries B.C.).’”

John Bright was so bold as to write: “There can really be little doubt that ancestors of Israel had been slaves in Egypt and had escaped in some marvelous way. Almost no one today would question it.” In finding burnt and destroyed towns in Palestine, G.E. Wright was positive these had been destroyed by the Israelites under Joshua:

“Sometime during the 13th century [B.C.] the city was destroyed by a tremendous conflagration. It was the privilege of the writer to participate in this excavation and … there was absolutely no mistaking the evidence of by far the worst destruction which the city had experienced in all its history. In some places the debris of fallen walls and charred ash-filled earth was almost five feet thick. The Canaanite city destroyed was a fine one with excellent houses, paved or plastered floors and drains. Compared with them the poor straggly houses of the next town [levels] were poverty itself. The break between the two is so complete that there can be no doubt that this was the Israelite destruction.”

Again, Wright as late as 1959/60 wrote:

“One of the remarkable results of archaeological research during the period between the two [World] Wars was the sudden emergence of the Patriarchal Age of Biblical History as one which could be fitted within an actually discernible period in the history of Western Asia.”

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43 Bright, *op.cit.*, p. 120
E.H. Maly in 1968 was so sure the biblical evidence was supported by solid archaeological evidence that he could write:

“With regard to the patriarchal history, the basic ‘facts’ included such items as the movements of the patriarchs, their occupations, their relations with their neighbors, and their marriages and deaths. There is every reason for the modern scientific historian to accept the basic family history, which served as the foundation for the authors’ religious history. The sciences of ancient Near Eastern history and especially of archaeology have shown that the underlying social, political, geographical and religious conditions in Genesis are precisely those of the Second Millennium [B.C.] … Consequently [they were] shepherds who originated, at least approximately, in Upper Mesopotamia, migrated to Canaan, lived out their lives much as described in Genesis, and at least some of whom descended into Egypt.”

Amy Dockser Marcus well sums up the Albrightian approach to archaeology as it relates to the Bible:

“Scholars such as the American William Foxwell Albright … and Nelson Glueck … chose not to implement new methods in their excavations. … Glueck, in fact, was one of the early pioneers in archaeological surveys, a technique that involves collecting pottery sherds on the ground that is still used today. But the fact remains that, whatever their methodology, the Bible, and finding a way to make archaeological discoveries correlate with the Bible, remained at [the core of their work.] ‘Discovery after discovery has established the accuracy of innumerable details and has brought increased recognition of the Bible as a source of history,’ wrote Albright in his 1932 classic work, Archaeology of Palestine and the Bible.”

So influential has this belief become throughout the historical community that the great popularizer of history, Will Durant, wrote the year Velikovsky’s Worlds in Collision was published that

“Each passing year adds to our store of knowledge, and provides us with more and more documents, inscriptions, monuments, and excavations which confirm the Bible’s historical accuracy … Science is now in a position to state categorically that the Bible is factual until proven otherwise.”

At this point of overkill, it must be pointed out that Velikovsky throughout most of his scholarly life imbibed the fruits of the Albright school. Velikovsky’s revisionist work, which maintained that the Bible was a chronologically and historically accurate document that could be used to align the rest of the history of

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47 Amy Dockser Marcus, The View from Nebo (Boston/NY/London 2000), p. 19
the ancient Near East, was in large measure an echo of the Albrightian milieu in which he was immersed. Those critics who suggest that his chronological revisions, many of which this author accepts where they fit into the short chronology of Heinsohn, Rose, and Sweeney, were unrelated to the historical materials of the time in which he wrote could also accuse Albright, Bright, Wright, Glueck, etc., of being out of touch with history. This is not done as openly as the critics of Velikovsky have done. The apparent reason is that these historians never strongly challenged the chronological foundations of history.

**THE FALL OF THE ALBRIGHT SCHOOL**

The downfall of the Albright school began in the 1970s with the work of John Van Seters and Thomas L. Thompson and was later followed by several other historians who, over time, examined and picked apart the entire opus of that historical view, leaving it largely in shambles and destroying the influence that this school of work had on the other historians as well as those who followed the debate. What these critics did at first was show that the foundations of the Pentateuch, which Albright and his followers placed in the second millennium B.C., exhibiting parallels with other nations dated to that time, were spurious. Then, after attacking these books, some of them went further to attack the rest of the Old Testament as being non-historical. Victor P. Hamilton describes Van Seters’ analysis of Albright and his school thus:

“Van Seters makes two major points, the first demolitional and the second reconstructive, and thus the division of his book [Abraham in History and Tradition] into two sections–Abraham (1) in History and (2) in Tradition … In his first section Van Seters makes his case for the fact that it is impossible, both on external and internal grounds, to date the Abraham story to any time during the 2nd millennium B.C. This case flies directly in the face of … Albright, et al.

“Thompson is as militant in denying any second-millennium B.C. context for the Abraham story, sometimes agreeing with Van Seters, sometimes disputing with him. Some have suggested that what Van Seters and Thompson have done is turn the clock back to Wellhausen. … If we understand correctly the difference between Wellhausen/Noth and Van Seters/Thompson, the former taught that the patriarchal traditions are historically unrecoverable, while the latter teach that the patriarchal traditions are unrecoverable because there never were any patriarchs! …

“Van Seters’s main objection [to the Albright school of] Speiser, Gordon, et al. is that hardly any of the social customs in the patriarchal narratives point exclusively to an early second-millennium B.C. background. He offers some criticisms and then presents an alternative. His criticisms are that either (a) too
much [biblical tradition] was read into the Nuzi texts to make them compatible with the Genesis stories, or (b) too much [second millennium tradition] was read into the Genesis stories to make them parallel to their Nuzi counterparts. In other words, the parallels [Albright and his school drew] are contrived, forced, and necessitate either trimming or augmentation in the stories and customs under consideration. Van Seters’s alternative is to suggest that there are much better parallels in the middle and late first-millennium B.C. cuneiform texts (i.e., Neo-Assyrian, Neo-Babylonian literature). This is the foundation stone in his edifice in later arguing for a seventh/sixth century B.C. Abraham tradition.”

In his critique of the Albright school, Van Seters shows:

“While there were a few dissenting voices raised against the mania [by Albright and his school] for finding second millennium parallels to the patriarchal stories and the early dating of these [biblical] traditions, the full-scale attack came from Thomas L. Thompson (The Historicity of the Patriarchal Narratives, 1974) and John Van Seters (Abraham in History and Tradition, 1975), that called into question the whole range of such parallels [used by Albright and his school] for the [validity of] the patriarchal stories of Genesis. In the case of similarities in social customs, sometimes the cuneiform texts were misconstrued [misinterpreted by Albright, etc.] to make them seem parallel with the biblical texts. In other cases, the biblical texts were interpreted in a very forced fashion to create the desired parallel. Thus, in one of Albright’s most celebrated examples, Abraham is represented as a great donkey caravaneer, trading between Mesopotamia and Egypt, when there is scarcely anything in Genesis that would suggest such a picture of the patriarch’s life. In many cases where examples of parallels in customs and place names [used by Albright, etc.] could be demonstrated between second millennium texts and the biblical account, these parallels were just as valid for the first millennium. Furthermore, there are features within the patriarchal stories that only make sense within the context of the first millennium and not the second millennium B.C.E.”

David M. Howard, et al., further report:

“Since the 1970s, … work launched by John Van Seters and Thomas L. Thompson [has] been influential in the research of the patriarchal narratives. … These scholars began to re-examine some of the conclusions made by Albright and [C.H.] Gordon and dismissed a variety of erroneous comparisons [parallels] … In opposition to scholars of the Albright school, like G.E. Wright who generally assumed the factuality of Hebrew narratives, Thompson reverted back to the critical method promoted by Wellhausen [whom Albright dismissed] that the narratives of Genesis are a reflection of the time in which they were written, not about the times they attempt to portray. Thompson also raised great questions

49 Victor P. Hamilton, The Book of Genesis (Grand Rapids MI 1990), pp. 63-64
about the contribution of archaeology: ‘For not only has “archaeology” not proven a single event of the patriarchal traditions to be historical, it has not shown any of the patriarchal traditions to be likely.’

“Van Seters had a more favorable opinion of the use of archaeology but argued that the conclusions of those sympathetic to the [Albright] American school were not consistent with the actual data. He argued that the archaeological evidence does not support a second millennium [B.C.] backdrop for the patriarchal narratives but rather a first millennium context. The great popularity and dissemination of the studies of Van Seters and Thompson have had a profound effect on many Old Testament scholars. To many, the old consensus established by the Albright school now lies in ruins.”

What must be pointed out before proceeding is the position of Van Seters, namely that the evidence from the Bible points directly to a setting in the “first millennium and not the second millennium” B.C. and “only makes sense within” this “context.” This is precisely what the short chronology demands with the history and chronology of the Bible. Since, according to Heinsohn, Sweeney, and Rose, there is practically no history until about 1200-1100 B.C., not only must the great empires of the ancient world be placed in the first millennium B.C., but so should the history of the Hebrew people. The response on the part of the old guard biblical historians to Van Seters and Thompson has been vicious to say the least. Kenneth A. Kitchen, a great proponent of the established chronology and of the chronology of biblical history, whom we will meet repeatedly below, raised a violent denunciation of Van Seters and Thompson. For example:

“Kenneth Kitchen [holds] that both Van Seters and Thompson are reactionaries rather than innovators trying to push the study of the extrabiblical support of the pentateuchal narratives back one hundred years [to the Wellhausen era]. Dever is critical of Thompson’s methodological approach toward the understanding of the history of Israel: ‘… this is irresponsible scholarship—simply ideological rhetoric, with no attempt at documentation or a well-balanced presentation that might enlighten the reader.”

Kitchen went so far as to assert Thompson, Van Seters, as well as Donald Redford, another critic of the biblical historical account, have “set the clock back 100 years … [for] like [Julius] Wellhausen, they concluded that the stories of the Patriarchs are fictitious creations—dating to the Babylonian Exile (6th century B.C.) or later—and are historically worthless.” The criticisms by Thompson have been

51 David M. Howard Jr., Michael A. Grisanti, eds., Giving the Sense: Understanding and Using Old Testament Historical Texts (Grand Rapids MI 2004), pp. 223-224
52 Ibid., p. 225
especially devastating and galling to biblical historians, but the fact remains that Albright and his school were shown to be severely in error, I repeat, because of the chronology upon which biblical history was erected: As P.R.S. Moorey put it, since the criticisms in “Thompson’s The Historicity of the Patriarchal Narratives (1974) … Albright’s reputation has never fully recovered…”\footnote{Peter Roger Stuart Moorey, A Century of Biblical Archaeology (Louisville KY 1991), p. 114} In fact, David Noel Freedman, a close associate and member of the Albright school, ultimately was driven to comment on how badly Albright has fared based on the criticisms of Van Seters and Thompson. William G. Dever, whom we will encounter again and again, not only as a critic of Thompson but as a staunch proponent of the theory that archaeology can, nevertheless, produce evidence that is largely sufficient in itself to support biblical history on its established chronological setting, states:

“I cannot end this critique of Biblical archaeology without noting the recent, widespread reaction against Albright and his school. This was apparent in Europe by the 1950s, and it is now seen in much of the literature in Biblical studies in America, beginning in the 1970s. The most striking instance was seen, I think, only a few months ago, at a symposium actually honoring Albright. A principal speaker was David Noel Freedman, one of the staunchest members and most eloquent defenders of the Albright school, who only a few years ago was promoting the cuneiform texts found recently by Italian excavators at Ebla, near Aleppo in Syria, as proof of the historicity of the Patriarchs. Freedman made the following remarks, which I will quote at some length, since his comments are candid and very revealing:

“The combination of the Bible and archaeology is somewhat artificial; the two have not really matched up very well. The Biblical scholar deals with one kind of material and the archaeologist with another. On rare but important occasions, there is significant contact, and both disciplines gain from the exchange of data and ideas. Often, however, there is no point of contact and nothing significant happens. On the whole, I believe that results of the interchange between archaeology and the Bible have been somewhat disappointing, though perhaps that was to be expected. Palestinian archaeology has had modest success in turning up monumental remains and inscriptional materials, but nothing like the quantity discovered in Mesopotamia and Egypt. Unwritten materials [artifacts] are extensive in Palestine, to be sure, but not always easy to interpret, and the Biblical connections remain elusive while confirmations are few and far between.

“Albright’s great plan and expectation to set the Bible firmly on the foundation of archaeology buttressed by verifiable data seems to have foundered or at least floundered. After all the digging, done and being done and yet to be done, how much has been accomplished? The fierce debates and arguments about the relevance of archaeology to the Bible and vice versa indicate that many issues
remain unresolved. Can anyone say anything with confidence about the patriarchs or the patriarchal age? The fact that skeptical voices now dominate the scene indicates that the Albrightian synthesis has become unglued and we are further from a solution than we ever were. Archaeology has not proved decisive or even greatly helpful in answering the questions most often asked and has failed to prove the historicity of Biblical persons and events, especially in the early periods.”

Although Dever believes Freedman may have gone too far in his assessment of the Bible’s historical/archaeological support, he, nevertheless, has concluded that “… we cannot trace in detail the efforts of Biblical archaeology over the past sixty years to confirm the historicity of personalities and events of these eras. The most recent survey of the archaeological background of the Patriarchal period is one that I was recently asked to do for a basic handbook of Israelite and Judean history. I set out thinking that perhaps it was possible, after all, to defend Albright’s attempt to locate the Patriarchs within a particular archaeological-historical phase in Palestine (i.e., the Middle Bronze I or perhaps II period). After an exhaustive survey of the evidence and of recent scholarly opinion, however, I had to conclude that we are farther than ever from a solution to this problem. On the one hand, the narratives of Genesis are a composite of many layers [interpretations] of oral and written tradition, from many different time periods and social circumstances. It is thus impossible to isolate a kernel of truth and assign that to one specific period on the basis of [an] historical ‘fit.’ On the other hand, while archaeology has been able to document in general the pastoral nomadic lifestyle depicted in Genesis throughout the second millennium B.C. (and other periods), it has not brought to light any direct evidence to substantiate the story that an Abraham lived, that he migrated from Mesopotamia to Canaan, or that there was a Joseph who found his way to Egypt and rose to power there. The point is not that archaeology has disproved the historicity of the Patriarchs, but simply that it has not gotten beyond the literary tradition that we had all along in the Hebrew Bible. The tradition is made up of legends that still may be regarded as containing moral truths, but until now they have been of uncertain historical provenance.

“On Albright’s favorite theme, ‘Moses and monotheism,’ the silence of archaeology is even more profound. Absolutely no trace of Moses, or indeed of an Israelite presence in Egypt, has ever turned up. Of the exodus and the wandering in the wilderness—events so crucial in the Biblical recitation of the ‘mighty acts of God’—we have no evidence whatsoever; nor are we likely to have any, since slaves, serfs, and nomads leave few traces in the archaeological record. Recent Israeli excavations at Kadesh-barnea, the Sinai oasis where the Israelites are said to have encamped for forty years, have revealed an extensive settlement, but not so much as a potsherd earlier than the tenth century B.C. …

55 William G. Dever, Recent Discoveries and Biblical Research (Seattle/London 1990), pp. 25-26
“Finally, the Israelite settlement in Palestine … has received intense scholarly attention in the last two generations. Dozens of late Canaanite and early Israelite settlements have been excavated, and hundreds more have been surveyed in surface exploration. Yet here again the evidence is largely negative. In particular, the ‘conquest model’ of Albright and Wright, derived principally from the book of Joshua, has been largely discredited. That Israel did emerge in Canaan in the early Iron Age is beyond doubt. But archaeology has not shown that the process of settlement followed a series of destructions, miraculous or otherwise.”

In essence, Dever claims that prior to the time of the monachies of David and Solomon there is no historical validity of those earlier periods in the Old Testament. But afterwards, he and his colleagues, except for the minimalists, claim there is a real history of Israel running from around 1000 down to Greek-Hellenistic times. And he repeats this claim in his book, What Did The Biblical Writers Know and When Did They Know It? (pp. 98-9) This, too, the minimalists have challenged. They claim that the earliest dates for compiling the biblical texts are from Persian and Hellenistic times or from the seventh century B.C. at best. On this point, we are informed “Van Seters … took the argument even further by showing that the biblical stories themselves could not be seen as early, but must be dated sometime in the sixth century BCE or later.” Freedman et al., suggest that “Some [scholars] … would like to push the starting point [of Hebrew history] into the Persian and Hellenistic period (Thompson, Philip R. Davies).” Norman Karol Gottwald echoes this:

“A fourth alternative is that many or all of the traditions [of the Bible] were created under scribal/priestly auspices during the reconstruction of Judah [in the post-exilic period] as an enclave within the Persian or Hellenistic empire as part of the foundation myth of this religiocultural community” [and] “Amid the variety of views on this issue, there is the widespread concurrence that [even] prestate [Israelite] traditions were still being created in Persian/Hellenistic times when the concluding touches were put on the biblical narrative traditions.”

Megan Bishop Moore adds that:

“Thompson agrees that the terminus a quo for the Hebrew Bible must be the Persian period, noting that external sources for the history of Judaism, such as the Elephantine texts, also date to this period and, like Davies, imagines that the Bible

56 Ibid., pp. 24-25
57 Thompson, The Mythic Past, op.cit., p. XII
59 Gottwald, op.cit., p. 160
was brought together as an aid to [Judaic] community formation [not for history].
More likely, however, in his opinion, is a Hellenistic date for the Bible."

If this linguistic/philological evidence is valid, how could the biblical writers accurately know, let alone describe, events that happened three, four, or up to seven centuries earlier? And even worse, know details of these narratives accurately? The answer from Thompson, Lemche, Davies, etc. is: this is not possible nor even probable and therefore the entire Bible contains nothing valid as history. Their challenge is defiant and overturns centuries of biblical research.

Frederick H. Cryer presents the problem inherent in rejecting the earlier books of the Bible for containing problems and contradictions but then accepting the later historical books which also contain problems and contradictions:

“Adherents of the hybrid approach [accepting some biblical books while rejecting others] replace the Biblical anno mundi chronology on an ad hoc basis by one in which the ‘patriarchs’ are dated to about 1900-1600 BC, the sojourn in Egypt to c. 1600-1250 BC, the exodus to around 1250 and wilderness wandering and conquest of the land [of Palestine] to around 1200, the period of judges to c. 1200-1000, the period of monarchy from 1000-721 (in the northern kingdom)–587 (in Judah) and rebuilding of the temple to around 420 [B.C.]. These various chronological periods have usually been supplied as need[ed] by both archaeological and extra-Biblical (frequently textual) sources. But it is seldom considered that scholars advocating this approach simply discard the sections of the Old Testament chronology that conflict with modern-scientific knowledge without giving a thought to the fact that the lacunae and inaccuracies of the Israelite and Judaean chronology preserved in the books of kings and elsewhere are equally problematic. They [the problems] happen to be smaller. Logically, if one discards the former [the Pentateuch], then the latter, too, must be discarded. However, rationalization as it has been practiced is a questionable business intuitively rejecting some figures that strike a given scholar as non-‘historical’ while retaining others that strike him or her as plausible. It is worth mentioning in this context that plausibility is not a historical category but a criterion for evaluating the verisimilitude of fictional literature …

“In reality, none of the sources has been utilized on its own terms, but only in terms of some other source. THE TEXTS ARE ‘ILLUSTRATED’ WITH ARCHAEOLOGICAL MATERIALS AND THE [ARCHAEOLOGICAL] MATERIALS DERIVE THEIR INTERPRETATIVE FRAMEWORK FROM THE LITERARY RECORD: A VICIOUS CIRCLE OF IMPRESSIVE DIMENSIONS. And in reality most Old Testament scholars writing in earlier times have regarded the Old Testament as a privileged source…

60 M.B. Moore, op.cit., p. 91
“HOWEVER, TO THE HISTORIAN THERE ARE NO PRIVILEGED SOURCES.”

William G. Dever, who specifically holds to the proposition that one can reject the Pentateuch as non-historical but maintain the history of the Hebrew kings from Saul, David, Solomon though the divided monarchy to the exile and return to Palestine, also, like Kenneth Kitchen, has understood how much is at stake and responded to the minimalists with undisguised expressions of denunciation, vituperation, and academic rage. Alice Hunt reports on his reaction to the work of Thompson, Lemche, Davis, Cryer, and all the others in the minimalist camp:

“William G. Dever’s work [like that of the minimalists] is no less filled with contradictions … than that of Thompson … Lashing out at the so-called minimalists with name-calling of his own, Dever casts labels such as ‘disaffected anti-establishment figure,’ ‘revisionist,’ ‘small but vocal minority,’ ‘the new nihilists,’ ‘outsider,’ presenters of history that is ‘full of errors, misrepresentations and unbalanced judgments,’ ‘outrageous,’ and without ‘even a superficial acquaintance with socio-anthropology or archaeology.’ He can neither abide nor enter into a scholarly discussion with these so-called minimalists.”

The first part of Hunt’s book is a clear indictment of the behavior that has been meted out to those minimalists who challenged the old biblical paradigm.

Keith W. Whitelam tells us that Hershel Shanks, the Editor-in-chief of Biblical Archaeology Review calls the minimalists an “increasingly modish-virulent stain on biblical scholarship” while Rodd terms them “vociferous and extremist” and Rainey decries them as “dangerous ideologues” or “dilettantes … a term meant to depict those who are not serious scholars but merely dabble in scholarship.”

Amy Dockser Marcus informs us that:

“Niels Peter Lemche and Thomas Thompson, two members of the so-called [minimalist] Copenhagen School, are trying to figure out why their work angers so many people. They’ve been called liars, bad scholars, Israel haters, and anti-Semites. Their work has been compared to Holocaust revisionism. These last charges especially disturb them. Lemche’s family was in the Danish resistance [fighting Nazis] during World War II. His son dug at Megiddo in the summer under Israel Finkelstein … The anti-Semitism charge, they argue, is designed to

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end the possibility of any sort of debate about the issues. ‘Once you call someone anti-Semitic, where can any discussion possibly go?’ asks Lemche …

“Thompson, with his manic energy and his claims that he fears for his personal safety, sometimes seems a little obsessed. During the bus tour of Copenhagen, he pulls out a photocopied page from the latest article by William Dever, a University of Arizona archaeologist who has written extensively against the [minimalist] Copenhagen School’s ideas, calling them nihilist and revisionist. At one point, he describes Thompson as a ‘nasty little man with a nasty little life,’ a phrase that still makes Thompson livid.”

Unable to stop these scholars from publishing their work in peer-reviewed journals and university book publications, these biblical scholars, editors, and history writers can barely help venting their spleens. Henige in his chapter “When Might Makes Wrong” discusses the nature of Dever’s attacks:

“A piece by William G. Dever serves to illustrate the ways in which discourse can stand surrogate for reasoned argument (William G. Dever, “Save us from Postmodern Malarkey,” Biblical Archaeology Review, vol. 26, no. 2 (March-April 2007, pp. 28-35 and 68-69). Dever opposes the ‘minimalists,’ who claim that the so-called historical books of the Old Testament are not very historical after all, and he labels them ‘postmodern,’ a surefire way of condemning their modus operandi as ‘malarkey.’ Worse yet, from Dever’s point of view, they often brush off claims that archaeology can fill the vacuum [which is the core of Dever’s response to them]. Unlike standard-issue postmodernists, who believe that no source can tell us enough, and so there is no point in trying, the minimalists do not argue that all history must be a literary construct, only that these biblical texts are literary at heart and date from so late as to jeopardize their value as primary sources. This is no different than the tack taken by critical historians everywhere.

“Dever continues [hopefully wishing] that ‘[m]ost Biblical scholars and virtually all archaeologists have tended to dismiss revisionism as a passing fad, not worthy of being addressed seriously [as if majorities determine truth].’ He is prepared to be more generous: ‘[w]e cannot, however, avoid the basic historiographical issues that the revisionists have raised.’ Dever does not so much address the issues, however, as those who have raised them. To keep readers in line [even before he attempts to answer them], he passes judgment early in his discussion: ‘What weighs in finally is not “truth,” for there is none, but rhetoric, the more extreme the better [which is just what he is presenting].’ To prove his point, Dever quotes or paraphrases several “typical statements,” but fails to cite a source for any of them [which is highly unscholarly]. Just two paragraphs later, he castigates the latest work of one of the more prominent minimalists as ‘hardly … scholarship, since it does not contain a single reference to support any of the countless cavalier assertions that are made [which is exactly what Dever did].’

64 Marcus, op.cit., pp. 118-119
One can only marvel at the lack of self-awareness. This assault on extremism is odd for someone who has gone so far as to [make the extreme] claim that ‘we [archaeologists] will write a history of ancient Israel, and we will also write the only competent histories of ancient Palestine’.”

J. David Pleins also castigates Dever’s approach to biblical archaeology:

“Dever’s latest book only adds fuel to the fire. Despite protesting that his brand of archaeology shores up the Hebrew Bible against the onslaught of the ‘minimalists’ (who appear to wish to throw out the Bible entirely), the fact is that Dever’s ‘Hebrew Bible’ has taken a hard fall on the battlefield of archaeological analysis. While he seeks links between the biblical texts and an endless array of artifacts from the time of the monarchy in Israel, many parts of the Hebrew Bible are ‘missing in action.’ Gone is Genesis’s patriarchal age; gone are the Exodus and Conquest stories, gone is the editorial frame of the Deuteronomistic History; gone are the ‘late’ and ‘non-Israelite’ wisdom texts; gone are Ruth, Esther, Job, and Daniel (‘historical novellae’ with contrived ‘real-life’ settings); gone are the ‘Oriental love-songs’ of Song of Songs; gone are the Priestly books of Leviticus and Numbers (which are ‘preoccupied with notions of ritual purity’); gone are the latecomers among the minor prophets. Indeed, apart from the books 1 and 2 Samuel, and 2 Kings, and a few of the prophetic texts, it looks like the bulk of the Bible has gone AWOL.

“Far from bringing the Bible and archaeology together, Dever has demonstrated how much the Bible needs to be read on another basis. If archaeology is the only key, then the lockbox Dever has put his Bible into must be quite small. Sadly, this is what can happen to the Hebrew Bible when archaeologists dissect the text. While they claim to put the Bible on a richer historical basis, in fact the text is sent through the secular archaeologist’s shredder to emerge in fragments that no longer fit together as a meaningful literary tradition.”

Thompson described the influence that the maximalists had on his career after writing his book on the Patriarchal Narratives in 1974:

“In 1975, I left Germany to return to the [United] States. The controversies over my book on the patriarchs shut me out of university teaching. I became a full-time house-painter and handyman, my weekends and evenings given to the study of Old Testament narrative and the Pentateuch. After nearly a decade of such isolation, my exclusion from the field reached an unexpected end. I was appointed by the Catholic Biblical Association as annual professor to the Ecole Biblique in Jerusalem for 1985. The climate of biblical scholarship had shifted. Sociology and anthropology had grown strong in historical studies… My understanding of the patriarchal narratives was no longer controversial. It had become part of the mainstream of the field. …

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65 Henige, *op.cit.*, pp. 163-164
66 Pleins, *op.cit.*, p. 164
“In 1987, I began work on the question of Israel’s origins in an effort to show that such a history was possible [wherein Thompson showed] there could not have been a ‘United Monarchy’ with Saul, David and Solomon in Jerusalem during the tenth century BCE. I published my complete study in 1992 under the title The Early History of the Israelite People.

“Reactions to this book [which questioned the part of the Bible that many or most historians believed had a sound historical basis] were even stronger than those to my book on the patriarchs had been. … A review of my book appeared on the front page of the London newspaper, The Independent on Sunday. I was coming up for tenure at [my new assignment] Marquette University, where officials were already very unhappy over my research. Publicity stirred up conservative theological dogma, and my work was found ‘incompatible with the Catholic mission of the university.’ While this breach of academic freedom could have led to personal disaster, it proved to be an unequivocal blessing. I was called to take up a chair in Old Testament [Studies] at the [academically free] University of Copenhagen where I have been now since 1993.”

Thompson has written that “there never was a ‘United Monarchy’ in history and it is meaningless to speak of pre-exilic prophets and their writings. The history of Iron Age Palestine [ca. 1000 B.C.] today knows of Israel only as a small highland patronate lying north of Jerusalem and south of the Jezreel Valley.” And the literature is replete with statements regarding the fact that there is not much monumental evidence in Jerusalem for the time of David and Solomon, while others suggest the evidence elsewhere supports the United Monarchy archaeology. Again, Herschel Shanks, the Editor-in-Chief of Biblical Archaeological Review, has a rationalization to explain this problem away. As Henige delineates:

“Arguments from silence [where no evidence exists] are often attacked by those who find them unpersuasive or unpalatable. Herschel Shanks refers to a work on the Torah in which the author [David S. Sperling, The Original Torah (NY 1998, pp. 3-4)] writes ‘[t]he reader should be aware that we have no direct evidence of the existence of characters best known to readers of the Bible including—but not limited to—Abraham, Deborah, Gideon, David, Goliath, and Solomon.’ Shanks does not deny the truth of this; instead he argues that ‘[t]he absence of specific reference to these figures in contemporaneous extra-Biblical sources is next to irrelevant. For a simple reason: We would not expect them to be mentioned in the sparse written records that we have.’

“Shanks does not insist that all the individuals existed, but he knows, better than most, that very many—and very ephemeral—Egyptian, Assyrian, and Babylonian rulers and non-rulers left remains, as did citizens [of little importance]”

67 Thomas L. Thompson, The Mythic Past (London 1999), pp. XIV-XV
68 Ibid., p. XV
of places like Ebla and Ugarit that antedated the rise of Israel and existed on no larger a scale [than Jerusalem]. It is fair to ask, where are the discoveries for Israel and Judah that would rival those of Ebla, Ugarit, Alalakh, and others? We have contemporary records of many pharaohs who ruled only a few years; why then have we [only David's name on an ossuary and] nothing [else] for David and Solomon, who supposedly ruled eighty years between them?**69**

Nevertheless, the work of the minimalists has had its impact on the public, particularly the Jewish Conservatives in the United States. According to Michael Massing's article “As Rabbis Face Facts, Bible Tales Are Wilting,” in The New York Times, March 2002, p. B27:

“Abraham, the Jewish patriarch, probably never existed. Nor did Moses. The entire Exodus story as recounted in the Bible probably never occurred. The same is true of the tumbling of the walls of Jericho. And David [is] far from being the fearless king who built Jerusalem into a mighty capital ... Such startling propositions – the product of findings by archaeologists digging in Israel and its environs over the last 25 years – have gained wide acceptance among non-Orthodox rabbis. But there has been no attempt to disseminate these ideas or to discuss them with the laity – until now.

“The United Synagogue of Conservative Judaism, which represents the 1.5 million Conservative Jews in the United States, has just issued a new Torah and commentary ... Called 'Etz Hayim' ... it offers an interpretation that incorporates the latest findings from archaeology, philology, anthropology and the study of ancient cultures. [T]he editors [offer] a view of the Bible as a human rather than divine document.

“... it seems unlikely that the story of Genesis originated in Palestine ... More likely, Mr. [Robert] Wexler [president of Judaism in Los Angeles] says, it arose in Mesopotamia, the influence of which is most apparent in the story of the Flood, which probably grew out of the periodic overflowing of the Tigris and Euphrates rivers. The story of Noah, Mr. Wexler adds, was probably borrowed from the Mesopotamian epic Gilgamesh.

“Equally striking for many readers will be the essay ‘Biblical Archaeology,’ by Lee I. Levine, a professor at the Hebrew University in Jerusalem. ‘There is no reference in Egyptian sources to Israel’s sojourn in that country,’ he writes, ‘and the evidence that does exist is negligible and indirect.’ The few indirect pieces of evidence, like the use of Egyptian names, he adds, ‘are far from adequate to corroborate the historicity of the biblical account.’ Similarly ambiguous, Mr. Levine writes, is the evidence of the conquest and settlement of Canaan, the ancient name for the area including Israel. Excavations showing that Jericho was unwalled and uninhabited, he says, ‘clearly seem to contradict the violent and complete conquest portrayed in the Book of Joshua.’ What’s more, he says, there

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69 Henige, *op.cit.*, p. 182
is an ‘almost total absence of archaeological evidence’ backing up the Bible’s grand descriptions of the Jerusalem of David and Solomon.

“The notion that the Bible is not literally true ‘is more or less settled and understood among most Conservative rabbis,’ observed David Wolpe, a rabbi at Sinai Temple in Los Angeles and a contributor to ‘Etz Hayim.’ But some congregants, he said, ‘may not like the stark airing of it.’ Last Passover, in a sermon to 2,200 congregants at his synagogue, Rabbi Wolpe frankly said that ‘virtually every modern archaeologist’ agrees ‘that the way the Bible describes the Exodus is not the way that it happened, if it happened at all.’ The rabbi offered what he called a ‘litany of disillusion’ about the narrative, including contradictions, improbabilities, chronological lapses and the absence of corroborating evidence. In fact, he said, archaeologists digging in the Sinai have ‘found no trace of the tribes of Israel – not one shard of pottery.’

Concluding his article, Massing explains:

“Before the introduction of ‘Etz Hayim,’ the Conservative movement relied on the Torah commentary of Joseph Hertz, the chief rabbi of the British Commonwealth. By 1936, when it was issued, the Hebrew Bible had come under intense scrutiny from scholars like Julius Wellhausen of Germany, who raised many questions about the text’s authorship and accuracy. Hertz, working in an era of rampant anti-Semitism and of Christian efforts to demonstrate the inferiority of the ‘Old’ Testament to the ‘New,’ dismissed all doubts about the integrity of the text. Maintaining that no people would have invented for themselves so ‘disgraceful’ a past as that of being slaves in a foreign land, [Hertz] wrote that ‘of all Oriental chronicles, it is only the Biblical annals that deserve the name of history.’

“The Hertz approach had little competition until 1981, when the Union of American Hebrew Congregations, the official arm of Reform Judaism, published its own Torah commentary. Edited by Rabbi Gunther Plaut, it took note of the growing body of archaeological and textual evidence that called the accuracy of the biblical account into question. The ‘tales’ of Genesis, it flatly stated, were a mix of ‘myth, legend, distant memory and search for origins, bound together by the strands of a central theological concept.’ But Exodus, it insisted, belonged in ‘the realm of history.’ ‘While there are scholars who consider the Exodus story to be ‘folk tales,’ the commentary observed, ‘this is a minority view.’

“Twenty years later, the weight of scholarly evidence questioning the Exodus narrative had become so great that the minority view had become the majority one.”
SCIENCE, TECHNOLOGY, AND BIBLICAL CHRONOLOGY

“No problem associated with 2 Kings, and indeed the O[ldest] T[estament] in its entirety, is more complicated than that of chronology, that is, the placing of events recorded in the O[ldest] T[estament] in their proper sequence and assigning them their proper moment in the broader history of the A[ncient] N[ear] E[ast].”

Thomas R. Hobbs, 2 Kings
(Waco 1985), p. XXXIX

“Read all the books of the Old Testament, and you will find such discord as to the number of years of the kings of Judah and Israel, that to attempt to clear up this question will appear rather the occupation of a man of leisure than of a scholar.”

Saint Jerome in Edwin R. Thiele,
Mysterious Numbers of the Hebrew Kings
(Grand Rapids MI 1994), p. 39

How is one to resolve this fundamental dichotomy between the minimalists and the maximalists? The problem, strictly in terms of the established chronology, is that there is no question that the Old Testament was written in the Persian and Hellenistic epochs, ca. 550-200 B.C., or somewhat later. But the events described by that historical biblical literature in large measure occurred from around 1000 to 500 B.C. It is obvious that the writers of the Bible in Persian-Hellenistic times could not have known in detail the events that transpired so much earlier, and herein lies the dilemma. Furthermore, the very literature of the Bible includes philology as well as material descriptions of that supposedly dead past that also clearly reflect Persian-Hellenistic philological and material elements. It thus appears that these writers projected their own times back into the Israelite past or, most significantly, that the events described occurred in Persian-Hellenistic times. This latter conclusion, namely that the biblical history of many of the kings of Israel after Saul, David and Solomon, occurred in the Persian-Hellenistic epochs, is just what we will show to be the solution to this dilemma. That is, as with all the other chronologies of the ancient world, the chronology of the Bible must also be incorrect and must be moved closer to the present! But this chronological correction, most importantly, must be founded on pillars of science and technology, as these are integrated directly with the ancient nations that interacted with biblical Israel and Judah. In short, because the biblical chronology is massively disconnected from reality, there could never have developed a proper or adequate knowledge of the historicity of the Bible. Edwin R. Thiele, who has worked extensively on the problem of biblical chronology, explains the issue thus:
“For more than two thousand years Hebrew chronology has been a serious problem for Old Testament scholars. Every effort to weave the chronological data of the kings of Israel and Judah into some sort of harmonious scheme seemed doomed to failure. The numbers for the one kingdom could not, it seemed, be made to agree with the numbers of the other. The data concerning the synchronisms appeared in hopeless contradiction with the data as to the lengths of reign. Dates established by the biblical numbers seemed to be constantly out of line with the dates of Israel’s neighbors [such as the Neo-Assyrians].

“As long as Old Testament study remains the serious subject that it is, and as long as chronology retains so vital a place in this field of study, efforts to solve the seemingly inscrutable riddle of Hebrew chronology will be in order. …

“The greatest drawback in the study of Old Testament chronology has been, and still remains, a tendency to hold to certain preconceived opinions without first endeavoring to ascertain just what the Hebrews did in the matter of chronological procedure. …”70

According to Thiele:

“The only basis for a sound chronology of the period to be discussed is a completely unbiased use of the biblical statements in the light of all other knowledge we can bring to bear on this problem, notably the history and chronology of the ancient Near East.”71

“… Without exact chronology there can be no exact history. Until a correct chronology of a nation has been established, the events of that nation cannot be correctly integrated into the events of neighboring states. If history is to be a true and exact science, then it is of fundamental importance to construct a sound chronological framework about which may be fitted the events of states and the international world.

“Of particular interest and importance is the history of the Hebrew nation. If the establishment of a correct chronology is important for any nation, then it is important for the Hebrews. Never will the manifold details of the Old Testament writings be correctly integrated into each other, never will the events of the Old Testament record be properly fitted into the events of the Near Eastern world, and never will the vital messages of the Old Testament be thoroughly or correctly understood until there has been established a sound chronology for Old Testament times. If Old Testament scholarship is to achieve the degree of confidence that is its due, then it must move forward on the basis of a sound chronology. Not until there has been established for the Old Testament a chronological pattern that is consistent with both itself and the world of that time will there exist a foundation that will make possible that degree of sound and scientific study so necessary in this important field.”72

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71 Ibid., p. 16
72 Ibid., p. 33
How, then, does Thiele attempt to correlate Hebrew history with the surrounding nations? And on what basis is this correlation to be founded? According to Thiele, that foundation or pillar of the past is astronomy:

“If the chronological materials recorded in the Hebrew Scriptures are basically sound, they will agree with whatever is sound in the annals of neighboring states … and that will be in agreement with the required astronomical data, then we may be certain that we are on the track of that correct and absolute chronology …”\(^73\)

This is what we will do in the following work. But before doing so, we must first evaluate the chronology that Thiele and all the other historians accept. What Thiele did was integrate the calendrical materials for the states of Israel and Judah in such a way that they agree with one another year by year. As William A. Irwin expressed it:

“The astonishing fact is that he [Thiele] demonstrates conclusively the precise and dependable accuracy of Hebrew chronology of the times of the [two] kingdoms. … The unique feature of Professor Thiele’s work is that he has attained his results by the most rigid application of scholarly facts and methods. He has brought to bear on the problem all relevant knowledge of the history and chronology of the ancient Orient, plus whatever is provided by the most approved methods of biblical study. Having done so, he has shown that the seeming inconsistencies and mathematical contradictions no more than hinted at … really are nothing of the sort but are integral elements in a sound and accurate chronological system. …

“… [T]he validity of his own findings rests on the simple fact that they work! They take account of all the data provided by the biblical record and organize them in a system that is rational, consistent, and precise. His findings harmonize with all that is known of relevant [Palestinian, Mesopotamian, and Egyptian] chronology of the entire world of the Bible.”\(^74\)

That is, indeed, the historians’ general view of Thiele’s great work. Internally, at least, it seems to be valid, though we suggest the entire history of Israel must be moved en bloc 274 years closer to the present, based on astronomy. But let us briefly examine the method Thiele employs to unite the calendars of Israel and Judah. Gershon Galil informs us:

“In 1944-45, two studies were published in the United States which greatly influenced the research of the chronology of the period of the divided monarchy [of Israel]: the works of E.R. Thiele and W.F. Albright. Their proposals were accepted by many researchers, and to the present, almost half a century after their publication, most scholars are of the opinion that the system of Thiele or that of Albright is the most plausible for reconstructing the chronological frame of the

\(^73\) Ibid., p. 35
period under discussion. … Thiele attempted to reconcile the contradictions inherent in these data and to prove their reliability in the following diverse ways: (1) coregencies existed both in Judah and in Israel; (2) at times, the years of reign were counted from the coronation of a king during his father’s lifetime, while at other times, they were counted only from the beginning of his reign as an independent sovereign; (3) there was a gap of half a year between the new year in Judah and that in Israel—in the Northern Kingdom the new year began on 1 Nisan, while in the Southern Kingdom it was celebrated on 1 Tishri; (4) there were instances in which in Judah the years of the kings of Israel were counted in accordance with the Judahite calendar, and there were cases in which the Northern Kingdom enumerated the years of the kings of Judah in accordance with the calendar of the kings of Israel; (5) the system of counting in the two kingdoms was not constant but rather was changed a number of times during this period—the Israelite kingdom initially employed the antedating system (until the time of Joash), and afterwards the postdating one; while in Judah postdating was used until the reign of Jehoram; antedating from Jehoram to Amaziah, and once again postdating from Amaziah to the end of the First Temple period.”

Thiele’s chronology has been generally accepted by biblical scholars across a broad spectrum, as Floyd Nolen Jones, a fundamentalist, shows:

“… Edwin R. Thiele (1895-1986) … has professed to have resolved the issues concerning the chronology of the period of the kings of the divided monarchy of the Hebrews. For nearly half a century his dates, and to a far lesser degree those of Professor William F. Albright, have dominated this segment of Bible chronology to the extent that nearly all Bible commentaries, dictionaries, encyclopedias, etc. in the marketplace reflect his views. Thiele’s dates are used and sanctioned by nearly every Bible college and seminary, conservative or liberal, on the globe today.”

Galil claims that “Thiele’s view was accepted by many researchers, including K.A. Kitchen and T.C. Mitchell, … Hallo, … H.S. Gehman, … Rainey, … de Vries, … R.J. Coggins, … and others.”

The reason for this acceptance is that Thiele integrated a great deal not only of biblical chronology but, most significantly, Neo-Assyrian astronomical/chronological evidence into a coherent whole in which all the evidence seemed to mesh. That is, he connected biblical chronology directly with Neo-Assyrian kings who were linked via archaeoastronomical evidence to precise dates. But, as we understand from the materials outlined in Pillars of the Past volume II, chapter 2, the Neo-Assyrians must be moved closer to the present, according to Lynn Rose’s work by about 274 years.

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75 Gershon Galil, The Chronology of the Kings of Israel and Judah (Leiden, the Netherlands/NY/Köln, Germany 1996), pp. 3-4
76 Floyd Nolen Jones, Chronology of the Old Testament (Green Forest AR 2004), p. 114
77 Galil, op.cit., p. 3 fn
Astronomical dating of the Neo-Assyrians became the anchor points for the biblical chronology of the Hebrew kings of the divided monarchy. That is, Thiele chose the astronomical/empirical data of the Neo-Assyrian kings upon which to erect his chronology and not strictly the biblical dates for the reigns of the Hebrew kings:

“… Between the absolute chronology of the Hebrews and that of their neighbors there can be no conflict. If biblical chronology seems to be at variance with Assyrian chronology, it may be because of errors in the Hebrew records, but it may also be because the data preserved in those [biblical] records are not correctly understood.”

As one can see, Thiele denied the precise validity of the biblical record, or more accurately claimed that it was “at variance with Assyrian chronology because of errors in the Hebrew records [or] because the data preserved in these are not correctly understood.” He was suggesting exactly what we have presented in these volumes of <i>Pillars of the Past</i>, namely that scientific evidence overrides documentary evidence and that the documents must be either in “error” or corrected to fit the science, or seen as fiction!

There are several direct mentions of Hebrew kings in the Neo-Assyrian records and it was upon these precise and direct connections between the Neo-Assyrian kings with the Hebrew kings that Thiele built his chronology. In this respect, not only was biblical chronology tied to Mesopotamia, but this has profound implications for biblical connections with the Egyptians and Egyptian chronology. Thus Thiele informs us:

“… in the seventh year of Hoshea, Shalmaneser began his siege of Samaria (2 Kings 18:9); in the fourteenth year of Hezekiah, [the Neo-Assyrian king] Sennacherib captured the fortified cities of Judah (2 Kings 18:13; Isa. 36:1); … Thus [the Babylonian King] Nebuchadnezzar [II] began his siege of Jerusalem in the ninth year of Zedekiah, on the tenth day of the tenth month (2 Kings 25:1; Jer. 52:4); …

“Events recorded in both Hebrew and secular history are always of interest. In the fifth year of Rehoboam, Shishak of Egypt attacked Jerusalem (1 Kings 14:25; 2 Chron. 12:2). During the reign of Ahaz, [the Neo-Assyrian king] Tiglath-Pileser III captured Damascus and received tribute from the king of Judah (2 Kings 16:7-9), and Menahem of Israel likewise paid tribute to Pul, i.e., Tiglath-Pileser III (2 Kings 15:19). Sennacherib attacked Jerusalem and the fortified cities of Judah in the fourteenth year of Hezekiah (2 Kings 18:13, Isa. 36:1). All these events are referred to in the records [outside Israel and Judah] of the ancient East.”

Thiele accepted that the Neo-Assyrians had been properly astronomically dated and reports:

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78 Thiele, <i>op.cit.</i>, p. 34
79 <i>Ibid.,</i> p. 35
“In the early period of the Hebrew monarchies the most frequent and definite contacts were with Neo-Assyria, and in the later period most contacts were with Neo-Babylonia. Fortunately, the chronologies of these two nations, at least for the period with which we are most concerned, have been definitely established [on astronomical grounds]. There were also frequent Hebrew contacts with Egypt, Aram, and other lesser states, but the contacts were in most instances very indefinite in point of time, and the chronologies of those nations are likewise far from being positively established.

“[Neo-Assyrian chronology back to the beginning of the ninth century B.C. rests on a highly dependable [astronomical data] basis. The various [astronomical data] items essential to a sound chronology were present, and so scholars have been able to produce a sound chronological system for that nation [and thereby to its connections with Israel and Judah].”

80 Thiele then goes into all the various astronomical and other aspects of Neo-Assyrian chronology that support that contention.81

Of particular interest is the connection of the Hebrew king, Rehoboam, who is connected in biblical chronology with Shishak of Egypt, both dated by Thiele to around 931-913 B.C. But on the basis of Rose’s analysis, the dates of both kings must be moved 274 years closer to the present, or just prior to Persian times, as must all the Hebrew kings who followed Rehoboam in Judah and Jeroboam I who reigned in Israel.

What we have done here, as did Thiele and all those who have accepted his chronology based on their acceptance of the astronomical links between the Neo-Assyrians and the biblical kings, is move all biblical history closer to the present by 274 years. On the basis of Rose’s work this entire matter is settled. The Neo-Assyrian records are absolute and therefore the Hebrews and all other surrounding national chronologies are required to conform with that scientific structure. As with Thiele, this new chronological arrangement is obvious and clear-cut, a vital and total chronological integrated meshing of the history of that period and as such it deserves unqualified trust. Unless Rose’s astronomical evidence is shown to be in error, no other chronology of the Bible is therefore warranted.

Here, then, we return to the debate between the maximalists and the minimalists. The philological evidence presented by Wellhausen, Van Seters, and Thompson undeniably proves that the history of the biblical kings and older materials must be placed somewhat prior to Persian and into Hellenistic times. This philological fact presented by the minimalists is undeniable. The astronomical/empirical evidence presented by Rose undeniably proves that the history of the biblical kings must also

80 Ibid., p. 67
81 Ibid., pp. 67-68
be placed just prior to Persian and into Hellenistic times. Therefore the philological evidence for the chronology of the biblical kings completely meshes with and is integrated with the astronomical evidence for the biblical kings. The minimalist-maximalist debate is resolved. That is, the writing of the Bible occurred at the very same time these historic events would have been taking place! There is no great temporal disconnection between the biblical writings and the events described in this epoch. The argument of the minimalists is no longer valid. The argument of the maximalists that these events were passed down orally from generation to generation and reflect the former period with many revisions or alterations is also no longer valid. That part of the debate is closed. In the words of Thiele:

“It is only proper that the dates we have given here for the rulers of Israel and Judah [correlated with the dates of Neo-Assyrian kings] should be subjected to every possible test. If these dates are final and absolute, they have nothing to fear from the most careful and exhaustive research. The kings of Judah and Israel have in the years gone by had part in many a chronological fray. We have every reason to believe that their days of such conflict are almost over. Our hope is that when the smoke of the battle clears and the din of the final conflict has subsided, each of these valiant stalwarts of old may occupy his rightful place in history.”

The exacting nature of Thiele’s work and his organization of the dates of the Hebrew kings of the divided monarchy after Solomon with the Neo-Assyrian kings directly connects the chronology of Palestine with the chronology of Mesopotamia, in absolute terms. The question that then arises is: can Hebrew chronology be directly connected with the chronology of Egypt based, of course, on the short chronology? This we will present in the chapter on the Exodus, where we will show there are numerous direct connections. The reader will understand that the chronology of the Neo-Assyrian/Persian/Hellenistic epoch correlates with the history/chronology of Hebrew Palestine and connects all these civilizations with that of Egypt. We have a set of gears that mesh together across the ancient Near East and are a chronological whole that does not exist with any other chronology, neither with the established one, nor that of Peter James et al., nor with David Rohl’s, nor with anyone else’s.

Simply put, the philology proves the history of the Bible’s divided monarchy was written in Persian and Hellenistic times and the astronomy of the Neo-Assyrians proves the events of that divided monarchy occurred in Persian and Hellenistic times!

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82 Ibid., pp. 211-2
PHILISTINES DATED TO PERSIAN TIMES

Since the Israelite monarchy is dated to the time of the Persians down into the Hellenist era, it follows that the Philistines must also be dated to this period. That is, the Philistines were part of the Persian empire and there were interactions not only between the Philistines and the Hebrews but both peoples were dominated by the Neo-Assyrians who Rose has shown were the Persian rulers of Assyria. These interactions are briefly summarized by Jonathan Michael Golden who discusses “Superpowers from the East”:

“Indigenous [Palestinian/Lebanese/Syrian] state structures … would not last long, and during the last few decades of the eighth century B.C.E. [which we date 274 years closer to the present] most of the southern Levant was under constant threat of a new imperial power, the Neo-Assyrians. In 734 B.C.E. Tiglath Pileser III launched campaigns into both Phoenicia and Philistia, turning his attention next to the Galilee, conquering that region and exiling its people. … Maintaining a consistent presence in the region and occasionally using force, the Neo-Assyrian [Persian] kings forged the ‘global’ empire of its day and for a period … dominated what was called pax Assyriaca, an economic policy explained in ideological terms. For a brief time the Judean King Hezekiah [probably with Persian help or sanction] gained political control over a number of cities, including the Philistine center of Ekron, but Sennacherib reasserted Neo-Assyrian [Persian] ascendancy in all of Philistia and beyond …

“Much of what archaeologists know about this period derives from the annals of two later Neo-Assyrian kings, Esarhaddon and Ashurbanipal. The accuracy of both the historical and Biblical accounts of the [Neo-]Assyrian onslaught is corroborated by archaeological evidence. … Neo-Assyrian power in the region began to fade, however …83

Thus we can see that not only do the Neo-Assyrians move the Egyptians of the Nubian 25th Dynasty closer to the present, into Persian time, as well as the Hebrews during the divided monarchy, but they do the same for the Philistines. This proves—based on Rose’s astronomical evidence—that all these regions—Assyria, 25th Dynasty and Nubian Egyptians, Hebrews, and Philistines—all lived in Persian times. Therefore, once again, the forensic historical evidence shows that there were no Sea Peoples in the 12th century B.C. to invade Egypt and that the Philistines—supposedly a group of the Sea Peoples—did not come to the Levant shortly thereafter. All these chronological connections are derived from forensic evidence and then confirmed and corroborated by documentary and archaeological

83 Jonathan Michael Golden, Ancient Canaan and Israel (Santa Barbara CA 2004), pp. 169-170
Evidence which is the proper methodology that should be employed to accurately delineate the chronology and history of these nations.

It was pointed out to me by Lynn Rose that I give the impression that all the monarchical Hebrew kings are dated to the Persian and Hellenistic epochs. That is not at all what I intended to convey to the reader. Above I mentioned that Hebrew history related to the period of the United Monarchy under David and Solomon preceded Persian times and therefore that the biblical writings about them, although written in later times, reflect what was known about them, with historical embellishment from the period prior to the Persians. While the bulk of Hebrew monarchical history was indeed written in the Persian and Hellenistic eras and largely took place at these times, the historical events of David and Solomon definitely did not occur in Persian or Hellenistic times but came somewhat before, by about a century to a century and a half.

**BIBLICAL DARK AGES**

As with other ancient nations, when a historical period is overly lengthened, what ensues are Dark Ages. Because the history of Israel is connected to the established chronology, it too is shrouded by these non-existent, empty periods. But what is also a most unusual aspect of biblical history is that most of the earlier periods assigned to it from around 1200 to 700 B.C. take place during the Dark Ages that descended upon Greece, Anatolia, Babylonia, etc. While historians claim that the Sea Peoples around 1200 B.C. swept across the lands of the Hittites, Syrians, and Palestine up to the gates of Egypt, which brought a 500-year Dark Age to all these regions, Palestine, or, more accurately, biblical Palestine, experienced the glories of the reigns of David and Solomon, and the kingdoms of Judah and Israel. Thus, while this Dark Age saw the seeming obliteration of several major civilizations for centuries, Israel rose to power. In fact, we will see that there were two Dark Ages, one at the commencement of the monarchies of Saul, David, and Solomon, and another towards the end of biblical history during the Exile. And in like manner, as with other nations’ Dark Ages, the historians have failed to grasp that these Dark Ages are a creation of their embracing the lengthy established chronology. With respect to the first Dark Age, James Maxwell Miller explains:

“Widespread disturbances and population upheavals underway approximately 1200 BCE mark the end of the Late Bronze Age. Mycenaean civilization collapsed, the Hittite empire came to an end, and Egypt entered another period of decentralization—the Third Intermediate Period. Ugarit, Alalakh [in Syria] and other major cities throughout the ancient world were destroyed. Among the peoples … who may have contributed to the collapse of the Egyptian and Hittite empires, were
the so-called Sea Peoples. Ramesses III ... defended Egypt’s frontiers against the Sea People and mounted a last-ditch effort to reassert Egyptian authority in Palestine ... Realistically, therefore, it may be appropriate to date the end of the Late Bronze Age in Palestine a half century or so after 1200 BCE.

“Then Syria-Palestine, and indeed most of the Middle East, entered a ‘dark age’ that would last through the first three centuries of the Iron Age [to ca. 850 B.C.]. These opening centuries of the Iron Age may be regarded as dark-age centuries both in the sense that it was a time of urban fragmentation and decline, and in the sense that we know so little about what was going on. Contemporary written records are scarce and archaeological remains are meager ... Israel’s origins are to be sought in these dark-age centuries.”

George E. Mendenhall describes this Dark Age thus:

“There is little agreement among biblical scholars about the nature of Israelite society from the time it is first mentioned in the Merneptah Stela (ca. 1208 B.C.) to the time of kings David and Solomon two hundred years later. Even the Israelite scribes six hundred years later who wrote about it had difficulty comprehending its original social character. For example, the author of the book of Joshua envisioned it less as a religious community and more along the lines of a political state or ‘nation,’ whose leader Joshua is depicted as (but never actually labeled) a king. …

“This twelve-tribe federation existed during a ‘dark age’ that archaeologists label [from] roughly 1200-1000 B.C.”

Not only does a Dark Age cover the period after the Exodus through to Joshua and Judges, the same exists for the realms of David and especially Solomon, except for a stone ossuary that notes on it the “house of David.” But as for what one would expect to find in Jerusalem regarding this nation, there is nothing to suggest it was the capital of the Jewish state. Peter James et al. describe what was expected when archaeologists dug into this site to find David and Solomon:

“The wealth of King Solomon is described in such graphic detail in the Bible that his name is still a byword for opulence. His reign marked the zenith of ancient Israel’s wealth, power and territorial extent. To the north and east, kings of Syria and Transjordan paid him homage, while Hiram I, ruler of the great Phoenician city-state of Tyre, was his ally, both politically and commercially. Solomon’s marriage to a daughter of an unnamed Egyptian pharaoh gave him security to the south and unprecedented prestige for a Levantine king.

“Solomon’s reign, which ended c. 930 B.C., is described in the Bible as forty years of almost uninterrupted peace, stabilized by a system of diplomatic alliances, strategically placed fortresses and a newly developed chariotry. Trade generated

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85 George E. Mendenhall, Ancient Israel’s Faith and History: An Introduction to the Bible in Context (Louisville KY 2001), p. 73
wealth: ‘all the kings of Arabia’ paid Solomon duties on the spice traffic passing through his domains. Ezion-geber at the head of the Gulf of Aqaba was developed as a port, and his navy, with the help of Phoenician maritime expertise, undertook commercial expeditions to distant lands, ‘bringing gold and silver, ivory, and apes and peacocks’ (1 Kgs. 10:22).

“The riches gathered by Solomon were spent principally on a massive building programme. His primary achievement was the magnificent Temple … He rebuilt the cities of Hazor, Megiddo and Gezer, and founded Tadmor (Palmyra), a trading centre deep in the Syrian desert (1 Kgs. 5; 7:1-12; 9:15-19).”

If this biblical description is correct in terms of place and chronology, there should have been found abundant evidence in the ground to account for this opulent period in and around Jerusalem, but such is not the case, which has been the despair of scholars. Israel Finkelstein and Neil Asher Silberman put the problem of archaeology of this period into the starkest terms:

“And we still have no hard archaeological evidence—despite the unparalleled biblical descriptions of its grandeur—that Jerusalem was anything more than a modest highland village in the time of David, Solomon, and Rehobeam.”

This conclusion reverberates throughout biblical literature, not only for David and Solomon, but back into the earlier period, 200 years into the past and beyond. According to Margreet Steiner:

“Based on the content of the Amarna correspondence [from and to Egypt] several scholars have concluded that the Urusalim [Jerusalem] from which Abdi-heba sent his letters to the pharaoh [Akhenaten] was an important and large city. It is assumed to be the center of a city state [in Palestine], the seat of the ruler of a dimorphic chiefdom, or the commercial center for the immediate region. … However, hardly any archaeological finds from the fourteenth century B.C.E. have turned up during the many excavations that have been carried out in and around Jerusalem. No trace has ever been found of a fortified town—no city wall, no gates, no palaces, no houses. Moreover almost no stray [pottery] sherds dating from the fourteenth century B.C.E. have been found in many later fills and debris layers. In my opinion, the only logical conclusion that can be drawn is that no fortified town existed in Jerusalem during the period of the Amarna letters. Archaeologically speaking, Jerusalem was simply not occupied during this period of the Late Bronze Age. This whole situation seems to be one of the many instances when texts and archaeology contradict each other.”

In the same book, Ann E. Killebrew tells us:

86 Peter James et al., op. cit., p. 188
87 Finkelstein and Silberman, op. cit., p. 158
“The most contested period of time, both archaeologically and biblically, relates to our understanding of Jerusalem during the tenth century [B.C.], specifically to the reigns of David and Solomon. Thus far no physical remains have been found in over a century of excavations that come near to matching the biblical magnificence of the Solomon’s Jerusalem that served as the capital of a ‘united monarchy’ …

“… [T]he physical reality of Jerusalem (no matter which chronology is followed) is far from the city described by the Bible. Heroic efforts to interpret a grander tenth-century Jerusalem based on missing evidence are methodologically flawed and at best misleading, especially to non-archaeologists … [These are] the highly idealized and romantic notions of a glorious Jerusalem [rather than] a historically accurate description of a tenth-century reality.”

James et al. brutally describe the archaeological situation citing M. Magnusson, “on archaeological grounds the commercial and political ascendancy ascribed to Solomon has also been dismissed as a fantasy, an empire which existed only ‘on papyrus’.” What the established chronology has led historians to do is seek for “idealized and romantic notions of … historically accurate … reality,” for a “fantasy” of “an empire which existed only ‘on papyrus’.”

The name “House of David” has been found on an ossuary at Tel Dan and certain historians have attempted to turn this inscription into a kingdom. This was tried by Kenneth Kitchen to create a clan for David. Of his attempt to do so Steven L. McKenzie shows:

“The Tel Dan inscription inspired another sighting of David’s name on a long-known text. That text is the relief of Pharaoh Shoshenq (called Shishak in the Bible …) carved on the temple of Amun in the ancient city of Thebes. … It contains a long list of names of places that Shoshenq claims to have captured.

“The British Egyptologist Kenneth Kitchen has very recently suggested that David’s name is in that list. The name occurs in an expression that Kitchen translates ‘highland/heights of David’… . The immediate context, he says, is a set of places in southern Judah and the Negev (the southern part of Palestine) where, the Bible reports, David was active when he was fleeing Saul … . The area, Kitchen concludes, must have been known by David’s name.

“This occurrence of David’s name is even less certain than the one on the Mesha Stele. Much of the relief is damaged and illegible. Of the names that can be read, many cannot be identified for certain with any known sites in Palestine. Not all scholars agree with Kitchen that the names on the relief reflect any consistent geographical order [of Shoshenq’s conquests]. In addition, the word translated

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90 James et al., op.cit., p. 169
‘highland/heights’ is rare, and its exact meaning is uncertain. In an earlier publication Kitchen himself calls the reading of these words ‘obscure.’

“Kitchen’s reasoning is curious. It is highly unlikely that the highlands of southern Judah and the Negev bore David’s name simply because he spent time there. The term ‘the highland of David’ for his reign does not occur in the Bible or anywhere else. If this interpretation were correct, it would indicate the opposite of what Kitchen intends. The ‘highland of David’ would most naturally refer to an area within the territory of a clan or tribe. ‘David’ in this expression would then be a clan or its land—like Benjamin, Ephraim, or Judah—not an individual at all. If ‘David’ could refer to a clan or region, as Kitchen’s reading suggests, then he may never have existed as a historical figure. The character of David in the stories about him might be an abstraction of the clan treated as its ancestor—what biblical scholars call an ‘eponymous’ ancestor or tradition.

“Eponymous traditions are common in the Bible. In Genesis 10, for instance, the nations and peoples of the known world are treated as individuals in a genealogy that goes back to Noah and his sons… In Gen. 25:19-26 the nations of Edom and Israel are treated as individuals, Esau and Jacob. In Genesis 29-30 the twelve tribes of Israel are described as the twelve sons of Jacob/Israel. But outside of Kitchen’s interpretation of the Shoshenq relief, ‘David’ was never the designation for a clan or region. The biblical stories about David differ from those about eponymous figures such as Jacob and his [twelve] sons. There is no hint that David ever represented the dynasty or the nation of Judah. Rather all the stories are about him as an individual. They deal with the drives, motives, and deeds of an individual man rather than the representative of a group. Kitchen’s reading unintentionally highlights an important distinction between the name David and the expression ‘house of David.’ The latter is a political designation and refers to the dynasty or the Kingdom of Judah. But the name David in the Hebrew Bible always refers to the individual. David is the founder of the dynasty known as the ‘house’ or ‘seed’ of David.

“The occurrence of David’s name on the Shoshenq relief, then, is uncertain. Kitchen’s explanation of it is highly speculative and causes more problems than it solves. On the other hand ‘the house of David’ on the Tel Dan and Mesha steles most naturally refers to the nation or dynasty established by the individual named David.”

Kitchen, a strong supporter of biblical chronology and tradition, was attempting to find another mention of David outside the Bible in a foreign land that would indicate that the kingdom of David was of sufficient renown to have been inscribed on a stele in Egypt. What he did was create this allusion out of whole cloth all on his own inclination. This approach shows that there is a fundamentalist/historic streak in scholars like Kitchen who attempt, without scientific or technological evidence, to place the biblical narratives and historic kings not only on a

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foundation that seems a reality, but will employ broad, unfounded interpretation of the documentary evidence to make the Dark Age that surrounds Davidic Israel one containing light. They cannot and, I dare say, will not see that it is their allegiance to an erroneous chronology that has created these problems.

Typical of Kitchen’s approach is to ignore evidence that disagrees with the point he is making. Philip Johnston shows how Kitchen has turned the Dark Age in Palestine, Syria, and elsewhere into an empire for Israel:

“David’s empire (2 Sam. 8) was apparently enormous, including all the neighbouring territories and stretching to the distant north, through what is now Lebanon, Syria, and north-west Iraq as far as the Euphrates River. So geographically and demographically Israel became only a minority part of this [empire]. … Kenneth Kitchen has shown that the eleventh to tenth centuries BC witnessed a power vacuum [created by the Sea Peoples] between the major powers of Egypt, Hittites (central Turkey) and Mesopotamia. This led to several mini-empires, including that of David and Solomon.”

But if this empire existed, then Jerusalem would exhibit all the archaeological evidence of this fact. Kitchen does not prove this existed. In order to get around this vexed contradiction, biblical historians have sought to prove their case by finding other archaeological evidence in other cities said to have been rebuilt by Solomon, namely Hazor, Megiddo, and Gezer. A great deal has been written about the other evidence which James et al. sum up:

“Solomonic remains have been sought in the period known as Iron Age IIA, dated c. 1000-900 BC, and particularly at Hazor, Megiddo and Gezer. The first remains to be confidently labelled ‘Solomonic’ were the palace and stables at Megiddo, protected by a substantial gateway and walls. Following this, Yigael Yadin … discovered another Iron Age IIA gateway at Hazor [stating]:

“… the gate’s plan – comprising six chambers and two towers – as well as its dimensions were identical to those of the gate discovered earlier at Megiddo and ascribed by its excavators to the city of Solomon [Jerusalem]. Excitement in our camp intensified. This was real proof! Not only were our deductions in ascribing this stratum to Solomon correct, but the gate was also confirmation of the authenticity of the Biblical verse describing Solomon’s activity in these two cities.’

“Yadin followed this success [by searching through the site reports of Gezer] where he discovered the plan of a gateway and walls ‘exactly like those found in Megiddo and Hazor.’ Renewed work at Gezer showed the gate to be [also] of Iron Age IIA date and the excavator William Dever wrote exultantly: ‘The sealed pottery from the floors and the makeup below were characteristic red-burnished ware of the late tenth century BC. Solomon did indeed rebuild Gezer!’

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92 Philip Johnston, *IVP Introduction to the Bible* (Nottingham UK 2006), p. 83
“For Yadin, and nearly all biblical archaeologists, this pattern of evidence is completely satisfying. The Bible states that Solomon built at Hazor, Megiddo and Gezer; at each place walls and gateways of identical construction and similar dimensions were discovered, all associated with the Iron Age IIA pottery thought to have been used in Solomon’s time. The attractive idea of the ‘Solomonic cities’ has now firmly entrenched itself, not only in the minds of scholars, but in the imagination of the public. Certainly the gateways of Hazor, Megiddo and Gezer could have been made by the same builders. But were they really the servants of Solomon? Further gateways with the same plan have now been found at Lachish, not recorded as a centre of Solomonic activity, and Ashdod in Philistia. The Bible is clear that Solomon’s direct rule did not extend over the Philistines, making the occurrence of ‘Solomonic buildings’ in Ashdod rather surprising.

“Moreover, the level of material culture associated with these Iron Age IIA structures is hardly reminiscent of the glories of Solomon’s reign. In the words of the American biblical archaeologist James Pritchard:

“‘These “cities”, even by ancient Near Eastern standards … were far from what one might call urban centres; they were more like villages. Within the walls of roughly cut stones there were floors of beaten earth or plaster. Artefacts of bone, stone, clay, an occasional metal tool or weapon, suggest a cultural level which was apparently lacking in both artistic sophistication and wealth. As yet no evidence has been found for [Solomonic] use of chariots or the metal trappings for the harnesses of horses. As for [Solomonic] gold and other precious metals, its occurrence is limited to an occasional earring or other article of personal adornment. From the tenth-century level … at Megiddo not a single gold item is recorded by the excavators’.”

Israel Finkelstein and Neil Asher Silberman report how the biblical model of Hazor, Megiddo and Gezer as evidence of Solomon crumbled:

“Yet this harmonized archaeological image of a golden age of the [David/Solomon] united monarchy did not last long. Two decades after Yadin demonstrated an apparently perfect match between Bible and archaeology, the various elements of the theory started to crumble, one by one.

“[As for the dating, t]he first to go down were the gates. A detailed study of the Megiddo gate by David Ussishkin showed that it was built later than the gates of Hazor and Gezer [which are closer to the present]. In addition similar gates were found in much later periods and at clearly non-Israelite sites … Even the basis for the dating of the Solomonic levels was shown to be the result of circular logic: the pottery and other artifacts found in the gate levels were dated to the tenth century BCE because of the association of the gates with the biblical verse about the building project of King Solomon. Later ardent defenders of the ‘Solomonic grandeur’ theory simply forgot about this circular reasoning when they argued that the biblical verse

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93 James et al., op.cit., pp. 189-191
(and the great Solomonic kingdom) must be historical, since the gates and other impressive structures were found in levels dating from the tenth century BCE!

“New data from ongoing excavations in Israel and a reanalysis of old finds undermined the rest of Yadin’s basis for ‘Solomonic’ archaeology. Less than ten miles to the east of Megiddo is the site of Jezreel, the location of a palace of the Omride dynasty, described in the Bible … The historical existence of the Omrides is supported by [Neo-Assyrian/Persian] records and the evidence of the Mesha and Tel Dan stelae [the latter of which has the inscription “House of David”]. Jezreel was excavated in the 1990s by David Ussishkin and John Woodhead, who uncovered a large fortified enclosure that they readily identified as an Omride royal compound, strikingly similar in conception to the royal acropolis of Samaria, the capital of the Omride dynasty. …

“Surprisingly, the pottery types found in the Jezreel compound are identical to the pottery of the city of the ashlar palace at Megiddo, which was supposed to have been destroyed by Pharaoh Shishak almost a century before the fall of the Omrides. Could it be that Yadin’s ‘Solomonic’ city at Megiddo was in fact an Omride city … like Jezreel [built] long after the time of Solomon?”

The answer apparently is yes. Excavations carried out by “Clarence Fisher (at Samaria and Megiddo) and John Crowfoot (at Samaria)” showed that “There are unmistakable similarities in the building methods between the Samaria palace and at least one of the two Megiddo palaces.” “The ashlar blocks in the palace at Samaria and the southern palace at Megiddo bear similar masons’ marks unknown at any other Iron Age site in Israel.” The authors conclude that “the monuments that have traditionally been attributed to Solomon and seen as symbols of the greatness of his state were in fact built by the kings of the Omride dynasty…”

What should strike the cognizant reader is that the Omrides dated to “the first half of the ninth century BCE” were reported in the Neo-Assyrian/Persian records, which places them 274 years closer to the present, based on the astronomical evidence presented by Rose. And Finkelstein and Silberman admit that “similar gates [as those at Megiddo, Gezer, and Hazor] were built in the later phases of the Iron Age, UNTIL THE SEVENTH CENTURY B.C.E.” or that such gates based on the established chronology were being built in the 600s B.C., or 300 or more years closer to the present!

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94 Israel Finkelstein, Neil Asher Silberman, David and Solomon (NY/London etc. 2006), pp. 278-280
95 Ibid., p. 280
96 Ibid.
97 Ibid., p. 281
What the biblical historians and archaeologists did was, having failed to find evidence for David and Solomon’s Jerusalem, they began looking elsewhere, namely at Megiddo, Gezer, and Hazor, for the missing evidence that should have been found at Jerusalem. Rather than making their case in Jerusalem, they switched gears and sought the missing proof elsewhere. Since they assumed the Bible had to be correct, finding the gates and walls at these villages led them to the illogical conclusion that although no evidence at Jerusalem existed to prove David and Solomon both historically and chronologically, finding almost identical gateways and walls at these other sites proved the existence of David and Solomon at Jerusalem historically and chronologically. The illogicality of their reasoning is profound and can at best be described by a well-known anecdote, such as the one discussed by Matthew A. Baum:

“This means heeding the lesson of the parable of the Drunkard’s Search … This story concerns a man who drops his car keys while attempting to enter his car after a night of heavy drinking. Rather than looking [for them] near his car where it is dark, he crosses the street to look for his keys under the nearest streetlight. When a nearby police officer inquires why the drunkard is looking for his keys across the street from where he dropped them, the drunkard responds that it’s easier to see under the light. The drunkard reasons that even though the keys are unlikely to be found under the streetlight … he has a better chance finding them there in the light than near the car, in the dark.”

Since the biblical historians couldn’t find Solomon and David in Jerusalem, they went looking in Megiddo, Gezer, and Hazor. Of course their delight at reasoning this way had disastrous results. First, their application of logic was inappropriate. Second, they took the easy way out by avoiding looking for the necessary evidence in Jerusalem, but that search and the others at the distant sites all ended in futility. This “streetlight mental syndrome” actually reflects the bias in their research methodology where they eschew the reality of Jerusalem and herald the unreality of these other sites.

The overall picture of this Dark Age is described by Finkelstein and Silberman:

“The suggestion of some scholars that ‘absence of evidence is not evidence of absence’ can be easily countered when we consider the general picture. Over a century of excavations in the City of David have produced surprisingly meager remains from the late sixteenth to mid-eighth centuries BCE [800 years]. They amount to no more than a few walls and a modest quantity of pottery sherds mostly found in erosion debris. The situation has been found to be the same at every excavated site in Jerusalem. The suggestion that substantial tenth-century BCE building remains did exist in Jerusalem but were obliterated by erosion or

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99 Matthew A. Baum, *Soft news goes to war...* (Princeton NJ 2003), p. 283
massive building activity in later generations [the thesis Ev Cochrane offered for areas in Mesopotamia in volume II] is simply untenable, since impressive structures from both the earlier Middle Bronze Age (c. 2000-1500 BCE) and the later Iron Age II (c. 750-586 BCE) have survived.”¹⁰⁰

It simply never occurred to historians that two great Dark Ages do not exist and are a manufactured construct of their own to maintain the established chronology. Kitchen nevertheless has attempted to prove this Dark Age in Palestine must be filled by biblical history. In order to prove Israel existed at this time as an empire, he thus is forced to explain why there is no evidence for the existence of it in Israel. Here is how he proves that nothing is something, or no evidence is evidence:

“It is often remarked that, outside the pages of the Hebrew Bible, there is no continuity between the isolated mention of ‘Israel’ on the victory stela of the pharaoh Merenptah in 1209 BC and the ninth-century ‘Divided Monarchy’ of Israel and Judah, when we find Ahab of Israel listed as one of many opponents of Shalmaneser III at the Battle of Qarqar in 853 BC. Therefore, it is alleged, we should treat Merenptah’s Israel as merely a ‘proto-Israel’ (W.G. Dever’s currently favourite term) rather than the real thing …

“However, the lack of continuity does not reside in the history of early Israel itself, but in the failure in witness by external, nonbiblical sources, and for very specific reasons. After Merenptah, only one other pharaoh—Rameses III …—was involved in the Levant until the mid-tenth century BC. Virtually all of the topographical lists from his reign are simply re-editions of those of earlier kings, and thus do not reflect peoples, places, or conditions of his own time. The same is true of the Syrian war reliefs in his temple at Karnak and Medinet Habu in Thebes. The only specific sources for his dealings with the Levant are (1) the texts and scenes of his conflict with the Sea Peoples … and (2) the strictly historical summary in his testament in Papyrus Harris I. This does at least add knowledge of his having chastised the inhabitants of Se’ir, but little else. One may add [to this] taxation ostraca from … southwest Canaan. There was here no conflict with Israel up in the hills, and hence no occasion to mention them. And no pharaoh ever again went to war in Canaan until after 970 BC … and then in 925 BC as did Shoshenq I (‘Shishak’) when specific peoples and states were not mentioned, only settlements … Thereafter, all specific Egyptian records of wars in Palestine cease entirely. In parallel with this, after Tukulti-Ninurta I of Assyria … again no Assyrian army even aspired to reach as far west as the North Mediterranean coast until Assurnasirpal II did so in 882 BC, with the brief exceptions of Tiglath-Pileser I in about 1100 BC and Assur-bel-kala soon after. … [N]o Assyrian king or chronicler had clear contact with, or had occasion to say anything about, peoples in Palestine during the 300/350 years between 1200 and 900/860 BC. And still less so Babylonia, caught up in purely local rivalries with Assyria to her north and Elam

¹⁰⁰ Finkelstein and Silberman, David and Solomon, op.cit., p. 95
to her southeast. In these circumstances of Great Power-eclipse … it is a total waste of time to complain of no mentions of either early Israel consolidating in Canaan or a locally expanding Israel (David and Solomon) reaching into Transjordan and south or even central Syria. Such data never existed because there was no occasion for them; but Israel did exist, as the OT [Old Testament] documentation makes clear, supported by relevant indirect data.

“And what about Israel’s closer neighbors in Phoenicia and Aram, or Transjordan? The facts are very simple. No archives of inscriptions have ever yet been found anywhere in Iron Age Phoenicia … Nothing whatsoever has come from Damascus … And very few major Aramean inscriptions come from anywhere else …”

This is what Kitchen’s evidence amounts to: No nation near or distant mentions Israel during this period. No written or archaeological evidence exists in Jerusalem and Israel which was supposedly building its nation at this time. In Israel/Jerusalem there are neither written records nor archaeological remains reflecting this growing thriving nation during this Dark Age. Therefore, Israel, according to Kitchen, existed without internal or external evidence that proves anything one way or the other for this time. Kitchen of course knows an Israelite empire in fact existed. How does he know? Because according to him “Israel did exist, as the OT [Old Testament] documentation makes clear.” In other words, Kitchen knows Israel existed during this Dark Age because “the Bible tells him so!” When one does not have written or archeological evidence in the ground in Israel or elsewhere for that period, one hasn’t any evidence of anything. Contrary to Kitchen’s lack of evidence and logic, one knows something exists because there is evidence for it, and one knows something does not exist when there is no evidence for it.

In terms of the established chronology, the Dark Age in Palestine runs from the Amarna Age, ca. 1500, to 850 B.C., or about 650 years. But this is not the end of the problem, because there is a second Palestinian Dark Age running from the time of the Hebrew exile in Babylon down to the time of the Maccabees, lasting about 250 years. Bob Becking et al. show:

“It has often been stated that the 5th cent. BCE, the time where tradition places Ezra and Nehemiah, is a dark age. But if we have regard to the fictitious character of both Ezra and Nehemiah, and to the Hellenistic date of the books ascribed to them, this age is even darker than is often thought.”

In this same respect, James Alan Montgomery reports:

“We now come to the most difficult field of the whole of our historical quest, the age in which we must look for the definite separation of the Samaritan community

[which settled in Palestine during the Hebrew exile in Babylon]. This is the ‘Dark Age’ of Jewish history, covering the Exile, the Return [to Palestine], with its several stages, and the remaining obscure period until the fall of the Persian empire. There have survived many prophetic books and fragments [supposedly] belonging to this age; its history is set forth in the Book of Ezra-Nehemia. But nowhere, in the eyes of Biblical criticism, does so much uncertainty lie concerning the worth and meaning of the [biblical] historical data as with regard to this latter book; and nowhere is there such confusion and conflict of critical theories attempting to reconstruct the actual history, as amongst the studies of this period.”

Emmet J. Sweeney summarized this Dark Age:

“Commentators frequently note that the Jews, most assiduous of record-keepers, left not a single document [in Palestine] 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.”

So not only do we have a 600-year or more Dark Age from ca. 1500 to 850 B.C., we have a 250-year Dark Age from the fifth to the mid-second century B.C. In all we have at least 850 years of Hebrew history that are Dark Ages. Above and beyond all that, the astronomical evidence for the Neo-Assyrians/Persians places them in Persian and Hellenistic times. All this means that Hebrew history and chronology must be shortened along with the other civilizations of the ancient time. We will, nevertheless, return to David and Solomon below in terms of the short chronology.

CLIMATE CHANGE, THE PIG, THE CAMEL, AND CHRONOLOGY

“The level of the Dead Sea dropped more than three hundred feet.”
Mark Bowen, Thin Ice
(NY 2005), p. 387

If, as we have shown, biblical history aligns itself astronomically with Neo-Assyrian/Persian times and does not have up to 1000 years of Dark Ages, it should also correlate with the climatic change in the 8th century B.C. that aridified the ancient Near East. This would be a natural fall-out that is derived from the short

103 James Alan Montgomery, The Samaritans, The Earliest Jewish Sect: Their History, Theology and Literature (Philadelphia PA 1907), pp. 57-58
chronology. Not only does the entire Near East dry out after the 8th century B.C., but so, too, should Palestine, and this should be reflected in several ways. We therefore turn to describe this climate change in Palestine and the response of the people to that new, drier environment. Thompson, who made a detailed study of agriculture in this region, describes the more favorable, lush conditions that existed there before the change:

“The ecology of the central hills [in Palestine] had previously provided barriers to wide-spread settlement that placed a cap on the capacity of the population to grow throughout most of the second millennium BCE. Over the four hundred years before the Mycenaeans [conventionally dated to 1200 B.C.], drought struck most of the highland [which] had been covered with untamed forests.”

What were these lush conditions like in Palestine prior to the onset of drought? Thompson has told us the “highland had been covered with untamed forests.” Alexandra Nibbi, in this respect, cites the work of Aharon Horowitz as to the type of forest vegetation that covered the Palestine-Lebanon-Syrian region:

“He recorded dry phases around 2250 and 950 BC. This study found that pollens present indicated a mixture of trees for northern Israel predominantly quercus [oak], pinus [pine], olea [evergreen trees and shrubs], pistacia [pistachio], none of them especially prevalent … Horowitz concluded that, in general, the flora of Israel used to resemble that of present-day Lebanon. The Lebanese-type forest, in which oaks prevail, apparently occupied the mountain areas of central and northern Israel, while to the south, olives seem to have flourished, together with Mediterranean-type maquis [dense shrub vegetation].”

The reader should note two elements of Nibbi’s presentation. First, there were two distinct dry periods that occurred which Horowitz, employing the established chronology, dated to 2250 and 950 B.C. but which we have dated throughout these three volumes to around 1500 and 800-700 B.C. Second, after the 800-700 B.C. date the vegetation in Israel did not resemble the Lebanon-type oak forest genre but it no longer existed in such great numbers or entirely disappeared. This second point indicates that the final climate change was one of great aridity and most significantly it was a permanent change as with all the other ancient nations presented in this series. Thompson further tells us:

“From the period of approximately 1200 to 1000 B.C. … there is abundant evidence in support of a long period of drought and recurrent famine [in Palestine and elsewhere] … The extensive deterioration of the Mediterranean basin shoreline is closely correlated with a global climatic change. In contrast to the dominant climatic regime after 1000 B.C., a Sub-Atlantic period of aridity, with an approximately 20% decrease in rainfall and rising temperatures of 2.0°-3.0° C.

105 Thompson, The Mythic Past, op.cit., p. 161
106 Nibbi, op.cit., p. 10
[Celsius] above normal, had become particularly acute... During the early first-millennium ... rainfall patterns in the Near East again reached a level comparable to that of modern times. At the close of the Late Bronze period ... a sharp increase in aridity took place throughout the Near East.”

But presently rainfall is hardly sufficient in most of the Near East to conduct agriculture without some form of irrigation. Thompson continues:

“The effects of even minor droughts on marginal areas that lie close to the [latitudinal] border of aridity have often been disastrous. Whereas in better watered regions the effects of drought appear only in terms of a scarcity in food supply, in regions such as southern and eastern Palestine (located ... on the border of aridity demarcating agricultural from steppelands) even relatively minor fluctuations of climate, when maintained or recurrent over years, can result in serious ecological and economic dislocation with radical political and social consequences. Desiccation becomes particularly marked in areas where irrigation is not employed. Although downward trends in rainfall fluctuations have a particularly harsh impact on steppe and marginal zones such as the coastal plain, the Beersheva Basin and the Judean hills, prime agricultural areas such as the central riverain valleys or upland regions of typically very high rainfall patterns display a greater resistance to such droughts, and consequently display a greater stability in settlement. In a year of severe aridity when rainfall in the Upper Galilee drops 15-20% below the norm, the reduced precipitation is still adequate to support most forms of dry agriculture, albeit at lower levels of productivity. However, the same weather cycle may lower rainfall in the Beersheva Basin by as much as 50% or more, drying up grasslands and, if the drought persists over years, transforming agricultural land to steppe and steppelands to desert. In truly arid and steppe regions (where pastoralism is a more dominant aspect of the economy) the population is most severely affected by drought when its severity and duration cause both a lowering of the water table and a reduced availability of grasslands, inevitably leading to overgrazing and subsequent severe erosion, creating ecological deterioration from which the region may take centuries to recover.”

Here, then, is the problem: Thompson has a drought that supposedly lasts 200 years. During this time the agricultural lands turn either to steppe or desert. People are forced to turn more and more to pastoralism to survive, and raise flocks of sheep, goats, or perhaps cattle. But these have a very deleterious effect on the land by grazing or more accurately overgrazing which inevitably leads to “subsequent severe erosion creating ecological deterioration from which the region may take centuries to recover.” That being the case, the land in Palestine subject to these conditions would after around 1000 B.C. take between 200 to 300 years to recover,

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107 Thompson, *op.cit.*, p. 215
or there would have been desert and near desert conditions in that region until around 750-700 B.C., a period of 250 or more years. This covers the region all throughout the Dark Ages that supposedly struck Greece, Anatolia, etc. But since there were no such Dark Ages, the drought would have occurred around 750-700 B.C. or later, and the climate did not shift back to much better agricultural conditions. This destruction of the land goes even further, according to Thompson:

“Even in those more stable regions that normally support a form of Mediterranean economy, a change in the rainfall patterns, when extended over a lengthy period can cause long standing, radical changes in vegetation; for example, a study of wood charcoals from archaeological sites in the highland area just north of the Negev indicates a shift from predominantly Mediterranean to Saharan vegetation forms. This change in types of vegetation corresponds to the transition from the end of the Late Bronze to the Iron I period. In other areas, where irrigation agriculture is supported or where rainfall is normally abundant, the onset of a period of severe aridity can be particularly disruptive and ultimately effect [sic] long term survival, because of a lack of indigenous drought resistant types of vegetation in the immediate area. Both water and wind erosion also dramatically increase with drought conditions.”

In essence, all these various regions had probably been so degraded that desert or desert-like conditions prevailed. The soil was largely eroded or blown away and it would have taken centuries for new growth of xerophytes [plants that are able to survive in arid climates] to pioneer into these regions to very gradually convert the sand to a soil richer in organic materials to allow other types of plants to take advantage of the richer conditions to grow and so on. One does not turn a desert or desert-like sandy environment into one that allows more lush vegetation to grow overnight but over centuries. Thompson adds:

“In 1966 R. Carpenter first proposed a global climatic change as the cause of the Mycenaean decline and collapse that closed the Late Bronze Age in the Aegean … The dissertation of D.L. Donley confirmed that the existence of a climatic change … that had brought about drought conditions as posited by Carpenter for Mycenae, was synchronous throughout the hemisphere.”

As in the Sahara, in the Indus and other regions there had to be adjustments by the population to these new conditions, or the region had to be abandoned. The Garamantes in the Libyan desert interior built “nearly a thousand miles of irrigation galleries, rough-hewn out of limestone to channel the flow [of water] from underground springs.” The Babylonians on the southern plain of Mesopotamia dug thousands of irrigation canals to water that area. The peoples of Arabia built dams to catch whatever water flowed from winter rains to irrigate their

109 Ibid., pp. 216-217
110 Ibid., pp. 218-219
land. Over time, however, these reclamation projects failed for one reason or other. In large measure, most regions were abandoned. In Palestine, under similar conditions, much land was abandoned and new techniques were employed by the people there to carry on agriculture. Elsewhere Thompson reports:

“Farming in Palestine entered a period of crisis. Towns were abandoned … Some were destroyed by earthquake and fire, others by military force. All faced the threat of economic and political collapse. Some, like Hazor, were partially destroyed, but then resettled in impoverished conditions. Others, like the town of Taanah in the Jezreel valley, were abandoned completely. Still others, like neighboring Megiddo, were able to maintain their occupation throughout this period of change and transition. The agricultural heartland of Palestine faced significant disintegration. The lowlands suffered a severe drop in total population, though many farmers were able to hang on by opening new lands in less productive areas. The population in these lowland valleys was scattered in smaller, more viable units spread over increasingly larger areas. Many of the displaced lowlanders were encouraged by Egypt’s continuing need for timber and olive oil to move out of the lowlands and into the lower Galilee and the central highland south of Jezreel to cut timber, and after clearing new land, to create new [agricultural] terraces and orchards.”\(^\text{111}\)

It was this terracing that enabled the Palestinian population to rebuild their economy with help from Egyptian trade. Egypt needed timber and olive oil; the Palestinians needed grain. This interaction stabilized the region and led to improvements in agriculture.\(^\text{112}\) As Thompson further shows:

“This was far from a subsistence economy. The settlers were hardly independent. The different regions for herding and terracing all required cooperation. Opening the land to production involved not only land clearance, but also, in the case of terracing, a decade-long process of land development. The cash crops involved in herding and olive oil production … were the most important crops of these highland regions …

“Where terracing and olives governed the economy, and where the total population had been considerably smaller, the population [then] grew by a factor from 2.5 to 5.

“Both olive and fruit production required settlers to solve the ecological problem related to limited water resources. There were also problems of land clearance and terrace building. Once a core [agricultural] settlement was established in each subregion … only ecological capacity seems to have limited the expansion of settlement growth…”\(^\text{113}\)
Building terraces and cisterns to collect winter rain with canals or channels to conduct the water to the terraces, goes well back into Palestinian history, but its augmentation and amplification was caused by the great drought, we maintain because of how well it fits all the other scientific evidence for the short chronology, it occurred around 800-700 B.C. or later. As for dating the age of these terraces, we are told that “…dating terracing is notoriously difficult. Many scholars date terraces from the architecture of related structures, which, as these scholars know, can be unreliable but is the only available information. The Negev of Israel is one zone of ancient terracing that also depended upon manipulating a whole watershed to harvest enough water for agriculture. There a variety of systems collected [rainfall] runoff and diverted [it] onto terraced fields to produce crops in a region generally too arid for rain-fed agriculture.”

A number of such terraces in Palestine are described by Arieh Singer in this manner:

“Humans … tried to cope … by taking advantage of the large amounts of water available from the increasing runoff and cultivating with them small patches of land on protected sites of the gully bottoms or where natural or artificial barriers on the slopes reduced soil erosion or even allowed soil accumulation to a depth permitting cultivation. Thus the impact of man on the desert landscape of the central Negev has been quite considerable. Hundreds of thousands of stone mounds were built on the hillslopes and thousands of check-dams were constructed in numerous wadis for the purpose of runoff agriculture. These man-made constructions in the landscape caused sedimentation in the terraced wadis. … The impact of ancient man, therefore, on the desert landscape of the region can be regarded as very positive. The ancient man-made constructions contributed to erosion control in the terraced valleys and enlarged the regional carrying capacity [for agricultural produce]. Thus, ancient man stabilized the soils in the wadis, increased their thickness, and succeeded in greening the desert through rainwater-harvesting techniques.”

That is, all this terracing, damming, and control of rainfall runoff occurred after the desiccation of the Near East after 750-700 B.C. This being the case, we can explain the Hebrew prohibition of the pig. By placing the historical and religious state of Israel after the great drought, it becomes clear that to raise pigs would be too costly in terms of providing them with grain. It was at this time that all the various civilizations in the ancient Near East also experienced the same desiccation and rather than expend grain to raise and fatten pigs, that grain could be and was employed to feed people. By having monarchical biblical history take place almost entirely in Persian and Hellenistic times, the Israelites were simply forced to do

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114 Krech, et al., op.cit., p. 1197
what nearly all their neighboring cultures were forced to do. Brian Hesse and Paula Wapnish have done extensive research on the pig and its relationship as an identifying marker of the Hebrews in Palestine. They state:

“Since we believe that religious legislation dealing with food has roots in wider cultural contexts of behavior, … the potential of pig remains as a marker of past social identity [of the Hebrews] must be evaluated empirically as well as theoretically. The exploitation of the pig was one of the most variable aspects of animal use in the ancient Near East.”\textsuperscript{116}

In terms of the established chronology, Hesse and Wapnish make the following point: “As we will observe, pigs became less visible in the record as the second millennium BCE proceeded, a pattern that continued through the Iron Age with the exceptions noted.”\textsuperscript{117}

To sum up this work, Amy Dockser Marcus tells us:

“In the past few years, they [Hesse and Wapnish] have focused their attention in particular on pig remains, and on discovering how avoidance of the animal became a signal part of Jewish … ethnic identity. Many archaeologists, particularly the Israelis, have been eagerly following their work, hoping that it might help ascertain when the Israelites emerged as a distinct ethnic group. Israel Finkelstein has led the charge on this issue, enthusiastically declaring in one article that ‘food taboos, more precisely pig taboos, are emerging as the main, if not the only avenue that can shed light on ethnic boundaries in the [Iron Age I period]. Specifically, this may be the most valuable tool for the study of ethnicity of a given … site.’

“Many archaeologists have assumed that the presence or absence of pig bones at different sites might be a useful index of ethnic identity in a given region. In many respects, this line of reasoning makes eminent sense. Food laws are one of the central and most visible expressions of Jewish religious community, and none is better known than the prohibition against eating pork. It is mentioned in Deuteronomy, which declares, ‘And the pig, because it divides the hoof but does not chew the cud, is unclean for you. You shall not eat their meat and you shall not touch their carcasses.’ A similar prohibition is reiterated in Leviticus. For years, as Hesse and Wapnish recount in one of their studies, archaeologists, sociologists, and historians all have been searching for the ‘one “true” rationale that lies behind the origin of pig disdain as expressed in the Bible.’ The debate had grown contentious and there were numerous theories from which to choose … Hesse and Wapnish rejected these explanations and proposed a different approach. They wanted to look beyond ideology and instead try to identify the myriad of forces—including economic, political, and ecological—that might have influenced the use or rejection of the pig.

“When their research was completed, they reached a startling conclusion, one at odds with the conclusive results that Finkelstein optimistically had predicted in

\textsuperscript{116} Hesse and Wapnish, \textit{op.cit.}, p. 240
\textsuperscript{117} Ibid., p. 247
his paper. After studying bone remains at archaeology sites throughout the Middle East, they determined that during the biblical period virtually no one in the region was eating pig. Similarly, the refusal to use pigs as sacrifices in official religious rituals hadn’t been limited to the Israelites, but was a common feature of religions throughout the Middle East.”

In terms of the short chronology, this prohibition comes after the great drought that befalls the ancient Near East after the eighth century B.C. The Hebrews vis-à-vis the short chronology are placed chronologically in Palestine as a distinct religious group around late Assyrian [not Neo-Assyrian/Persian] times prior to this climatic change and then directly after the great desiccation. Thus, they, like all the various other societies which developed this prohibition, were responding to that climatic change. In Palestine this is particularly shown by the Philistines who did eat pork but abandoned it in the time frame provided by the short chronology. Marcus in this important respect informs us that:

“… it was difficult to propose any valid generalizations about who used pigs and why. Some archaeologists had assumed early on that pig bones, which were found in southern coastal cities in Palestine known to have been conquered by the Philistines … might be a key to determining the presence of Philistines at other sites. However, the study by Hesse and Wapnish showed that while pig use was found in some Philistine cities in Canaan, such as the port of Askelon and the more mountainous sites of Ekron and Timna, not all Philistine sites had pig bones. The most interesting finding was that pig use by the Philistines took place only within the first century or two after their arrival in Canaan. By the time the Babylonian ruler Nebuchadnezzar [II] destroyed Askelon in 604 B.C.E. during one of his campaigns in Palestine, the Philistines had generally stopped eating pork, just like their [Hebrew] neighbors.”

This makes perfect sense in terms of the short chronology. The Israelites, Philistines, Babylonians, etc., all gave up eating pork very roughly over a period of a century or so after the great aridity, described above and elsewhere in these volumes, which created conditions that made it economically and ecologically irresponsible to raise pigs. The great climate change and the prohibition of pork in these regions go hand in hand with the short chronology. Israel was not unique in this sense but shared, and succumbed to, the same climate conditions as did all its neighbors.

In terms of the camel in biblical history and chronology, the very same conditions that led to the prohibition of the pig led to the growth and domestic use of the camel with the drying out of the Near East after the eighth century B.C. There is, indeed, an anachronism in the Bible which is cleared up by the short chronology: it is the camel. Finkelstein and Silberman explain:

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118 Marcus, op.cit., pp. 22-24
119 Ibid., pp. 24-25
“The biblical text reveals some clear clues that can narrow down the time of its final composition. Take the repeated mention of camels, for instance. The stories of the patriarchs are packed with camels, usually herds of camels; but as in the story of Joseph’s sale by his brothers into slavery (Genesis 37:25), camels are also described as beasts of burden used in caravan trade. We now know [based on the established chronology] through archaeological research that camels were not domesticated as beasts of burden earlier than the late second millennium [B.C.] and were not widely used in that capacity in the ancient Near East until well after 1000 BCE. And an even more telling detail—the camel caravan carrying ‘gum, balm, and myrrh’ [to Egypt] in the Joseph story—reveals an obvious familiarity with the main products of the lucrative Arabian trade that flourished under the supervision of the Assyrian empire in the eighth-seventh centuries BCE.

“Indeed, excavations at the site of Tell Jemmeh in the southern coastal plain of Israel—a particularly important entrepôt on the main caravan route between Arabia and the Mediterranean—revealed a dramatic increase in the number of camel bones in the seventh century. The bones were almost exclusively of mature animals, suggesting that they were from traveling beasts of burden, not from locally raised herds (among which the bones of young animals would also be found). Indeed, precisely at this time [Neo-]Assyrian sources describe camels being used as pack animals in caravans. It was only then that camels became a common enough feature of the landscape to be included as an incidental detail in a literary narrative.”

In this respect the camel is not an anachronism in Hebrew history. A.R. Millard in his defense of biblical history has argued that the inclusion of the camels discussed in Genesis allows that these beasts of burden existed in the patriarchal period of the second millennium B.C., suggesting “It is as logical to treat the passages in Gen. 12:16, 24 as valuable evidence for the presence of camels at that [more ancient] time as to view them as anachronistic.”

The Hebrews lived from late Assyrian [not Neo-Assyrian] times, in the first millennium down into the Persian and Hellenistic periods and thus had direct contact with, and knowledge of, the camel.

There may also be a unique and fascinating aspect related to the domestication and widespread use of camels in the Middle East in that late era. That has to do with the plagues that swept the region at that time, in terms of the short chronology. With the great development of cities and widespread trade, various diseases/plagues would have spread around the ancient Near East. What we would expect, therefore, is that these plagues would be associated with specific nations and these, in turn, would in the short chronology be dated to the period of about the eighth century B.C. (the 700s) and continue forward in time. This is just what we find. For example, with respect to the Philistines we have a report of a plague in

120 Finkelstein and Silberman, The Bible Unearthed, op.cit., p. 37
“… connection with rodents in the passage (5:6, in a sentence preserved in the Septuagint 6:4) [which] suggests that the affliction [of the Philistines] is infectious and is possibly bubonic plague. The Hebrew term [for the symptoms observed] translated ‘tumors’ could easily be used to represent the boils (buboes) that are symptomatic of the plague. There is some question, however, whether bubonic plague is attested in the ancient Near East at this early date … others have proposed that the plague be understood as bacillary dysentery which can be transmitted through food infected by mice. If this is correct, however, the connection of the swellings is unclear.”

The authors can only suggest that such a plague is of too early a date based on the established chronology. In the short chronology no such impediment opposes this connection to bubonic plague. H.W.F. Saggs briefly reports that the Old Babylonians who, we have shown in volume II, lived in Persian times, discuss plagues, as do the Hittites, whom we have identified as Lydians in volume I. Both lived somewhat close to the Persians. Both experienced plagues which fall after 750 B.C.:

“For long south Mesopotamia had the most heavily populated cities and the greatest concentration of them. Conditions were ideal for the outbreak of epidemics, including bubonic plague. Epidemic disease was such an accepted part of the scheme of things that there were prominent plague gods whose duty was to punish cities by this affliction. The word for epidemic disease was mutanu, meaning something like certain death, from mutu, the ordinary word for ‘death.’ The term denoted fatal pestilence generally rather than any specific disease, and could be applied to animal epidemics as well as to human. Akkadian [Assyrian] texts abound with references to mutanu. An old Babylonian letter reports ‘There is at present pestilence in the city, but it is not the pestilence of the god Nergal.’ Obviously Nergal’s pestilence was some specific epidemic, perhaps bubonic plague. An omen [of that time] speaks of plague gods marching with the troops, perhaps a reference to typhus, since that disease often breaks out in armies. Other texts speak of afflicted cities with daily deaths, whole countries hit by fatal epidemic and epidemics continuing for years. …

“The late fourteenth-century Hittite king in a prayer to the gods of Hatti-land gave an account of the course of such an epidemic, probably bubonic plague or typhus. It began with a military attack by Hatti-land on … a part of Syria held by Egypt. The Hittites won the war and brought back Egyptian prisoners. But then fatal plague broke out among the prisoners and spread to the Hittites. At the time of the prayer it had raged for twenty years.”

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122 The IVP Bible Background Commentary: Old Testament, John H. Walton et al., eds. (Downers Grove IL 2000), p. 228
123 Saggs, op.cit., pp. 124-125
This material is presented as tentative and needs much more work before anything definitive may be said regarding epidemics with the growth of the ancient Industrial Revolution.

**GENETICS AND BIBLICAL CHRONOLOGY**

Both with astronomical dating and climatic evidence we have shown that biblical history must be placed mostly in Persian and Hellenistic times in accordance with the philological evidence.

The great impediment related to finding and accepting an accurate biblical chronology stems mainly from having erected it on faulty foundations. Thus there are now three forms of evidence that place the history of many of the Divided Monarchy kings just prior to and down into the Persian and Hellenistic epochs—philology, astronomy, and climatology. Beyond this, we now turn to genetics to continue evaluating the validity of the established chronology, as opposed to that of Heinsohn, Sweeney, Rose and in part Velikovsky’s revisions. If the established chronology is valid, genetic evidence will indicate, not precisely, but generally, that the Exodus took place in the second millennium B.C. That is, if the Hebrews were a separate people in Egypt around 1300 B.C., as the conventional chronology indicates, and then as a separate people migrated to Palestine and settled there, and thereafter were a separate, established people, tied by bonds of religious faith to each other as well as through marriage, the genetic evidence would show that they lived as far back as the mid-second millennium B.C. or somewhat earlier. If, on the other hand, the short chronology is correct, then the Hebrews are mainly a first millennium B.C. people connected by faith and marriage to each other, and the genetic evidence should indicate this, showing their status as a separate people goes back to some point in the first millennium B.C.

Although the genetic evidence is not precise, it is germane in the sense that it does provide a general time period in which the Hebrews, as a separate entity, existed among the ancient nations of the Near East. The overall date for the national organization of the Hebrew people as a separate cultural and ethnological unit will be exhibited in genetic evidence because Jews have strongly tended to see themselves as isolated from, and very different to, the pagan peoples all around them and therefore tended to intermarry within their religious/cultural group for millennia. Their religion, which had only one invisible god in those ancient pagan times made them unique and they were seen as a separate people. To many of the pagan peoples of the ancient world, they were a godless people without idols and having one invisible god. Thus even in ancient times they had little cultural or religious connection with the peoples around them and intermarriage with pagans
was akin to cultural and religious suicide. Even though the Hebrews lived outside of Palestine, this did not change, and where they settled in large numbers, such as Alexandria or Rome, etc., they still were isolated in ghettos where, up to 1800 A.D., they generally remained a separate people all through their history in Europe. Historically, their separation, isolation and ban on intermarriage and the resultant genetic make-up has made the Jews an excellent laboratory subject to trace the depth in time of their historic/chronological origin. As Dr Harry Ostrer, Chairman of the Human Genetics Program at the New York University School of Medicine states in Yaakov Kleiman’s book DNA & Tradition:

“Did you ever wonder if 2000 years of recorded history could be preserved in the genetic record? Recent work from genetic labs has validated the Biblical record of a Semitic people who chose a Jewish way of life several thousand years ago. These observations are the biological equivalent to the discovery of the Dead Sea Scrolls, suggesting that, despite 2000 years of Diaspora, the relatedness of the Jews of Eastern European (‘Ashkenazim’), North African (‘Sephardim’) and Middle Eastern (‘Oriental’) origin can be demonstrated by genetic marker analysis.”

If these three different groups of Jews, Eastern European ‘Ashkenazim,’ North African ‘Sephardim,’ and Middle Eastern ‘Orientals,’ each with different appearances and separated in large measure from each other for thousands of years, in different regions, are descended from one group, then they should have in common certain distinctive genetic markers that prove this common origin. Of particular importance, that origin should go back to the time the Jewish culture/religion had established itself. This is particularly evident in the group known as the Kohanim or Cohens who were the first high priests:

“Jewish tradition, based on statements in the Bible, is that all Kohanim are direct descendents of Aaron. Males of the tribe of Levi, of which Aaron and Moses were descendants, were assigned special religious responsibilities. The male descendants of Aaron were then selected to serve as priests – Kohanim. The family line has not been broken until today, passed from father to son without interruption from Aaron for 3,300 years – more than 100 generations!”

Therefore, if the Kohanim were descended from one individual, they should exhibit this in their genetic make-up. In addressing this scientific question, Nicholas Wade, a science reporter for the New York Times, reports:

“The population history of Jews has been studied more than that of most other groups and has yielded one surprise after another. The population’s first remarkable feature, from which all the others follow, is that Jews have to a significant extent married among themselves over the centuries. Jewish

124 Yaakov Kleiman, DNA & Tradition: The Genetic Link to the Ancient Hebrews (Englewood NJ/Prestwich UK 2004), pp. 10-11

125 Ibid., p. 18
communities, in other words, have been largely endogamous, at least until recent times, which means the population’s gene pool has had time to develop its own private history, and this genetic history has shed light on many historical events.

“An important consequence of endogamy is that the gene pool is not diluted through intermarriage and so the selective pressures that may act on a population are able, over time, to favor specific genetic variations. A striking possibility, plausible though not yet confirmed, is that one particular Jewish community, the Ashkenazim of northern and central Europe, lived for a long time under a harsh selective pressure that raised certain variant genes to high frequency. These variant genes are well known to physicians because of their serious side-effects—when inherited from both parents, they cause a variety of serious diseases.”

That is, when a group is endogamous, certain genetic traits are preserved and can create genetic diseases. Genetic diseases when they are found predominantly within a particular area or population are an excellent indication of endogamy. For example, people from Greece and Italy because of their generally long history of intermarriage within their culture, tend to have a much higher percentage of a genetic disorder known as thalassemia. In the Jewish culture/religion, there are four genetic diseases that tend to be endemic to Jews. These show

“… a distinctive set of Mendelian diseases. The mutations that cause these diseases can hit at random anywhere in the genome, so that it would not be expected to favor any particular category of gene. But no fewer than four of the Ashkenazic [Eastern European Jewish] Mendelian diseases affect the cell’s management of chemicals known as sphingolipids, so called because their discoverer could not resolve the sphinx-like riddle of what they did. The four sphingolipid diseases are Tay-Sachs, Gaucher, Niemann-Pick and mucolipidosis type IV.”

Thus there is clear genetic evidence that shows that, due to the long history of Jewish intermarriage, over time certain genes became prevalent in that community that, when given by both father and mother to a child, lead to these sphingolipid diseases.

The significant question that follows this understanding is: are there markers related in this genetic group that can pin-point long-term Jewish inheritance and that reflect the longevity of the Hebrews as a distinct people? This was found in the male line of the lineage as Wade further informs us:

“The patrilineal [male] priestly tradition still exists and has afforded geneticists another deep insight into Jewish history. Cohens and levites continue to carry out ceremonial roles in certain congregations. Cohens are first called to the reading of the Torah in synagogue, and are asked on special occasions to bless the congregation. (The cohen’s blessing signaled by holding up the hand with a split

\[126\] Nicholas Wade, Before the Dawn: Recovering the Lost History of Our Ancestors (NY 2006), p. 145
\[127\] Ibid., p. 252
between the middle and the ring fingers, is familiar to many non-Jews; it was adapted by [the actor] Leonard Nimoy, who remembered seeing it as a boy in synagogue, as the Vulcan greeting for his role as Spock in Star Trek).

“Oral tradition holds that all cohens, or cohanim, are descended from Aaron, the brother of Moses and the first high priest. The Jewish priesthood is thought [based on the established chronology] to have been established some 3,300 years ago and to have passed from father to son ever since. This fact was on the mind of Karl Skorecki, a medical researcher at the Technion-Israel Institute of Technology in Haifa. One morning when he was sitting in synagogue and the Torah was being read, the cohen doing the first reading was a Sephardic Jew. Skorecki, whose family is Ashkenazic, himself comes from a line of cohanim. The thought occurred to him that though he and the Sephardi differed strongly in physical appearance, they must both have inherited some Y chromosome from Aaron, if oral tradition was correct.

“Skorecki called Michael Hammer, the University of Arizona geneticist, who agreed with his inference and set about analyzing the Y chromosomes of cohanim from both the Ashkenazic and Sephardic communities. Despite the millennium or so for which the two communities have been separate, and the geographical distance between them, Hammer and his colleagues found that the cohanim of both groups did indeed possess a distinctive genetic signature.

“The signature is a set of DNA sequences at two specific sites on the male chromosome. It is known as the cohen modal haploid, a geneticist’s phrase meaning the set of DNA variations typical of cohens. The Hammer team detected the cohen modal haploid in 45% of Ashkenazic cohanim and in 70% of Sephardic cohanim. The finding substantially confirmed the oral tradition that cohanim are descended from a single individual. This person was presumably a founding high priest and could perhaps have been Aaron himself if indeed there was an Aaron …”

“To learn more about when the ancestor of all the cohanim might have lived, another team of geneticists including Skorecki and David Goldstein has looked at the variations that have developed on the cohen modal haplotype. The Goldstein team estimates that about 106 generations must have occurred to account for the observed amount of variation that has built up on the cohen modal haplotype. Assuming 30 years per generation, this means the ancestor of the cohanim lived some 3,180 years ago (or 2,650 years ago, if a generation time of 25 years is preferred).”

In this same regard, Mario Livio reports:

“What Skorecki and his colleagues realized was that in the DNA, the Y chromosome passes solely from father to son, precisely like the status of a cohen. By studying certain markers on the Y chromosome of 306 Jewish men, including 106 cohenim, Skorecki, Thomas and their collaborators were able to show that the cohenim indeed have some Y chromosome features that make them distinct from

\[128\] Ibid., pp. 248-249
other Jews. Moreover the researchers found suggestive evidence that the shared genetic material stems from an ancestor who lived between 2,100 to 3,250 years ago [or between 100 and 1250 B.C.].”

Kleiman in this respect writes: “These [Jewish] populations may best represent the paternal gene pool of the ancient Jewish/Hebrew population dating back to the First Temple period, before the Babylonian exile (approx. 2,500 years ago).”

Ernest L. Abel presents the length of this genetic lineage, stating “Using sophisticated models of genetic change, the scientists traced the origin of the [cohanim] priestly Y chromosome to some time about … 2000 to 3000 years ago.”

The various estimates are generally closely related to from ca. 2100 to 3250 years ago. However, these estimates are based strictly on the genetic evidence which gives about a 1000 to 1300 year span during which this ancestor or these ancestors originated. But because we have precise astronomical evidence for the chronology of the Hebrew kings from just prior to and into Persian-Hellenistic times, we can narrow down this estimate considerably within the span of the first millennium. This is corroborated by and correlated with the Hebrew prohibition of eating pork some time after 800 B.C. and Hebrew knowledge of the camel that reinforces the same chronology. Because the scientific genetic evidence of Jewish population longevity into the past must be congruent with these other forms of forensic historical evidence, the chronology all corroborates, correlates, and converges for a date well inside the first millennium B.C. There is no scientific chronological evidence to support the dating of the Hebrews into the second millennium B.C. that corroborates, correlates, and converges to support the established Hebrew chronology. The minimalists and maximalists have no such evidence to support any such contentions.

129 Mario Livio, op.cit., p. 187
130 Kleiman, op.cit., p. 30
CHAPTER 8
THE ALPHABET AND HEBREW CHRONOLOGY

As with the scientific evidence cited above, the alphabet, which originated in Canaan, must also correlate, corroborate, converge, and be congruent with our chronology as well. According to the established chronology, the earliest alphabet originated in the early part of the second millennium B.C. The Heinsohn, Sweeney, and Rose short chronology model moves that alphabetic origin forward in time by about 1000 years to around 800-700 B.C., at the onset of the ancient Industrial Revolution in the Near East. Therefore, the evidence regarding the alphabet must be shown to either agree with the established chronology or with that of Heinsohn, Sweeney, and Rose, or with neither. Again, as with so much else, it will be evident that this alphabetic material agrees with the short chronology. Of the earliest alphabet, based directly on the established chronology, Philip J. Long and Lawrence E. Stager report:

“The alphabet was invented by the Canaanites early in the second millennium BCE. The first alphabetic system, consisting of the twenty-two-letter Phoenician script (also [used] in the standard Hebrew-Aramaic alphabet), materialized about 1100 BCE. The presence of alphabetic writing in ancient Israel does not mean the populace was literate at that time. Determining the spread of literacy in ancient Israel is a difficult undertaking.

“Among the earliest examples of alphabetic writing were the Proto-Sinaitic inscriptions written in a linear alphabetic script. In 1905, W.M. Flinders Petrie discovered eleven inscriptions at modern Serabit el-Khadem in the southern Sinai Peninsula. These inscriptions, incised in rock and on stone, may have been carved by Canaanite laborers [or slaves] working the Egyptian turquoise mines in the area. Petrie dated the Proto-Sinaitic inscriptions to the early fifteenth century BCE and suggested the script represented a linear alphabet. Albright dated them provisionally between 1525 and 1475 BCE. Egyptologist Alan Gardiner dated them to the eighteenth century [B.C.], corresponding with the Middle Kingdom of Egypt, and he seems to be correct. The total number of these inscriptions now exceeds forty-five. A new discovery of alphabetic writing, carved in limestone cliffs in Wadi el-Hol, across from the royal city of Thebes in Upper Egypt, dates between 1900 and 1800 BCE. It seems likely that the Serabit el-Khadem writing, which is typologically similar, should be dated that early as well. Detailed study remains to be done.”

Along the same lines, John F. Healey reports:

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1 Philip J. King, Laurence E. Stager, Life in Biblical Israel (Louisville KY 2001), p. 302
“The writers of the Proto-Sinaitic or Proto-Canaanite inscriptions apparently did take this [advanced] step [of alphabetic writing] in the early to middle second millennium BC. The evidence is difficult and scholars do not agree on all points. The texts in question first became well known through a series of short inscriptions of c. 1700 BC onwards, carved by miners … in Sinai. Because the number of signs in these inscriptions was so small (less than thirty), it quickly became clear that this script was an alphabet and not a syllabary [syllabic script which can contain perhaps 100 or more syllabic signs for writing, such as in the Linear B script].

“Subsequently, other examples have been found in Palestine (Shechem, Gezer, Lachish), so we can be certain that we are dealing with a fairly widespread phenomenon. While we can never hope to know who invented the new so-called linear alphabet, two things seem clear. Firstly, there is clearly an Egyptian inspiration behind the invention, since there are some similarities of signs and the basic acrophonic principle [of using a sign to denote a sound] (which has no parallel in cuneiform) must have come from knowledge of the Egyptian script. Secondly, the texts are in Canaanite West Semitic, not Egyptian, so we can be fairly sure of an origin of the script in the Semitic area which had close cultural contact with Egypt.”

Here then we come to problems related to having the alphabet invented around 1800-1700 B.C. or earlier. Not only was it written in the Sinai, but in Shechem, Gezer, and Lachish in Palestine, and because such finds are rare, this script must have been employed in other areas of these regions as well, though it has not been found yet. In this respect, William M. Schniedewind’s comment is germane to the question:

“The alphabet had the power to democratize writing and made it possible for literacy to spread beyond the scribal classes [that wrote in hieroglyphic or cuneiform scripts]. This innovation also took the mystery out of writing… Although alphabetic writing made it much easier to learn to read and write, more than a millennium passed after its invention before we have evidence that literacy actually spread significantly beyond the scribal classes. Contrary to what has been suggested by the anthropologist Jack Goody and the classical scholar Eric Havelock, the invention of the alphabet did not automatically result in the spread of literacy … This was not because of imperfections of the early alphabets, but because these social developments are much more complex than can be accounted for simply by the invention of the alphabet. This said, we should not underestimate the potential of the alphabet as a technology that could transform society.”

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2 John F. Healey, The Early Alphabet (Berkeley CA 1990), pp. 16-17
4 William M. Schniedewind, How the Bible Became a Book: The Textualization of Ancient Israel (Cambridge UK/NY et al., 2004), p. 38
As with other technologies discussed in these three volumes, the invention of alphabetic writing went through something like a 1000-year Dark Age before it had any real impact on the societies of the ancient Near East. But according to the established chronology, the first peoples to employ these alphabetic scripts, the Phoenicians, Hebrews, and Aramaeans, only did so around 1100 B.C., or 700 to 800 years later. What historians are suggesting is that the alphabet was invented around 1800 B.C. but took 700 to 1000 years or more to become the written form of expression used by the various peoples of the Palestine region.

According to Finkelstein and Silberman, it wasn’t “UNTIL THE EIGHTH CENTURY BCE, WHEN THE FIRST SIGNS OF WIDESPREAD LITERACY APPEAR IN JUDAH.” In order to use an alphabet to write, one must learn, as does every school child, to say the ABCs, from a person or in some sort of school. This immensely important form of communication does not linger unused for 700 to 1000 or more years before becoming the primary vehicle of writing in a society. Having alphabetic scripts of slightly different forms in Shechem, Gezer, and Lachish indicates that there were some means of teaching students (via scribes or priests) their ABCs. What the proponents of the established chronology have bestowed upon history is a kind of alphabetic “undevelopment” that lasted between these earliest writings and the establishment of a full alphabetic communication program in those societies. Why would any society create schools or have tutors teach the alphabet to write and then fail to employ that immensely important communication tool fully in all its mercantile, taxation and other governmental record keeping? Why would these same societies cling to a much more difficult and cumbersome writing system, such as cuneiform, when a simpler method accomplished as much more easily? Record keeping was a fundamental pillar of all these societies, but for some unknown or obscure reason historians suggest that for almost a full millennium or more years not one of these societies nor its educated class recognized the value of this revolutionary development.

Interestingly, in Mycenae, supposedly during this very same alphabetic Dark Age, Linear B script was employed for just such record keeping. This simply does not make any kind of chronological sense. Consider that in the Middle Ages monks copied books by hand until Gutenberg invented movable type and almost in the historical twinkling of an eye, monk-copied books became obsolete, replaced by printed books. Into the 1800’s business communications were written by hand until the invention of the typewriter, and again, almost in the historical blink of an eye, the typewriter replaced the more difficult and cumbersome handwritten notes. In fact, even the dates assigned to these early alphabetic inscriptions are considered to

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5 Finkelstein, Silberman, David and Solomon, op.cit., p. 53 (capitalization added)
be highly conjectural, as Whitt explains, although the established chronology largely guides the range of time possible:

“Most of our knowledge about the early alphabetic inscriptions is uncertain. The inscriptions are very short and fragmentary, and only a few have an intelligible meaning. They were written before a standardized alphabet had been developed and thus show a wide variation in the direction [in which] the letters [were written]. Especially when the archaeological context is not certain, dating these early inscriptions is highly conjectural and not subject to any real controls. Although it is clear that the later letter forms are a simplification of a pictographic script, it does not follow that inscriptions with more pictographic letters are necessarily earlier than those [less] simplified letter forms. There is no reason that a more pictographic script could not have continued on in some locations even after the script had been simplified elsewhere. This makes dating by paleography more hazardous than is usually admitted, meaning that the consensus opinion on the topic—that there is a range of error of a century or so at the end of the second millennium [B.C.]—is probably too optimistic.”

Thus, at best, there is a “consensus,” not “proof,” that the dating of these early alphabets 600 to 700 years after they were supposedly first formulated is too optimistically in error by a century or so. As for the Hebrew alphabet, we run into the same problem as with the Egyptian use of various scripts all at the same time.

Frederick Cryer in this regard shows:

“Moreover, the Hebrew of the Old Testament itself divides into a standard Hebrew and a so-called ‘Late Biblical Hebrew’ which, however, cannot be shown to be later than the ‘standard’ type as the oldest extant manuscripts, found in the caves in the vicinity of Khirbet Qumran, are all contemporaneous witnesses…

“Furthermore, the Hebrew-language documents found in Qumran and in Masada and from elsewhere from around the intertestamental period reveal the existence of: 1 a characteristic ‘Qumran Hebrew’ … 2 a variety of proto-Mishraic Hebrew … 3 at least two different orthographies … 4 at least two scripts (one based on Aramaic ‘square’ script and one deriving from Phoenician tradition that go back to pre-exilic times … The difference could as easily be accounted for by social or regional distinctions within Syria-Palestinian society, as by the assumption that texts in so many different types of Hebrew can only be explained [by] an extensive historical development.”

The very same problem exists for the Phoenician alphabet:

“As with the earliest [Canaanite] alphabetic inscriptions, using paleography to date inscriptions written in the standard Phoenician script is highly problematic;

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6 Whitt, op.cit., pp. 2383-2384
7 Cruz-Uribe, op.cit., p. 192
8 Cryer, “Magic in Ancient Syria-Palestine”, op.cit., p. 112
this is especially true for those inscriptions thought to be from the ninth century [800’s B.C.] and earlier. The main problems are that most of these inscriptions do not come from clearly stratified archaeological contexts; that the inscriptions are too few, too short, and too fragmentary to give trustworthy typological sequences, and that there is no way to determine whether the peculiar features of a certain inscription are due to local variations in script or to its date [that has been assigned by philologists, archaeologists, or historians].”

In addition to the problem of why it took 700 to 800 years for the early Canaanite, Hebrew, Phoenician and other alphabets to be fully employed by these states for records, laws, religious rites, etc., the very dating that is being used to fix them in time is highly problematic. But there is an even worse problem related to all these later standard alphabetic scripts: The times when they existed are unknown. Furthermore, there is simply no archaeological evidence for the Iron Age cultures where these alphabetic writings have been employed. With regard to Phoenicia, Peter James et al. point out:

“A … situation prevails in Phoenicia, where the reported wealth and building feats of the great Hiramic age, contemporary with those of Solomon in Israel, have not been matched on the ground. For example, at Byblos archaeologists have long lamented the ‘curious fact’ of the general ‘absence of stratified levels from the Iron Age’. This has usually been blamed on the lack of excavation at the main ancient towns, which continue to be inhabited, as well as the current political situation. Nevertheless, the recent, albeit very small-scale, work at Tyre and Sarepta confirms the poor picture of early Iron Age Phoenician archaeology. The 10th-century finds from Tyre are meagre, while the evidence recovered from Sarepta is very limited … The uncomfortable conclusion, for those who adhere to both the conventional chronology and the biblical account, was spelt out by James Muhly:

“This is really quite remarkable. The great age of Phoenician mercantile activity, the time of Hiram I, of Solomon and the biblical accounts relating to Ezion-geber, the Tarshish fleet and three-year voyages to the Land of Ophir, is simply not documented in the archaeological record from Tyre and Sarepta’.”

In spite of this fact, H.W.F. Saggs wrote as though the archaeological evidence was, in fact, in evidence in Phoenicia:

“Shortly before 1000 BC the writing system which began as proto-Canaanite had stabilized into the form which we refer to as Phoenician. Most of the earliest Phoenician inscriptions come from Byblos, the most important being the inscription on the coffin of King Ahiram, of about 1000 BC.”

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9 Ibid., p. 2385
10 James et al., op.cit., p. 192
11 Saggs, op.cit., p. 83
With respect to Byblos, Velikovsky cites Nina Jidejian’s *Byblos Through the Ages*, p. 57, as follows:

“‘The results of excavations at Byblos have shown a curious fact which has been a source of discussion among scholars. In the excavated area at Byblos there is a complete absence of stratified levels of the Iron Age, that is for the period 1200-600 B.C.’

‘[Velikovsky continues:] There was found no stratified level to bridge the time between Ramses II and Nebuchadnezzar [II], or more than six hundred years on the conventional timetable. [Jidejian adds:] ‘The excavators were unable to perceive any stratification of the Iron Age, a period which must have been one of prosperity and intense commercial activity.’ It is known, for instance, that an emissary of Egyptian priests, named Wenamon, visited the place and the palace of the local king, supposedly in the eleventh century, but no vestiges of that palace have been discovered and only ‘large foundation stones of a building of the Persian period (550-330 B.C.) were unearthed to the east of the site. … [T]here are only a few fragments from Byblos to cover the Early and Middle Iron Ages’ or the said period from -1200 to -600.”

That is, the Phoenicians’ non-existent cities and peoples of the Early and Middle Iron Age were able to maintain their alphabetic script down to Greek times and pass it along to the Greeks. This remarkable feat is presented by Marc Van de Mieroop simply by saying, “the role of the Phoenicians in the spread of the alphabet is their most renowned accomplishment. Having preserved the use of the script in the Dark Age after 1200 [B.C.], the Phoenicians inspired all the alphabetic writing systems of their neighbors.” A very similar situation exists for the Hebrew alphabet in Jerusalem during the Early Iron Age. Based on the established chronology, which we discussed above, there was no city there at the time, showing a Dark Age from 1450 to 900 B.C. which we need not go into again.

Nearly all modern historians have postulated a Dark Age across most of the Near East from 1200 to 600 B.C., and in the same breath they say that the ancient alphabets of these regions became standardized and were passed along to the people of Greece or elsewhere while they apparently had no cities, no schools, no urban populations. We have the very same problem with the development of mathematics. It too was developed in the early second millennium B.C. and like the early alphabets lay dormant for almost a thousand years. There is no continuity of either mathematics or scripts across this abyss. The intermittent finds are simply dated by historians to fit the established chronology.

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12 Velikovsky, *Ramses II...*, op.cit., pp-80-81
THE UGARIT ABECEDARY

There is yet a further ancient alphabetic script found in the city of Ugarit, modern Ras Shamra on the northern coast of Syria. In this respect, Cyrus H. Gordon and Gary A. Rendsburg briefly explain:

“The most important corpus of ancient Near Eastern literature for the study of the Bible is the group of texts discovered at the north Syrian port of Ugarit. The documents date to the Amarna Age [ca. 1500 B.C.], during which time the city of Ugarit emerged as a major cosmopolitan center. The texts are written by stylus on clay [like cuneiform] in the fashion of the Mesopotamian scribes, but the system of spelling is alphabetic: each sign stands for a single sound, which is a Canaanite contribution … Among the texts found at Ugarit were copies of the ABCs taught in the Ugaritic schools; their fixed order of the letters is the one from which our own English ABC is ultimately derived.

“The Ugaritic language belongs to the northwest branch of Semitic, along with Hebrew, the other Canaanite dialects and Aramaic.”

Here we encounter another problem: while everywhere else in the Palestine region the Hebrew alphabet was not employed for trade, law, religious or other purposes for another 700 to 800 years, the people of Ugarit developed schools and taught their own form of the alphabet for all these purposes. According to Nicholas Ostler, “Hebrew is a close relative of Ugaritic, but not as close as Phoenician.” He adds: “Hebrew and Ugaritic were close enough, then, to share some fixed phrases.” Now we know that the Bible was written in Persian and Hellenistic times, as pointed out by the minimalists, and we are further told that the Ugaritic alphabet was employed from about 1550 to 1200 B.C., based on the established chronology. That means there was a 600-year break between the writings of Ugarit and those of the Hebrew Bible. It is here, once again, that we encounter the problem of language change over time. Phrases, idioms, and other forms of written and oral expression do not remain fixed over a 600-year-long period. What historians and philologists are suggesting is that after more than half a millennium Ugaritic and biblical Hebrew expressions did not change greatly but remained strikingly similar if not identical. Andrew Robinson writes that the Ugaritic

“… literary and religious texts written with only 27 signs … bear striking similarities, in theme and even in phrasing, to stories from parts of the Old

15 Ibid.
Testament. It seems that these biblical stories were written down many centuries before they were written in Hebrew [by the scribes of Ugarit].”

Gordon and Rendsburg echo this statement of the closeness between the two alphabetic literatures: “That Ugaritic is the greatest literary discovery from antiquity since the decipherment of the Egyptian hieroglyphs and Mesopotamian cuneiform is generally recognized. That it lies closer than any other literature to the Hebrew Bible is also well known.” Let us briefly examine one such expression in an Ugaritic text cited by Ostler:

“Now consider how the goddess Anath of Ugarit decks herself out to meet the emissaries of Baal:

‘She draws some water and bathes;
Sky-dew of the fatness of earth,
Spray of the Rider of the clouds;
Dew that the heavens do shed
Spray that is shed by the stars.’

“The words for ‘Sky-dew of the fatness of the earth’ are ṭi šmm šmn’rs. This is precisely what Isaac promises to Jacob (and denies to Esau) in the blessing

‘wə yīten hā alohîm m-ṭl ha-šānim û-mišmani hā-‘ares.’

“May God give you of dew of heaven and of the fatness of earth.”

After 600 or more years such an expression would have gone out of use or been replaced by another. For example, in the opening line of Chaucer’s Canterbury Tales he writes, transliterated into modern English “When April with the sweet root…”. Who, writing over 600 years later, in our present time, ever speaks commonly of “April with the sweet root”? We don’t use this expression because it has lost its cogency. Yet philologists expect us to accept that Ugaritic expressions stayed fixed in their usage and were commonly employed in the Syrian-Palestinian area 600 or more years later. Baruch Margalit specifies this problem in his chapter on “Ugaritic literature and the Hebrew Bible” thus:

“The discovery of the Ugaritic texts and their study during the past six decades have undoubtedly left an indelible imprint on our perception as on our comprehension of the Israelite literature preserved in the (so-called) Old Testament. The imprint [of Ugaritic in the Bible] is evident in the lexicographic clarification of individual words and expressions; in the elucidation of the mechanics as well as the dynamics of poetic style and structure; and, perhaps above all, in the history of religion …

17 Gordon and Rendsburg, op. cit., p. 93
18 Ostler, op. cit., p. 73
“It results in large measure from the dawning realization by biblical scholars of the spatial as well as temporal gaps between ancient Israel and the Late Bronze kingdom of Ugarit, a realization fuelled by the prevalent opinion among the leading Ugaritic specialists that the city of Ugarit was never part of the geopolitical unit known as ‘Canaan,’ that even the language we call ‘Ugaritic’ can not be classified without qualification as ‘Canaanite.’ It is this ‘de-Canaanization’ of Ugaritic literature which has precipitated a decline in the special interest of Ugaritic studies on the part of Old Testament scholars … The result is a gradual parting of the ways…”

Unable to connect the clearly related element found in Ugaritic literature with the Hebrew Bible, the historians have claimed the two were never connected in spite of the fact that the various names of gods, idiomatic or related expressions, etc., are clearly found in both. It is the established chronology that has forced this irrational decision on the historical community.

One element of the irrationality is the extraordinarily close organization of the Hebrew and Ugaritic alphabets as described by Cyrus H. Gordon:

“The scribes of Ugarit required an educational system to train them from the bottom up. The simplest school texts found there are ABC tablets listing the letters of the local alphabet in their fixed invariable order. The Phoenician alphabet of twenty-two letters is derived from a longer Ugaritic ABC of thirty letters. Contrary to the strict alphabetic principle [where each letter represents one sound, the Ugaritic alphabet ‘has three different aleph’ or A signs] the last three letters of the Ugaritic ABC are appendages so that twenty-seven remain for our consideration. Five sounds in the repertoire of [these] twenty-seven came to converge with other sounds because of sound shifts in standard Phoenician. The remarkable fact is that when those five sounds are eliminated, the remaining twenty-two letters appear at Ugarit in precisely the same order as they are still preserved in the Hebrew Alphabet.”

That is, the letters in the Hebrew alphabet 600 years later are in the same order as the letters in the Ugaritic alphabet 600 years earlier. Why, one must ask, would the Hebrews, who supposedly had nothing to do with Ugarit, organize their alphabet so that all Hebrew letters were in the same order as those of Ugarit? The obvious answer is that there was borrowing between these alphabets by scribes who learned these letters in an almost identical order. The probability that such ordered alphabets should be so nearly identical defies this being a mere happenstance. In fact, historians maintain the Greeks obtained their alphabet from the Phoenicians because the letter orders in both alphabets are identical!

Yet, as the short chronology has both cultures living in the first millennium B.C., there is contemporaneity and there would have been direct connections

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philologically between Ugarit and Palestine. And it is fully admitted by John Wansbrough that it is a very great problem to tie together the chronology of Ugarit with the various surrounding nations:

“The full chronology of Ugarit is almost entirely notional: ‘fourteenth century B.C.E.’ based on [18th Dynasty] Amarna [documents], is symbolic of a possible millennium [spanning] 2200-1200 [B.C.]. ‘Significance,’ in other words [for this chronology], has been read into, not out of, the traces [of evidence]. …

“So confronted, the historian of the ancient Near East has been compelled to adopt at least one—often more—of a number of strategies for [justifying] these data. In theory his choice might appear to be unlimited, in practice it has been unexpectedly restricted. Reason for this must lie somewhere in the acceptance of a [chronological] paradigm for assessing discrete and random witness (archaeology is after all notoriously unpredictable): i.e. it can only be read in terms of a prefigured system of [chronological] co-ordinates. Selection of the system will in turn depend on what is already available. For Ugarit the choice comprehended [took into account] several (vaguely) contemporary [chronological] models (themselves hardly certain in their political and socio-economic conditions): e.g. Hittite, Aegean, Cypriot, Canaanite, and Egyptian. The manner in which Ugaritic data have been slotted into these unstable [chronological] structures inspired only qualified confidence. … Once the general situation had been staked out in the interstices of surrounding archaeology, it seemed easy enough to fill the gaps by recourse to a series of case studies, each the product of a separate comparison with materials [of outside cultures] quite disparate in time and space.

“Perhaps the most remarkable, and certainly the best known, have been those adduced to support a reconstruction of culture in Ugarit: its language is described as Proto-West Semitic … its literature is deemed Canaanite epic (mostly via Biblical Hebrew [of Persian, Hellenistic times], and its religion interpreted as a version of ancient Near Eastern mythology … while none of these postulates is entirely without substance, the first two [Ugarit’s Proto-West Semitic language and Canaanite-Hebrew literature] might be thought to suffer from a kind of diachronic disability [of being distant from one another over a large time span], and the third from a generous proportion of unaccounted for onomastic [expressions]. At least two characteristics of the procedure inherent in this [chronological] exercise are salient: (1) the early metamorphosis of the philologist’s hypothesis [that Ugaritic literature is many centuries older than Hebrew literature] into the historian’s ‘fact’; and (2) the reconstruction of [the history of] Ugarit as a source or vehicle of subsequent evolution. The
methodological significance of both is enormous. Together they constitute the paradigm of historical explanation [of Ugarit’s chronology and history].”

On this evidence Velikovsky has written much and has rightly suggested that both Ugaritic and Hebrew were contemporary (or closely contemporary):

“The new view, predominant since the excavations at Ras Shamra [Ugarit], also regards many social, religious, and cultural elements of the Scriptures as copies, but of Canaanitic originals: since they were already in existence some six hundred years before the time the Bible claims for them, they could not be of Jewish origin. The Canaanites [it is suggested] paved the way to Jewish concepts in religion; their poetry had a high moral standard; their language, alphabet, style, and rhythm were inherited by the Jews; the ethos of social justice and the pathos of prophecy were Canaanitic hundreds of years before they became Israelite. These and similar deductions were dictated by the age attributed to the tablets of Ras Shamra [Ugarit]. In face of the striking parallels between the language, style, poetical forms, technical expressions, moral ideas, religious thought, temple ordinances, social institutions, treasury of legends and traditions, medical knowledge, apparel, and jewelry, as reflected in the Ras Shamra tablets and in the pages of the Scriptures, the logical conclusion would have been that the tablets and the Books of the Scriptures containing these parallels are of the same age. But such a deduction was not thought of, owing to the obstacles of [the established] chronology…”

Not only are Hebrew words and expressions found in Ugaritic texts of 1400-1200 B.C., based on the established chronology, but, shockingly, they are also found in Egyptian texts of the same time, as Velikovsky wrote in Ramses II and His Time (NY 1978), pages 61-63. He tells of a 19th Dynasty Egyptian scribe writing Hebrew during the 14th century B.C. In brief, there are numerous Classical Hebrew words found in the Egyptian language, as for example in the epic Poem of Pentaur, commemorating the heroic deeds of Ramses II. Hebrew words were used instead of their equivalents in Egyptian. It was as if there was a “Hebrew College” in Egypt to train Egyptian scribes to write Hebrew and Phoenician. The scribe Hori wrote a response to another scribe who attacked him. Hori proved his deep learning of Hebrew and Palestinian geographical locations. The problem, as with Ugaritic of the same period, is that the Hebrew of ca. 1300 B.C. remained completely unchanged for the 600 to 700 years that separated Hori from the period that historians say was the time this Classical Hebrew was actually being written, in the 8th/7th century B.C. In the words of Sweeney, “It is the

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21 John Wansbrough, „Res Ipsa Loquitur: History and Mimesis,” Methods and Theory in the Study of Islamic Origins, Herbert Berg, ed. (Leiden, the Netherlands 2003), pp. 11-12
22 Velikovsky, Ages in Chaos, op. cit., pp. 195-196
23 Ginenthal, op. cit., pp. 477-479
equivalent of finding a document from the time of Chaucer written in modern English! Yet such an implausible, not to say fantastic idea, is passed over in textbooks without comment.”

In both Ugarit and Egypt the almost identical, linguistically impossible situation exists. While there are clear words, phrases, expressions of classical Hebrew in both sets of texts, the historians say that 600 to 700 years separates the Hebrew employed in both from the time this Hebrew was actually written. Furthermore, the Egyptian Amarna texts of less than 100 years earlier are written to Palestinian kings, but these contain Canaanite words, as pointed out by Ian Young:

“The Amarna letters, written in the fourteenth century BC from the vassal states of Syria-Palestine to the Egyptian Pharaoh, are probably the most important pre-Biblical sources. As we saw … the Canaanite language which lies behind the Akkadian syllabary of the letters is not only a consistent literary prestige language throughout the whole area, but is also in important respects carried on by the Israelite literary prestige language, Biblical Hebrew. This language is most clearly seen in the Canaanite glosses [words written in the margins for explanation]. Throughout the Amarna texts Akkadian words are glossed by Canaanite words.

“Many scholars go beyond the glosses, however. It seems certain that the Akkadian of the Amarna letters is mixed in with a good degree of the local literary language. For example, the standard Akkadian first person singular pronoun is anaku. In the Amarna letters we sometimes have a-nu-ki which corresponds with Biblical Hebrew ‘anoki (Dhorme 1914:357). The evidence, in fact, seems to indicate that the Amarna scribes consciously evolved a hybrid language, a sort of pidgin Akkadian … We must be cautious in that forms represented in the Amarna letters might be neither Akkadian nor Canaanite but rather a peculiar cross between literary Akkadian and literary Canaanite, a hybrid which is neither one nor the other.”

Velikovsky devoted three chapters to the Amarna Letters of which some 380 still survive, written by vassals in Syria and Palestine to two kings of Egypt. Sweeney summarizes Velikovsky’s material thus:

“All of the letters, even those written by Egyptian scribes in Egypt, were composed in the Akkadian language, which proved that Akkadian was then the lingua franca of the entire Near East at the time. Those written in Palestine and Syria often contained, to varying degrees, an admixture of Canaanite vocabulary and syntax. This demonstrated that Akkadian was not the first language of the scribes and that obviously they had learned the language in some form of scribal schools or colleges.

“The Canaanite elements in the texts proved to be interesting. Scholars were surprised to find precise parallels with biblical Hebrew. Even Hebrew expressions

24 Sweeney, Ramessides..., The Velikovskian special edition (Forest Hills NY 2000), p. 44
25 Ian Young, Diversity in Pre-exilic Hebrew (Tübingen, Germany 1993), pp. 174-175
and popular sayings, found throughout the Old Testament, occurred. Thus in one letter to the pharaoh, Labayu, the ruler of the central hill country around Shechem, wrote, in good Hebrew; “If ants are smitten, they do not accept [the smiting] quietly, but they bite the hand of the man who smites them.” Albright recognised in this a close parallel with two biblical Proverbs mentioning ants (6:6 and 30:2). Orientalists were particularly struck by the parallels between the Letters and many expressions in the books of the biblical Prophets.

“Thus loyalty is expressed by the metaphor, ‘to lay the neck to the yoke and bear it’, found in the letters of Baal-miir and Yakhtiri. Precisely the same expression is found in Jeremiah 27:11.

“The submission of an enemy is expressed as ‘to eat dust’ in the letter of the men of Iqarta as well as in Isaiah 49:23.

“The king’s ‘face’ is set against his enemy, or the king ‘casts down’ an enemy’s face, or he throws the enemy out of his hand – identical in the letters of Rib-Addi and in Genesis 19:21 and I Samuel 25:29. Rib-Addi’s ‘face is friendly towards the king’; ‘he has directed his face towards the glory of the king, and would see his gracious face’. In the words of S.A. Cook, ‘Biblical ideas of “face” and “presence” will at once be recalled.’

“The same writer noted that, ‘Just as Ikhnaton’s hymn reminds us of Psalm 104, so Psalm 139:7f is suggested by the words of Tagi (Letter 264): “As for us, consider! My two eyes are upon thee. If we go up into heaven (shamema), or if we descend into the earth, yet is our head (rushunu) in thine hands.”’

“‘The footstool of his feet’ is an expression found in both a letter and in Psalm 110. Akhi-Yawi writes, ‘A brother art thou and love is in thy bowels and in thy heart,’ very similar to Jeremiah 4:19. ‘The city weeps and its tears run down, and there is none taking hold of our hand.’ (ie there is no helper). These words, written by the people of Tunip remind us of Lamentations 1:2 and Isaiah 42:6.

“Rib-Addi’s appeals to the pharaoh’s good name contain turns of speech used also in Deuteronomy 9:27f and in Joshua 7:9. When he says he has confessed his sins, he uses the words, ‘opened his sins’, an expression also found in Proverbs 28:13. When he writes that he will die ‘if there is not another heart’ in the king, he uses an expression also found in I Samuel 10:9 and Ezekiel 11:19.

“The king of Jerusalem wrote to pharaoh that ‘because he has his name upon Jerusalem for ever, the city cannot be neglected’, words reminiscent of a passage in Jeremiah 14:9. He also wrote, ‘See! The king, my lord, has set his name at the rising of the sun and at the setting of the sun,’ almost identical to an expression found in Malachi 1:11.

“These and numerous other parallels moved Cook to write: ‘the repeated lyrical utterances of Rib-Addi and Abdi-Khiba are early examples of the unrestrained laments of the later Israelites who appeal, not to a divine king of Egypt, their overlord, but to Yahweh.’ Yet it was the chronology alone that
prompted this statement. Believing the correspondences to predate the period of the Israelite kings by about five centuries, historians were amazed at the parallels, both in the language used and in the various expressions and idioms employed. Indeed, were it not for the chronology, they would simply have termed the language of the letters ‘Hebrew’.\footnote{Sweeney, \textit{Empire of Thebes} (NY 2006), pp. 75-77}

Just as with the alphabetic language of Ugarit, there are close expressions, idioms, and words employed in the Egyptian Amarna letters that reflect Hebrew expressions, idioms, and words employed in the Bible. And again the historians expect one to accept that these words, expressions and idioms remained virtually the same for 600 years or more. But just as with the missing strata of the Phoenicians and Israelites when their alphabetic closely related languages were supposed to exist, as discussed above, we run into a parallel situation with the Amarna Letters. That is, the cities in Palestine and Syria did not exist at the time these letters were sent. In this respect Sweeney reports:\footnote{Ibid., pp. 77-8}

“If scholars were surprised to find the Canaanites of the 15\textsuperscript{th} and 14\textsuperscript{th} centuries BC using Hebrew of the 8\textsuperscript{th} and 7\textsuperscript{th} centuries, they were perhaps even more non-plussed to find mentioned towns and settlements which should not, according to everything that was known about the history of the region, have existed until the time of the biblical kings.

“This was particularly so with regard to the city of Jerusalem, which appeared in the letters as Urusalim. Until the discovery of the [Amarna] archive, it was widely believed that Jerusalem was not known by this name until after its conquest by David, right at the start of the Kings period. It had generally been accepted that prior to this the city had been called Jebus, or Salem. …

“Another unexpected discovery amongst the correspondences was the mention of a city named Batrana. This was immediately recognised as ancient Botrys (modern Batroun), which Menander of Ephesus had claimed was built by king Ithobalos (Ethbaal), the father-in-law of the Israelite king Ahab.”

Here we have well-known cities mentioned in the Amarna Letters that simply did not exist in the second millennium B.C. based on the established chronology. But they are named; thus, by moving the time of these Egyptian pharaohs and vassal Palestinian kings into the first millennium, all the various contradictions created by the erroneous established chronology vanish!

In the cases of Ugarit and Egypt of the 15th-14th centuries B.C., Classical Hebrew is found not in very isolated cases, but is evident in so many cases as to make it inordinately improbable and linguistically/philologically impossible that these three languages were not contemporary; they had to belong to ca. the mid-
first millennium B.C. Archaeologically the very same correlations exist to connect Ugarit to the first millennium B.C. and not the second. As scientifically and technologically documented in these volumes, the 12th Egyptian Dynasty must be placed in the first millennium B.C. Thus, at Ugarit we found relics of the 12th Dynasty that on the basis of the forensic evidence require that Ugarit must also be placed in the first millennium. In this respect, James Hastings et al. directly state, “Ugarit came under the Suzerainty of Egypt’s vigorous 12th Dynasty.”

The archaeological evidence for this is presented by Wilfred G.E. Watson and Nicholas Wyatt:

“[Ugarit] yielded one of the largest collections of Middle Kingdom statues and objects in Asia: two sphinxes of [12th Dynasty king] Amenemhet III, a statue of the vizier Sesotris-Ankh with his mother and sister, a statuette of the princess Khenemet-nefer-khedjet, the wife of Sesotris II, and a bust of an unidentified 12th Dynasty queen … In addition, there are inscribed seals, pearls and beads, and a large quantity of uninscribed objects.”

In terms of tribute, the Hittites/Lydians demanded certain products from Ugarit. Here Amélie Kuhrt shows:

“As well as basic food (grain, wine, olives), Ugarit also produced surpluses: stacks of amphorae in warehouses at the quayside imply the production of olive oil in commercial quantities; … Ugarit also had specialised craft industries: fine, purple-dyed linen and wool garments (the colour derived from murex shellfish), and bales of cloth in considerable quantities were manufactured; they were specifically demanded by the Hittites as a substantial part of the tribute from Ugarit… The Hittite/Lydian treaty with Ugarit also shows, incidentally, that Ugarit was considered capable of raising considerable quantities of gold and silver, both in bullion and in the form of vessels made of precious metals. … Ivory-carving and inlay was probably another skill which the king of Ugarit could command. The forests of the region supplied wood used for the production of fine furniture and luxury items, as well as for building…”

We have shown that there was an ancient Industrial Revolution in the ancient Near East from around 750 to 300 B.C. and this encompassed far-reaching trade relationships across the ancient world. This trade stretched over much of the Near East, as Alan Ralph Millard explains:

“Ugarit’s wealth came from trade. The city stood at the end of a road from Babylon, up the Euphrates and across to the Mediterranean. From Ugarit ships sailed to Cyprus and Crete, to the southern coast of Turkey, and down the coast of

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28 James Hastings et al., Dictionary of the Bible (NY 1963), p. 120
29 Wilfred G.E. Watson and Nicholas Wyatt, Handbook of Ugaritic Studies (Leiden, the Netherlands 1999), p. 616
30 Kuhrt, op.cit., p. 302
Canaan to Egypt. Not surprisingly, influences from all these regions appear in the art and culture of Ugarit.

“They are most obvious in writing, for beside Babylonian and Ugaritic, Hittite and Hurrian were also written in cuneiform, Egyptian appears on metal and stone … while the Hittite hieroglyphs and a syllabic script from Cyprus [a form of Linear A] are also found at Ugarit.”

There is no doubt that the syllabic Linear A script from Cyprus is a first millennium form of writing. It is assumed this syllabic form is quite old only because it is found at Ugarit. But the fact of the matter is that it is not attested outside Cyprus in the second millennium B.C. As Paul Hellander reveals “The Cypro-syllabic script … has to far been attested in Cyprus only BETWEEN THE 8TH AND 3RD CENTURIES BC.”

That being the case, there should also exist trade relationships between Ugarit and the Mycenaean and Minoan cultures which we also have located in the first millennium, based on technological and other forms of evidence in volume II of this series. That, too, is just what is found according to Gert Jan van Wijngaarden:

“Mycenaean and Minoan ceramic vessels and Mycenaean figurines are the only imports at Ugarit which are of certain Aegean origin. … Indeed the main conclusion of this chapter is that Mycenaean pottery was fairly common at Ugarit: it has been found in all excavated parts of Ras Shamra …”

And this further correlates with the shipwreck at Ulu Burun also dated to 1400 B.C., as Stephanie Lynn Budin explains:

“… the excavator [of the Ulu Burun shipwreck]–George Bass–noted fifty-two [amphorae vessels] were of a style that not only was prevalent in the trading city of Ugarit, but also [has] parallels on the Greek Mainland [Mycenae].”

That is, the scripts to which alphabetic Ugaritic is related must all be placed in the first millennium B.C. based on scientific, technological and, importantly, linguistic evidence. This places Ugarit directly inside the ancient Industrial Revolution between around 750-300 when this great expansion of trade just discussed developed.

The Ugaritic alphabetic language is tied to the Middle Kingdom by objects of the 12th Dynasty along with 12th Dynasty hieroglyphs. And the 12th Dynasty is dated to well inside the first millennium B.C. by Sothic dating and other

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31 Alan Ralph Millard, Discoveries from Bible Times: Archaeological Treasures Throw Light on the Bible (Oxford UK 1997), p. 87
32 Paul Hellander, Lonely Planet: Cyprus (Footscray, VIC, Australia 2003), p. 12 (capitalization added)
33 Gert Jan van Wijngaarden, „The Role of Mycenaean Pottery in the Material Culture of Ugarit,“ Use and Appreciation of Mycenaean Pottery in the Levant, Cyprus and Italy (1600-1200 BC) (Amsterdam, the Netherlands 2002), p. 71
34 Stephanie Lynn Budin, The Ancient Greeks: New Perspectives (Santa Barbara CA 2004), p. 96
technological evidence. The Ugaritic alphabetic language is tied by documents to the Hittites who used a form of Hittite that was identical to one of the languages of the Lydians of the first millennium, and technological evidence directly related to the Hittites/Lydians also places them in the first millennium. The Ugaritic alphabetic language is tied by documents to the Cypro-syllabic Linear A script used on Cyprus which is only attested to between the 8th and 3rd centuries B.C. This requires that the forms of Akkadian and Babylonian found at Ugarit are also of the first millennium B.C. These multi-correlations of languages to that of alphabetic Ugaritic are an overwhelming form of evidence that corroborates and converges to prove that Ugarit existed in the first millennium B.C. and not the second. While historians have agreed that Ugarit fell to the Sea Peoples in 1200 B.C. this is clearly not the case. As Amélie Kuhrt explains:

“What is impossible to demonstrate on the basis of the Ugarit letters is who exactly was responsible for the fall of the city of Ugarit and its destruction. It is worthy remembering that Claude Schaeffer was himself convinced that the city was destroyed by an earthquake.

“Hatti and Ugarit are the two states whose collapse has been thought to be clearly attributable to the ‘sea-peoples’, although it is difficult to do so conclusively.”

There is yet other evidence that ties Ugarit to the Greeks of the first millennium B.C. and not to the second. Here Velikovsky points out:

“On some of the tablets there is a ‘copyright mark’: it is a statement that these tablets were made at the order of Nikmed, king of Ugarit.

“Nikomedes is an old Greek name. The similarity between the name Nikomedes, regarded as originally an Ionian name, and the name of the Ugaritic King Nikmed, is so obvious that, after deciphering the name of the king, two scholars, working independently, related it to the Greek name. Other scholars, however, rejected this equation of the name of the king Nikmed (who also wrote his name Nikmes and Nikmedes) with Nikomed (Nikomedes) of the Greeks, asking how an Ionian name could have been in use in the fourteenth century before this era. Those who made the identification were unable to defend their position against the mathematics of conventional chronology.

“Ugarit was a maritime commercial city; its population was composed of various ethnic groups. One document found there describes the expulsion of King Nikmed and all the foreign groups in the city. Among them were [the] people of … Jm’an. The last name was identified by the decipherers as Jamanu, which is well known from the Assyrian inscriptions, and means Ionians. This interpretation of Jm’an was disputed for no other reason than that in the fourteenth century a reference to Ionians would have been impossible. In the same inscription, at a

35 Kuhrt, op.cit., p. 392
point where the names of the expelled are repeated, the name Didyme appears. The decipherers took it to be the name of the city of Didyma in Ionia. This city was renowned for its cult of Apollo Didymeus. Again, the name of the deity Didymeus (Ddms) was inscribed on another Ras Shamra tablet; the decipherers, turning neither left nor right, translated it ‘Apollon Didymeus’. Now antiquities have been brought from the site of Didyma, originating from the eighth century [B.C.]. But in the fifteenth or fourteenth century neither Ionians nor the shrine of Apollo Didymeus could have been mentioned [because at that time they did not exist]. [The established chronology could not square with the Ionian names of Nikomed, or the name of the Ionian city of Didyma, or the Greek cult of the god of that city, or the very name Ionians in the Ras Shamra texts–but all these were there, and no explanation was put forth in place of the rejected theory about an Ionian colony from the city of Didyma … that came to Ugarit and was expelled together with the king of Ionian origin, Nikmed. It could only be stated that there was not a grain of probability in such a reading of texts belonging to the middle of the second millennium [B.C.].]

Again, we run into the same problem of Ugaritic texts containing undeniable elements of 8th-7th century Greece (Ionia) just as they had with classical first millennium biblical Hebrew. And this is precisely the problem of historians denying that where and when Greece or Greeks are mentioned in texts that supposedly belong to the second or even third millennia B.C., the linguists are in error. The only reason to do so is the crutch of the established chronology. As we pointed out in volume I of this series:

“… it seems that the Old Kingdom Egyptians were trading with the Greeks and therefore knew them. This would never have been possible in the third [or even second] millennium B.C., but would certainly be true for the first. In this respect W. Stevenson Smith [‘The Old Kingdom of Egypt and the beginning of the First Intermediate Period,’ Cambridge Ancient History, vol. 1, part 2, 3rd ed. (Cambridge 1971), pp. 180-81] discusses the fact that the Old Kingdom 5th Dynasty employed the hieroglyph ‘Haunebut’ which refers to the Greeks of the Aegean, [stating] ‘that it is unlikely that the Aegean or its inhabitants were meant by the term “Haunebut” in the Pyramid Texts or in the inscriptions of the time of Cheops and Sahure’.”

To put it bluntly, historians who have argued that documents and their contents to a very great degree are one of the basic pillars for establishing historical truth refuse to accept this pillar when it goes against the chronology that is the heart of their historical belief system. “Jm’an” at Ugarit cannot mean Ionia, they say, in spite of the fact that that is exactly what it means, just as “Haunebut” in Egyptian texts

36 Velikovsky, Ages in Chaos, op.cit., pp. 185-186
37 Ginenthal, Pillars..., vol. I, op.cit., p.173
cannot mean Greek or Greece in spite of the fact that that is exactly what it means. In this same context historians also say that the Egyptian word for “Israel” in an Egyptian text does not mean “Israel” but something else. Henige does just this:

“A stele of the pharaoh Merenptah … illustrates the penchant to make much of little. Dating to the end of the thirteenth century BCE, the stele mentions a polity called ‘Israel’ (as reconstructed from hieroglyphs anyway). Many biblical scholars take this as evidence—all there is, really—of a biblical Israel [that existed] two centuries before and two centuries after the date of the stele itself. In short, they treat it as testimony to four hundred years of continuous history. They do not notice, or do not heed, that the relevant passage reads something like ‘Israel is laid waste [and] his seed is not,’ apparently stating Merenptah’s claim to have extirpated the inhabitants of whatever polity this was. If the stele is evidence of anything, it is that Merenptah claimed to have wiped out a group of people with a name similar to that which the bible later gave to people who purportedly left Egypt and who several centuries later established a state in the same area.”

In other words, none of the texts that mention the states of Greece, Ionia, Israel, mean what they say they mean! They apparently mean only what the historians say they mean! The historians are never in error in this regard, only the words’ meanings in the documents are! Oscar Handlin, the great philosopher and critic of historical analysis, discussing Velikovsky, makes it clear that historians only trust one another and all are committed to the established chronology:

“If historians turned their backs upon the theories of Immanuel Velikovsky … it was on the basis not of scrutiny of the evidence but of judgment by trusted experts in neighboring departments.”

The inverted logic used by historians to dispel not only Velikovsky’s evidence for dating Ugarit is to assume the established chronology is correct and then deny any evidence, scientific, technological, linguistic, etc., that contradicts that assumption and find reasons to support the contention held by trusted historians. Francis Brown, as long ago as 1885, in his book *Assyriology: Its Use and Abuse in Old Testament Study* (NY) pages 27-8, warned historians of doing just this:

“The vice of this method of handling the inscriptions lies here: that it involves playing fast-and-loose with well-attested historical documents; hailing them eagerly when they say at once what they want them to say, but discrediting them with all your might when their utterances are troublesome…”

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38 Henige, *op.cit.* , p. 72
40 quoted in Henige, *op.cit.*, p. 91
THE DOMESTICATED CAMEL AND UGARIT’S CHRONOLOGY

In volume II we discussed, and below we will discuss, the coming of the domesticated camel into Mesopotamia, which even in the established chronology did not occur until around 900 B.C. Therefore, it would have been impossible for the people of Ugarit to have known of domesticated camels because, according to that self-same established chronology, that small city state was destroyed around 1200 B.C. Nevertheless according to Gleason Leonard Archer et al. “…that the camel was known and listed among the domesticated animals during the Old Babylonian period is supported by a Sumerian lexical text from Ugarit …”41 This evidence is also presented by W.G. Lambert, “The Domesticated Camel in the Second Millennium—Evidence from Alalakh and Ugarit,” BASOR, vol. 110 (1960), pp. 42-43. Since the domesticated camel did not come into Mesopotamia prior to ca. 900 B.C., it is impossible for the people of Ugarit to have documented these animals. Historians who uphold the established chronology either ignore this fact or suggest that camels must have been domesticated and used for transportation prior to B.C. But since we date the Neo-Assyrians/Persians to the 7th century B.C., the camel was domesticated then, as pointed out in volume II, pages 491-512.

TIN, IRON, AND THE CHRONOLOGY OF UGARIT

More than the evidence of the first millennium camel at Ugarit, there is also the problem of tin bronzes in that city prior by at least 100 years to the time that tin to make bronze comes to that region after 1100 B.C. However, in the short chronology tin comes into Mesopotamia around 800-700 B.C. In either case, there should be no tin artifacts in Ugarit strata. Nevertheless, Eliezer D. Oren points out: “Of the two javelin heads from level III at Ras Shamra [Ugarit] dated by [Claude] Schaeffer to 2300/2100 B.C. … the first contained 92.98% copper and 4.41% tin [which occurs naturally], while the second had 88.90% copper and 9.67% tin [which is man-made bronze] … Eight other metal artifacts from level II at Ras Shamra contained an average of 88.50% copper and 7.50% tin [also man-made bronze]…”42

Here, then, is the problem as outlined by Watson and Wyatt: “There is a problem in connection with copper artifacts and the production of bronze at Ugarit since there were no local sources of metal … It seems that metalworking had a long tradition in Ugarit …”43

41 Gleason Leonard Archer et al., A Tribute to Gleason Archer (Chicago IL 1986), p. 145
43 Watson and Wyatt, op.cit., p. 452
In terms of carburized iron, which, according to the established chronology, was developed by the Hittites some time after 1400 B.C., Andrew Ede tells us an “[iron] battle-axe from Ugarit, in Syria, was probably forged between 1450 and 1350 B.C.E. … When the Hittites began to produce iron tools and weapons, around 1400 B.C.E., they tried to keep their metalworking methods secret, but with the end of their empire in Anatolia (modern Turkey) around 1200 B.C.E., the skills went with the blacksmiths as they dispersed into the new empires that grew up in the region.”

The problem is that historians claim that the Hittite empire and Ugarit fell at about the same time by a marauding group known as the Sea Peoples. So how could the Hittite blacksmiths have dispersed to a land that was to fall immediately afterward, and teach the process of iron melting? They would have to build advanced furnaces, or have carried their own to the new land. But then they would need to have iron mines opened to be exploited.

It is certainly probable that a few iron smiths escaped from Hittite cities to Ugarit, but then, where did they mine their iron ores? These smiths would not only have to move their furnaces or rebuild them during the chaos of the Sea Peoples’ rampage, but then find new sources of iron ore to mine at the same time. However, since we have dated the Hittites to Lydian times, or well into the Greek epoch, we repeat a technical fact cited earlier, above, by John Garstang: “THE HITTITE SOLDIERS OF THIS LATE PERIOD WORE ARMOUR WHICH ANTICIPATES ALMOST EXACTLY THE ATHENIAN PANOPLY OF THE FIFTH CENTURY [B.C.]” The technical evidence regarding the chronology of Ugarit places it in the mid-first millennium B.C. Therefore, its cuneiform alphabetic script also had to be written in the first millennium B.C., and all the connections of the various alphabetic systems of the Hebrews, Phoenicians, etc., were developed and utilized in that same millennium. All these different forms of evidence correlate with, and corroborate, one another in a highly congruent manner and converge to place all the nations associated with alphabetic scripts in the first millennium.

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45 Garstang, *op.cit.*, p. 294 (capitalization added)
UGARIT, ASTRONOMY AND CHRONOLOGY

It is possible that some careless critic may raise the issue regarding a possible total solar eclipse at Ugarit and then claim that this astronomical event cannot be made to support and therefore contradicts on scientific grounds all that has just been written above about the dating of the chronology of Ugarit. Yet, the facts of this matter and the evidence purporting to support this solar eclipse are truly unsustainable for a host of reasons. This is explicitly stated by Herman Hunger and David Pingree:

“A fragment of mostly lunar omens in [the] Ugaritic [language] was found in [the town of] Ras Ibn Hani near Ugarit … The text is badly damaged so that many details are uncertain; but there is no doubt about its [astronomical] character. …

“The short Ugaritic text KTU 1.78 has been understood by several authors … as referring to a total solar eclipse. The text begins with a disputed word (or words) bṯṯ and then speaks of the Sun’s setting on the New Moon day of (the month) Ḥiyar. Then follow the words ṭḡṛḥ ṭšp, generally translated as ‘her (i.e. the Sun’s) gatekeeper was Rešep.’ The remaining sentence of the text refers to an investigation of livers. Assuming that Rešep here refers to the planet Mars (which is not at all certain, see Walker [1989]), one can assume here the astronomical situation of Mars being visible after sunset on the New Moon day of the sixth month in Ugarit. Such an observation could very well have been considered ominous and be interpreted by extispicy [liver omens] … It has been remarked, however, that the astronomical significance of the word ‘gatekeeper’ is not clear. Some modern scholars felt that a Mars observation was not rare enough to be an omen [because Mars could easily seen quite often after the Sun set] and tried to find a solar eclipse in the text. THE WORDING DOES NOT JUSTIFY THIS INTERPRETATION.’”

That is, one does not know with certainty that the setting of the Sun on the New Moon day is directly related to a solar eclipse, although this is a possible interpretation. One does not know whether Mars was the planet referred to because, as Hunger and Pingree state, this “is not at all certain,” and Mars would generally be clearly observed rather often at the horizon after the sun set which makes this ominous event too common to be a serious omen. Lastly, why didn’t the astrologers interpret this assumed astronomical event since they were the experts in astrology instead of the liver omen authorities? This point is raised by Ulla Koch-Westenholz:

46 Hermann Hunger, David Pingree, Astral Science in Mesopotamia (Leiden, the Netherlands, Boston MA, Köln, Germany 1999), p. 10 (capitalization added)
“... there is the famous and often discussed report which may contain the oldest dateable observation of a solar eclipse (May 3rd, 1375 B.C.). The interpretation of the text by Dietrich et al. shows that the eclipse was not treated as an astronomical omen to be interpreted according to the astrological omen series, but merely as an evil portent that had to be investigated by liver divination.”

Ann Jeffers suggests that instead of the planet Mars the Ugaritic text may refer to the star Aldebaran:

“Alternatively KTU 1.78 ... could refer to an eclipse of the Sun. If so, ršp may refer to the planet Mars. During a total eclipse of the sun the stars appear in the day-time; in this case, Aldebaran, a bright red star was visible and it was easily mistaken for the red planet Mars.”

B.F. Walker himself admits: “the eclipse can only be tentatively dated.”

Even J.F.A. Sawyer and F.R. Stephenson admit the eclipse on their date was not total at Ugarit but had a magnitude of 94 percent.

Paolo Xella admits: “At all events, the precise date of the [Ugarit eclipse] event (probably March 5th, 1223 BCE) is still debated by specialists and there is no unanimous opinion.”

More than that, the “May 3rd, 1375? B.C.” date is clearly without merit if one adheres strictly to the Ugaritic document as the authors point out:

“This text is treated most recently by T. de Jong and W.H. van Soldt ... According to their interpretation of the text, Mars was visible during the eclipse, which excludes the date proposed by D.-L.-S. [Dietrich-Loretz-Sanmartín]. They suggest instead the date March 5, 1223 [B.C.] – Walker, Nature [vol.] 338 (1989) p. 204 f says that the eclipse can only be tentatively dated.”

The entire conclusion is an exercise in assuming everything that needs to be proven before one can determine if this Ugaritic text deals with a total solar eclipse. Ev Cochrane holds that the gatekeeper identification of Reseph as Mars "seems secure in [view] of his well-attested identification with Nergal in Ugaritic texts.”

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47 Ulla Koch-Westenholz, Mesopotamian Astrology: An Introduction to Babylonian and Assyrian Celestial Divination (Copenhagen, Denmark 1995), p. 493 (emphasis added)
48 Ann Jeffers, Magic and Divination in Ancient Palestine and Syria (Leiden, the Netherlands/NY/Köln, Germany 1996), p. 148
52 Koch-Westenholz, loc.cit., fn. 3
“According to the consensus [no proven evidence] of leading scholars ... Sawyer and Stephenson, who were the first to see in the text an early description of a solar eclipse attended by Mars, offer the following translation:

“The Sun went down (in the day time) with Mars in attendance...”

On the other hand, Michael A. Covington and Jay Pasachoff suggest:

“What was the earliest eclipse recorded? Some scientists think that an engraved tablet now in Damascus, Syria, records the [Ugarit] eclipse of 1223 BC. If this translation is accurate, the tablet is the only surviving astronomical observation written in the Ugaritic language of Babylonia. The tablet refers to an event occurring at sunset, and it may not have been an eclipse after all. Since we don’t know the Ugaritic names of the planets nor do we know the calendar they used [at that time in Ugarit], the identification of the tablet with an eclipse remains speculative. And even if it was an eclipse, it might not have been total. In 1993, an Orientalist and a historian of science from the University of Chicago concluded that the text does not correspond to a solar eclipse at all.”

Note that Covington and Pasachoff state that “we do not know the calendar they used [at that time in Ugarit].” That, in and of itself, is a major problem regarding this eclipse: when one doesn’t know the order of the months in a calendar or when the year began, one cannot date an eclipse precisely, if at all. In this respect, Paolo Merlo and Paolo Xella explain: “With regard to the annual cycle ... however, unfortunately we are not yet completely certain of the exact sequence of the months of the Ugaritic calendar since the local names are considerably different from the standard names of the months of Mesopotamian tradition.”

John R. Hinnells states: “No calendar or list of months has survived from Ugarit. But the month names are known, and their order apart from two.”

Robert R. Stieglitz in fact shows that “The position of ... three [particular] Ugaritic months is conjectured to be months X-XII of the spring year [namely] January ... February ... March.”

On top of all these problems is that pointed out above regarding Delta T, or the slowing of the Earth’s rotation over time, which makes dating a solar eclipse older than ca. 700 B.C. extremely unreliable. Above we cited Robert R. Newton, who

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54 Ibid.
examined the Delta T value and was forced by the evidence to admit “we do not know whether the eclipse was visible at Ugarit or not, and we have no idea of the magnitude if it was visible. There is no basis for taking -1374 [or -1223 B.C.] to be the date of the record.”

Nevertheless, David Rohl, whose chronological revision lowers the history of Egypt by about 300 years, claims to have dated this eclipse so that it agrees with his time scheme. He concludes: “Astronomical retrocalculation produces the only possible date for the near-sunset solar eclipse observed and recorded on Tablet KTU-1.78. The [Ugarit] eclipse occurred on the 9th of May in 1012 BC …” Rohl has employed the retrocalculations of Wayne A. Mitchell to obtain these results. Paul Dunbavin has described Mitchell’s work briefly:

“The solution proposed by Mitchell was a lower date [of the Ugarit solar eclipse] of 9 May 1021 BC … The circumstances show an eclipse at sunset with the Sun in the horns of Taurus—indeed the Sun set while still in eclipse. … One anomaly, however, is that the path of totality does not pass directly over Ugarit and would have to have been viewed from further north, in southern Turkey. A small error in $\Delta T$ would make little difference in this instance, as the track is almost west to east [and thus would not move the eclipse south to reach Ugarit].

“Egyptologist David Rohl has attempted to use this eclipse (along with much other evidence) to propose a revision of Egyptian chronology…”

Nowhere does Rohl provide certain evidence that all the problems outlined above regarding this eclipse have in any way been fully resolved. As with his work on Sothic dating noted above, he has placed this eclipse into a date that would conform with his chronological revision and has claimed success by taking all the assumptions that fit his date as factual, when in fact it is preferential interpretation. When I consulted Professor Lynn Rose about this matter, he sent me the following letter dealing with Rohl, Mitchell, and their May 9, 1012 B.C. solar eclipse:

“You are correct in pointing out that $\Delta T$ is essentially unknown for the second millennium and even for the early first millennium. That is the conclusion that Stephenson and his various colleagues have reluctantly reached in their later years, despite the fact that they were much more sanguine in their youth. This change of heart and mind is well illustrated by the Ugarit text itself. In 1970, Stephenson was sure that he could retrocalculate a total solar eclipse near Ugarit on 5 May -1374. Later, he quietly dropped the entire matter. The lengthy Bibliography in his book

59 Newton, The Moon’s Acceleration..., op.cit., p. 465
60 Rohl, op.cit., pp. 237-40
61 Ibid., p. 240
lists at least twenty-five of his own publications, but does not even mention his Ugarit papers; he seems to have decided they were youthful follies, best forgotten.

“The insistence of Mitchell and Rohl on Sunset as the time of the eclipse seems unjustified. That is not the way most scholars have read it. Indeed, Sawyer and Stephenson put their 5 May -1374 eclipse at 6:10 a.m. They make it clear that in contexts such as these the word ‘rbt or ‘set’ is used in the sense of ‘is eclipsed.’ They even cite Amos, who is usually taken to be referring to an eclipse when he writes: ‘and I will make the sun go down at noon.’

“I do not know exactly how Mitchell and Rohl arrived at Sunset as the time of the eclipse. Did they first determine that the eclipse would have to be at Sunset, and then find confirmation in the 9 May -1011 eclipse? In this case, they seem to be completely at odds with many of the reputable scholars who have been examining the text for decades and have not believed that the eclipse had to be at Sunset. Or did Mitchell and Rohl first come upon the 9 May -1011 eclipse, and then decide that the text specified an eclipse of the setting Sun? In that case, they are cheating.

“Are Mitchell’s 1988 (at the latest) mainframes any more effective than twenty-first century desktops? Maybe, maybe not; it doesn’t really matter. But are his results, using Huber’s programs, any better for the second millennium than Starry Night or Planetary, Lunar and Stellar Visibility, Version 3.1? I doubt it. Version 3.1 shows a 96% solar eclipse at about 5:45 p.m. on 9 May -1011, with Sunset about 50 minutes later. The Starry Night graphic shows a total solar eclipse at 6:37 p.m., about 40 minutes before Sunset. I do not trust either of these programs for the second millennium – or Huber’s own programs, for that matter.

“I have not seen those older Huber programs that Rohl mentions, but in Huber and De Meis (2004), Huber himself reveals on page 207 that:

“‘We have assumed that the Moon’s orbital accelerations is -26”/cy^2 and that the difference between Ephemeris Time (ET) and Universal Time (UT) can be represented by

(ST82f) \[ \Delta T = ET - UT = -1925 - 81t + 32.5t^2 \text{ sec} \]

where \( t \) is measured in centuries since 1800 AD.’

“Both the Moon’s orbital acceleration and \( \Delta T \) itself display considerable variation and fluctuation, as well as overtones. Huber’s simple formulas ignore all of that, and are allowed to vary only uniformly and only with time. He is trying to draw very simple and regular curves through an unruly and recalcitrant mass of historical data that begins to peter out somewhere around 27 or 28 centuries ago. Even Huber has the decency to refrain from pushing his formulas into the second millennium; with uncharacteristic wisdom, he stops at -800.
“Under these circumstances, those ‘powerful university main-frame computers (vintage 1988 at the latest) are of little avail; even with a 2008 supercomputer, it would still be a matter of Garbage In, Garbage Out.”

All in all, although various dates have been put forth for the Ugaritic solar eclipse in the second millennium B.C., we face what appear to be insurmountable problems that deny these assertions. (1) We don’t have a calendar for Ugarit that is certain and ordered within the year; only 10 months are named but their precise order and how they fit into either a year that begins in March/April (spring) or September/October (fall), is not known. (2) We don’t have a known gatekeeper; perhaps Mars, perhaps Aldebaran have been suggested though not proven to be in fact the case. (3) We don’t know if the Sun was eclipsed in daylight or merely set. (4) We have dates selected for the eclipse as being May 3, 1375 B.C., or March 5, 1223 B.C., or even May 9, 1012 B.C., separated in time by 163 years. (5) The dates indicate that if there was an eclipse at Ugarit, it was partial and is therefore highly speculative. (6) The various authors of these dates have a Delta T value that is extremely tenuous for a date so long before 800-700 B.C. What is required for dating any such solar eclipse is precise astronomical data coming from relevant and clear sources. But this simply does not exist. The suggestion that such a total solar eclipse has been documented is an indefensible argument. This indicates that the entire procedure is valueless.

On the other hand, placing Ugarit in the 8th century B.C. correlates with several forms of evidence noted above, from science, technology, philology, and the alphabet, including the coming of the domesticated camel to that part of the world. And, finally, in terms of chronology, Ugarit dated to the first millennium B.C. should also have experienced the same climate change that followed the 8th century B.C. pole shift described above and elsewhere in these volumes. By moving Ugarit into the mid-first millennium B.C., we can show that it, too, experienced this aridification. Thompson presents just this case except that he adheres to the established chronology:

“D.L. Donley confirmed that the existence of a climatic change … that had brought about drought conditions as posited by [Rhys] Carpenter for Mycenae, was synchronous throughout the hemisphere. [William] Stiebing placed the peak of this drought between 1200-1190 BC and related it to references to drought and famine in the Hittite [Lydian] Empire mentioned in Egyptian texts involving grain sent to Anatolia from Ugarit during the reign of Merneptah. … F.R. Dupont has argued very persuasively that the civilization of Ugarit, although greatly weakened by the effects of the drought that had seriously depressed the entire region, had been destroyed neither by the drought itself nor by the invasions of

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63 Rose, personal communication, Dec. 30, 2007
‘Sea Peoples’ … Rather long standing drought conditions had brought about such widespread impoverishment at Ugarit, and a consequent weakening of the political structures, that the population of Ugarit was prevented from rebuilding after the city had been destroyed by [an] earthquake in 1182 B.C.”

Thus, not only does the scientific, technological, and philological evidence tie Ugarit to the chronology of other nations around it and to the first millennium B.C., but the climatic aridification does the same and explains why the people of Ugarit knew about the domestic camel. That is, the camel comes to this part of the Near East only after the aridification set in and made the camel an important element in trade across the newly-forming deserts.

ARCHAEOLOGY AND THE HEBREW ALPHABET

Based on all the evidence above, it is rather clear that all the various forms of alphabetic script belong to the 8th-7th centuries B.C. and that all these scripts will be found in contexts, based on the short chronology, that reflect this philological development. In Palestine itself, this written evidence becomes apparent in just this period, as described by Ephraim Stern.

“One of the surprising results of the excavations of Judah in the 7th century BCE is the unusual amount of epigraphic material from a period that lasted not much more than one hundred years. This material is abundant in comparison with both the preceding and the following periods. The Judaean state appears to have experienced an ‘explosion’ of writing in comparison with the rest of the Palestinian states of the period, which also enjoyed the prosperity of the age, and in comparison with the written finds in the Assyrian provinces in general. Taking into consideration the size of the Judaean kingdom during this period, this large body of written artifacts is truly astonishing.

“We shall now describe the various Judaean inscriptions … but the rarity of this phenomenon should be examined first. It seems that the major reason stems from the fact that the Judaean kingdom of this period was an extremely centralized administrative unit in which each activity needed the approval of the king in Jerusalem or of one of the senior officials. In order to make this possible, a sophisticated bureaucratic network of administrators was established to deal with all aspects of the kingdom’s activity. The remains of this bureaucracy and its echo can be traced in the archaeological record in the form of the many official seals and seal impressions issued by the royalty, the seals of the various officials and administrators of the kingdom, the system of taxation, and the surprisingly accurate system of weights and measures. Remains of a well-organized system for reporting among the various components of this administration were also

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64 Thompson, Early History, op. cit., p. 219
unearthed. It seems to have been almost impossible, even in the remotest small fort, to send any amount of rations without recording them in a detailed manner. If we add to this the ordinary daily reports and reporting on special events we obtain a picture of a sophisticated bureaucracy involving every aspect of state life. The question arises, however, why this administration did not appear earlier.”65 We will address this question in the next chapter on “Biblical History.”

TRANSMISSION OF THE ALPHABET TO GREECE

Because of the established chronology which proposes that alphabetic writing begins well in the second millennium B.C., the transmission of this communications tool to the Greeks is fraught with chronological problems. But, as we will demonstrate, the short chronology of Heinsohn, Sweeney, and Rose, and in large part of Velikovsky, resolves them and correlates them with all the other forms of forensic and historical evidence to explain how and why this transmission occurred in the 8th-7th centuries B.C. The philologists and historians also generally agree about the time of this transmission but when they do so in terms of the established chronology, they are confronted by serious problems. These we will now address.

The extremely close relationship between the Greek and Phoenician alphabets is well described by P.V. Jones:

“The single most influential example of interaction between Greeks and near eastern cultures was surely the development of the Greek alphabet, the basis of ours. The Greeks invented their alphabet in the 8th century BC on the model of the Phoenician alphabet. Very broadly, Greeks took Phoenician symbols and where necessary changed both their look and their value to suit Greek needs. Major changes included:

- Phoenician script runs right-to-left. In time Greeks settled on left-to-right, and thus reversed all Phoenician non-symmetrical signs. So e.g. Phoenician gimel (׳) became Greek gamma (Γ)
- Greeks created separate signs to represent vowels. They took the Phoenician consonants aleph (alpha), he (epsilon), yod (iota) and ain (omicron) and turned them into vowels AEIO, and then invented Y to complete the hand. This was the first time that proper vowel signs became an established part of an alphabet, and was a very important development.
- They created H (long E) and Ω (long O).
- Since the Phoenicians did not have aspirates, Φ, Χ (and Y) were added, too.
- None of this happened at once. Different Greek states used different versions of the alphabet for some time, both in appearance and number of letters (as

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we know from inscriptions. But by about 370 BC the Greek alphabet (α, β, Phoenician aleph, beth) had stabilized into the one we use today.

“Or rather, into the one we do not use today. Our script bears no relation to that of the ancient Greeks. Ancient Greeks wrote in capital letter (as did the Romans who took their alphabet and changed it from the Greek), with no punctuation and no gaps between the words …”

In volume II we illustrated that the Greek language was related to that of the Linear B syllabic used at Mycenae and Crete, and that this transfer occurred after the Greeks obtained their alphabet around 700-600 B.C. or later. It is now evident because of recent excavations on Crete that that island was probably the source of the Greek alphabet as explained by Harald Haarmann:

“The oldest version of the Greek alphabet originated in Crete. This statement, however, does not imply that the alphabet spread from there throughout the Aegean and to the mainland. The origins of Greek alphabetic writing in Crete are secured by the chronology of recent finds which illustrate the great age of the Cretan version of the alphabet. It has long been believed that the Phoenician script became known to the Greeks through their trade relations with ports on the Syrian and Palestinian coast. Important places for intercultural exchange of this type may have been Ugarit and Byblos. The idea of a direct adoption of Phoenician writing by the Greeks at places in the Near East cannot be corroborated by any find.

“In recent years, archaeology has been providing more evidence that Crete had strong relations with Phoenicia from the tenth century BC onwards (Helm 1980:94f). The number of Phoenician goods increases considerably during that period. Among the items of Phoenician origin is a fairly old specimen of Phoenician writing which has been discovered on the island. This is the Phoenician inscription on a bronze bowl from Tekke near Knossos dating back to the end of the tenth century BC … This is the oldest document of Phoenician writing so far unearthed in Greek territory (Sznycer 1979).

“This document leads one to the conclusion that Phoenician writing was known to the Cretans as early as the tenth century BC …”

The problem inherent in this finding is that there is no evidence of Iron Age, 1000-850 B.C. material culture in Phoenicia, as shown above. Thus, how could non-existent Phoenician cities carry on trade and spread their alphabet to Crete?

It has generally been assumed that the Greeks as they traded with Phoenicia obtained their alphabet around 730 B.C. (to be noted below). But the Greeks supposedly dominated Crete at that time–1000 to 850 B.C.–why did it take them

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almost 200 to 400 years to accept alphabetic writing? Haarmann discusses this
discovery:

“In Crete, there is also evidence for the initial attempt to adopt Phoenician writing
for recording Eteocretan and Greek … with respect to the recording of Greek Crete
holds a record, and it is this evidence of the oldest Greek inscription ever found
(Duhoux 1981:288;168). This document is still older than the hitherto believed oldest
[alphabetic] text in Greek, written on the Dipylon jug from Athens …

“It is not surprising that Crete was the region where the oldest known version
of the Greek alphabet originated. Since the second millennium BC, Crete was the
spring-board of the Aegean impact on the cultures of the eastern Mediterranean,
and eastern influences found their way into the Aegean archipelago via Cretan
mediation. The so-called ‘dark ages’ (from the eleventh to the ninth centuries BC)
were not dark in Crete, where cultural evolution continued during the fusion
process of Greco-Minoan synthesis. In Crete, Minoans and Mycenaean Greeks
experimented with writing, as did the Dorian Greeks. Bilingualism and the
dynamics of a fusion culture made people susceptible to the knowledge of the new
technology, alphabetic writing, which was introduced to the island. The old linear
writing systems were not forgotten during the phase when the Phoenician writing
was being adopted and remodeled for the needs of the languages in Crete, as the
appearance of linear signs in the third century BC shows.”68

This, in a way, is the approach taken by this author in volume II. However, we
wish to date the Phoenician alphabetic system closer to the present, to between
about 800-600 B.C., and thus do the same for the Cretans who obtained that script.
What we are attempting to show is that all these developments in alphabetic
writing were started in this time period, and that the Greeks obtained their
Phoenician script at this very time when the alphabet was being created in different
forms in Syria, Palestine, etc. It was all part of the great intellectual revolution
which accompanied the ancient Industrial Revolution. All these developments are
part and parcel of this revolutionary epoch. And in fact the Greeks only adopted
and adapted the Phoenician alphabet about 200 years closer to the present, as
Andrew Robinson shows:

“There are two major difficulties in deciding the date of invention of the Greek
alphabet. First, the earliest known alphabetic Greek inscription [was originally]
dated about 730 BC. Second, there are no known practical business documents for
over 200 years after the invention of the Greek alphabet.”69

The point to stress is that the Greeks at about 730 B.C. employed forms of
Phoenician alphabetic writing that go back to 1000 B.C. and do so after the

68 Ibid., pp. 133-134
69 Robinson, op.cit., p. 167
Phoenician alphabet had evolved for 370 years into a different form. This created a chronological problem. According to Robert K. Logan:

“[Joseph] Naveh provides an even greater challenge to the scholars of Greek culture with his dating of the transfer of the Phoenician alphabet to the Greeks based on his studies of Semitic epigraphy. Naveh contends that the transfer occurs as early as 1100 B.C., whereas earlier scholars had placed this event some three or four hundred years later [and with the 200 year break between 730 B.C. and the use of the alphabet in Greek business documents the event took place 500 to 600 years later].

“Based on the Cadmus myth, the similarity of early Greek letters and Phoenician letters, the identical order of the two alphabets, the similarity of the [letter] names alpha [Greek] and aleph [Phoenician] and the similarity of the sound values of these corresponding letters, scholars are in unanimous agreement about the Phoenician origin of the Greek alphabet. Only the date is the source of disagreement. The earliest [then] known Greek inscriptions are placed in the eighth century B.C., which is the date many scholars believe the transfer of the Phoenician alphabet took place. Rhys Carpenter in 1933 was the first to set this date. His technique was simple. Obviously the Greek alphabet must have branched off from the Semitic at the point where the chronologically contemporary resemblances are strongest. He chose the period between 732 and 719 B.C., however, his idea of a borrowing at one fixed point in time is probably too simple.

“Naveh accepts the [then known] fact that the absence of evidence of a Greek alphabet before the eighth century B.C. argues strongly for its inception at this time. However, he points out a number of cases in Semitic epigraphy where no examples of known scripts have been uncovered for periods varying from one hundred to two hundred years. To support his argument, he points out that the earliest Greek alphabetic inscriptions were written in both horizontal directions [right-to-left as well as left-to-right] and also in boustrophedon [first line right-to-left, next line left-to-right, next line right-to-left, and so on, like an ox plowing a field]. In fact, the term *boustrophe* is a Greek term coined to describe early alphabetic writing. He also points out that by the eighth century B.C., the time of the purported transfer [of the Phoenician alphabet to Greece], all the Semitic scripts with which the Greeks could have had possible contact [no longer used the boustrophedon form of writing and] had settled into a right to left horizontal pattern.

“He considers it highly unlikely that the Greek alphabetic script first began right to left [as did the Semitic writing of that time], then evolved into boustrophedon, and then eventually reversed directions [going to a right-to-left form]. THERE IS NO KNOWN CASE WHERE AN ALPHABET SCRIPT WAS TRANSFERRED FROM ONE CULTURE TO ANOTHER AND REVERSED DIRECTION … Ethiopic, which is written from left to right like Greek, was
derived from Proto-Arabic, which split off from Proto-Canaanite when both were written in either direction. Phoenician eventually evolved a right to left script and Ethiopic a left to right. This, claims Naveh, is the likely pattern with Greek also. He contends that the transfer from Phoenician to Greek occurred when the Phoenician script was still in its Proto-Canaanite form and hence not yet settled in any direction. Greek and Phoenician then evolved into horizontal scripts written in opposite directions. Naveh places the transfer at about 1050 B.C.”

In addition, Logan shows other proofs by Naveh who “… provides additional evidence for this dating by comparing letters and showing that the Archaic Greek letters resemble the Proto-Canaanite letters of 1050 B.C. ‘The Greek letters are considerably less cursive than the eighth- or even ninth-century Phoenician letters.’ He also points out that the Archaic Greek scripts show an enormous amount of local variation while the eighth century B.C. Phoenician script had already evolved into a single national form. This local variation, which persists until the fourth century B.C., when Ionian [script as opposed to the other Greek forms] becomes the uniformly adopted Classic Greek script, argues for an earlier adoption [by the Greeks of the Phoenician alphabet] than the eighth century B.C.”

People that adopt a form of writing from another culture will adapt it in some ways to suit their linguistic needs, but they nevertheless follow its form, such as the order of the letters of its alphabet. Historians assume that the Greeks not only reversed the Phoenicians’ direction of script writing but then changed its direction and wrote it in the older boustrophedon form when “there is no known case where an alphabetic script was transferred from one culture to another and reversed direction” of writing almost immediately or wrote in the original direction and then the opposite one. Why would the Greeks borrow the much older letter forms of the Phoenician alphabet of 1050 B.C. in the 700s B.C. when, in fact, these forms were no longer being used? These highly improbable, historically implausible approaches to alphabetic borrowing are not rational forms of human behavior. They’re irrational, unreasonable, and anti-historical approaches, all chosen specifically to maintain an irrational, unreasonable chronology.

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71 Ibid., p. 42
CHAPTER 9

A TENTATIVE BIBLICAL CHRONOLOGY

“Where there is nothing to give written help, recourse to circumstantial evidence is appropriate and demonstrating that is the aim of my essay. The results can never resolve questions of the historic or legendary nature of any biblical statement. By showing some of those statements actually reflect attested ancient practices [and evidence], they do give plausibility to the Hebrew text, a plausibility which has usually been ignored or denied …”


The following materials are largely historical in nature and are derived by following the short chronology to its natural end. The evidence that follows, being mainly historical, cannot, therefore, be taken as definitive, and thus needs to be augmented by further forensic research to determine its validity. Because this historical analysis fits the short chronology, it is worthy of consideration, but will only stand the test of time when it is supported by additional evidence. In this regard, it is interpretative and subject to correction based on either new knowledge or new scientific methods of analysis.

LOOKING FOR DAVID AND SOLOMON

Now that we have moved Hebrew history/chronology forward in time by some 274 years, certain historical elements of biblical history can be better understood and seemingly confirmed in this new time frame. The missing evidence for the historical reality of the great Hebrew monarchs David and Solomon that appear to be non-existent in the historical and archaeological record can emerge, creating a place for them in this new time scheme that did not exist in the established chronology. The possible importance of this cannot be too greatly emphasized, because it is just this missing evidence, in the main, which has been employed by the minimalists to negate the validity of David and Solomon as historical figures
and to maintain that they are fictitious inventions of later biblical writers. Because there is also no archaeological evidence of relatively large kingdoms ruled over by these great Hebrew monarchs, the minimalists have convincingly argued that the lack of non-biblical historical or archaeological evidence clearly denies their reality. However, in terms of the short chronology, moving David and Solomon some 274 years closer to the present indicates that the missing archaeological stratum is improperly dated: it is to be found around the 8th-7th centuries B.C. and not the 10th. Once David and Solomon are kings of ca. the 8th-7th centuries B.C., the archaeology of their possible great empire is undeniably there. Finkelestein and Silberman sum up the position of the minimalists thus:

“Did David and Solomon exist?”

“This question, put so baldly, may sound intentionally provocative. David and Solomon are such central religious icons to both Judaism and Christianity that the recent assertions of radical biblical critics that King David is ‘no more a historical figure than King Arthur,’ have been greeted in many religious and scholarly circles with outrage and disdain. Biblical historians such as Thomas Thompson and Niels Peter Lemche of the University of Copenhagen and Philip Davies of the University of Sheffield, dubbed ‘biblical minimalists’ by their detractors, have argued that David and Solomon, the united monarchy of Israel, and indeed the entire biblical description of the history of Israel are no more than elaborate, skillful ideological constructs produced by priestly circles in Jerusalem in post-exilic or even Hellenistic times.

“Yet from a purely literary and archaeological standpoint, the minimalists have some points in their favor. A close reading of the biblical description of the days of Solomon clearly suggests that this was a portrayal of an idealized past, a glorious Golden Age. The reports of Solomon’s fabulous wealth … and his legendary harem (housing seven hundred wives and princesses and three hundred concubines, according to I Kings 11:13) are details too exaggerated to be true. Moreover, for all their reported wealth and power, neither David nor Solomon is mentioned in a single known Egyptian or Mesopotamian text. And the archaeological evidence in Jerusalem for the famous building projects of Solomon is non-existent.”1

James Barr puts the minimalists’ argument even more emphatically with stronger biblical consequences:

“Let us take an example of the sort of argument that has been used [to attack biblical history]. Here is a statement by the Danish scholar Lemche: ‘The traditional materials about David cannot be regarded as an attempt to write history, as such. Rather, they represent an ideological programmatic composition which defends the assumptions of power by the Davidic dynasty.’ Extending this somewhat further, we may find that we have no truly historical confirmation that there ever was a

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1 Finkelstein, Silberman, *The Bible Unearthed*, op. cit., p. 128
David or a Solomon or the united kingdom of Israel and Judah over which they are said to have ruled. Still less did the united kingdom of Israel and Judah control a substantial empire extending far to the north and east. Much of what is told in the Bible about the earlier periods may be an expression of religious belief and ideology rather than anything approaching true historical writing.

“To appreciate the importance of the argument as presented, especially, by [Philip] Davies, one has to see how radical it is. It is not just, as one might at first imagine, an argument that the people of Israel existed over all this time, but that its ‘historical’ narratives were inaccurate. It is an argument that there was no people ‘Israel’ there at all, not a people whose lineaments can be discerned, whether by critical or by non-critical means, from the biblical texts. ‘Ancient Israel’ is thus properly enclosed in quotation marks, for it is a literary construct, something that exists in the Bible but is the product of the imagination of scribal writers from long afterwards, who were seeking to construct an ‘ancient’ history in order to provide legitimation for their own view of, and plans for, their own quite different society … this view, exemplified by Davies, well illustrates the questions that are raised.”

In an attempt to mitigate these facts, Alan Millard argues:

“Now the failure to find any inscriptions of Solomon is also noted as a counter-indication to the biblical portrayal of him. ‘Why has there not been found a single inscription, not even a fragment from Solomon himself?’ The answer lies in … part of the accidental element in archaeology, and in the ancient practice of re-using old building blocks, irrespective of their previous role … When a rapid calculation reveals that only 16 out of 113 kings ruling in the Levant between 1000 and 600 BC, including the kings of Israel and Judah, are known from their own inscriptions, the absence of Solomon becomes less remarkable. It is less significant still when we recall that there has yet to be found in Palestine a monument inscribed with the name of another powerful and wealthy ruler of the land, many of whose buildings are still visible, King Herod … The lack of physical remains is not an insurmountable objection to accepting the statements of the Hebrew historians.”

In the same vein, Daniel Hillel asks: “If the story of David is untrue, composed merely as a paean to glorify the putative founder of the Judean dynasty, why would the writer have included such embarrassing details as David’s leadership of an outlaw band engaged in extortion (1 Samuel 25); David’s alliance with Achish, king of Philistia, who was an enemy of Israel (1 Samuel 27); and David’s immoral affair with Bathsheba and his atrocious act of arranging the death of her husband (2

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3 Millard, *op.cit.*, p. 46
Indeed, the Bible reads like no other ancient historical narrative, exposing the Hebrew kings as liars, frauds, and murderers. It has also been argued that other ancient documents are rarely questioned as to their validity, but only the Bible needs outside confirmation. In all these instances, whatever is offered to sustain the Bible as history, is not accepted as evidence. We maintain that both the Bible and other ancient historical documents must meet the same standards to be accepted as historically valid. These arguments for the Bible’s validity may or may not be correct, but that does not matter; without clear-cut evidence to settle these suppositions we do not have a forensic historical case.

The arguments against the historical validity of Kings David and Solomon rest on two pillars that are needed but are missing in the evidence. The first is the lack of archaeological evidence of their empire in Palestine and particularly Jerusalem. As we cited Finkelstein and Silberman above, “the archaeological evidence in Jerusalem for the famous building projects of Solomon is non-existent.”

The second argument relates to the fact that, according to the minimalists, there was no mention of either David or Solomon outside of the Bible. But, this was to change in 1993, as Finkelstein and Silberman explain:

“Yet in the summer of 1993, at the biblical site of Tel Dan in northern Israel, a fragmentary artifact was discovered that would change forever the nature of the debate. It was the ‘House of David’ inscription, part of a black basalt monument, found broken and reused in a later stratum as a building stone. Written in Aramaic, the language of the Aramean kingdoms of Syria, it related the details of an invasion of Israel by an Aramean king whose name is not mentioned on the fragments that have so far been discovered. But there is hardly a question that it tells the story of the assault of Hazael, king of Damascus, on the northern kingdom of Israel … This war took place in the era when Israel and Judah were separate kingdoms, and the outcome was a bitter defeat for both.

“The most important part of the inscription is Hazael’s boasting description of his enemies:


“This is dramatic evidence of the fame of the Davidic dynasty less than a hundred years after the reign of David’s son Solomon. The fact that Judah (or perhaps its capital, Jerusalem) is referred to with only a mention of its ruling house is clear evidence that the reputation of David was not a literary invention of a much later period. Furthermore, the French scholar André Lemaire has recently

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suggested that a similar reference to the house of David can be found on the famous inscription of Mesha, king of Moab … to the east of the Dead Sea.”

Steve L. McKenzie discusses the Mesha stele in a balanced way:

“The Tel Dan Stele prompted the announcement of the discovery of the same expression, ‘the house of David,’ in another previously known inscription. The Mesha stele or Moabite stone is the greatest tragedy in the history of the archaeology of Palestine… It was found [completely] intact in 1868 among the ruins of Dibon, the ancient capital of Moab, the country on the other side of the Dead Sea from Israel … It was the most spectacular artifact ever found in Palestine, and the European powers were quickly embroiled in a bitter competition to acquire it. The Bedouin tribe that controlled it felt threatened by its presence and decided to get rid of it. They hoisted the stone in the air and dipped it alternately in fire and water until it broke to pieces. Most of the inscription of more than thirty lines was later reconstructed from recovered fragments and a ‘squeeze’ (an impression [of the inscription that was] left on a plaster-soaked paper [applied to the stele]). The squeeze was also fragmentary, having been torn away in the middle of a gunfight [for it] before the stone was destroyed. Both the reconstruction and the squeeze have been in the Louvre Museum in Paris ever since.

“The monument was commissioned by Mesha, king of Moab … it is [believed to be] contemporary with the Tel Dan stele. …

“The basic content of the inscription has been known since the last century. However, for the past few years, the French scholar André Lemaire has been studying both the monument fragments and the squeeze. Living in Paris, he has a unique opportunity to examine it closely and repeatedly. He hopes, therefore, to solve some of the problems of the reconstruction and produce a definite edition of the inscription.

“Lemaire has found what he believes to be an additional occurrence of the expression ‘the house of David.’ The phrase occurs … in line 31 … the relevant lines are as follows:

“‘And the house [of Da]vyit dwelt in Horonen…’

“Since the monument is broken … only about half of the phrase is visible.”

The letter “D”, daleth, is missing from David’s name and thus one must be careful regarding the validity of this inscription, although Kenneth Kitchen expressed approval of Lemaire’s restoration, as did Simon B. Parker, who cited Kitchen but indicated caution by employing a question mark. For a full discussion of this inscription, see Lawrence S. Mykytiuk, Identifying Biblical Persons in Northwest Semitic Inscriptions of 1200-539 BC (Atlanta GA 2004), pp. 265-277.

Elsewhere, Finkelstein and Silberman write of the Tel Dan inscription:

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5 Finkelstein, Silberman, *The Bible Unearthed*, op.cit., p. 129
6 McKenzie, op.cit., pp. 13-14
“It was a fragment of a triumphal inscription written in Aramaic, its ancient letters chiseled in black basalt. In the following year, two more fragments of the stele were discovered … The king it commemorates was most probably Hazael, ruler of Aram Damascus, who was known both from the Bible and Assyrian records … His battles against Israel are recorded in the book of Kings …

“… this inscription offers a unique perspective on the turbulent politics of the region … It records, from the Aramean side, the territorial conflict between Israel and Damascus …

“In words chiseled into the stone … Hazael claimed to have killed the king of Israel and his ally, the king of the ‘House of David.’ It is the first use of the name David in any source outside the Bible … It most probably refers to the deaths of King Jehoram of Israel and Ahaziah of the ‘House of David.’ The minimalists’ contention that biblical history was a late and wholly creative composition and that David was a fictional figure was dealt a serious blow.

“The ‘House of David’ inscription … testifies to the existence of a line of kings who … traced their legitimacy back to David. Hazael used the common genre of this period, of referring to a state after the name of the founder of its ruling dynasty. But the mention of the royal name—though confirming the existence of a dynastic founder named David, offers no new information about the man himself …

“In short, the Tel Dan inscription provides an independent witness to the historical existence of a dynasty founded by a ruler named David, from just a few generations after the era in which he presumably lived.”

While the Tel Dan inscription has given the maximalists the support for King David as a valid historical figure, the problem they still face is that outside the Bible they can find no foreign expression related to Solomon. But let us recall that earlier we were able to point out that the cities of Jerusalem and Botrys, which are mentioned in the Amarna Letters of the 18th Dynasty, place that Dynasty well inside the first millennium B.C. In fact, we can now suggest that the Jerusalem of these letters may have contained a temple named for Solomon. This was outlined by Emmet Sweeney, citing Jules Lewy’s “The Sulman Temple in Jerusalem,” in The Journal of Biblical Archaeology, vol. 59 (1940), pp. 159 ff. As Sweeney points out, it was only the established chronology that forced Lewy and the rest of the historians to dismiss this direct citation referring to the Temple of Solomon:

“Letter 290, from the king of Jerusalem, referred to a place read as Bet-NIN.IB. Originally, this was believed to be a reference to Assyria (House of Nineveh). However, in 1940, the eminent Assyriologist Professor Jules Lewy suggested that Bet-NIN.IB be translated as “Temple of Shulman” and that this was

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7 Finkelstein, Silberman, David and Solomon, op.cit., pp. 264-266
an alternative name for Jerusalem itself at the time.\textsuperscript{8} In the letter in question, Abdi-Hiba complained that the land was falling to the invading bands (\textit{habiru}), “and now, in addition, the capital of the country of Jerusalem – its name is Bit Sulmani – the king’s city, has broken away.” Beth Sulman in Hebrew, as Professor Lewy correctly translated, is Temple of Sulman. Believing the Amarna Letters to date from the 14th century BC, Lewy could not, of course, surmise that the edifice was the Temple of Solomon and therefore supposed it to be a Canaanite place of worship of a god found in Akkadian sources as Shulmi, Shulmanu, or Salamu. He wrote, “Aside from proving the existence of a Sulman temple in Jerusalem in the first part of the 14th century BC, this statement of the ruler of the region leaves us in no doubt that the city was then known not only as Jerusalem, but also as Bet Sulman.” He saw it as “significant” that it was only the name Jerusalem that “reappears after the end of the occupation of the city by the Jebusites, which the Sulman temple, in all probability, did not survive.”\textsuperscript{9}

Dwardu Cardona has argued “[that] Bet (or Bit) Shulman should be understood to refer to the Kingdom of Judah is not a well-reasoned hypothesis … [i]n el-Amarna letter 74, Rib-Addi asks that his warriors should assemble in Bet Ninib/Shulman … an area that stretched between Jerusalem and the Negeb [which] would have been too ambiguous an order.”\textsuperscript{10} Cardona argues that the “el-Amarna letter 290 \textit{does} incorporate the [Akkadian hieroglyphic] determinative–‘dingir’–for divinity … –viz ‘shulmani’ … Shulman has to be understood as the name of a deity and there can be no connection with either the palace or temple of Solomon beyond the fact that Solomon was named for the same deity.”\textsuperscript{11} He argues further “that Bet Shulman was merely another designation for Urusalim, the one derived from Assyrian, the other from Chaldean. The Chaldean name passed into Hebrew, the Assyrian did not. The one popular; the other not.”\textsuperscript{12} But because Palestine/Retenu was “God’s Land” to the Egyptians it is highly probable that the temple in its capital would have been designated ‘dingir’–and does suggest (not prove) that Bet Shulman was the temple of a god-like king of Retenu: Solomon.

In this regard, Eva Danelius reports, citing Naville:

“The governor [of a province of Punt] ‘is a tall, well-shaped man … his hair flaxen … his nose aquiline, his beard long and pointed. … The Puntites are painted red, but not so dark as the Egyptians.’ Chabas thought that the Puntites

\begin{itemize}
\item \textsuperscript{8} Jules Lewy “The Sulman Temple in Jerusalem” \textit{The Journal of Biblical Archaeology} 59 (1940), pp. 519ff.
\item \textsuperscript{9} Sweeney, \textit{Empire of Thebes, op.cit.}, pp. 46-47
\item \textsuperscript{10} Dwardu Cardona, “Bet Shulman,” \textit{KRONOS} vol. X, no. 2 (winter 1985), pp. 90-1
\item \textsuperscript{11} \textit{Ibid.}, p. 90
\item \textsuperscript{12} \textit{Ibid.}, pp. 90-1
\end{itemize}
belonged to the Semitic stock, but Naville doubted it. According to the reproductions in Naville, the governor has a beard trimmed like that of the Egyptian gods, and Pharaohs. It seems that the clean-shaven Egyptians were thoroughly impressed by the fact that the inhabitants of ‘God’s Land’—an alternate name for Punt—wore beards like that, and they referred to them as ‘the bearded ones,’ a title given to no other people. Breasted, however, writes: ‘Southerners of God’s Land,’ instead of translating literally: ‘the Bearded Ones of God’s Land.’ In doing so, Breasted blurs the connection hinted at by the Egyptian designation, that not only the Land reached by Hatshepsut’s expedition was God’s, but that its inhabitants, too, in their outer appearance lived up to its reputation—F. Petrie, on the other hand, stated simply: ‘The physiognomy of the Puntites is finely rendered … the form of the beard is that of the Egyptian gods’; and E. Zyhlarz calls them ‘the God-like bearded ones of God’s Land.’”

Here, we have the people of Punt seen as god-like and this can explain why the Temple of Shulman was held to be divine.

Again, the various forms of evidence seem to indicate that Jerusalem was known as Bet Sulman, or the House of Solomon, in the first millennium B.C. Thus, we not only have sources outside of the Bible for the existence of the House of David, in the Tel Dan ossuary, but also for the possible Temple of Solomon in the Amarna Letters sent by the king of Jerusalem.

What is therefore now necessary is to find the archaeological and other evidence that supports these identifications.

Finkelstein and Silberman report on the lack of archaeological evidence for the years 1000-900 B.C. of any of this in Israel, especially Jerusalem:

“The grandiose descriptions of Solomonic wealth and unchallenged royal power are absurdly discordant with the historical reality of the small, out-of-the-way hill country kingdom that possessed no literacy, no massive construction works, no extensive administration, and not the slightest sign of commercial prosperity. Of course one might argue that admiration for the kinds of achievements attributed to Solomon might have been conceivable in even the poorest or most backward of kingdoms. But the biblical narrative is filled with so many specific details about trade transactions, monetary values, and complex royal administration that its authors seem to be describing a reality they knew from personal experience—not merely dreaming of an invented and imagined utopia.

“It is a world of effortless international connections and the celebration of commercial prosperity… Indeed, the stories of Solomon’s negotiations with King Hiram of Tyre to help build the Temple, his international trade in thoroughbred horses, his lucrative maritime expeditions, and the gifts of precious goods from the

queen of Sheba … celebrate the values and vision of what we would call today a
globalized economy.” ¹⁴

As we are well aware, this evidence must exist in the 8th and 7th centuries B.C.
because there is no such material in the 10th century B.C. In this respect not only
must we bring forward the inscriptive evidence—the Bible—by about 274 years,
we must also move the chronology of the archaeological evidence that exists in
Jerusalem and Palestine as well. Since the scriptural evidence is disconnected from
David and Solomon, it can then be relocated in time to mesh with the other
evidence. That is, biblical monarchical history begins in the 8th century B.C. as all
the evidence in this volume and the others indicates, and thus the archaeological
evidence which has been interpreted as dating prior to this time must perforce be
situated in this period. The question is: Is there evidence for a thriving
cosmopolitan Jerusalem in the first millennium B.C.? The answer in this case is,
most assuredly, yes! That material, like the Amarna evidence for Jerusalem, has
been misassigned to other Hebrew kings instead of possibly David and Solomon.

When we move David and Solomon closer to the present by 274 years, their
reigns would fall not in the tenth century, but from the last decades of the eighth
century through most of the first half of the seventh. It is in this later time range
that we must find the archaeological evidence of their empire in the ancient
Industrial Revolutionary period.

Finkelstein and Silberman report:

“[I]t is precisely at this time that the first archaeological signs of state formation
are evident in Judah. Archaeological surveys have revealed that the number of
scattered agricultural villages (though still modest) was steadily growing. In the
Judahite lowlands, permanent centers of administration, controlling specific
regions or specialized aspects of the economy, were first constructed … In the rich
grain-growing lands of the Shephelah in the west—the traditional breadbasket of
Judah—two impressive citadels were constructed, requiring the organization of
considerable labor, and [these] were far more imposing in appearance than any
previous settlements in that region in the Early Iron Age. At Lachish, excavations
by British archaeologists in the 1930s and a subsequent Israeli expedition directed
by David Ussishkin revealed a massive podium that supported a fortified complex
containing storerooms and a palace; at Beth-shemesh, slightly farther to the north,
evidence of another massive construction effort has recently been uncovered by a
Tel Aviv University team headed by Shlomo Bunimovitz and Zvi Lederman. It
includes a system of massive fortifications and an elaborate subterranean water
system that would enable the residents of this important site in the rich Sorek
Valley to withstand a protracted siege.

¹⁴ Finkelstein, Silberman, David and Solomon, op.cit., p. 153
“Even more telling is the sudden appearance of evidence for centralized administration in the Beer-sheba Valley … At both Arad, on the eastern end of the valley, and Tell Beer-sheba in the west, permanent fortresses were constructed … They seem to represent an effort to take control over the trade routes that passed through the Beer-sheba Valley and to protect the southern borderlands of the kingdom…”

With respect to Jerusalem, we discover very interesting evidence regarding its existence as reported by Finkelstein:

“If one needs to summarize over a century of exploration in Jerusalem, the proper statement regarding the Bronze and Iron Ages would be that archaeology revealed evidence for major building activity in two periods only: the Middle Bronze II-III and the Late Iron II (the eighth-seventh centuries B.C.E.). … The interval between these periods, which covers the Late Bronze, the Iron I, and the Early Iron II (ca. 1550-750 B.C.E.) provides indication of habitation but almost no signs of monumental building operations [between these periods].”

That is, we have about an 800-year archaeological break in Jerusalem’s stratigraphy between ca. 1550 and 750 B.C. very similar to that at Tall Munbaqa. Matthew Sturgis describes this hiatus in archaeology thus:

“…Ronnie Reich, the archaeologist leading the IAA dig [at Jerusalem reports]. His discoveries [related to David and Solomon] have tended to fall either side of the mark. They are either several centuries too early, or several centuries too late … Reich explains, ‘… our digging inside the wall produced only finds from the eighth century BCE, two centuries after the time of David and Solomon …’

“If we go by the pottery,” explains Reich, ‘and that is the only evidence there is, then we have a problem. We don’t have a single sherd in the close vicinity of the [Gihon] spring dating to the tenth century BCE, the days of David.’ There is a huge gap in the archaeological record: ‘We have only the Canaanites in the eighteenth century BCE, and the Israelites in the eighth century BCE, a millennium later, but in-between we don’t have anything. That nobody during the tenth century [B.C.] broke a bucket or a jar or anything made of pottery near the site is difficult to accept.’ If archaeology teaches one thing, it is that people in ancient times broke pottery in huge quantities. It is a bizarre anomaly. Reich describes it as a confrontation between texts and archaeological finds.”

There we have it. The dating of Jerusalem is not based on evidence. Because of this methodological approach, going hand in hand with the established chronology,

15 Finkelstein, Silberman, op.cit., pp. 103-4
17 Matthew Sturgis, It Ain’t Necessarily So: Investigating the Truth of the Biblical Past (London 2001), pp. 113-134
the historians have invented a 1000-year Dark Age when practically no pottery was ever broken at that site, although historians say people did live there.

Only a dozen or so such pottery sherds were found in the ground to cover this 800-year period. Is it remotely possible that even in a village over almost a millennium of habitation only one or two clay pots, or a few more, would have been broken? In fact, archaeologist Joseph Zias admitted that “not a single potsherd [among the dozen] could be definitely dated to the tenth century B.C.”

What is clearly required is that this 800-year archaeological Dark Age never existed, indicating Jerusalem of the time of David and Solomon belongs to the late 8th to well into the 7th century B.C. Finkelstein goes on to explicitly state:

“There is no question that in the second half of the eighth century B.C.E. the built-up area of Jerusalem expanded from the City of David to the Western Hill and the city reached its maximum size in biblical times. At the same time dozens of settlements of all size ranks—from regional towns to small villages and tiny farmsteads—appeared in the hill country of Judah…”

It is here we can date Jerusalem’s growth from a small town to a cosmopolitan city. It is also at this time, because the climate changed to one of great aridity, that we date the agricultural terraces that allowed the enlarged population to thrive. However, the dating of the agricultural terraces, because of the established chronology, is highly conjectural, as Finkelstein shows:

“... I wish to comment briefly on two construction elements uncovered on the eastern slope of the City of David. I refer to the system of stone terraces, unearthed by both [Kathleen] Kenyon and [Yigal] Shiloh, and to the ‘stepped stone structure’... Both have been mentioned time and again in relation to one or more of the ‘interval’ periods: clarification of the confusion regarding their relationship—whether they were built together or at two different periods—and date is key for any discussion of the archaeology of Jerusalem from the Late Bronze to the early Iron II.

“Cahill and Tarler argued that the two were built together in the thirteenth–twelfth centuries B.C.E., while Kenyon, Shiloh, and Steiner proposed that they were built in two different periods. Kenyon and Shiloh dated the stone terraces to the thirteenth–twelfth century and the stepped stone structure to the tenth or ninth century B.C.E."

Finkelstein has his own dates: “… I would opt for an Iron I date for the stone terraces and a ninth … or even eighth-century date for its renovation …” In essence, the dates for these structures range between the 13th down to the 9th

18 Nicholas Clapp, Sheba: Through the Desert in Search of the Legendary Queen (Boston MA/NY 2001), p. 102
19 Finkelstein, “The Rise of Jerusalem…”, op.cit., p. 82
20 Ibid., pp. 84-85
21 Ibid., p. 86
century B.C., a period of 400 years. But at the very same time no-one has explained why with all this construction going on in and around Jerusalem, the city of Jerusalem was archaeologically sterile. This brings us to the epoch where we place David and Solomon, which was given to a later biblical dynasty known as the Omrides. Finkelstein and Silberman have called this dynasty “Israel’s Forgotten First Kingdom.”

It is our contention that this forgotten kingdom, assigned to the Omride kings who followed David and Solomon some centuries later, actually belongs to these earlier, possible monarchs, and should be placed in the 8th-7th centuries B.C. Thus we ask the readers to think of David and Solomon’s kingdom as one reads the following:

“The Omrides are remembered as among the most despised characters of biblical history. Yet the new archaeological vision of the kingdom of Israel offers an entirely different perspective on their reigns. Indeed, had the biblical authors and editors been historians in the modern sense, they might have said that Ahab was a mighty king who first [like Solomon] brought the kingdom of Israel to prominence on the world stage and that his marriage to the daughter of the Phoenician king Ethbaal [like Solomon’s marriage to an Egyptian princess] was a brilliant stroke of international diplomacy. They might have said that the Omrides built magnificent cities to serve as administrative centers of their expanding kingdom. They might have said that Ahab and Omri, his father before him [like Solomon and David, his father before him], succeeded in building one of the most powerful armies in the region—with which they conquered territories in the far north and in Transjordan. Of course, they might also have noted that Omri and Ahab [like David and Solomon] were not particularly pious and that they sometimes were capricious and acted brutally. But the same could be said of virtually every other monarch of the ancient Near East.

“Indeed, Israel, as a state [at that time] enjoyed natural wealth and extensive trade connections that made it largely indistinguishable from other prosperous kingdoms of the region … Israel [then] had the necessary organization to undertake monumental building projects, to establish a professional army and bureaucracy, and to develop a complex settlement hierarchy of cities, towns, and villages—which make it the first full-fledged Israelite kingdom. Its character, goals, and achievements were dramatically different from those of the kingdom of Judah. Therefore, they have been almost totally obscured by the Bible’s condemnation, which supports the later claims of the southern, Davidic dynasty for predominance by demeaning and misrepresenting nearly everything that the northern, Omride dynasty did.”

The reason for this enormous growth and power is related to the fact that the Hebrews had devised an irrigation system (probably learned while they were in Egypt

22 Finkelstein, Silberman, *The Bible Unearthed*, op.cit., p. 169 ff
23 Ibid., pp. 169-170
with the Hyksos and understood how irrigation worked). Furthermore, we also date the Hebrew nation to the time of the ancient Industrial Revolution, so that they were taking part in the vast trade which enriched their kingdom, allowing them to accumulate wealth to undertake large building projects. These economic changes had a ripple effect that reverberated throughout that ancient land, and demand for food for the growing, prosperous population required that the outlying districts had to undergo cultivation, which in turn enabled these districts and towns to prosper, grow, and build. In this way, the short chronology explains this efflorescence of Israel.

Part of the wealth which Solomon’s kingdom attained may be attributed to mining operations carried out in and near Israel. Thomas Levy of the University of California at San Diego, professor of anthropology and Judaic studies, has carried out excavations in the southern Jordan site of Khirbet en Nahas which was aired on the Public Broadcasting Station program Nova and National Geographic Television, titled “Quest for Solomon’s Mines,” on Tuesday, November 23, 2010 at 8 p.m. According to the narrator,

“His [Thomas Levy’s] search has led him down from the highlands [of Edom] into the baking desert cauldron of the Dead Sea Rift Valley. It was here, in the no-man’s land between ancient Israel and Edom, that he discovered [in an area called Wadi Feynan … an entire valley covered with a mysterious black rock. This was solidified slag, the waste product of metal smelting and on a massive scale.

“Nearby, multiple shafts dug through rock and, far underground, tunnels, stretching deep inside the hills. And everywhere a striking blue-green rock: the unmistakable evidence of natural copper.

“The slag, the mines, the copper, it all added up. This was an ancient copper mining and smelting complex, perhaps the source of wealth behind the Edomite kingdom. …

“Thomas Levy: ‘Khirbet en Nahas, in Arabic, means ‘the ruins of copper.’ As you can see around us, the site is just covered with heaps of black industrial slag. …

“Narrator: “But … small-scale village [metal] production is not what Tom has discovered at Khirbet en Nahas. Over years of excavation, his team … has revealed the remains of a massive operation, a copper producing factory.

“’The site was enormous. Its massive walls, buildings and slag heaps covered an area of 25 acres. Up to a thousand men worked here, day and night, feeding the furnaces where the copper was smelted.’”

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24 Nova/Quest for Solomon’s Mines, Transcript, Internet, pp. 2-3
Importantly, an ostracon was found containing writing from which Greg Bearman got a clear image “[b]y combining and processing photos taken at many different wavelengths.”

According to the narrator:

“Struggling to piece together the words which the letters form, the experts can hardly contain their excitement.

“EXPERT: “This is definitely a Hebrew word [meaning] Don’t do.”

“NARRATOR: “They can make out other Hebrew words too: ‘eved,’ worship; ‘shafat,’ judge; ‘nakam,’ revenge; and ‘melekh,’ king.

“The writing is Canaanite, but the words are Hebrew.” …

“NARRATOR: “It makes the ostracon a historic find, a remarkable testament to the birth of Hebrew writing in the process of being systematized.”

The writing has not, so far, been translated. It is hoped that further excavation will unearth more inscriptions which will tell more about the site and perhaps about kings David and Solomon. The artifacts there have been radiocarbon dated to the 10th and 9th centuries, with which we disagree, but the evidence found is tantalizing and needs more study.

Therefore, the archaeologists have a powerful empire in Israel based on the Omride evidence, but they have failed to relate it to the astronomy of the New Assyrians. That is, there is a direct relationship of the Omride kings to a Neo-Assyrian king who thus must be dated 300 or so years closer to the present. As Finkelstein and Silberman show:

“… there is some dramatic evidence of the Omrides’ power from Assyria itself. Shalmaneser III, one of the greatest [Neo-]Assyrian kings, who ruled in the years 859-824 BCE, offers perhaps the clearest (if entirely unintentional) praise for the power of the Omride dynasty. In the year 853 BCE, Shalmaneser led a major Assyrian invasion force westward to intimidate and possibly conquer the smaller states of Syria, Phoenicia, and Israel. His advancing armies were confronted by an anti-Assyrian coalition … in western Syria. Shalmaneser boasted of his great victory in an important ancient text known as the Monolith Inscription, found in the 1840s by the English explorer Austen Henry Layard at the ancient site of Nimrud. The dark stone monument, thickly inscribed with cuneiform characters, proudly recorded the forces ranged against Shalmaneser: ‘1,200 chariots, 1,200 cavalry men, 20,000 foot soldiers of Hadadezer of Damascus … 2,000 chariots, 10,000 foot soldiers OF AHAB, THE ISRAELITE …”

When we bring the Neo-Assyrian king Shalmaneser III closer to the present by 274 years, we have to move the Omride king Ahab forward in time by the same

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25 Ibid., p. 7
26 Ibid., p. 8
27 Finkelstein, Silberman, *The Bible Unearthed*, op.cit., p. 178 (capitalization added)
number of years, which places Ahab in the very early 6th century. The importance of this new dating of Ahab cannot be stressed too much. Placing him and the Omride kings in this time period means that they could not be placed in the archaeological strata and period the established chronology assigns to them in the 9th century B.C. It means that the archaeological materials that were given to the Omrides must be given to the kings who preceded them, namely David and Solomon, etc. Given that the archaeological remains of the 8th-7th centuries exhibit distinct evidence of a cosmopolitan Jerusalem and that these came prior to the Omride dynasty, this cosmopolitan kingdom could only be the kingdom possibly ruled over by Israel’s two great kings, David and Solomon.

In this respect an excavation carried out by Eilat Mazar is clearly related to this, as pointed out by Dore Gold:

“In the summer of 2005 Israeli archaeologist Eilat Mazar excavated an immense stone structure south of the Temple Mount—just where the Bible relates that King David’s palace stood. This has not yet been either proved or disproved as David’s palace, but construction of a structure of such enormous dimensions was clearly beyond the means of a petty village chieftain. With walls between six and eight feet thick, it sat atop what archaeologists called a ‘stepped-stone fortification’ that Mazar judged to be the height of a twelve-story building. The unique style of hewn stones discovered nearby was rare for central Israel at the time, but consistent with Phoenician construction work of the sort that would have been brought in by King Hiram of Tyre [a contemporary of Solomon]” …

“Pottery fragments show that the building was utilized for several hundred years through the sixth century BCE … Mazar also found an ancient bulla used as a seal for official documents, implying that the structure was some kind of governmental facility. The seal read ‘Belonging to Yechual ben Shelemiah ben Shovi,’ a name mentioned in the Book of Jeremiah (37:3) as a minister to King Zedekiah, the last king of Judah from the house of David.”^28

Since there was no material evidence in Jerusalem for a 10th-9th century B.C. empire, placing this palace in, say, the 8th century B.C. tentatively places David and this great structure in its proper place in the short chronology. I. Finkelstein, A. Mazar and B.B. Schmidt say this building has no “parallel anywhere in the land of Israel between the twelfth and early-ninth centuries B.C.E.”^29 which places it after these dates.

Niels Peter Lemche in this regard argues:

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“From the perspective of the tenth century, David and Solomon are invisible. It is as if they never lived. In spite of this, Old Testament scholars, relying solely on the Bible, argue that the argument against the assumption that there was a Davidic-Solomonic empire, is an argumentation ex silentio, an argument from silence, which is logically a false argument. From a formal point of view, it seems to be a false conclusion. The opposing party often refers to the fact that ‘absence of evidence is not evidence of absence.’ Although this may look like a valid position, it is based on mistaken assumptions. The advocates of this position make the logical mistake of mixing two categories, a text and physical reality. In the biblical narratives, David and Solomon are great kings who ruled Palestine and most of Syria. This narrative is not affected by the lack of physical evidence of the presence of these kings. The problem arises at the moment when the content of the biblical narratives is considered to have historical implications as well, because here other factors rule. Here the narrative content of the stories of the books of Samuel and Kings must be considered secondary and physical remains from the tenth century BCE primary. If the primary evidence speaks against the presence of a mighty Israelite empire in the tenth century, the biblical narrative has little to do with the historical conditions of the tenth century BCE. It belongs in a different period and should be understood in the light of the period when it came into being. …

“One school maintains that in the tenth century BCE Jerusalem was a city, although quite small …

“The second archaeological school is more radical and argues that until this day not a single trace of Jerusalem being settled in the tenth century has been found—not a single shard.

“The conclusion seems to be inevitable: … the narratives about the first kings of Israel in the books of Samuel and Kings are not history. They do not allow the historian to reconstruct the history of Israel in the period around the year 1000 BCE. … [W]e have no other traces of their presence …”

We do have physical evidence “of a mighty Israelite empire,” but in the 8th-7th centuries B.C., not the tenth. This indicates, but does not prove, that David and Solomon existed. To paraphrase Lemche:

This conclusion leads to serious consequences for the biblical tradition about the unity of the Israelite tribes dating back to the earliest times of their history. There is at present no [8th-7th century] period in the reconstructed Israelite history that allows for the existence of this tradition in a national epic. The consequences are even more compelling when we move onward to the following periods in Israel’s history.31

30 Niels Peter Lemche, The Old Testament Between Theology and History (Louisville KY 2008), pp. 144-146
31 Ibid., p. 146
The strength of this shortened chronology is that it connects Israel’s history with archaeology which no other chronology can do. It does exhibit physical evidence to complete our thesis. What is wanting is clear evidence for Solomon beyond the possible identification of Bet Shulman as the temple of Solomon which makes our thesis tentative.

Finally, we come to the Egyptian side of the chronological equation that places David and Solomon in the 8th-7th centuries, and the Omrides in the 6th century B.C. This part of the equation therefore deals with one of Solomon’s sons, Rehoboam, who, as the Bible reports, was conquered by the Egyptian pharaoh Shishak. Most biblical scholars see Shishak as the pharaoh Sheshonq I. According to I Kings, 14:25-28,

“In the fifth year of King Rehobo’am, Shishak king of Egypt came up against Jerusalem; he took away the treasures of the house of the Lord and the treasures of the king’s house, he took away everything. He took away all the shields of gold which Solomon had made; and King Rehobo’am made in their stead shields of bronze, and committed them to the hands of the officers of the guard, who kept the door of the king’s house.”

Velikovsky, however, has claimed that Shishak is actually Tuthmosis III, who also conquered Palestine and defeated Rehoboam, and that the evidence attributed to the Egyptian pharaoh Shoshenq I actually belongs to Tuthmosis III. Velikovsky does not claim that they are one and the same king. Shoshenq I is the founder of the 22nd Dynasty which reigned somewhat prior to the Ptolemies, as discussed above in the chapter on the Sea Peoples. Tuthmosis III is taken to have reigned ca. 1504-1450 B.C. However, when we remove the 800-year Dark Age discussed with dating of the Nubian Dynasty, this brings Tuthmosis III down to ca. 704-650 B.C.: the very time of Solomon and Rehoboam. This is not at all a unique position since certain scholars do exactly the same thing.

Velikovsky maintained that Shoshenq I copied the Tuthmosis III material, taking credit for the invasion of Palestine, and that historians have fallen into the trap of accepting Shoshenq’s events as real. According to Nicola Schreiber in her chapter “The 10th Century and the Problem of Shishak”:

“The problems associated with the use of the Shoshenq evidence to provide an absolute chronology of Iron Age Israel have, however, been much discussed. First, some scholars have proposed a wholesale disassociation of Shishak [of the Bible] with the pharaoh Shoshenq I in etymological terms (James et al., 1992, 229-231). This is not convincing. Although the ‘nun’ character is missing from the Hebrew version of the name, the Akkadian rendering of the name [as] susinqu, and the

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32 The Holy Bible (Toronto/NY/Edinburgh 1952), p. 277
33 Velikovsky, Ages in Chaos, op.cit., pp. 143-77
later Greek name [for Shoshenq I] sesonchosis, are close enough etymologically to be one and the same (Baines, pers. comm.). Of the Shoshenq pharaohs of the 22nd Dynasty, the inscriber at Karnak is clearly most likely to be ‘Shishak’ of the Bible.

“Second, the absolute dating of Shoshenq’s reign is in danger of [being rendered by reasoning which is] circular… Hughes suggests that ‘the seemingly impressive agreement between Egyptian dates for Shoshenq’s campaign [to Palestine] in and around the 20th or 21st year of his reign, and the Biblical dates—the 5th year of King Rehoboam—results from the fact that Egyptian chronologists, without always admitting it, have commonly based their chronology of this period on the Biblical synchronism for Shoshenq’s invasion. (Hughes 1990, 192)”

In essence, the position that Velikovsky has taken, to disassociate Shoshenq I from the biblical Shishak, is also presented in the literature. Although the dates for Shoshenq’s reign vary a little, Schreiber and the proponents of the Shishak-Shoshenq connection would agree: “Despite these problems, however, absolute dating of Sheshonq I’s reign to some time in the second half of the 10th century is well established.” Nevertheless, that date is ineluctably linked to the reign of Rehoboam, and Rehoboam’s reign is inextricably bound up with that of the other Hebrew kings, and these in turn are absolutely connected chronologically to astronomical dates of the Neo-Assyrian/Persian kings that interacted with various Hebrew monarchs. All this denies Rehoboam being dated to the 10th century. He must be dated 274 years closer to the present. The Egyptian king that invaded Palestine had to also rule around 650 to 600 B.C.

Historically, Tuthmosis III during his campaigns came into contact with the Mitanni/Medes, also dated in volume II, chapter 3, well into the first millennium B.C., based on various forms of technological and other evidence. Therefore, the Egyptian pharaoh that invaded Palestine would have contact or relations in one form or another with the Mitanni/Medes. This in no way correlates with Sheshonq I but clearly does with Tuthmosis III. Trevor Bryce discusses this:

“With the renewal of Egyptian campaigns … under Tuthmosis III, Mitanni’s territorial holdings once more came under serious threat. No doubt seeing Tuthmosis [III] as some kind of liberator from the threat of Mitanni despotism [various nations] hastened to establish diplomatic relations with him, sending him gifts and tribute.”

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34 Nicola Schreiber, *The Cypro-Phoenician pottery of the Iron Age* (Leiden, the Netherlands/ Boston MA 2003), pp. 87-88
As for Shoshenq I’s supposed relationship with Rehoboam and Mitanni, Donald B. Redford agrees with Velikovsky’s position that he copied this material from Tuthmosis III’s reliefs:

“It would be most gratifying to have on hand a version of the Egyptian account of Sheshonq’s invasion. At present we have only a fragmentary stela from Karnak and the triumphal scene [of his conquest] on the south wall of the hypostyle. The stela yields only a few scattered phrases, suggesting an Asiatic initiative for the battle; but it is still questionable whether it refers to the campaign in Rehoboam’s fifth year at all. The triumphal scene likewise refers to Asiatics ‘who have taken to attacking thy [the king’s] frontiers,’ but these are referred to by the stereotyped ancient phrases ‘the Montiu of Asia,’ and once as ‘the battalions of the army of Mitanni.’ Since one can scarcely credit Sheshonq with having fought his way as far north as the plains of Mesopotamia, the phrase [‘the battalions of the army of Mitanni’] must have been drawn, somewhat carelessly, from the military reliefs of Tuthmosis III on the same temple.”37

There is no scientific or technological evidence that ties Shoshenq to Rehoboam chronologically as there is for Tuthmosis III, and that forensic evidence supports Velikovsky’s thesis quite strongly.

**HATSHEPSUT, THE QUEEN OF SHEBA, AND THE LOCATION OF PUNT**

With Tuthmosis III dated to the time of the Hebrew king Rehoboam, the son of Solomon, in the 7th century B.C., the predecessor of Tuthmosis III would rule Egypt around the same time that Rehoboam’s father ruled in Palestine. Queen Hatshepsut, who came before him, would, therefore, have reigned in Egypt during the time of Solomon. Both would have lived at the same time. In terms of chronology, the expedition to the Land of Punt would have coincided with the visit of the Queen of Sheba to King Solomon. The chronology demands this, but this must be supported by scientific as well as historical evidence.

This connection of Solomon with Hatshepsut/Queen of Sheba is a major thesis of Velikovsky, and will be examined in the following discussion.38 Emmet Sweeney, in part, summarizes Velikovsky’s theory thus:

“About thirty years after the war of liberation, Egypt came to be ruled by a woman. This was Hatshepsut, the stepmother of the underage Thutmose III: And it is here that we come to one of the most spectacular of Velikovsky’s concepts. … It was, for

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38 Velikovsky, *Ages in Chaos, op.cit.*, pp. 103-141
Velikovsky, but a short step from placing Hatshepsut at the same time as Solomon, to making her identical to Solomon’s famous royal visitor, the Queen of Sheba.

“There is no question that Velikovsky’s equation of Hatshepsut with the Queen of Sheba was one of his most attractive and dramatic insights. … For centuries the identity of the queen, who appears but briefly in the Book of Kings, has prompted endless speculating and romanticizing. Whole mythologies have been built around her. For the people of Ethiopia [and for certain historians] she is – along with her host Solomon – the ancestor of the native monarchy. The southern Arabs [and other historians] vie with the Ethiopians in claiming her; and they too regard her as their royal forebear. The passing centuries have not diminished her allure, and to this day she holds a unique fascination for many people. Every year sees the publication of new studies propounding supposedly fresh insights into the identity of the bewitching queen. Yet for all that, we know precious little about her. Theories proliferate, but hard facts remain hard to come by.

“Our only real knowledge of the Queen comes in a very brief description of her visit to Solomon in the Book of Kings. We are told simply that the ruler of Sheba, having heard of the fame of Solomon, came to “try him with hard questions.” (I Kings 10:1) She is said to have entered Jerusalem in a great train, carrying enormous quantities of treasure. She was then shown around the city, after which Solomon answered her questions. Everything she saw and heard impressed her, and, upon being presented with a great quantity of gifts, she departed to her own country. And that is all. The entire story occupies no more than thirteen biblical verses.

“With such meagre information to go on, it is little wonder that the mythologists and romanticizers have had such scope. … [I]n 1952, Velikovsky produced his dramatic hypothesis. Not only did the Queen of Sheba really exist, but she was one of the most powerful monarchs of her age, and [as she is Hatshepsut] we are [therefore] in possession not only of her monuments, but actual portraits of her! If this were to be correct, if the Queen of Sheba really was identical to Hatshepsut, then Velikovsky, by this discovery alone, had made one of the greatest contributions ever to our understanding of ancient Near Eastern history.”

Jeff J. Williams and Cindy Parry summarize Velikovsky’s argument thus:

“Where did the Queen of Sheba come from? Josephus tells us she was the queen of Egypt and Ethiopia. According to the conventional chronology there was no queen ruling Egypt at the time of Solomon. But according to Dr Velikovsky’s revised synchronical chronology, Hatshepsut was ruling Egypt at this very time! Was Hatshepsut the Queen of Sheba? …

“The expedition of Queen Hatshepsut to the land of Punt is compared [by Velikovsky] to the story of the Queen of Sheba, and is found to match in every detail. The local governor who met the herald of Hatshepsut is named Paruah in the bas-reliefs of Deir el-Bahri. The local governor of Ezion-Geber during the

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39 Sweeney, *op. cit.*, pp. 26-27
reign of Solomon was named Paruah. The gifts given and received by Queen Hatshepsut match those described in scriptures as given to the Queen of Sheba. Even the strange plants and animals Hatshepsut received in the land of Punt are known to have been brought to Jerusalem by the navy of Solomon.

“One other parallel is worth noting. In the mural of Queen Hatshepsut, three distinct racial types are shown as the inhabitants of the ‘Holy Land.’ The most dominant racial type is north semitic, the second is a group of light-skinned men called the ‘chiefs of Irem,’ and the third are dark-skinned negroid types called the men of Khenthen-ofer. If the ‘Holy Land’ was indeed Israel, who were these other men? From the record of Josephus we learn that in addition to apes and peacocks, the sailors of King Solomon brought negroes [probably slaves] from the land of Ophir to Jerusalem. King Hiram of Tyre cooperated with Solomon to a great extent; the ‘chiefs of Irem’ were the delegation sent by King Hiram to give gifts to the Egyptian Queen.

“One last piece of evidence needs to be mentioned. When Queen Hatshepsut left Egypt for the Holy Land, she travelled over land from Thebes to the port of el Qoseir, on the Red Sea, and thence by ship to the port of Ezion-Geber, up the Arabah valley to the Dead Sea, and the city of Jericho, and then up the road to Jerusalem. The land journey, however, is difficult to transport large quantities of goods such as the many presents given by Solomon to the Queen of Sheba. In order to transport such a quantity of goods, it would be easier to take them to the Mediterranean port of Joppa, and then by ship across the Mediterranean and up the Nile directly to Thebes. In the mural at Deir el-Bahri, it shows the ships returning from the Holy Land landing at the city of Thebes, not the port of el Qoseir. This would be impossible if the Land of Punt and the Holy Land were in Arabia, or on the east coast of Africa.”

Along these lines, Eva Danelius tells us:

“The Governor of Punt and his wife are introduced by name: he is called P’-r-hw; her name was Eti (or Ati). According to Naville, these names have ‘no ethnographical significance.’ Naville was wrong: As Velikovsky has shown, these names are Biblical names from the period of David, and Solomon. A certain Paruah was the father of one of Solomon’s twelve governors; a man named Atai was a member of the Ierahmielim, a Bedouin tribe affiliated with Judah, who lived in the northern Negev. Another man of the same name was one of the ‘men of might’ who joined David when he hid in the wilderness above the Dead Sea, and in Ziklag, because of Saul. According to Hebrew usage, the daughter might have been presented with her father’s name, Bat (=daughter of) Atai.”

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41 Danelius, *op. cit.*, pp. 13-4
The probability of finding a governor of Ezion-Geber and his wife having Hebrew names strikingly like the names on the Hatshepsut reliefs of the governor of Punt and his wife is inordinately small. What is the probability that the names of the biblical governor and his wife should be so like that of the governor of Punt and his wife who met Hatshepsut’s envoy? The probability that this striking similarity between the names could have occurred by chance is remote. It is strong evidence that links Hatshepsut’s expedition to Punt to Israel in the age of Solomon.

Directly related to this is the location of the land of Punt to which the expedition journeyed to give and receive gifts, as noted on Hatshepsut’s temple at Deir el-Bahri. The historians have generally been divided in placing Punt either directly to the south, on the horn of Africa in Ethiopia, or somewhat further south of that area, or in Arabia south-east of Egypt on the opposite side of the Red Sea from Ethiopia. None of them, so far as I have learned, have placed Punt to the northeast of Egypt in and around Palestine and Lebanon. The crucial historical point in such a discussion is: What did the Egyptians actually say respecting Punt’s location? According to Sweeney, “not a single Egyptian source places Punt unquestionably in the south.” In this regard, Dimitri Meeks in a well-researched paper, “Locating Punt,” discusses the various arguments of historians for placing Punt in either Ethiopia or Arabia; Meeks suggests Arabia for Punt’s location. For example, he states that one

“… argument has been used to prove that Punt was within Africa. The evidence consists of the various versions and copies of a list of toponyms [geographic locations] first recorded from the reign of Tuthmosis III. This catalogue presents the foreign countries and cities known at the time and classified according to the four points of the compass, in theory all subject to Pharaoh. In this series devoted to the lands of the South, Punt and the cities in it are mentioned alongside the land of Kush, Irem, Wawat or Medja … no. 48 being Punt [perhaps the land furthest north of these] … At first sight this makes Punt a country linked to the Nile. Yet the list also mentions the Libyans … The listing is therefore concerned with the south in a wider sense …”

There are, however, other inscriptions from Egypt that locate Punt to the north of Egypt, as Meeks notes:

“Other geographical lists, that of Amenophis III, in Soleb (in Nubia) and its successors locate Punt closely to the north of Egypt among places [where] Punt appears to be either between Pehal and Shosou, or in the sequence [of places] Mitanni, Shosou, Kadesh, Punt, Qutna, Tahse, Yenoam [in the northeast;]

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42 Ibid., p. 45
43 Dimitri Meeks, op. cit., pp. 56-7
curiously these sequences have sometimes been taken unquestioningly [by historians] to be the result of error on the part of the scribes and sculptors…

“Numerous texts, from at least the Middle Kingdom [12th Dynasty for example] onwards, describe Punt as the land of the rising sun and locate it in the East, equating it with the eastern horizon.”

Nevertheless, Meeks points out that in the time of the Ptolemies:

“Once the construction of the temple of Edfu was completed, the priests decided to have inscribed on its walls an historic account of the events which marked the … years that the work had taken … In connection with the dynastic feuds between Ptolemy IX Soter and Ptolemy X Alexander I, they note laconically in relation to the latter: ‘he fled to Punt, his older brother took possession of Egypt and was crowned king again’ … Other sources record that Ptolemy X [then] fled to Cyprus, but Egyptologists have only mentioned the apparent [south-north] contradiction without further comment … However ignorant one might assume Egyptian priests to be, they could scarcely have confused an island [well known to be] north of Egypt with the depths of Africa [or Arabia].”

That is, Ptolemy X Alexander I left Egypt to travel first to Palestine and thence to Cyprus. But this possibility, and especially the fact that Cyprus is unquestionably north of Egypt, which is in total contradiction to the Ethiopian or Arabian southern location to Egypt, the Egyptologists mention but fail to discuss, which suggests that they cannot deal with such direct evidence that contradicts their views of Punt’s location.

Since the forensic evidence places Hatshepsut in the first millennium around 700-600 B.C., the separation of her dynasty—the 18th—from Ptolemaic times is somewhat more than 300 years. In that case, it is evident that the Ptolemies had known as a fact the location of Punt was north of Egypt. Yet historians, having placed Hatshepsut 800 or so years back in time to around 1500 B.C., must assume that in this earlier period the Egyptians knew it was not to the north but to their south. Just as the locations of Dilmun, Magan, and Meluhha, which are known in the first millennium B.C. to be located in India, Egypt, and Ethiopia, have been located somewhere else in the second or third millennium. In terms of the short chronology, the first millennium B.C. locations are the correct ones because there is no forensic evidence for civilization much beyond 1200-1100 B.C. So too this applies to Punt which is located north of Egypt in the first millennium B.C.

Meeks also discusses Punt during the time of the early Romans, claiming:

“Another source worth examining is a geographical list carved at the beginning of the Roman period on the inner wall of the Kom Ombo temple, in southern

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44 Ibid., p. 57
45 Ibid., p. 69
upper Egypt … In a second series of names, Punt is listed between Upper Retienou, corresponding to Palestine, and Pa Bekhen, the mountainous northern part of Mesopotamia … on the one side and Beiber or Babylon (?)—or a place name in southern Palestine …”\(^{46}\)

Meeks adds: “All these texts agree in assigning Punt and its inhabitants to the Near East in more or less direct contact with the lands of the Mediterranean coast.”\(^{47}\) In essence, there is direct written evidence that places Punt in and around Palestine and not in any way in Ethiopia or Arabia, as the historians would have one believe. In fact, the historians simply ignore this in their deliberations and discussions. Meeks puts it thus:

“Texts locating Punt beyond doubt to the south are in the minority, but they are the only ones cited in the current consensus on the location of the ‘country.’ All the other texts [that locate Punt to the northeast or to the east], despite their large number, have been ignored. Punt, we are told by the Egyptians, is situated—in relation to the Nile Valley—both to the north, in contact with the countries of the Near East, of the Mediterranean area and also to the east, or southeast…”\(^{48}\)

Ralph E. Juergens and Lewis M. Greenberg have analyzed the name of “Punt” in terms of its linguistic relationship to the ancient Egyptian, Greek, and Hebrew languages and have shown, citing several authorities, that Punt, in all three languages, clearly refers to Palestine/Phoenicia.\(^{49}\) Their work, needless to say, correlates quite well with the evidence we have just presented. Why should not only the names of the governor of Punt and his wife on the Hatshepsut reliefs be strikingly like those of the Hebrew governor and his wife of Solomon’s time, and at the same time the name Punt, as delineated by Juergens and Greenberg, be strikingly like the Greek and Egyptian names for Palestine/Phoenicia? And why should the Ptolemaic and Roman writings also place Punt in Palestine/Phoenicia? Again the probabilities speak quite strongly against the placement of Punt in either southern Arabia or Somalia.

In this respect, those historians who have failed to admit these fact have not met their professional obligation which the Roman historian Marcus Tullius Cicero in *De Oratore* III, XXXVIII explained was their duty. “The first law of the historian is that he shall never dare utter an untruth. The second is that he shall suppress nothing which is true.” The failure of scholars to even discuss or explain why they have omitted direct evidence related to Punt’s location in Syria, Lebanon, and

\(^{46}\) Ibid., p. 64

\(^{47}\) Ibid., p. 65

\(^{48}\) Ibid., p. 58

\(^{49}\) Ralph E. Juergens, Lewis M. Greenberg, “A Note on the ‘Land of Punt’,” *KRONOS*, vol. 1, no. 2 (June 1975), pp. 89-93
Palestine is a negation of both the first and second laws of historical writing which denies the truth they were sworn to uphold.

Even two of Velikovsky’s major critics who denied the Palestine/Punt location admitted that “… a whole series of documents [John] Bimson admits place Punt in the north, and specifically associate the region with known cities in Syria”50 whilst “even [David] Lorton has to concede, Punt is always described as being to the east of Egypt.”51

What is suggested by this author is that Punt was in a large land area to the east of Egypt and ran from Syria, Lebanon, Palestine and somewhat farther to the south and that the Egyptians in general located Punt in this overall region, but not in Ethiopia nor in Arabia as the evidence will show.

**PUNTITES: AFRICANS OR SEMITES?**

The nature of the racial characteristics of the Puntites is extremely important in determining whether the expedition traveled to a country of Africans or to one inhabited by Semites. Do the people depicted as Puntites exhibit racial characteristics which can decide this question? The answer is clearly in the affirmative and that answer precludes the possibility that the people of Punt are black Africans from Ethiopia or African lands farther to the south. In this respect Tuthmosis III’s inscription contains evidence that is quite revealing, as Meeks shows:

“The lists of Tuthmosis III and similar sources are usually considered as a straightforward inventory of toponyms [geographic locations]. However, to do so disregards their pictorial aspect: each place name is inscribed in an oval surmounted by a human head… The head [appearance] served to classify the toponym according to ethnic category, each traditionally defined by a face with characteristic features and attributes such as hair style, or the absence of beards. This is how the ancient Egyptians distinguished between Asiatics, Libyans and Nubians … The heads of the Puntite toponyms (with Libyan or oriental features) differ markedly from those of the Kushite toponyms (African features) …

“A copy of this list inscribed during the reign of Amenophis II [a son of Tuthmosis III] divided these same southern lands into two quite distinct categories. The ovals in the first have heads can be identified as belonging to Africans, while those in the second are identical to those on the ovals containing Near Eastern toponyms. Punt and its toponyms belong to the second category. Fokhry … attributed this unusual detail to an error on the part of the sculptor, but

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the care in carving the heads has attracted comment from others. It seems more plausible that the complete set of lists would have grouped together all the toponyms from the south, regardless of whether they were located on one side of the Red Sea or the other, but distinguished the African toponyms from the Arabian or Libyan [or Palestinian] ones by their different heads.”

With respect to the appearance of the people of Punt in Hatshepsut’s temple at Deir el-Bahri, Meeks further reports:

“The general appearance of Puntites in the temple of Hatshepsut and other sources is similar to those people of the Near East and their skin is the same colour as that of the Egyptians … When Puntites and Africans are depicted on one and the same monument, care is taken to bring out the physical differences between them, as in the tomb of Amenomose at Thebes … Here the Puntites have none of the characteristics of the Africans, on the contrary, they exhibit some features which are peculiar to them alone, and others which are found elsewhere. … Proponents … recognized that the Puntites were not black Africans.”

Thus in these depictions of Puntites and Africans together there exists a difference that Meeks explains:

“The Puntites and Nubians are clearly regarded as different ethnic groups, living in different places… The black-skinned population whose leaders (‘the great men of Punt’ … referred to in the texts) are always slender individuals with pale skin and thin short beards. The presence of black Africans in a secondary position in relation to the population of ‘notables’ with the paler skins may evoke the trade of slaves.”

All in all, Meeks summarizes the evidence thus:

“The hypothesis of an African location for the land of Punt is based on extremely fragile grounds. It is contradicted by numerous texts and has only become an established fact in Egyptology because no one has taken into account the full range of evidence on the subject regardless of place of origin or date. When all the evidence is assembled, the incoherent and implausible character of such an African hypothesis becomes self-evident. The only way to reconcile all the data is to locate Punt in the Arabian Peninsula. The territory of Punt began quite close to that of Egypt, once Sinai had been crossed, in Arabia Petraea or the Negev [of Palestine].”

To get around this problem, Meeks suggests “PROBABLY Punt incorporated in a rather imprecise manner the whole coastal zone of the Red Sea down as far as

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52 Meeks, op.cit., pp. 56-57
53 Ibid., p. 58
54 Ibid., p. 61
55 Ibid., p. 79
present-day Yemen.” But of course this is conjecture since his claim contains the word “probably.” We too shall take into account the full range of evidence on the subject regardless of place of origin or date, to show that, when all the evidence is assembled, the incoherent and implausible character of such an Arabian hypothesis for this expedition will become self-evident. The only way to reconcile all the data is to locate Punt in the region of Palestine and its environs. As Meeks admits, the territory of Punt began quite close to that of Egypt once Sinai had been crossed, which is directly adjacent to the Negev of Palestine.

THE FAUNA OF PUNT

The major argument for placing Punt in either Ethiopia or Arabia are the animals depicted in the Hatshepsut reliefs at Deir el-Bahri. Sweeney has well summarized the evidence as it relates to whether the animals in the reliefs are from Africa, Arabia, or Syria/Palestine:

“Both Bimson and Lorton … concentrated virtually all their efforts on the identity of Punt in an attempt to show that the expedition commemorated at Deir el-Bahri could not have been to Syria/Palestine.

“… the Deir el-Bahri reliefs do show a number of African people and apparently African animals, such as at least one rhinoceros and a giraffe. For Bimson, and for many of his readers, this was decisive evidence in proving an African location for the territory …

“… the animals very definitely do not belong to Africa. Giraffes of course are nowadays found only in Africa, and this has misled many people into seeing them as proof of a southern location for Punt. However, as Bimson himself admits, giraffes were found on the borders of Syria and Arabia in classical [Greek and possibly Roman] times – a fact noted by Diodorus. [Diodorus ii,50-51] Furthermore, the Bible itself (Deuteronomy 14:5) speaks of giraffes (which it calls the ‘camel-leopard’) in the region of Sinai and the Negev, whilst A. Nibbi notes the occurrence of a rock-cut drawing of a giraffe in Sinai. (Alessandra Nibbi “The Shipwrecked Sailor Again,” Göttinger Missellen 24 (1977) p.54) The giraffe then can at best show that Punt may have been in Africa.

“The rhinoceros however more probably points to Asia. Once again, as with the giraffe, people have simply thought ‘Rhinoceros – Africa’. But the rhinoceros portrayed at Deir el-Bahri appears to be of the Asian one-horned species, Rhinoceros unicornis, and cannot represent either of the two contemporary African species, both of which have two horns. The one-horned rhinoceros has never been attested in Africa. Again, this is a fact that Bimson himself concedes. [In a footnote, Sweeney

56 Ibid. (capitalization added)
notes: ‘Bimson ... suggests that the one-horned rhino may have once occurred in northern Africa, since early Egyptian hieroglyphs included a pictogram of such a creature. This is not impossible, since the rhinoceros, giraffe and elephant were all common in Egypt [before the great aridity] until near the end of the Early Dynastic period. But the same creatures also roamed Syria/Palestine, where they survived till ... later owing to the more favourable [northern] climatic conditions.’

“In antiquity the entire Near East was home to most of the creatures associated nowadays only with Africa. It is well-known, for example, that lions occurred in great abundance throughout the region [as when Samson killed a lion with his bare hands], and were extensively hunted for sport by Assyrian kings as well as Egyptian pharaohs. What is not so well known is that in ancient times basically all of the animals now associated with the African savannah roamed the Syria/Palestine region. These populations were remnants of an earlier time when the entire Sahara and Arabian deserts were well-watered grasslands [as discussed in Ginenthal, The Extinction of the Mammoth and in this volume, and elsewhere]. Thus the Illustrated Bible Dictionary [3 Vols. (Hodder and Stoughton, 1980] supplies the following rather surprising information about the non-human inhabitants of the [Syria/Palestine] area in biblical times:

“(a) Elephants. ‘The Asiatic elephant was once found as far west as the upper reaches of the Euphrates [northern Syria].’ Vol. 1 p.58

“(b) Lions. ‘At one time lions were found from Asia Minor through the Middle East and Persia to India ...’ Ibid.

“(c) Leopards and Cheetahs. ‘It is possible that Heb. namer refers to both the true leopard and the cheetah, or hunting leopard, and also to one or two other spotted wild cats of Palestine.’ Ibid.

“(d) Gazelles. ‘Two wild species [of gazelle] are found in Palestine: the dorcas and Palestine gazelles, both standing under 70 cm [28 inches].’ Ibid.

“(e) Hippopotami. The hippopotamus ‘lived in the lower Nile until the 12th century AD and, much earlier, in the Orontes river in Syria (and perhaps elsewhere in SW Asia) until after the time of Joseph, so it is well known in Bible lands.’ Ibid. p.61

“(f) Ostriches. ‘The ostrich finds mention in several [Bible] passages ... Jb 39: 13-18 is clearly a description of the ostrich, a bird which once lived in the Middle East.’ Ibid. p.62 (note: an Assyrian portrayal of this bird is shown on the same page).

“(g) Crocodiles. ‘In biblical times the Nile crocodile was found from source to mouth of the Nile. While its distribution north of Egypt in that period is unknown, returning Crusaders reported crocodiles in the Zerka river, which runs into the Mediterranean near Caesarea and is still known locally as the Crocodile river.’ Ibid. p.65”

57 Sweeney, op.cit., pp. 47-49
This evidence is well known in the literature on ancient Palestine. Briefly, because there is so much written on the animals in ancient and present-day Palestine, we will recapitulate:

**Hippopotamus**: Olga Krzyszkowska and Robert Morkot report:

“Aside from Egypt, small areas of Syria-Palestine also seem to have supported hippopotamus populations in antiquity. Of these, one was apparently located in the Amuq Basin and Orontes Valley, providing a ready source of hippopotamus ivory for Ugarit (Caubet and Poplin 1987:292-7) … Extinction perhaps occurred early in the first millennium BC. Osteological remains found at Tel Qasile (near modern Tel Aviv) suggest the presence of a second (rather small?) population which evidently survived as late as the fourth century BC (Haas 1953).”

**Ostrich**: Berthold Laufer in his book on ostrich egg-shell cups tells us ostriches were well represented among the fauna of Mesopotamia and Palestine:

“[In Mesopotamia regarding ostriches] the fact remains that [they are] clearly represented on seals and cylinders.

“One of [the] seals … of Urzana, king of Musasir, a contemporary of King Sargon … represents Assur, king of the great Assyrian gods … in the act of strangling two ostriches. On another seal … the god Marduk is shown in the act of exacting vengeance on an ostrich …

“In the language of the Sumerians the ostrich was known under the name gir-gid-da, which is explained as the long-legged bird’ … Other Assyrian designations of the bird are sha-ka-tuv, and se-ip-a-rik, the latter also meaning ‘long-legged.’”

“In Palestine the ostrich was well known to the Hebrews, and, as attested by several allusions to the bird in the Old Testament, must in ancient times have been frequent in Palestine. It is included among unclean birds in the Mosaic Code [Leviticus XI, 16; Deuteronomy XIV, 15], and its flesh was prohibited.”

**Crocodiles**: These were also known in Palestine according to George Livingston Robinson:

“Crocodiles have been seen at different times in Palestine. Pliny speaks of the *Nahr az-Zarka* (lit. ‘the blue river’) which flows across the Plain of Sharon north of Joppa as ‘the Crocodile River.’ Besides, the Hebrew words for these beasts bear no resemblance to the Egyptian names. One wonders why Egyptian terms to

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60 *Ibid.*, p. 6
express them were not employed. In fact, there are no Egyptian equivalents to the Hebrew expressions, and the Hebrew words remain still an enigma …”61

This means the Hebrews knew of the crocodile in their home land before ever entering Egypt and had given it a Hebrew name unrelated to the Egyptian one. Had they learned of the crocodile from the Egyptians, they would have employed an Egyptian word or cognate Egyptian term for it.

**Gazelle:** This type of deer was well known in Palestine as shown by Mark Ziese: “Many species and sub-species of gazelle are … common to Palestine … [such as] the mountain gazelle … Gazelles were hunted for food and sport …”62

Colbert C. Held and Mildred McDonald Held point out:

“Lions, tigers, leopards, cheetahs, and other felines were formerly common in parts of the Fertile Crescent and Iran, and other carnivores, such as the wolf, fox, jackal, and hyena, roamed much of the same area.

“All of these larger mammals have been decimated by nature and people … The crocodile disappeared about 1900, the ostrich in the early 1930s. … A considerable baboon population is [still] found in the woodlands of Asir in south-western Arabia. Hundreds of hamadryas baboons scurry through the brush … of the national park.”63

With regard to baboons, Ellsworth D. Foster, in *The World Book* (Ann Arbor MI 1917), page 525 tells us the “BAB-EL-MANDEB BABOON wandered to the plain of Shinar in Mesopotamia.” Matomah Alesha reports “baboons were known at the [Mesopotamian] court and given as gifts and tribute.”64

(h) **Giraffes.** As for the depiction of the giraffe on the walls of Deir el-Bahri, Meeks reports that it and the Asiatic rhinoceros

“… mingle with domestic animals … in the Punt village, although the poor preservation of the reliefs makes it impossible to determine whether they are captured or free …

“Yet these African animals … were often used as diplomatic gifts in antiquity and appreciated for their rarity … There are representations of giraffes from tombs of the New Kingdom and in the sixth century B.C. a giraffe figures among the gifts offered to the [Persian] King of Kings in Persepolis … Diodorus … describes the ‘cameleopards’ of Arabia and these may have been giraffes …”65

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64 Matomah Alesha, *Sako Ma: a look at the sacred monkey totem* (Tucson AZ 1997), p. 47
65 Meeks, *op.cit.*, p. 55
And indeed, Diodorus does say the “cameleopard” is the “Greek word for giraffe.”⁶⁶ Alexandra Nibbi in this regard shows “a rock-cut drawing of a giraffe in Sinai.”⁶⁷

THE FLORA OF PUNT

As with the fauna depicted on the walls of Deir el-Bahri, accepted as being southern forms from Africa, so too with the flora this same interpretation was employed to prove this African or Arabian land was a nation that cultivated frankincense, and myrrh also had to be derived from there since it grows there presently. Biogeographically it thus purported to coincide with the faunal evidence for Punt being in Arabia or perhaps Africa. As Jerome Murphy O’Connor and all the others tell us, “These fragrant gum-resins [myrrh and frankincense] are produced by trees that grow only in modern Yemen and Oman and across the Gulf of Aden in Somalia.”⁶⁸ On the basis of this modern-day growth, historians claim this is definitive proof of Punt being located in these southern regions.

Nevertheless, there is other evidence that shows quite clearly that the people in and around ancient Palestine grew these fragrances. In both cases, floral and faunal, the historians have interpreted the biogeography of the present day with that of the past. As Velikovsky remarks: “Frankincense grows in only a very few places, Somaliland and southern Arabia on opposite shores of the Red Sea being areas which produce it even today. The botanists were guides to the archaeologists [and historians] in their search of the land of Punt.”⁶⁹

Velikovsky then dealt with the question of whether the various trees, wood, and incense plants that Hatshepsut brought back to Egypt from Punt came from Lebanon/Palestine or from either Arabia or Africa:

“The question that arises is: Did Palestine produce myrrh … and frankincense, mentioned among the gifts Hatshepsut received in God’s Land?

“Myrrh and frankincense are repeatedly mentioned in Egyptian inscriptions as products of Punt. Frankincense (olibanum) falls in clear drops which, when gathered and formed into balls or sticks, turn white. … The less precious incense, ladanum, is yellow or brown in color.”⁷⁰

Yet James Henry Breasted discusses the wood products that also came from Punt which do not grow in Arabia or Somaliland as inscribed on the walls of Deir el-Bahri.

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⁶⁸ Jerome Murphy O’Connor, The Holy Land (reprint, Oxford UK/NY et al., 2005), p. 325
⁶⁹ Velikovsky, Ages in Chaos, op.cit., p. 172
⁷⁰ Ibid.
el-Bahri: “The myrrh of Punt has been brought to me ... all the luxurious marvels of this country were brought to my palace in one collection. ... They have brought me the choicest products ... of cedar, of juniper and of meru-wood; ... all the goodly sweet woods of God’s Land.”

Cedar, of course, was a highly prized wood in Egypt and has always traditionally been cited in the ancient texts as coming from Lebanon and not from Arabia or Somaliland. Yet Hatshepsut says she obtained “myrrh” as well as “cedar” from “Punt” called “God’s Land.” Here, no doubt, we encounter a clear contradiction to the historians’ paradigm that myrrh and frankincense had to have come to Egypt from Arabia or Somaliland shouting at them from the walls of Deir el-Bahri because the “cedar” wood is also among the “choicest products” that Hatshepsut brought home from Punt. But there is more evidence in this respect as pointed out by Nibbi:

“In the general discussion of the provenance of incense, usually assumed to be from the south, the argument has always begun by placing Punt in Somaliland or somewhere in the south, even though this has never been proved. Thus the texts have been interpreted from this standpoint with confusing and erroneous results. ... “From Theophrastus (Hist.Pl. 4, 4, 14) we learn that in his day, frankincense, myrrh and many other aromatic plants grew in the area which he called Syria.

“Among the plants that grow in Arabia, Syria and India the aromatic plants are somewhat exceptional and distinct from the plants of other lands; for instance, frankincense, myrrh ... and all other such plants.’ (translation A. Hort) ...”

Along these same lines, Kjeld Nielsen shows:

“According to Drower, Retenu in Egyptian sources is a general term for the territories north of Egypt. In modern geographic terms it corresponds to Syria-Palestine. We hear nothing about incense import from Retenu before Thutmosis III’s time ... Among the goods he receives, sntr [incense] is often mentioned. It generally occurs in lists of farm products belonging to Syria-Palestine ... On one occasion sntr is mentioned as being part of the harvest of the land of Retenu. This is a clear indication that sntr was considered a native product of Retenu. An ostracon from the Ramesseum bears the inscription ntr sntr ḫ3rw, which means “incense from ḫ3rw, Ḫ3rw being the name of the locality Khor in Syria-Palestine.”

The argument used to get around these direct statements—that incense, frankincense and myrrh did not come from where these said they did—is that it is considered to have come not directly from Syria, Lebanon, and Palestine but to these regions from southern Arabia “indirectly.” But as support for this Nielsen

72 Nibbi, op.cit., pp. 67-68
73 Kjeld Nielsen, The Incense of Israel (Leiden, the Netherlands 1986), p. 7
74 Ibid., p. 22
offers not one iota of proof. The belief in the paradigm that Punt is the southern source of incense is circularly reasoned as evidence, because there is no evidence. The question that never seems to have occurred to historians who support these “indirect” sources of incense to Syria, Lebanon, and Palestine from Arabia is: why didn’t the Arabian or Somali incense traders bring their goods directly to Egypt? Velikovsky further shows:

“Let us see whether the Scriptures give any indication that they were products of the Holy Land in the days of Solomon. In the ‘Song of songs, which is Solomon’s,’ the enamored prince says to the little shepherdess (4:6):

‘‘Until the day break, and the shadows flee away, I will get me to the mountain of myrrh, and to the hill of frankincense.’ …

“Lebanah (frankincense) near Beth-el (Judges 21:19) was probably the place where the incense plant grew. In the time of Thutmose III the rare plants of the Palestinian gardens were transferred to Egypt, as he himself told and pictured. Thereafter, in the days of Isaiah (60:6) and Jeremiah (6:20), incense was imported into Palestine from southern Arabia.”

In both Egyptian and biblical sources there are direct statements on record that historians clearly understand to say the source of Egypt’s frankincense and myrrh was around the Palestine region. As with so much else, they have also chosen to disregard this evidence.

One final point about the flora of Punt was presented by Eva Danelius which directly links Punt to southern Palestine:

“… the reliefs [of Hatshepsut’s temple as drawn by Naville] show an amazing similarity to characteristics of the northern end of the Gulf of Aqaba, unknown at the time when Velikovsky wrote his reconstruction but to a very small circle of explorers.

“The drawings show meticulous care for details to a degree justifying the supposition ‘that the expedition to Punt was accompanied by one or more draftsman [sic] who made careful studies of what they saw there …’ On the picture reproduced on plate LXIX, palm trees and other trees, high enough to shade houses on poles, and cattle, go right down to the edge of the water which swarms with fish and other aquatic animals. Such plant and animal life is ‘not to be found by the shore, nor do date palms grow in the sand and pebbles of the beach,’ concluded Naville, Maspero and other Egyptologists, who, therefore, looked for a suitable harbor ‘at some distance inland, safe from the high tides of the Red Sea.’ This opinion, however, is difficult to uphold in light of the fact that the aquatic animals reproduced are typical specimens of the Indian-Ocean-Red-Sea-Fauna.

“The Egyptologists were not the only ones to remark on this unusual combination of nature. Procopius, who lived in the sixth century A.D., mentions the date palms growing along the coast of Aila (=Aqaba) … And the well-known Arab writer Al-

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75 Velikovsky, op.cit., pp. 172-173
Mukadassi (10th century [A.D.]) describes the place just as it was shown on the mural of Hatshepsut: ‘There are many date-trees, and an abundance of fish.”

Again, why would the point of landing of Hatshepsut’s expedition, shown in her reliefs, just happen to be strikingly like the Gulf of Aqaba in its floral and faunal life? This again is strongly indicative that the expedition sailed across the Red Sea, up the Gulf of Aqaba to Israel.

THE RED SEA ROUTE TO PUNT

It is assumed by historians following the established chronology that Hatshepsut and her predecessors were receiving incense from Arabia or Africa via trade either along the eastern side of the Red Sea by camel caravans on the “incense road” or by ships sailing the Red Sea. But, as we pointed out in vol. II, pages 491-511, camels did not become domesticated for trade until around 1000 B.C. in the established chronology and were unknown to Egyptians in the era of Hatshepsut. Surely, had she known of camels, these animals would have been pictured in the reliefs of Deir el-Bahri. Furthermore, the camel saddle necessary for the incense traffic did not exist in the time of Hatshepsut. Nevertheless, it can be, and has been, argued that Egyptian sailing vessels made the long, dangerous passage south on the Red Sea and then returned north to Egypt via the same long, dangerous route to bring frankincense, myrrh, cedar, etc., back to Egypt, then transported it across the eastern desert to the Nile and thence throughout Egypt. In general, historians maintain that travel to Punt either in or around African Somalia or Asian Yemen was a rather straightforward undertaking. Nielsen expresses this concept of such Red Sea voyages such:

“The historical texts confirm the impression received from the archaeological finds of incense and incense burners: the use of incense goes back to the earliest time. One of the earliest written sources referring to ‘ntyw [incense] from Punt is the Palermo stone. The king who imports ‘ntyw is Sahure, the 2nd king of the 5th Dynasty. The [Palermo] stone mentions the Egyptian connection with Punt as a most normal state of affairs… Contact with Punt was kept alive first of all by ship. Under Pepi II of the 6th Dynasty a certain Enekhet was slain by Asiatics while building ships for Punt. In the Old Kingdom a ship employed for voyages to Punt was called a gubliya, a term which originally must have designated a ship built for voyages [on the Mediterranean Sea] to Gubla or Byblos [called Byblos ships which may suggest these were built at Byblos]. However, the word soon lost its original meaning and came to signify a ship utilized for ocean-going traffic in contradistinction to the boats used on the Nile. From the Middle Kingdom [12th

76 Danelius, op.cit., p. 12
Dynasty], a couple of inscriptions commemorating voyages to Punt are preserved. One of them found in Wadi Hamamat belonging to a public official called Henu tells that Henu was sent to the Red Sea area to dispatch a ship to Punt … The mysterious story about the shipwrecked sailor records that the island where the sailor landed is the property of the prince of Punt, who owns ‘ntyw [incense] abundantly. Its exact position, however, is not known.”

In fact, it is believed that, as H.W.F. Saggs states, “such voyages [to Punt] were common, and one Sixth Dynasty official records taking part in eleven expeditions [to both Punt and Byblos].”

But were such long Red Sea voyages really all that common and easily achieved? Joyce Tyldesley expresses the dubious nature of such a common event:

“Expeditions to Punt had been a feature of several Middle Kingdom reigns and the trading missions of Mentuhotep III, Senwosret I and Amenemhat III [of the 12th Dynasty] had all successfully navigated their way to and from this fabulous land. The exact location of Punt is now [1996] a mystery, although the flora and fauna depicted in the reliefs indicate that it must have been an African country, probably situated along the Eritrean/Ethiopian [Somali] coast … Punt could therefore be reached via the Red Sea port of Qeseir which lay at the end of an arduous trek [from the Nile] [across] the desert from Coptos. The Egyptians, well accustomed to sailing up and down the Nile, were not particularly well versed in the hazard of sea travel, and the long voyage to Punt must have seemed something akin to a journey to the moon for present-day explorers. However, the rewards of such a journey clearly outweighed the risks, and missions to Punt [after Hatshepsut] continued during the reigns of Thutmosis III and Amenhotep III.”

In this respect, it should be noted that Amenemhat III was a 12th Dynasty king. Thus the evidence means, as Velikovsky stated above, that in later times “in the days of Isaiah (60:6) and Jeremiah (6:20) incense was imported into Palestine [and Egypt] from southern Arabia.” The prophets thus lived during a period when the camel could carry incense from Arabia via the “incense road” and the camel was known and depicted in Egyptian art of the later first millennium B.C. Trips down to the Indian Ocean via the Red Sea would have been a rarity because the camel caravan was safer, as we will see, and surer. In earlier times such a sea voyage of some 1200 miles/2000 kilometers each way was fraught with “risks.” It is the fact that these great dangers were rarely acknowledged in the literature that allow historians to suggest such voyages were common. With Punt across the Red Sea to the Negev of Palestine, a far shorter, safer voyage was easier to accomplish. Velikovsky states:

77 Nielsen, op.cit., p. 6
78 Saggs, op.cit., p. 134
“The shortest route from Thebes to Jerusalem is not along the Nile and the coast of the Mediterranean; the Red Sea route is only a little more than half its distance: from Thebes to Coptos, a short distance up the Nile, and then to el-Qoseir, a harbor on the Red Sea, then by ship across the Red Sea and along the Aqaba (Aelana) Gulf and from Aqaba overland to Jerusalem.”

U. Vermeulen and D. de Smet describe some of the perils of a Red Sea voyage:

“[There are] dangers ships encountered in sailing from the south to the northern Red Sea. Classical Muslim geographers and travelers mention the erratic winds which blow in the Red Sea and the hazards involved in navigating this sea. Our sources all comment about the difficulties encountered with shallow waters and the numerous rocks, [underwater] corals and islands. [The north-south direction was safer and the pilot would in all probability sail down the centre to avoid the treacherous banks and reefs near the coasts for which the Red Sea is notorious. The route, therefore, became difficult with islands, rocks and irregular currents, not to mention the danger of sailing in the dark of the night [and still keeping the ship away from these dangers].”

H.W. Tilman further reports the navigational hazards of a Red Sea voyage by modern yachts:

“Even without pirates the Red Sea provides enough problems for the small-boat sailor… strong currents, baffling winds, steep seas, scattered reefs and [in modern times] partially lit shores. Another navigational hazard [in daylight] is caused by refraction [of light from the surrounding desert lands] … On one occasion a light with a range of 14 miles was seen when we were 37 miles distant … Thus owing to the displacement of the horizon, sights, particularly sun sights are liable to considerable error.”

Lin and Larry Pardey add to this list of dangers:

“North- or southbound, the Red Sea can be a real test of your seamanship and navigation skills. Sandstorms can reduce visibility to less than 100 yards, though during the most favorable transit months sandstorms are not too common. Currents throughout the Red Sea are variable, especially so near the reefs …

“Because the Red Sea is flanked by hot deserts, a refraction phenomenon … causes daytime sights to be in error up to 20 miles east or west [from shore].

“To [be] becalmed … in the confined areas of the Gulf of Suez … can be a real problem.”

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80 Velikovsky, op.cit., p. 113
82 H.W. Tilman, Eight Sailing/Mountain-Exploration Book (Leicester UK/Seattle WA 1987), p. 619
The problem was to keep away from the shore with its rocks and submerged reefs. At night this was not possible, so ships anchored near shore or at the shore overnight, but during daylight the distance to the shore was distorted by refraction, so ships could run aground or be carried aground by the changing currents and erratic winds when the sailors misinterpreted these distances. What further exacerbated these problems was the reduced ability to turn these ancient ships quickly when dangers approached, because “Ancient sailing vessels were extremely limited in maneuverability.”

Therefore even if one could see the danger ahead, a strong current and accompanying wind into the direction of the hazard would not necessarily allow the ship, even manned by rowers, to avoid striking it. Even in the times of the Ptolemies, when ships were more advanced than the ancient Egyptian vessels, the very same problems existed, as J.P. Mahaffey states: “The navigation of the Red Sea is very carefully described, with all its dangers, which the Ptolemies sought to diminish by leaving wrecked ships where they had stranded by way of warning.”

There were no charts showing the rocks, reefs, islands, currents, and other dangers. There were no global positioning data available. The ships were to a great extent at the mercy of the elements and therefore voyages down and back up the length of the Red Sea were an extraordinarily dangerous undertaking. Again, the short trip to the Negev was far, far less risky. William J. Bernstein sums up the problem thus: “The traders [of incense] might have sailed the Red Sea down its entire length, but as we’ve already seen, shallow waters … and adverse winds cursed this route. It was safer and more reliable to use the overland path north along the Arabian coast of the Red Sea, then west through the Sinai.”

It is here suggested that Red Sea voyages south and north to Arabia or Africa were undertaken late in ancient times and that around 600 to 500 B.C. was the approximate period when such trips were made. Prior to that time, ships sailed north. Yes, it is possible that such dangerous voyages may have been made to the south, but the evidence, as we will see again and again, is against such journeys.

Is there evidence for the Egyptians sailing the Red Sea and if so, particularly when did such travels occur? The evidence is fairly clear that they did so, just as

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84 Ernst Julius Kiel et al., *Mnemosyne*, Bibliotheca Classica Batava, reprint (Leiden, the Netherlands 1930), p. 62
we claimed, in the mid-first millennium B.C., that is during the 12th Dynasty/Middle Kingdom times. As reported on the Internet under “World’s Oldest Ship Timbers Found in Egyptian Desert,”

“The oldest remains of seafaring ships in the world have been found in caves at the edge of the Egyptian desert along with cargo boxes that suggest ancient Egyptians sailed nearly 1,000 miles on rough waters to get treasures from a place they called God’s Land, or Punt.

“Florida State University anthropology professor Cheryl Ward has determined that wooden planks found in the manmade caves are about 4,000 years old – making them the world’s most ancient ship timbers. Shipworms that had tunneled into the planks indicated the ships had weathered a long voyage of a few months, likely to the fabled southern Red Sea trading center of Punt, a place referenced in hieroglyphics on empty cargo boxes found in the caves, Ward said.

“‘The archaeological site is like a mothballed military base, and the artifacts there tell a story of some of the best organized administrators the world has ever seen,’ she said. ‘It’s a site that has kept its secrets for 40 centuries.’

“Ward, an expert on ancient shipbuilding who previously was a member of famed Titanic explorer Robert Ballard’s Black Sea project team, joined archaeologists Kathryn Bard of Boston University and Rodolfo Fattovich of the University of Naples l’Orientale as the chief maritime archaeologist at the site, a sand-covered bluff along the Red Sea called Wadi Gawasis, in December. The project, which Ward will detail in an upcoming issue of the International Journal of Nautical Archaeology, was conducted with the support of Zahi Hawass, secretary-general of Egypt’s Supreme Council of Antiquities.

“Scholars have long known that Egyptians traveled to Punt but they have debated its exact location and whether the Egyptians reached Punt by land or by sea. Some had thought the ancient Egyptians did not have the naval technology to travel long distances by sea, but the findings at the Wadi Gawasis confirm that Egyptians sailed a 2,000-mile round trip voyage to Punt, putting it in what is today Ethiopia or Yemen, Ward said.

“The Wadi Gawasis site, located about 13 miles south of the modern city of Port Safaga, was an industrial shipyard of sorts with six rock-cut caves that the ancient Egyptians used as work and storage rooms to protect their equipment from the harsh desert conditions, Ward said. Along with timber and cargo boxes, the archaeologists found large stone anchors, shards of storage jars and more than 80 perfectly preserved coils of rope in the caves that had been sealed off until the next expedition – one that obviously never came.

“The team also found a stela, or limestone tablet, of Pharaoh Amenemhat III, who ruled between 1844-1797 B.C., inscribed with all five of his royal names. The plaque provided further evidence that discoveries found at the site date to Egypt’s Middle Kingdom period. A period of civil unrest and political instability likely put a halt to
further exploration, Ward said, and the Wadi Gawasis site was long forgotten.

“While in use, though, the ancient shipyard was central to a sophisticated government operation for the expeditions to Punt that Ward likened to NASA's space program. She theorized that ships were originally built at a Nile shipyard, then disassembled and carried across 90 miles of desert to the Red Sea, where they were put back together and launched on the voyage.”

Of course Amenemhat III is a 12th Dynasty pharaoh, which confirms our thesis.

**THE CAMEL AND THE LOCATION OF PUNT**

The thesis we present is that Arabia or Somalia as a source of frankincense and myrrh only became the source for these resins in the mid- to latter part of the first millennium B.C. It was at this time that the domesticated camel was employed to carry these goods of Somalia and especially southern Arabia to Egypt, Mesopotamia, Syria, Lebanon, and Palestine in such amounts as to undercut the prices for them in these northern areas. The proof to a great degree is related to when the camel became known in Egypt and this evidence is crucial for locating Punt in the Levant in the time of Hatshepsut.

As noted earlier, the camel is a very late comer to Egypt, dating roughly after 600 B.C. Hatshepsut and those others who allegedly sailed to Punt in either Arabia or Somalia, *even based on the established chronology would have seen camels there*. Richard Bulliet outlines the question:

“From the camel’s point of view, the horn of Africa is not so much a triangle as an island. To the south the tsetse fly begins to be found in Somalia about four degrees north of the equator, and although tsetse fly country is not continuous, it offers an effective barrier to the spread of the camel southward where, in any case, high humidity would make camel breeding difficult. To the west is the high Ethiopian plateau to which the camel might possibly become adapted since mountain breeds are known elsewhere, but there is no evidence of camel breeding having ever been practiced [so that the camel could not have been domesticated in the mountains and thence brought to the horn of Africa]. To the east, of course, is the Indian Ocean, the Gulf of Aden, and the Bab al-Mandab straight at the mouth of the Red Sea … Only along the Red Sea coast at the northern end of the region, where the Ethiopian highlands meet the sea, is there a possible land route for camels to go elsewhere in Africa, and the land just south of that isthmus… [the] Afar triangle […] is far from being prime land even for camels. Given this high degree of geographical isolation, there are only two plausible ways in which the camel could have reached the horn of Africa: either it came south by land along the [western Red

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Sea] coast, which would make it a southern offshoot of Sudanese or Saharan camel culture, or it came across the [Red S]ea from the Arabian peninsula. The peculiar usage pattern [of using the camel strictly for its milk] of the Somali effectively rules out the former possibility. All Sudanese and Saharan camel people ride their animals … [the Somalis do not, or very rarely, ride them]."\(^{89}\) There is an excellent scientific reason that prohibits the movement of the camel south from Egypt or the Sudan, which Bulliet explains thus:

“… on the physiological side, the reproductive cycle of the camel is governed by a male rutting period of two to three months’ duration once a year during the best grazing season and a twelve to thirteen month gestation period which causes mating and calving to coincide. What triggers the period of rut is not known, but since it takes place during the rainy season, whatever part of the year that is in a given locale, and since camels do not seem to reproduce well in wet climates, some connection with the level of humidity [for rutting to begin] is indicated. Whatever the exact cause of the onset of rut may be, however, animals that are moved from one area of seasonal rainfall to another area where the rainfall occurs in a different season cease almost entirely to reproduce. In the case of Somalia and the Sudan [to the north] just such a difference in climate regime exists. The Sudan is summer rainfall country, with July and August being the wettest months at Khartoum, while Somalia’s climate is governed by the monsoons which bring the heaviest rains, which are not very heavy in May and June, and lighter rains in October, November, and December. Consequently, it is unlikely that any efforts by camel breeders north of the coastal isthmus [of the Red Sea] to penetrate south would have been pursued once it was discovered that [camel] reproduction was adversely affected."\(^{90}\)

Yet the location of African Punt is generally given as south of the isthmus of the Red Sea. Bulliet therefore concludes:

“Therefore it is out of the question that Somali usage [of camels] could have been copied from earlier stages of Saharan or Sudanese domestication [Somalis use the camel strictly for milk while the others ride camels and the camel could not be moved south because it will then not reproduce]. With Arabia the opposite case holds. The camel appears to be indigenous to Arabia, and present or historically determinable patterns of camel use [for milk consumption] may well, and in all probability do, derive from earlier stages of camel domestication within the Arabian peninsula.”\(^{91}\)

Since the earliest form of camel domestication in Arabia was for milk and not riding, the fact that on the horn of Africa camels are employed for their milk

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\(^{90}\) *Ibid.*, pp. 41-42

\(^{91}\) *Ibid.*, p. 42
implies that camels came across the Red Sea to Africa quite early. Had they come later, the people of Somalia would have been taught to breed camels not strictly for their milk but for riding and transportation. As Bulliet adds:

“Therefore the Somali camels most probably stem from original stock imported from southern Arabia at a time when camel domestication in that area was in a relatively early stage [when camels were milked, not ridden] which was preserved in the isolation of Somalia after being superseded in its original area.”

When did this take place? It relates to Egyptian voyages to Punt supposedly dated from 2500 to 1500 B.C. And this is critical. If there were camels in both Arabia and the horn of Africa at that epoch, we run into a major contradiction to the established chronology and to the concept that Egypt was trading with these areas. Bulliet informs us that “a tentative deduction [based on the evidence available] can be made that the camel came to … Somalia at [a] date between 2500 and 1500 B.C. …” Brian M. Fagan and Charlotte Beck along these same lines state: “The camel subsequently spread to Somalia between 2500 and 1500 B.C. …” Isichei states these Somalis “do not ride camels … They rely heavily on their milk and value them as a way of accumulating wealth [as other Africans do with cattle].”

That being the case, both Hatshepsut who reigned, according to the established chronology, 1498-1483 B.C., and Tuthmosis III, who reigned after her, and all the other travelers to Punt from the mid-second millennium B.C. onward would have seen camels and they would have been known in Egypt from at least 1500 B.C. and more reasonably earlier. Placing Punt in either southern Arabia or across the gulf in the horn of Africa requires that Hatshepsut and all those who came after her had to have known the camel. Neither Hatshepsut, nor Tuthmosis III, nor any of the other visitors to Punt depicted the camel, nor wrote about it, because they did not travel south to where they would have seen this animal until around 600 B.C. To go to Punt, they had to travel northeast to Syria, Lebanon, and Palestine where frankincense, myrrh, and cedar grew.

Since these ca. pre-600 B.C. voyages were to the northeast, then of course they would not have encountered camels which were unknown at these times. Until proponents of the view that Punt was located well to the south of the Red Sea can prove their case, and explain away this problem and the others noted above, the conclusion remains that Punt prior to 600 B.C. was located northeast of Egypt. In terms of Velikovsky’s thesis that Hatshepsut was the Queen of Sheba who visited

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92 Ibid.
93 Ibid., p. 45
Solomon, the chronology for this connection is rather good. Solomon lived somewhat before 600 B.C., prior to the introduction of domesticated camels with the camel saddle, and so too did Hatshepsut.

In terms of chronology, we earlier described the evidence that at Ugarit there was found an inscription that deals with provisioning fodder for camels, which places Ugarit well within the first millennium B.C. With respect to incense and its trade we also find that Ugarit as well as Mitanni/Media also received these aromatic products. Because historians have saddled themselves with the erroneous belief that incense had to come via camel from Arabia or Somalia in the second millennium B.C., they cannot conceive that these resins came from later times. Here Nielsen reports:

“In the 2nd half of the 2nd millennium B.C. the first Semitic references to myrrh in the Syrian area appears. Myrrh oil occurs in Ugarit and Mitanni. How has myrrh arrived in Ugarit and Mitanni? … The myrrh in Ugarit and Mitanni may have arrived by either of these [land or Red Sea] routes or by both. Since the Egyptians hardly have acted as middlemen, there is reason to believe that Arabs have established themselves as merchants from S[outhern] A[rabia] in the 2nd millennium B.C. …”

In this respect we can see how the date of the use of the domesticated camel places not only Ugarit well into the first millennium B.C., but the Mitanni, whom we equate, as do Heinsohn and Sweeney, with the Medes, also belong well into the first millennium B.C.

If one locates Punt at the southern part of the Red Sea, we encounter another major contradiction to this hypothesis. Sweeney sums up this problem thus:

“The Egyptological establishment is nowadays fairly unanimous in placing Punt at the southern end of the Red Sea, either in Eritrea or Somalia, or a combination of these two places. … Yet such a location, we have seen, immediately raises the enormous problem of accounting for the fact that Thutmose III claimed to have conquered Punt – all of Punt – in his first year. No one in his right mind would suggest that any pharaoh ever ruled these territories; aside from the logistical problems of a military expedition to such remote regions, there is no archaeological justification for such a supposition. Not a trace of anything that could be construed as implying Egyptian rule, or even substantial contact with Egypt, has ever appeared.

“Yet the first year of Thutmose III’s reign did see major military activity: the conquest of Israel/Canaan. He could, and did, claim to have conquered all of that territory.

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96 Nielsen, op.cit., p. 22
“But locating Punt in Eritrea/Somalia raises an even greater problem. Actually, it is an insurmountable one. … The Old Kingdom references make it clear that Punt was a territory well-known to the Egyptians, and there is strong suggestion that it was a country which had attained a level of civilisation comparable to that of the Egyptians themselves. Certainly by the time of the New Kingdom Punt was a trading partner of Egypt, and the record of Hatshepsut’s expedition suggests a cultured region inhabited by people of mixed Hamitic/Semitic stock, who cultivated exotic plants like incense and mined for gold and other precious materials. As such, the archaeologist would expect to find, in Punt, the remains of a thriving Bronze Age culture. Indeed, if the reports of Punt emanating from Old Kingdom inscriptions are anything to go by, he would expect to find plentiful remains even of an Early or at least Middle Bronze Age civilisation. Now, we ask ourselves the crucial question: Can Eritrea/Somalia produce the required remains?

“The answer, sadly for Velikovsky’s critics, is a resounding no!

“Neither Eritrea nor Somalia has received the type of intense archaeological attention accorded to Egypt or Syria/Palestine. Nevertheless, a substantial amount of work has been done in both territories; and from this a fairly detailed picture of the region’s cultural and ethnic history has emerged. We now know, for example, that the area was colonised by groups of Arabs from across the Red Sea at various times during the first millennium BC. From the intermixing of these newcomers and natives there developed the great and venerable civilisation we now call “Ethiopia”. But this Ethiopia had nothing whatsoever to do with the Ethiopia of the Bible, which was Nubia.

“The Arab incomers were literate metal-workers who introduced high civilisation into what had previously been a Neolithic territory. The crucial question for us is: When did they arrive? Ironically enough, John Bimson devotes considerable space to this topic in his critique of Velikovsky. He sought to show that these Arab immigrants were the “Puntites” with whom Hatshepsut and the other Egyptians traded. For all that, he has to admit that the main Arab settlement took place only in the 8th century BC! (though according to Velikovsky’s chronology Hatshepsut’s expedition would have occurred in the 10th century). In order to get round this difficulty he quotes a number of sources which claim Arab settlement from the early Iron Age. The problem is (and Bimson himself is well aware of this) that Solomon’s early Iron Age, of the 10th century, is an Iron Age that exists only on paper. It has no archaeological confirmation. That is precisely why Velikovsky was compelled to identify Late Bronze Age Palestine with the Palestine of David and Solomon. In short, the Arab settlements in Eritrea would have needed to commence in the archaeological Bronze Age for Hatshepsut and Thutmose III to have found any high culture there. But no such settlements have ever been found. And of course the problem becomes even more acute when we remember that there was frequent intercourse between Egypt and Punt during the
Old Kingdom. Eritrea/Somalia should therefore, if it was Punt, have had an Early Bronze civilisation. The non-existence of any Bronze Age culture in this area, should, in itself, be sufficient to bury once and for all the notion of an African location. The only southern region that could qualify would be Yemen, but this only exacerbates the problem of Thutmose III’s conquest of the territory.”

Timothy Insoll corroborates Sweeney, stating:

“… material evidence for these possible early Egyptian-[African-]Punt contacts is lacking, for items of Egyptian provenance found in either Ethiopia or Eritrea and dating before the Ptolemaic period are unknown… . In fact, the bulk of objects of Egyptian provenance date from the Aksumite period, i.e. after the first century CE … .

“In contrast, archaeological evidence testifying to … contacts between South Arabians and Ethiopia appear to have substantial time depth [but only] stretching as far back as the eighth century BCE, as indicated by excavations of sites with an ‘unmistakable South Arabian appearance’ … .”

Moustafa Gadalla concludes that:

“Erroneous statements have been repeated again and again about the Punt Expedition, as portrayed in Hatshepsut’s Commemorative Temple on the west bank of … Thebes. There are those [historians] who insist on giving answers where there are no answers and as a result, they have publicized that Punt is Ethiopia [Eritrea] or Somaliland. There is not a single piece of evidence to support their supposed geographic location of Punt.”

To try to get around this impasse certain historians and in particular Kenneth Kitchen have attempted to date strata found in Arabia back to these earlier periods but not older than around 1200 B.C. or somewhat earlier, strictly on archaeological grounds while ignoring philological evidence that thoroughly contradicts that archaeological interpretation. Clapp discusses the fact that alphabetic writing was found in a mound/tell there called Haïar bin Humeid at a particularly deep stratum by Gus Van Beek who attempted to date this level to around 1200 B.C. or earlier. Because certain historians and archaeologists who are proponents of an Arabian Punt location needed just such evidence, it was accepted by them, but not by those who examined the alphabetic script:

“Van Beek’s final report [for a 1200 B.C. date for that level] and findings were well received by his American colleagues but were criticized … and dismissed abroad. In a review in a French journal, epigrapher Jaqueline Pirenne could only

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97 Sweeney, *op.cit.*, pp. 56-7
98 Timothy Insoll, *The Archaeology of Islam in Sub-Saharan Africa* (Cambridge UK/NY et al., 2003), p. 40
conclude that he had seriously confused his strata. In her opinion the script on this monogram could not possibly date before 700 BC, since its letters were clearly derived from the Greek alphabet, which dated to approximately 800 BC. The … Académie Française backed her view, as did academics in Britain, Italy, and Germany. In their opinion civilization in southern Arabia dated to no earlier than 800 BC, so there could have been no significant Sabean [Sheba] state … [dated earlier].”

These ancient Arabians, according to certain historians dated to around 1400 to 1200 B.C., invented an alphabet with letters that for all the world are Greek in character. But those who support the older date for this Sheba/Punt location cannot explain how the people there somehow invented these Greek-type letters some 600 years before the Greeks employed them. Kenneth Kitchen’s *Documentation for Ancient Arabia* (Liverpool UK 1994), p. 132 ff insists, however, that the interpretation of the archaeology trumps these Greek-type letters.

Since this Arabian alphabet particularly with the Greek-type letters KHM must be dated to around the 8th-7th centuries B.C., the dispersal of that alphabet to Africa on the opposite shore of the Gulf of Hormuz had to have occurred some time after this period. And in fact that is exactly what was found to be the case. Luigi Pareti informs us: “About the third century AD southern Arabian writing passed also to Ethiopia, where the Ethiopian alphabet presents such [alphabetical] modifications.”

Above and beyond all this, not only were the southern Arabians using certain Greek-type letters for their script; surprisingly they also employed cuneiform lettering, identical to the Ugaritic alphabetic script. Kitchen writes:

“The date of origin of the ancient South-Arabian script has been considered anew in recent years. Some 30 to 40 years ago, Cross suggested its emergence in the 14th/13th centuries BC, while recently, Sass has argued for the 11th/10th centuries BC. For more than one reason, a ‘middle date’ may be wiser, *i.e.* about the 13th/12th centuries BC. In the first place, we have the remarkable case of the tablet from Beth-Shemesh [western Arabia] inscribed with letters in Ugaritic script, but in the order of the Old-South-Arabian alphabet [which does not follow that of the late Phoenician, Hebrew or Aramaic one in the same order but has a different order]!”

That is, not only are Greek-type alphabetic letters employed by the southern Arabians before the Greeks began to use these, but somehow the southern Arabians were able to influence their western neighbors to employ the Ugaritic cuneiform

100 Clapp, *Sheba, op.cit.*, p. 173
alphabet, but in the southern Arabian Greek-type script order. Rather than conclude that both Greek-type alphabetic letters and Ugaritic cuneiform alphabetic letters originated in Greece and Ugarit of the first millennium B.C. as discussed above and had to be dated to that time on technological as well as linguistic grounds, Kitchen and some of his colleagues argue these actually Greek-type and Ugaritic cuneiform alphabetic letters were of late second to early first millennium date.

Where all this breaks down chronologically is when Kitchen and his colleagues relate this trade to the Neo-Assyrians whom, in volume II, we astronomically dated to Persian times. Their argument that this trade can be absolutely dated at least to the early first millennium B.C. because the Arabians were trading with the Neo-Assyrian kings must perforce be moved closer to the present by almost 300 years. At this point, Kitchen “hoists himself with his own petard.”

“… one other phenomenon has to be admitted, the rapid rise of the camel to prominence for relatively long-distance travel … Besides the 200 camels from Saba in ca. 750 BC, we have Gindibu of ‘Arabia’ contributing a fighting-force with 1,000 camels to the allies that confronted [the Neo-Assyrian king] Shalmaneser III at the Battle of Qarqar in 853 BC. There is limited but sufficient evidence taking back the knowledge of camels and restricted use of them a long way back beyond the 9th century BC. … Suffice it to mention Old-Babylonian attestation [of the camel] (early 2nd millennium BC), and the late 19th-Dynasty [camel] model from a Ramesside tomb … in Egypt, for example. …”

The Old Babylonians, as we have shown in volume II based on empirical, technological and other grounds, also existed in Persian times. Thus this calling upon the Old Babylonians, really of the Persian epoch, again contradicts the chronology of Kitchen and his colleagues but supports that of Heinsohn, Rose, and Sweeney. All trading roads from Arabia in this respect lead to the mid-first millennium B.C.

**INCENSE DARK AGES IN EGYPT**

As with everything else related to Dark Ages, Egypt goes through long periods when there is no trade or communication with Punt. The first major disconnection between Egypt and Punt occurs during the Hyksos/Akkadian/Assyrian epoch. On this divorce, Lionel Casson shows: “One area that the Egyptians early established contact with was Ethiopia and Somalia, or Punt, as they called it. … The water route [to Punt] was interrupted during the years that Egypt suffered under the Hyksos invasion.”

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103 Ibid., pp. 135-136
to ‘Punt’ … which had been interrupted by the Hyksos.”

On this point, Sweeney explains: “Yet the Hyksos (in Avaris/Sinai) could scarcely have prevented communication between the southern Egyptians [not under Hyksos domination] in Thebes and the people of Eritrea or Somalia.”

The only possible way the Hyksos situated in the northern part of Egypt could completely halt all trade to Punt was if Punt was located northeast of Egypt.

The second Punt Incense Dark Age also derives from the established chronology. Kitchen admits “After the time of Ramses III [conventionally dated to ca. 1182-1152 B.C.], early in the twelfth century BC, virtually all historical references to Punt (or expeditions) cease completely, and the name of Punt (except for one XXVIth Dynasty mention) becomes an empty fossil in purely traditional lists and allusions. Punt seemingly disappeared from history. What happened?”

Having created this enigma by resorting to the established chronology, Kitchen, like other historians, begins to grasp for straws which are really inventions without a scintilla of proof to back them up, but tries to make the problem go away:

“In the first place … only rarely between Ramesses III and the XXVIth Dynasty was Egypt politically united under rulers strong enough to mount expeditions to such distant lands as Punt. And even if such expeditions still just occasionally took place, they are not the kind of event to find any mention in the very narrow range of textual sources available to us during the period in question.

Second, the trade may, in fact, have died out for other reasons. During the first millennium BC, Egypt may have obtained supplies of aromatics from the neighbouring Levant [that is, Syria, Lebanon, and Palestine], drawing on the trade that then came from Saba [in Arabia or Africa] along the caravan route from Saba to Palestine. So, links between Egypt and Punt withered away.”

Consider what Kitchen is suggesting. He claims the Egyptians did trade with Saba, in earlier times, but that the traders of Saba failed to learn where Egypt was located, perhaps to the south, or west, or east, or north of their land, and remained either ignorant or oblivious to the location of the Egyptian states that brought them wealth through trade. Thus, when they reached the Sinai, with their camel caravans, rather than turning west to get to Egypt, a few days’ travel conservatively by camel, they continued northward instead for a few days to Retenu or Palestine. This trade would have gone on even with weak pharaohs and as noted earlier

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105 Werner Keller, *The Bible as History* (NY 1956), p. 206
106 Sweeney, *op.cit.*, p. 59
camels would have been known after the time of Ramses III. That a noted historian should offer such excuses to explain the incense Dark Age is lamentable.

The problem only gets worse for the established chronology and Kitchen because during these times the Egyptians had control not only of Palestine but of the Sinai desert as well. Finkelstein and Silberman explain:

“In the thirteenth century, Egypt was at the peak of its authority … The Egyptian grip over Canaan was firm; Egyptian strongholds were built in various places in the country, and Egyptian officials administered the affairs of the region. In the el-Amarna letters, which are dated a century before, we are told that a unit of fifty Egyptian soldiers was big enough to pacify unrest in Canaan. And throughout the period of the New Kingdom, large Egyptian armies marched through Canaan to the north, as far as the Euphrates in Syria. Therefore, the main overland road that went from the [Nile] delta along the coast of northern Sinai to Gaza and then into the heart of Canaan was of utmost importance to the pharaonic regime.

“The most potentially vulnerable stretch of the road–which crossed the arid and dangerous desert of northern Sinai between the [Nile] delta and Gaza–was the most protected. A sophisticated system of Egyptian forts, granaries, and wells was established at a day’s march distance along the entire length of the road, which was called the Ways of Horus. These road stations enabled the imperial army to cross the Sinai peninsula conveniently and efficiently when necessary. The annals of the great Egyptian conqueror Thutmose III [who reigned just after Hatshepsut] tell us that he marched with his troops from the eastern delta to Gaza, a distance of about 250 kilometers [155 miles], in ten days.”

Thus, even if the Puntite incense traders came by camel to Palestine during the time of Hatshepsut, they would have encountered the Egyptians and would have traded with them, and therefore the Egyptians would have known the camel and discussed it and depicted it on their temple, burial place, or other walls for all the world’s Egyptologists to find.

If the Egyptians went to southern Arabia by sea to trade for incense, or if the southern Arabians came to Egypt by camel, the Egyptians would have known the camel, written about it, and depicted it. If the Egyptians went to Somalia by sea to trade for incense they would have known and depicted the camel. No matter which way the Egyptologists turned, any trade carried on with these southern sites would have brought them into direct contact with camels. Only if Punt was located to the north in and around Palestine, would the Egyptians have been able to obtain incense, wood and other items, and not come into contact with the camel. This evidence once again corroborates and correlates with the other evidence to show that Punt was located not in southern Arabia or Somalia, but in and around Palestine.

109 Finkelstein, Silberman, The Bible Unearthed, op.cit., p. 60
Without a shred of archaeological evidence to support the supposition that Punt was located in southern Arabia, Eritrea, or Somalia, historians press forward with the hypothesis that Punt must be either in one or the other location. It simply never occurs to them that there is no archaeological evidence in Punt, where it is supposed to be. In this respect, David Henige raises an interesting point:

“While archaeologists debate among themselves endlessly, there is remarkably little direct colloquy between archaeologists and historians, despite the symbiotic relationship that exists in their respective work and conclusions. As Umberto Albarella put it: ‘… a quick browse through the literature can easily show that the archaeologists and historians often ignore each other’s evidence.’ In particular, such discussions should feature canons of proof, where, it seems to me, historians occupy higher ground but need to know how much higher. Merrilee Salmon, a philosopher of archaeology, offers a clue to the size of the abyss: ‘[g]iven scientific fallibilism, expressions such as “verification” and “validation” of hypotheses, “proof,” and “incontrovertible evidence” are not intended to suggest that absolute certainty is attainable. In the context of scientific inquiry, the limits of these terms are well understood.’ This double-speak is alarming for anyone who exercises the right to apply everyday meanings to everyday words, and suggests that historians need to worry whenever they encounter any [contrary] archaeological argument [or evidence to their historical conclusions]. Is it being made ‘in the context of scientific inquiry?’ Do words like ‘proof’ mean what the rest of us think they mean?

“Undeterred, archaeologists press forward. Discussing the vexed case of the location of a polity [like Punt or one] mentioned in the Chinese annals, Okazaki Takashi is optimistic [about having located it]: ‘[o]n the other hand archaeological findings cannot yet furnish a definite answer [for its location] either.’ With even more optimism, an Israeli archaeologist has been quoted as saying: ‘[w]e found almost certain proof that the story of the entry into Israel [by the Israelites led by Joshua] is very believable. The relevant materials are in the field. We only have to find them.’ With such visionary hopes and ambitions … [they] will never really ask how they know, and will forget that their epistemologies are not supposed to differ so much from those of the rest of us.”\(^\text{110}\)

Throughout the literature on the location of Punt, historians argue that it is either in southern Arabia or Eritrea-Somali Africa. The evidence for these locations, they suggest, “is very believable. The relevant materials are in the field. We only have to find that evidence. …” It is all visionary hopes and ambitions because they never ask how they know this when they still have to find that evidence and still indicate to the world and to each other that their non-existent evidence proves their claims. They are, as Henige suggests, offering “double-

\(^{110}\) Henige, op.cit., pp. 75-76 (emphasis in original)
speak” as proof: “we know where Punt is located because of the non-existent archaeological evidence.” A perfect example of this form of historical double-speak about Punt is presented by Dimitri Meeks:

“Punt ‘exists’ as if in a void, previously Arabian, now African, but a void, nevertheless, the exact whereabouts of which remain more or less unknown. Punt exists in research, but its outline does not materialize on any map.”

While telling us “Punt ‘exists’ as if in a void” and “exists in research” and “does not materialize on any map,” he still claims its “exact whereabouts … remain more or less unknown.” But its “exact whereabouts” are not “more or less unknown,” its whereabouts are to historians who place Punt south of Egypt “completely unknown.” Something that is “unknown” is “unknown,” not “more unknown” or “less unknown” but, in that selfsame word, “UNKNOWN.” When Meeks adds the adjectives “more or less,” he leaves a double message that there is still more or less knowledge of Punt’s location than that of the “void” he wrapped around it. A void cannot be more or less unknown. A void is completely unknown. But historians of all persuasions cannot face that simple fact because, we believe, to accept it leaves the door open to locating Punt in another area than Arabia or Africa.

What historians refuse to know and acknowledge fully is that there is abundant and solid evidence that indicates the location of Punt in and around Palestine at the times of the Old Kingdom and of Queen Hatshepsut.

1. While some texts place Punt either to the east or south as Meeks has shown, “texts locating Punt beyond doubt to the south are in the minority, but they are the only ones cited in the current consensus. All the other texts that locate Punt [to the northeast or the east] despite their large number have been ignored.” He further told us, “All these texts agree in assigning Punt and its inhabitants to the Near East in more or less direct contact with the lands of the Mediterranean coast.” Ignoring or suppressing this Mediterranean coast location in the current consensus betrays the fundamental irresponsibility of the historians’ profession.

2. The lists of Tuthmosis III and similar sources, on which the names of the various locations can be seen together with the heads of the Puntites and others, contain depictions of different peoples. “The ovals in the first have heads which can be identified as belonging to Africans, while those in the second are identical to those on ovals containing Near Eastern toponyms. Punt and its toponyms belong to the second category.” Thus the largest number of citations place Punt to the north along the Mediterranean coast and the people depicted in the lists as Puntites look like people of the Near East.

111 Meeks, op.cit., pp. 54-55
3. The fauna of Punt in all respects is Asian and/or closely located around and in Syria/Palestine. The rhinoceros is clearly that of the Asian variety and the giraffe is without doubt an Asian one. All this evidence along with the northern location of Punt along the Mediterranean coast and the Near Eastern appearance of the Puntites have been swept under the rug because it did not fit the southern Punt location paradigm.

4. The flora of Punt: “After his fifth visit of inspection to conquered Syria and Palestine, Tuthmosis III listed frankincense … following his ninth visit he stated he had received at “Retenu … ‘dry myrrh’ …” Nibbi reported “From Theophrastus … we learn that in his day, frankincense, myrrh and many other aromatic plants grew in the area which we call Syria.” Nielsen further shows Egyptian inscriptions discussing incense which give a clear “indication that sntr [incense] was considered a native product of Retenu [Palestine].” That is, frankincense and myrrh are directly mentioned as being products of Syria/Palestine by Tuthmosis III and Theophrastus. Yet historians have again failed to take this evidence into account with all the rest.

5. Travel to Punt as located in southern Arabia or the horn of Africa via the Red Sea route or else by camel caravan involves the problem of sailing the dangerous waters of that body or the chronological contradiction of desert transport by the camel that was not at all, in terms of the established chronology, being used for that purpose in Hatshepsut’ time, which brings us to:

6. If the expedition sent by Hatshepsut and all those that preceded it had sailed south to Arabia or the horn of Africa to obtain incense they would have encountered domesticated camels which were kept like cattle as wealth for their milk, wool, leather, etc. But during all that time not a mention or description of the camel appears in Egypt until the mid-first millennium B.C. And this, too, the historians dismiss with assumptions that they know they cannot prove.

7. The finding of Greek-type alphabetic letters in southern Arabia supposedly dating to 1200 B.C., when Greece did not obtain its alphabet until 700-600 B.C., 500 to 400 years later, is similarly cast aside by some conservative historians by employing historically based archaeology which is clearly interpretive. The additional finding of Ugaritic cuneiform alphabetic letters again makes little sense since we have shown above that the Ugaritic writings contain idiomatic phraseology found in the Hebrew Bible that was written over 600 years later, in Persian/Hellenistic times. When languages are separated by hundreds of miles in space and 600 years or more in time, they do not employ so many of the very same expressions. Above and beyond all that, Ugarit dated to 1400-1200 B.C. contained 12th Dynasty relics. As Rose’s Sothic dating evidence shows, the 12th Dynasty
only existed from around 500 B.C. The 12th Dynasty of about 500 to 331 B.C. does not leave relics in 1400-1200 B.C. strata, etc., etc., as outlined above.

8. There are no Egyptian relics in either of the southern incense states of Arabia or the horn of Africa. As Timothy Insoll told us: “… material evidence for these possible early Egyptian-[African]-Punt contacts is lacking, for items of Egyptian provenance found in either Ethiopia or Eritrea and dating before the Ptolemaic period are unknown … archaeological evidence testifying to … contacts between South Arabians and Ethiopia appear[s] to [be only] stretching as far back as the eighth century BCE,” which explains the Greek-style letters there and the Ugaritic cuneiform alphabet as well.

9. The two Dark Ages of trade between Egypt and Punt dissolve with the short chronology. As Casson stated, “the water route was interrupted during the Hyksos invasion.” If Punt lay to the south of Egypt, the Thebans in Egypt’s south could still have traded with Arabia and the horn of Africa via the Red Sea. The Hyksos/Akkadians/Assyrians who dominated northern Egypt and Sinai could clearly have cut off such trade, or used it exclusively for themselves, isolating Theban Egypt from Syria, Lebanon, and Palestine.

10. The second Dark Age for Egyptian trade with Punt comes after Ramses II until around Ptolemaic times, about 900 years later. But we know that Ramses III fought the Sea Peoples/Greek Haunebu and the Persians/Pereset in the 4th century B.C. just prior to the Ptolemies and thus there is again no Dark Age.

11. In all the time the Arabians or Africans traded with Egypt, the Egyptians failed to learn about the camel, discuss it, or even picture it. This is never explained.

12. Southern Arabia and perhaps Somalia became the focal points of the incense trade between Egypt and the rest of the Near East around Hellenistic times and thereafter. Of the written sources, Nielsen states: “The youngest [source of this trade is that of] Ptolemy, then going back to the time of Pliny, the anonymous author of *Periplus Maris Erythraei*, Strabo and his source Eratosthenes, and Theophrastus.”

This being the case, according to Nielsen, we arrive at the connection of Egypt’s trade with Punt located in Arabia or Somalia only after the domestic camel and the camel saddle permitted desert transportation in the first millennium B.C.

In terms of the Bible, Hatshepsut must be brought forward to the 8th-7th centuries B.C. to trade with Palestine, Syria, or Lebanon for frankincense and myrrh but not to know of the camel. With Rehoboam dated to the time of Hatshepsut’s stepson Tuthmosis III, it is clear that her expedition had to have journeyed to Punt, God’s Land, Retenu, which was the name given to Palestine during the reign of Solomon. On every level of evidence the connections of Egypt

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112 Nielsen, *op. cit.*, p. 19
with Palestine/Punt in those ancient times correlate with the scientific and technological evidence.
ADDENDA

It has been pointed out to us that we have not dealt with adequately with certain aspects of the relief of Hatshepsut at the temple of Deir el-Bahri, such as the governor’s wife Ati or Eti who appears to be of the black race. This was discussed in Edouard Naville’s “Hatshepsut’s Naval Expedition to the Land of Punt,” in The Temple of Deir el-Bahari, vol. 3 (London 1989). Many take the picture of this obese woman as being of the black race by her appearance. In this respect, Naville, writing in 1906, discussing the Puntites, writes:

“As for the Puntites, the regular inhabitants of the Land of Punt are called [a hieroglyphic notation given] without the sign of a foreign nation. The Puntite is a tall well-shaped man of a type which certainly belongs to the Caucasian race; his hair is flaxen, and is divided into well-made plaits; his nose is aquiline, his beard long and pointed; he wears only a loin cloth, with a belt in which a dagger is fixed. … They are painted red [not black or brown] but with the red of Horus, which [is] not exactly like that of the ancient Egyptians.”¹¹³

Naville makes it quite clear “whatever may be the meaning of the [Egyptian hieroglyphic] word, it seems to designate the real Puntite population of Hamite race to distinguish it from the negroes who also inhabited the land of Punt.”¹¹⁴

With respect to Ati or Eti, he states “Her stoutness and deformity might be supposed at first sight to be the result of disease, if we did not know from the narratives of travellers of our own time that this kind of figure is the ideal type of female beauty among the … tribes of Central Africa. We can thus trace to a very high antiquity this … taste, which was adopted by the Puntites, although they were probably not native Africans.”¹¹⁵ Let us further recall that the Puntites are painted red and not black or brown. In this respect Amelia Ann Blanford Edwards writes: “Maspero suggests that the Princess Ati may be suffering from elephantiasis; but Mariette is of opinion that the Egyptian artist has here represented not merely the wife of the chief, but the most admired type of the women of the Somali race. The complexions of [her] whole family are painted of a brick red, and their hair black, thus showing that they [the chief, his wife Ati/Eti and their children] are not of negro race.”¹¹⁶

Thus Naville agrees with Meeks whom we cited above that the Puntites are of the Hamitic, which we claim is the Semitic, race. These people are dressed in very different clothing than the Asiatics shown in the rock tomb of Beni Hassan with long woven dresses or skirts of many colorful designs. The Puntites depicted at

¹¹³ Naville, op. cit., pp. 34-35
¹¹⁴ Ibid., p. 36
¹¹⁵ Ibid., pp. 36-37
Deir el-Bahri exhibit nothing like these garments. But this only shows that the people of Punt dressed differently at different places. Interestingly, the Asiatics drawn in the rock tomb of Beni Hassan are leading donkeys; so, too, there is a donkey shown on the relief of Hatshepsut’s tomb about which Naville claims: “The ass which carried the ponderous princess shows that it was the beast of burden of the [Puntite] country.” However, as we have shown above, the camel was both the beast of burden and a well known animal of Arabia and Somalia. Why, then, is it never associated with Punt?

As to the possibility that Ati, the wife of the governor of Punt was an African woman who was obese as a matter of beauty among certain tribes, Eva Danelius further shows:

“A detail on the murals which seemed to hinder its identification with an Asiatic country consists of the picture of the ‘abnormally fleshy wife’ of the Puntites’ governor. The obesity of the wife was considered proof that the scene was in Africa, where wives are artificially fed and fattened. This supposition, however, has also become obsolete. In a very thorough study, made possible thanks to the ‘superior gift of observation of the Egyptians,’ it could be shown that the unhappy woman suffered from a progressive dystrophy of the muscles, a malady the cause of which is not known and against which no remedy has been found to this day. The sickness is hereditary and the first signs of it seem to have been observed by the artist also on her daughter, who follows behind her.”

Danelius cites Emma Brunner-Traut’s article “Die Krankheit der Fürstin von Punt,” in Die Welt des Orients (Göttingen), Bd. II (1954-59), pp. 307-311. “Der Fall der Fürstin von Punt stellt den ältesten Beleg für das Leiden dar. Wir danken ihm der überragenden Beobachtungsgabe der Ägypter und ihrer treuen Berichterstattung” (p. 310). This translates as: “The case of the princess of Punt represents the first evidence of this disease. We owe it to the eminent power of observation of the Egyptians and their faithful reporting.”

Thus, there is no evidence that suggests the wife of the governor of Punt is African. It has been further argued that Hatshepsut would not have left her country because, while she was gone, a cabal of priests and governors would have chosen to dethrone her, or a strong leader, such as Tuthmosis III, could have done so. In this respect Velikovsky explains:

“Queen Hatshepsut undertook the journey like a devout pilgrim who, hearing an inner bidding, takes staff in hand:

“… a command was heard from the great throne, an oracle of the god himself, that the ways to Punt should be searched out, that the highways to the myrrh-terraces should be penetrated:

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117 Naville, *op.cit.*, p. 32
118 Danelius, *op.cit.*, part III, pp. 12-13
“‘I [Hatshepsut] will lead the army on water and on land, to bring marvels from God’s land for this god, for the fashioner of her beauty …’ [Breasted, in Record, Vol. II, sec. 285]”\textsuperscript{119}

Velikovsky again cites Breasted’s Record, Vol. II, sec. 288, as follows: “I have led them [the company of the expedition] on water and on land, to explore the waters of inaccessible channels, and I have reached the myrrh-terraces.”\textsuperscript{120} Why did Hatshepsut write this on the walls of her temple unless she had made the trip?

When she speaks about the myrrh terraces, we noted above that terraces to hold water for cultivation were employed only after the 8th century B.C. pole shift and aridification of Palestine and the rest of the ancient lands. Danelius describes Nelson Glueck’s “three-hour aerial reconnaissance over part of the Negev in March, 1959.”\textsuperscript{121}

“On this flight, Glueck paid special attention to the way in which water conservation had been practiced, which alone would make cultivation possible in a region where rainwater was as scarce as in most parts of the Nile Valley.

“We could see from the plane how sterile hilltops and slopes had been pressed into service as catchment areas to help collect as many drops of rainwater as possible,’ observed Glueck, who believed that ‘the art of terracing wadi-boxes was extensively practiced at least as early as the time of the Judaean kingdom between the 10th and 6th centuries B.C.’ To this, Glueck added: ‘The fact remains that we can pinpoint in the Negev the existence of Judaean terraces and cisterns, of Judaean fortresses and agricultural villages …’ He could even observe, from his seat in the piper plane, that ‘the water-tight construction of some of the Iron II cisterns had lasted down to that same day, some of them being filled with water from the rains of late February and early March, 1959.’”\textsuperscript{122}

In this case it is clear that Hatshepsut would have seen thousands of terraces, probably with myrrh trees, as she proceeded along on her journey. We suggest all these geographical aspects of Punt make excellent sense in terms of identifying Punt with Palestine.

Another critical point raised is that the houses of the Puntites are made of wicker and wood and stand on stilts and must therefore be African. But ancient people who lived in swamp lands all over the world built their houses of these materials on stilts. Louise Spilsbury states, in discussing Mesopotamia, “The first settlers built their reed and mud huts, often on stilts.”\textsuperscript{123} In this regard, Emmet Sweeney suggests:

\textsuperscript{119} Velikovsky, Ages in Chaos, op.cit., p. 117
\textsuperscript{120} Ibid., p. 118
\textsuperscript{121} Eva Danelius, “The Identification of the Biblical ‘Queen of Sheba’ with Hatshepsut, ‘Queen of Egypt and Ethiopia’,” part II, KRONOS vol. I no. 3 (Fall 1976), p. 10
\textsuperscript{122} Danelius, op.cit., part II, p. 11
\textsuperscript{123} Louise Spilsbury, Changing Climate: Living with the Weather (Chicago IL 2006), p. 9
“Aside from the animals and negroes, the famous houses on stilts, at the foot of the myrrh terraces, were probably most influential in making scholars think of Africa. But in antiquity houses on stilts were by no means uncommon. Entire villages on stilts were found in ... Europe ... Why then should not some of the inhabitants of Palestine/Phoenicia dwell in like habitations? And where else would such dwellings be found but in the ‘marshes of Asia’ adjacent to God’s Land? In ancient (and more modern) times the area of Lake Hula constituted ‘one vast morass’, periodically susceptible to flooding in the aftermath of heavy rains to the north. ... Even as recently as the early 20th century the inhabitants of the area, the Ghor Arabs, led a life typical of marsh-dwellers. Of the latter [reeds from the marsh] too ‘many of their fragile houses [were] ... built.’”

As we have stated at the start of this chapter, the “materials are largely historical in nature and are derived by following the short chronology to its natural end. The evidence that follows, being mainly historical, cannot, therefore, be taken as definitive, and thus needs to be augmented by further forensic research to determine its validity. Because this historical analysis fits the short chronology, it is worthy of consideration.” On the other hand we have also presented a great deal of scientific evidence alongside this material that cannot be made to fit any of the chronologies of the establishment or the historical interpretations that proponents of the established chronology offer.

The author has studied the history of shipbuilding and the seaworthiness of Egyptian river vessels. Although one of these ships has been recreated, it has never been tested by being sent on a sea voyage down the Red Sea. Many questions need to be addressed and we look forward to sincere criticisms by our colleagues regarding this chapter, which we will address. Much more forensic evidence needs to be found to flesh out this period of history. Although we have found evidence that King David existed and Solomon might have existed, this is the problem inherent in our hypothesis. King Solomon exists only in the Bible and before anything can be vouchsafed for his reign, extra-biblical evidence beyond the “Temple of Shulman” needs to be found. While there is a great deal of evidence that points to Hatshepsut’s expedition going to Israel, we have no evidence that she met with Solomon. While supporters of this aspect of Velikovsky’s thesis take the written passages of the Bible about this meeting as valid, it is not proven. Absence of evidence is absence of evidence. Forensic historical evidence for this material may or may not exist, and may or may not ever be found. In this case, Velikovsky’s citation from Seneca applies:

\[ Quota pars operis tanti nobis committitur \]

\(^{124}\) Sweeney, *op.cit.*, pp. 50-1
CHAPTER 10
THE EIGHTH CENTURY CATASTROPHE, FLOODS, AND CHRONOLOGY

Velikovsky in *Worlds in Collision* maintained that the Earth had undergone two periods of celestial catastrophes. The first two events occurred around 1500-1400 B.C., which we touched upon in the first chapter. The second period of catastrophes was lower in its destructiveness, and these occurred around 800 to 700 B.C. These second events finally tilted the axis of the geographic pole far less than the first and the destruction caused by them was also far less in extent. Years ago, friend and anthropologist Roger Wescott told this author that a major difficulty for Velikovsky’s theory was that it would be extremely difficult to determine the physical evidence separating these two fairly close periods of cataclysm. It is further suggested here that Velikovsky attributed certain characteristics of the second period to those of the first. It must furthermore be pointed out that the first overwhelming period of cataclysm happened a few hundred years prior to the development of ancient civilizations, based on the short chronology, though Emmet Sweeney suggests it occurred during the First Intermediate Period between the Old Kingdom and the Middle Kingdom in Egypt.

Nevertheless, Velikovsky spoke of small pole shifts in the 8th century which disrupted civilization but did not destroy it, and the effects of this period of catastrophe and the evidence for it will now be presented.

According to Velikovsky, the first period of catastrophe around 1500-1400 B.C. led to an immense amount of flooding over major portions of the continents. This occurred because there was a sudden major tilting of the poles along with meteorite impacts in the oceans. Velikovsky further posited a second, much smaller, pole-shift which created far smaller flooding. If this is indeed the case, then these second, 800-700 B.C. inundations should have taken place in historical times, and thus would be taken by people of the ancient Near East as the Flood of Noah or of Gilgamesh in Mesopotamia, or of Deucalion in Greece. James L. Kugel in analyzing the Bible’s 29th Psalm states:

“The psalm seems to describe God’s arrival amidst a storm coming off the sea. First he is ‘over the waters,’ ‘thundering … over the deep;’ then the storm [or flood] hits the land, ‘shattering the cedars,’ making the whole earth tremble …

“After scholars came to know the literature of biblical Israel’s northern neighbor Ugarit, this psalm took on a new look. To put it bluntly, Psalm 29
seemed to many like a cheap knock-off of an originally northern Canaanite hymn, in which the name of Baal had simply been scratched out and replaced with the name of Israel’s national deity.”¹

Raymond H. Dillard and Temper Longman, in discussing Psalm 29, claim that in both cases there is the “image of God sitting on a throne that is situated above a flood.”² Richard J. Clifford, S.J. describes the literature at Ugarit which connects it with a flood. “It is thus not surprising to find Mesopotamian influence on Canaanite [Ugaritic] and biblical literature. A good example … is the creation-flood story … found at Ugarit.”³ Thus, we will show, Ugarit also experienced this 8th century B.C. flood.

We suggest that the evidence will show that lands adjacent to the sea would have been subjected to great inundations in the 8th century B.C. We will attempt, therefore, to connect several of these with that date.

In this respect there appears to be evidence of an inundation from the Indian Ocean at that time that drowned the southern Mesopotamian plain during Sumerian/Chaldean times, and an identical one that struck the Harappan/Indus civilization at the same time, and further, an identical flood that drowned the Chinese civilization. All these floods, we contend, were created by the same catastrophe. A small tilt of the Earth’s axis or immense sub-oceanic seismic events would cause floods all over the Earth. This is what is found at these three civilizations located at the southern coastal regions of Asia adjacent to the Indian and Pacific Ocean. Historians and archaeologists imbued with the belief that the established chronology is correct have therefore repeatedly claimed that these inundations were local events taking place at three different times and caused by great rivers overflowing the surrounding lands. The concept that these floods were simultaneous and were generated by oceanic waters flowing onto the land is much too catastrophic a concept and thus is unthinkable, and the literature is rife with statements that these events were local river floods which we will discuss below to show that these were much too great to be caused by rivers overflowing their banks. Before any critics suggest these events are not chronologically synchronous, we expect these critics to scientifically and technologically prove that the chronology to which they adhere is valid. The mere utterance that their chronology disproves the case we make is not evidence of anything.

¹ James L. Kugel, How to Read the Bible (NY 2007), p. 529
THE MESOPOTAMIAN FLOOD

The evidence of the southern Mesopotamian flood was unearthed by Sir Charles Leonard Woolley and is well summarized by Werner Keller who described the excavations at Ur wherein he shows that beneath the Royal Tombs,

“... a few hundred thrusts of the spade made it quite plain that further layers of rubble lay below. How far into the past could these silent chronometers take them [the excavators]?”

“When had the very first human settlement arisen on virgin soil under this [burial] mound? Woolley had to know. To make certain, he very slowly and carefully sank shafts and stood over them to examine the soil which came up from the underlying strata … ‘Directly under the floor of one of the [royal Ur] tombs we found in a layer of charred wood ash numerous clay tablets, which were covered with characters of a much older type than the inscriptions on the [royal] graves. Judging by the nature of the writing, the tablets could be assigned … they were therefore two or three centuries earlier than the [royal] tombs.’

“The shafts [then] went deeper. New strata with fragments of jars, pots, and bowls, kept appearing. The [pottery] experts noticed that the pottery remained surprisingly enough unchanged. It looked exactly like that which had been found in the graves of the kings. Therefore, it seemed that for [two or three] centuries Sumerian [Chaldean] civilization had undergone no radical change …”

The Royal Tombs, as we have shown, were built in the first millennium B.C. by the Scythian allies of the Assyrians, and Keller has told us that they were two or three centuries later than the cuneiform inscriptions well beneath them. Nevertheless, the pottery in the lower level, separated from that of the upper tombs by the flood level, was “exactly like that found in the graves of the kings.” This is significant for our chronological understanding. Over hundreds of years ancient pottery forms do not stay exactly the same. To suggest that in this unique case the people of Mesopotamia did not have changing tastes for the styles of their pottery but used the exact same forms defies history and the human desire for novelty which existed everywhere else in the ancient world. That is, there was a far shorter time span between the different older and younger cuneiform scripts. The Scythians had clearly removed the top layer of soil with these pottery sherds to build up the mound in which they buried their royalty. As proof of this we learn from Keller that Woolley dug two other shafts, one on the low-lying surface, the second on a natural hill, to determine the chronology:

\[4\] Werner Keller, *op.cit.*, p. 28
“Woolley made his men dig a shaft through the rubble where the old settlements lay and [down from the top of a] natural hill, that is to say on a considerably higher level than the [flood] stratum of mud.

“At just about the same level as in the two other [low-lying] shafts the shards of wheel-turned vessels ended suddenly. Immediately beneath them came handmade clay pots. It was exactly as Woolley had supposed and expected. Naturally [on the hill shaft] an intermediate [flood] layer of mud was missing.”

That is, the flood event came only about 100 years or so prior to the time when the Scythian Royal Tombs were dug. Prior to the flood the people were far less advanced than those who came after them. The people who settled on top of the flood layers had metal, wheel-turned pottery, as well as cuneiform writing. It is obvious that these advances of metallurgy, pottery technology, and epigraphy did not develop in less than a century or so above the more primitive culture beneath. These developments had to have a prior history of 300 to 500 years.

What we have, in terms of the short chronology, is that the Sumerian/Chaldean culture developed these advances beginning about 300 to 500 years earlier, or around 1000 to 1200 B.C., somewhere in Mesopotamia or elsewhere, and then moved onto and into the southern plain directly after the flood had drowned most of the people there, leaving it almost unpopulated. Thus the more advanced culture’s materials lay directly above those of the earlier, less advanced people. That requires that the Sumerian/Chaldean culture began 300 to 500 years earlier and that they were contemporary with these more primitive people. After they had settled the region, as we noted in volume I and elsewhere, the climate became permanently arid, and canal irrigation agriculture that began around 750 B.C. led to salinization of the entire southern plain about 400 years later. These different forms of evidence fix the date of the flood around 800 to 750 B.C.

The question that follows is: How deep were the flood waters that inundated the southern Babylonian plain, and could the Tigris and Euphrates Rivers have produced this depth of water on this plain? What we will now present is direct measurable evidence that it would have been impossible for both the Tigris and Euphrates to carry the necessary amount of water to produce the levels of silt laid down at Ur, at the same time flood the southern plain at Ur from 40 to 25 feet or 12 to 7.6 meters, and elsewhere on the plain to be level with this depth at Ur. To find this we return to Woolley whose diggers continued to go deeper.

“When after several days some of Woolley’s workmen called out to him ‘We are at ground level,’ he let himself down onto the floor of the shaft to satisfy himself. Traces of any kind of settlement did in fact break off abruptly in the shaft. The last fragments of household utensils lay on the smooth flat surface of the base of the pit.
Here and there were charred remains … He carefully prodded the ground on the floor of the shaft and stopped short. It was sand, pure sand of a kind that could only have been deposited by water. Mud in a place like that? Woolley tried to find an explanation: it must be the accumulated silt of the Euphrates in bygone days. This stratum must have come into existence when the great river thrust its delta far out into the Persian Gulf, just as it still does, creating new land out of the sea at the river mouth at a rate of seventy-five feet a year. When Ur was in its heyday, the Euphrates flowed so close to it that the great staged tower [the Ziqqurat] was reflected in its waters and the Gulf was visible from the temple on its summit. The first buildings must therefore have sprung up in the mud flats of the delta.

“Measurements of the adjacent area and more careful calculations, however, brought Woolley eventually to a quite different conclusion. ‘I saw that we were much too high up. It was most unlikely that the island on which the first settlement was built stood up so far out of the marsh.’

“The foot of the shaft, where the layer of mud began, was several yards above the river level. The mud, therefore, could not be river deposit.”

That is, the height of the water in the Euphrates being several yards, 25 to 40 feet or 7.6 to 12 meters, below the top of the mud/sand stratum meant the river had to rise not just to the top of the mud/sand stratum but several yards above it in order to deposit this depth of material. The river had to have risen over the land to a height of 40 to 80 feet. But as we will see below, the depth of the Euphrates is not in any way deep enough to ever rise to such a level on a broad flat plain about 100 miles/160 kilometers wide from east to west, and 400 miles/640 kilometers long from north to south.

“What then was the meaning of this remarkable stratum? Where did it come from? None of his [Woolley’s] associates could give him a satisfactory answer. They decided to dig on and make the shaft deeper. Woolley gazed intently as once more basket after basket came out of the trench and their contents were examined. Deeper and deeper went the spades into the ground: three feet, six feet—still pure mud. Suddenly, at nearly ten feet, the layer of mud stopped as abruptly as it had started. What would come now?

“The next baskets that came to the surface gave an answer that none of the expedition would have dreamed of. They could hardly believe their eyes. They had expected pure virgin soil, but what now emerged into the glaring sunshine was rubble and more rubble, ancient rubbish and countless potsherds. Under this clay/mud deposit almost ten feet thick, they had struck fresh evidence of human habitation. The appearance and quality of the pottery had noticeably altered. Above the mud stratum were jars and bowls that had obviously been turned on a potter’s wheel; here, on the contrary, they were hand-made. No matter how

6 ibid., pp. 28-29
carefully the baskets were sifted, amid increasing excitement, metal remains were nowhere to be found. The primitive implement that did emerge was made of hewn flint. It must belong to the [Neolithic] Stone Age!7

Thus, as with the floods at the Black Sea and the Mediterranean Sea, where a Neolithic village off the coast of Israel was found, this flood had intervened between a Neolithic culture and that of an ancient civilization. With the other evidence above and that in volumes I and II, this places the earlier flood event toward the middle of the second millennium B.C.

Let us return to the problem of where all the flood waters came from. According to Georges Roux, citing Woolley, “‘Eleven feet of silt,’ reasoned the archaeologist [Woolley], ‘would probably mean a flood no less than 25 feet deep[,] in the flat, low-lying land of Mesopotamia . . .’”8 Thus it is clear that at the very least, the flood waters that inundated the Mesopotamian plain at Ur were 25 feet or 7.6 meters deep.

One of the points that must be stressed is that the southern Mesopotamian plain is extremely flat. According to P.R.S. Moorey, “Mesopotamia is remarkably flat. In the distance of over 300 miles (480 kilometers) which separates Baghdad from the head of the [Persian] Gulf, it drops only by about 35 meters [115 feet].”9 Ian Wilson also reports, “Because of the flat, low-lying nature of Mesopotamia’s terrain its elevation drops by only 35 meters (115 feet) throughout the 480 kilometer (300 mile) distance from Baghdad to the Persian Gulf.”10 That means that for every 100 miles/161 kilometers the land slopes a little over 38 feet/12 meters. Every mile/1.6 km the land slopes 38/100 of a foot or just over 1/3 foot. Water, of course, on such a flat surface will not pile up to a height of 25 feet at Ur and perhaps as much as 40 feet without seeking this level everywhere else on that Mesopotamian plain.

In order for the waters at Ur to stand at least 25 feet above the plain it must stand at that same level wherever the plain is at about the same level as at Ur. Farther south of Ur the depth of the flood waters will be greater and greater while north of Ur the depth will become smaller and smaller. In this way, the water will be almost evenly distributed across the southern Mesopotamian plain.

Furthermore, this region is encompassed by mountains or highlands on three sides to the east, west, and north. Only the southern plain is flat where it meets the Persian Gulf. In essence, it is a trough, as described by Susan Pollock:

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7 ibid., pp. 29-30  
8 Roux, op.cit., p. 100  
9 P.R.S. Moorey, Ur of the Chaldees: A Revised and Updated Edition of Sir Leonard Woolley’s Excavations at Ur (Ithaca NY 1982), p. 32  
10 Ian Wilson, Before the Flood (NY 2004), p. 29
“Mesopotamia is, geologically speaking, a trough created as the Arabian shield has pushed up against the Asiatic landmass, raising the Zagros Mountains [to the east of the plain] and depressing the land southwest of them … Within this trench, the Tigris and the Euphrates Rivers and their tributaries have laid down enormous quantities of alluvial sediments, forming the Lower Mesopotamian Plain … Today the Lower Mesopotamian Plain stretches some 700 kilometers, from approximately the latitude of Ramadi and Baquba in the northwest to the Gulf, which has flooded its southern end.”11

Keller explains that “According to Woolley the disaster engulfed an area northwest of the Persian Gulf 400 miles long and 100 miles wide. Looking at the map today we should call it ‘a local occurrence,’ but for the inhabitants of the river plain it was, in those days, their whole world.”12 Ruth E. Moore concurs: “Genesis had fixed the Flood’s depth at twenty-six feet. In the flat valley Woolley calculated such a flood would have covered an area three hundred miles [480 kms] long and one hundred miles [160 kms] wide.”13

Now we were told by Woolley earlier that the top of the mud stratum was several yards or meters above the Euphrates River and that this river could not have produced a sufficient amount of water to raise it to a level of at least 25 feet or 7.6 meters above the plain. The reason is that there is not enough water in the entire Euphrates and Tigris Rivers, even at flood stage, to raise the level of water on a plain 30,000 to 40,000 square miles in area to anywhere near such depths. In 1909 Robert W. Rogers analyzed the width and depth of that Euphrates River in an article which we now cite:

“From the Khabur [river] to its mouth, a distance of 800 miles [1280 kms], the Euphrates receives no tributaries … When it receives the Khabur it is 400 yards wide and eighteen feet deep. FROM THAT POINT IT BEGINS TO DIMINISH IN VOLUME [because of evaporation and seepage into the Mesopotamian silt plain]. At Irzah or Werdi, seventy-five miles lower down, it is 350 yards wide and of the same depth; at Hadiseh, 140 miles below Werdi, it is 300 yards wide and of the same depth; at Hit, fifty miles below Hadiseh, its width has increased to 350 yards, but its depth has diminished to sixteen feet; at Felujiah, seventy-five miles from Hit, the depth is twenty feet but the width has diminished to 250 yards. From this point the contraction is rapid and striking. The Scklowijeh canal is given out upon the left, and some way further down the Hindiyeh [canal] branches off upon the right, each carrying, when the Euphrates is full, a large body of water. The consequence is that a Hilah, ninety miles below Felujiah, the stream is no more than 200 yards wide and fifteen feet deep; and at Lanlum, eighty-five miles lower

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12 Keller, *op.cit.*, p. 31-2
13 Moore, *op.cit.*, p. 32
down, it is reduced to 120 yards wide with a depth of no more than twelve feet. Some of the channels which take water out of the river afterward, return to it again, but it never attains its earlier greatness. The channel from Kurnah to El Khitr was found by Colonel Chesney to have ‘an average width of 200 yards and a depth of about eighteen or nineteen feet … [During the flood season] about the end of May … the water stand [is] about thirteen feet above low water … [during the dry spring-summer season].’

And let us remember that the river level had to rise several yards just to reach the highest point of the mud stratum. Thus it had to rise well above that point to deposit 8 to 11 feet of mud. Then the water had to spread out over the plain to reach a common level so that the water, as described by Woolley, could stand at the least 25 feet above the plain at Ur. “In fact, even if one were to grant the validity of only a local flood, great difficulty would remain … water seeks its own level. Mesopotamia is no watertight basin to be filled with flood waters.”

Not only would the water have to spread across the Mesopotamian plain seeking its own level, it would also at some time be running as a massive flow southward, debouching into the Persian Gulf. Even if we add enough water to cover so vast a region to such depth and the same if we double, triple or quadruple these amounts.

The levels given above were an average between the spring/summer dry season and the rainy fall/winter season. According to A. Locher, the Euphrates has “a maximum depth of fifteen feet during the dry season .” Thus at its more northern stretch, where the volume of the river is fullest, it is about 15 feet or 4.5 meters in depth. Farther south its depth is even lower than 15 feet which with an additional 13 feet during the rainy season makes its depth in the southern part of the plain at best around 20 feet. At no time during either the wet or dry season is there enough water in the entire Euphrates/Tigris river system to flood the entire Mesopotamian plain to a depth everywhere of at least 25 feet; 40,000 or 30,000 square miles of Mesopotamia would never have been inundated anywhere to such depths. We suggest that if the entire rivers from Baghdad to the Persian Gulf completely emptied onto the surrounding plain it would fail to create such depths. The only source of such an inundation are the Persian Gulf waters of the Indian Ocean.

\[\text{Footnotes:}\]
15 Bromley, ed., op.cit., p. 318
16 A. Locher, With Star and Crescent (Philadelphia PA 1980), p. 539
In this regard Keller reports “The sea had left its unmistakable traces in the shape of remains of little marine organisms embedded in the mud.”\(^{17}\) In this respect, G.M. Lees and N.L. Falcon, who accept that this flood was of a marine origin, state:

“Although numerous wells and bore-holes have been put down for various purposes in the Mesopotamian plains, very little attention has been given to the fossil content of the alluvium. It is always difficult, without expert knowledge and local experience to be certain whether fossils recorded by early travellers were actually in situ, or whether they had been washed into the alluvium from neighbouring rocks or even in some cases brought in by animal agencies (including human beings). Caution must therefore be exercised before such evidence can be safely interpreted in terms of marine or fresh-water conditions of sedimentation. This is particularly true of the hard parts of smaller [sea] organisms, such as foraminifera, which can be carried long distances by currents …

“Confining ourselves to evidence which is new and acceptable, we find that a few available facts support our hypothesis of local marine inundation due to subsidence [of the Mesopotamian plain].

“Dr. W.A. Macfadyen, to whom we are indebted for allowing us to refer to unpublished work, has found the marine foraminifera Buliminella, Streblus and Triloculina in alluvium at Amara, 95 miles north-north-west of Basra, ranging from a depth of 3 feet to a depth of 35 feet below the present level of the bed of the Tigris (stated to be 10 feet above sea level). At Qurmat Ali on the north-western outskirts of Basra, samples of alluvium from 2 feet and 16 feet below ground level contained a mixture of marine and fresh-water organisms … whereas samples from 10 feet and 64 feet below the surface produced only marine organisms…”\(^{18}\)

These measurements were made south of Ur and thus indicate that earlier that region was below sea level. But when this change occurred and how far the sea may have reached is still not very well determined. The marine evidence is thus highly questionable and cannot be employed as evidence one way or the other. The location of the ancient shore-line south of Ur was analyzed by de Morgan in 1900. Lees and Falcon explain:

“The archaeologists’ conception of the development of the Mesopotamian plains was supported at the turn of the present century by the geologist de Morgan who, having explained the general principles of delta formation, attempted to show how the advance of the deltas of the Euphrates, Tigris and Karun rivers conform to type. In reconstructing the early geography of the area south-west of Ahwaz, de Morgan discusses the voyage of Nearchus in 325 B.C. as described by Strabo, and also Sennacherib’s expedition against Elam in 696 B.C. [which must

\(^{17}\) Keller, *op.cit.*, p. 30

be moved closer to the present by 274 years, based on Rose’s astronomical work] as described in Assyrian documents. An attempt to fix localities, and the shore line, by a study of the various distances given in the accounts of these expeditions was summarized by de Morgan in two maps … but it must be borne in mind that no single place name in these old records can be identified; the maps are based only on guesses of localities.”

Therefore, the actual location of the Persian Gulf shore line with respect to its distance from Ur is not accurately or generally known for that time. However, even if at that time that shore line lay closer to Ur than at present, the region to be covered by the inundation, reduced to about 100 miles north of the Persian Gulf, would still have to flood an area 100 miles east to west by 300 to 400 miles north to south, or 30,000 to 40,000 square miles, so that these rivers spilling all of their waters onto the plain would never have raised to the level of 40 to 80 feet.

What critics of the flood theory argue is that the flood did not leave sediment all across the plain. Therefore, the flood was only local at Ur. As pointed out by Benjamin Eddin Scolnic:

“When Woolley got down to a level from the period 4000-3500 BCE, he found a stratum of mud ten feet thick. He concluded that this was evidence of Noah’s flood. Woolley’s conclusion is trumpeted by … popular discussions … What only scholars know is that Woolley dug five pits down through all the strata at Ur and only found water, or [mud] evidence of the flood, in two of them! The so-called universal flood did not even cover the whole site at Ur. When Woolley dug at a site only four miles from Ur there was no flood level [mud] at all.”

William H. Stiebing, Jr. raised the very same argument: “Others have shown that the flood at Ur was local and did not cover all areas of the city, let alone most of Mesopotamia.”

It is difficult to deal with comments so woefully ignorant. In the first place, Woolley told us the flood water level had to be at least 25 feet deep at Ur to lay down 8 to 10 feet of mud. And since water seeks its own level, all the area around Ur had to have been covered by 25 feet of water. In the second place Woolley fully explained why the mud stratum was only found at two areas where he had dug which Stiebing and Scolnic omitted from their criticism:

“…a flood does not, of course, pile up silt everywhere–on the contrary, where the current is strongest it may have a scouring effect; the silt is deposited where

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19 ibid., p. 24
20 Benjamin Eddin Scolnic, If the Egyptians Drowned in the Red Sea Where Are Pharaoh’s Chariots (Lanham MD/Oxford UK 2005), p. 41
the current is held up [slowed] by some obstacle. When ... duly plotted it was clear that the mud was heaped up against the north slope of the town mound [of Ur] which, rising above the plain, broke the force of the flood waters; on the plain east or west of the mound we should probably have found nothing. Eleven feet of silt—the maximum—would probably mean a flood not less than twenty-five feet deep; in the flat low-lying land of Mesopotamia, a flood of that depth would cover an area about three hundred miles long and a hundred miles across [based on de Morgan’s shore line explanation]; the whole of the fertile land between the Elamite mountains and the high Syrian desert would disappear, every village would be destroyed, and only a few of the old cities, set high on their built-up mounds, would survive the disaster.”

This evidence echoes throughout the scholarly literature. Alan Ralph Millard succinctly states: “At Ur, Woolley admitted, the silt did not cover the whole site. The great depth of clean soil he dug through appeared to be the result of water running against part of the ruin mound, perhaps over a long period of time.” In 1992, Julius Zarins wrote that in spite of the fact that Eridu is located in a low depression southwest of Ur, it is separated from Ur by “an eight-meter [26 foot] scarp of the Upper Fars formation ... possibly blocking any marine infilling into the depression.” J. Mellaart in this instance takes up the question of why Eridu was not covered by sediment:

“Most puzzling to the historian has been the failure to detect evidence at Eridu of the same fluviatile deposits at this time. But on reflexion this is not surprising, for the two sites are over 12 miles apart, and are separated by a limestone ridge. Moreover, whereas Eridu lay on the bank of a canal, Ur was situated near the main stream of the Euphrates. It must also be recalled that flood debris will only be left on a large scale when there is a barrier or obstacle to prevent it from being swept over the plains. Ur, as an island in the marsh, provided that obstacle—only thus can we account for the absence of similar evidence from the neighbouring sites of Eridu and elsewhere.”

Geologist Dorothy B. Vitaliano directly states:

“The thickness of the flood silt in the excavations in Mesopotamia does not prove that the waters stood deep and for a long time. It is not the depth of the water but the velocity that is the deciding factor in sedimentation. Fast-flowing water does not deposit sediment; on the contrary, it scours and erodes, picking up

23 Millard, *Discoveries...*, op.cit., p. 39
anything loose and carrying it along. The moment the current is checked for any reason, the particles fall to the bottom; first the heaviest, and, as the current slackens, progressively finer particles fall. Local obstructions, such as buildings or walls, may either speed up the current or impede it. If the flow is constricted by being forced around or between objects, its velocity is increased and so is its capacity to scour, and the obstruction may be undermined or swept away; but if the obstructions are so placed as to create a backwater, a sizable load may be dumped in the quiet spot while other places are receiving no sediment at all. This can explain why the alluvial layer laid down in the flood … is not found in all the pits that were dug through the early strata at Ur …”

Millard, in his explanation of the “Flood and flood stories” has argued “A 3–m[eter]–deep layer of clay at ‘Ur’ is the result of water standing for a long period, not, as Woolley asserted, evidence of ‘The Flood’.”27 Let us take Millard at his word for a moment. If the water around Ur stood at a level of 25 or more feet [7.6 meters], then it had no place to flow away fairly rapidly. That being the case, sediment would have built up all over the region and not just in the area blocked by large obstacles, but this is not the case. If, on the other hand, the same condition prevailed and only at the end of the flood the movement of the water increased, then it would have carried some of the silt blocked by the obstacle of Ur around that obstacle and left an alluvial fan-like line-of-sediment at both sides of the obstacle running down current southward. None of these conditions exist at Ur, and the concept that this was a massive but relatively slow moving body of water cannot have the Tigris/Euphrates rivers as its source because they do not have sufficient water to cover southern Mesopotamia to such a depth; an oceanic source, on the other hand, cannot stay for a long period because of the following evidence.

This inundation did not last very long because “if salty waters of the Persian Gulf mixed with fresh waters standing on the land, at least the southern part of the region [thereafter] would have been rendered sterile [from salt] after a year of inundation.”28 Vitaliano cites a geologist who suggested the cause of the flood was an oceanic inundation:

“The Viennese geologist Eduard Suess … proposed in 1904 that flooding in Mesopotamia might have been compounded to more catastrophic proportions if a typhoon drove the shallow waters of the Persian Gulf inland upon the delta, blocking the flow of the rivers already swollen. The recent typhoon disaster in East Pakistan emphasizes the inherent plausibility of such a suggestion. Suess

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26 Dorothy B. Vitaliano, Legends of the Earth (Bloomington IN/London 1973), p. 156
28 Bromley, op. cit., p. 318
further believed that an earthquake could have been responsible for the idea of the bursting out of subterranean water as an additional cause of the flood.”

Theresa Howard-Carter along these same lines writes:

“Nearly all the authorities who have attended to the flood question in writing before 1975 are generally proved right insofar as they merely refer to existence of floods in Mesopotamia. But recent research in geomorphology of the Gulf area now forces us to think in larger terms. That research documents what appears to have been a major inundation … at which time the waters of the Gulf reached a point north of Amara …

“The peaking of the waters … was a massive movement of the sea which is not to be confused with later small floods affecting specific sections of lower Iraq … Waters moving north from the Gulf would therefore follow a natural line north from Basra covering effectively the cities of Sumer …

“The evidence from literary sources appears in general terms to document this great flood, a flood intended by the gods to destroy mankind. This flood is a flooding sea from the south, accompanied by torrential winds and rains referred to as the ‘terrifying flood from the sea’ and ‘the inundation of the deep.’ It is to be noted that the epics insist upon a south storm …

“This giant of all floods occurred … at a point already distinguished archaeologically as the beginning of the Uruk period. This is stratigraphically demonstrable at Eridu, Ur, and Warka.”

Thus there are direct statements in the literature that the great flood came from the Persian Gulf. However, one of the major arguments against this thesis is solely based on the established chronology. Flood sediments have been found at the sites of Kish, Fara/Shuruppak, and Nineveh. Max Mallowan attempted to link these with that of Ur chronologically. But this failed, it is suggested, because that chronology is not correct. Here J. David Pleins explains:

“Mallowan hunted elsewhere for evidence, reconsidering the flood layers at Kish. While one of the Kish layers was later dismissed by Watelin as a candidate because clay seals of Gilgamesh were found beneath [that particular] layer, an earlier layer was taken by Mallowan to be the flood layer of the flood legends. The date fit for Mallowan was 2900 B.C.E. Putting the nail in the coffin, Mallowan also discerned a contemporaneous flood layer at Shuruppak [Fara] … The connection with Shuruppak [with those of Ur and Kish] would make sense.”

Consider the concept that a great tsunami swept north from the Indian Ocean up the Persian Gulf and across the southern Babylonian plain. It would sweep up all

29 Vitaliano, *op. cit.*, pp. 154-155
31 J. David Pleins, *When the Great Abyss Opened* (Oxford UK/NY 2003), p. 64
sorts of terrestrial surface soil and debris and deposit it inland. Since historians have failed to take such a concept into their considerations, they simply assume the flood came from riverine waters to the north. Because of this misinterpretation of the evidence, when they have made detailed analyses of materials related to flood deposits they have encountered problems. For example, Watelin, when he made just such a survey at Kish, discovered to his surprise that the flood materials he had expected to be there made no sense. They were not from the area locally nor could they be related to flooding of the Euphrates or of other sources.

But there is also an enormous problem for the so-called local flood at Kish. The sediment found there could not be local, with Kish being inundated by the Euphrates River. We are told of

“... Watelin, the excavator of Kish. It resulted in ‘fresh-water silt containing just those elements which are to be expected from the water of the Euphrates. (54) However, the silt caused a surprise because of ‘the absence of freshwater mollusks as well as by the absence of contemporary marine organisms, and by the presence of terrestrial mollusks in one specimen only.’ (55) This curious sterility was not to be expected for the tremendous flood of riverine origin required to accumulate up to 370 cm [12 feet] of silt. When, two decades later, geologists found out that the water of the Shatt el-Arab, around which the flood yielding sites [at Kish] are located, is practically silt-free, (56) the riverine theory could no longer be upheld. Even if there had been silt the velocity of a riverine flood ‘would incidentally inhibit the deposition of silt.’ (57) ‘The great recorded depth of the deposits at Ur over 3 m[eters, 10 feet], and at Shuruppak probably about 60 cm [23 inches] are significant as they would require lagoon-like conditions for a fairly long time.’ (58) Therefore, an Aeolian (wind-blown) origin was considered. The absence of lamellation seems to point in that direction. Yet, there was ‘a complete absence of any larger rounded particles which usually occur in Aeolian deposits.’ (59) In addition it was found that the flood deposits differed from the present desert sand.’ (60) Flood research ended in perplexity. Scholars had to go back to the drawing board where they now focused on the ‘impounding of the water through the operation of some hitherto unidentified phenomenon.’(61)32

The scientists simply do not know where these flood sediments came from. They admit they could not be from the Euphrates for good reasons, nor from wind deposits, nor from local desert sands. The only conclusion left is that this sterile deposit came from some distant foreign source and was carried there by water over a great distance. The belief that Kish contains local materials from in and around the area is now known to be impossible. The concept stands up well in terms of a

32 BAR International Series (Ann Arbor MI June 2, 2008), p. 176
great oceanic flood sweeping across the Mesopotamian plain carrying distant materials with it and leaving them far from their origin.

And how does one know which sites were contemporary with the Neolithic flood layer beneath Ur? One cannot say that other places in Mesopotamia were definitely not more advanced when this flood struck. Some may in time, in terms of the short chronology, be scientifically and technologically linked with each other. When one argues that all the floods occurred at different times, one is basing that thesis on the sole assumption that the established chronology has been proven valid. The evidence in these three volumes of *Pillars of the Past* has shown this assumption to be invalid. Thus, there is work ahead to be done to forensically link the flood sediments at different sites with one another if possible or explain them from other floods.

Woolley himself stood resolutely by his interpretation as pointed out by Piotr Bienkowsky and Alan Ralph Millard. “Woolley was quite inflexible and loath to modify his overall interpretation [of the great Ur flood covering all of the southern Mesopotamian plain] in the face of new evidence, despite being prepared to reconsider matters of detail.”

P.R.S. Moorey states: “Although this [great flood] equation remains uncertain, Woolley never questioned it.”

Woolley himself claims:

“This was the evidence we needed; a flood of a magnitude unparalleled in any later phase of Mesopotamian history; and since, as the pottery [types] proved, it had taken place some little while before the time of the Erech dynasty, this was the flood of the Sumerian [Chaldean] King-list and that of the Sumerian [Chaldean] legend, and that of Genesis.

“We have proved that the Flood really happened; but that does not mean that all the details of the Flood legend are true—we did not find Noah and we did not find his Ark! The Sumerian [Chaldean] version says (this is not mentioned in Genesis) that antediluvian man lived in huts made of reeds; under the Flood deposit we found the wreckage of reed huts … I reckoned that to throw up an eleven-foot pile of silt against the mound on which the primitive town of Ur stood, the water would have to be at least twenty-five feet deep; the account in Genesis says that the depth of flood water was fifteen cubits, which is roughly twenty-six feet. ‘Twenty-six feet?’ you may say, ‘that’s not much of a flood!’” Lower Mesopotamia is so flat and low-lying that a flood having that depth at Ur would spread over an area [based on de Morgan’s analysis] 300 miles long and 100 miles wide.

“[This flood was not a universal deluge; it was a vast flood in the valley of the Rivers Tigris and Euphrates. It drowned the whole of the habitable land between the eastern and the western deserts; for the people who lived there that was all the

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33 Bienkowski, Millard, *Dictionary …, op.cit.*, p. 322
34 Moorey, *A Century …, op.cit.*, p. 79
world. It wiped out villages and exterminated their inhabitants, and although some of the towns set upon mounds survived, it was but a scanty and dispirited remnant of that nation that watched the waters recede at last. No wonder that they saw in this disaster the gods’ punishment of a sinful generation and described as such in a great religious poem; and if, as may well have been the case, one household managed to escape by boat from the drowned lowlands, the head of that house would naturally be made the hero of the saga."

Woolley examined the scientific evidence as it related to the Mesopotamian flood and on the basis of science accepted that there had been a great deluge that drowned southern Mesopotamia. In that sense he was following the course we have taken in these volumes, and therefore stood his ground except for “details,” as do we. What is notable is his statement that this was “a flood of a magnitude unparalleled in any later phase of Mesopotamian history.” Woolley also wrote:

“Both at Ur and on other Mesopotamian sites there has been found evidence of local and temporary water action occurring at various times in history; sometimes this was no more than the effect of rain in an enclosed area, and never is there anything approaching what we found in our ‘Flood pit’ [at Ur]. There, it can safely be said, we have proof of an inundation unparalleled in any later period of Mesopotamian history.”

If this flood was caused by overflowing waters of the Tigris and/or Euphrates Rivers, why, during the next 5000 years (based on the established chronology) weren’t there other floods or even one comparable in magnitude to the one Woolley found? What historians, archaeologists, and even scientists have accepted and presented is that this was merely a large local flood. Yet if this was really the case, there would have been others comparable to it that occurred over five millennia in the same area. This event was unique in nature and, because of its catastrophic destruction, drowning tens of thousands of people, devastating many cities and hundreds of villages, and leaving a remnant of survivors, most of whom must have died of starvation or disease and whose land was then taken over by a more advanced society. All this makes far greater sense than the concept of small local floods or of the Black Sea flood theory of Pitman and Ryan.

“Taking the Mesopotamian flood layer as clues, Woolley’s work and Mallowan’s hypotheses leave us with provocative, albeit speculative candidates for the origin of biblical and Mesopotamian flood story tradition.

“Even if the flood stories have some roots in Mesopotamian flood conditions, the stories go well beyond the scope of a local flood limited to southern

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36 Woolley, *Excavations …*, op.cit., p. 35
Mesopotamia. It is hardly necessary to build a boat to save the entire [Black Sea] world when everyone could just walk a few miles to escape the peril …

“Of course Ryan and Pitman’s theory is not without chronological difficulties. The Mesopotamian finds of Woolley and Mallowan seem to be the better fit in terms of both the region and the chronology. By contrast, the vast time lapse and geographical disconnect for the Black Sea flood appear to make it the less likely candidate, if any single flood is really behind the tales.”

THE INDUS FLOOD

If there was a pole-shift, as Velikovsky suggested, around 800-750 B.C., and the Indian Ocean waters at the equatorial bulge moved both north and south, not only would this mountain of water have swept up the narrowing Persian Gulf into southern Mesopotamia and then in weeks or months back into the sea, the very same flooding catastrophe should have befallen the same type of low-lying flatlands adjacent to the Indian Ocean, namely the great Indus plain of modern Pakistan. And it should have happened at the very same time and it should have been of a magnitude that never occurred again over the supposed 5000 years of subsequent history in that region. All this we will show is exactly the case, and the time for it, as with the flood in southern Mesopotamia, occurred about 800-750 B.C. We outlined the chronology of Harappan civilization in volume II, pages 546-565. There the scientific, technological, and other evidence indicated that this culture ended around the time of a permanent climatic change in the 7th century B.C.; we will now show it was devastated by the same flood from the Indian Ocean that overwhelmed southern Mesopotamia.

The concept that the Indus civilization ended with some kind of catastrophe is well documented. For example Dhavendra Kumar states in this regard that “It is possible that the whole valley came to an abrupt end due to some natural catastrophe such as an earthquake or major floods.” Gerald James Larson reports: “Regarding the decline of the [Harappan] civilization … some have speculated that its decline was caused by invading nomadic Aryans [a thesis without evidence and no longer accepted] … Others have suggested that the civilization was the victim of a major natural disaster, possibly a flood, earthquake or famine.”

Bridget Allchin and Frank Raymond Allchin relate “… Mohenjo-daro … included three

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37 Pleins, op.cit., p. 65
38 Dhavendra Kumar, “The Indian Subcontinent and Human Genetics: An Introduction,” Genetic Disorders of the Indian Subcontinent, Dhavendra Kumar ed. (Dordrecht, the Netherlands 2004), p. 4
39 Gerald James Larson, India’s Agony Over Religion (Albany NY 1995), p. 56
principal structural levels of Marshall’s intermediate period [which] was succeeded by a great and disastrous flood ..."^40

The argument raised by John Keay is that

“... after 2000 B.C. [in the thesis we present, 800 B.C.], according to the archaeologists, came the floods. If they did not actually overwhelm this precocious civilisation, 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 metres [32 feet] and the water table followed it.”^41

Here, then, is the problem: If these various floods of the various rivers on the Indus plain buried Mohenjo-daro beneath 30 or more feet of silt, over the ensuing ca. 2800 years, based on the short chronology, or ca. 3800 years based on the established chronology, the continuing seasonal floods should have raised the level of the flood plain additional dozens of feet above Mohenjo-daro. But this is not the case. Here Shyam Singh Shashi outlines the contradiction and points out the problem inherent in the established chronology:

“These inundations have caused a general rise in the level of the Indus flood plain that has buried the earlier Harappan levels under at least 30 feet of silt. The lowest level explored so far does not necessarily go back to the earliest occupations of Mohenjo-daro, but that does not make any difference to the present argument whether this level is dated 2500 B.C. or somewhat later. The topmost Harappan occupation level coincides approximately with the flood plain level so far as the ‘lower and outlying parts of the city’ are concerned; this is of course the most recent Harappan level, that which corresponds most closely with the end of the culture. Taking the archaeologists’ estimate of approximately 1000 years duration [of Harappan civilization] a very strange fact emerges: during 1000 years duration of the city, 30 feet [9 meters] of silt were deposited; [however,] during the subsequent 3500 years [based on the established chronology, or 2800 years based on the short chronology to the present] no further silt has been deposited [over the city]. It is even possible that there has been some lowering by erosion of the flood plain during this second longer period. Clearly there is an anomaly here that cannot be explained in terms of abnormal floods, however irregular their occurrence.

“For flooding to continue to be a tenable cause of the inundations of Mohenjo-daro, it would be necessary to suppose that the flood regime of the Indus underwent an enormous and extremely sudden change at the end of the Harappan occupation of the site. The flood regime of the Indus, being dependent largely on snowfall and snowmelt in the Himalayas, is for that very reason independent of local climatic variations in the plains; for the snowmelt pattern of the Himalayas to

[^40]: Allchin and Allchin, _op. cit._ (Baltimore MD 1968), p. 143
[^41]: Keay, _op. cit._, p. 5
change which could hardly have been confined merely to the Indian subcontinent and of which there is no conclusive evidence from other parts of the world [in the three volumes of Pillars of the Past, climatic evidence shows conclusive climate changes at the same time]. In any case there is no reason to suppose that a sudden change in the flow regime did occur, for despite man’s control of the Indus by barrages and in other ways in recent years, the river is still every so often liable to large floods. It is relevant to note that the floods, in spite of much evidence of greatly increased soil erosion in the headwaters (and therefore high silt load) do not cause sudden significant changes in the flood plain level.”

If the riverine floods have over time caused Harappan cities to be buried under silt, these same floods over thousands of years after the end of this civilization should have continued to do the same and raised the flood plain even higher than about 30 feet. The case seems to be that there were early small riverine floods but at the end an oceanic wave flowed over and drowned these cities and many of their inhabitants. Because of this problem, Shashi was driven to question the chronology of the length of the Harappan culture:

“One of the premises, on which has been based the estimate of a long occupation of Mohenjo-daro, may therefore be entirely false, and it follows that the estimate itself may be wrong. The city may have been occupied for 1000 years, it may on the other hand have been occupied for as little as a century, or two [or more]. In the latter case the question of [why Harappan civilization remained essentially the same in a 1000 period of] stagnation would not arise.”

In terms of the short chronology, since Harappan civilization began about 1200-1100 B.C. and ended about 400 to 300 years later, one would expect very little if any change and the stagnation problem would also not arise. Shashi has discussed the evidence that “the Indus underwent an enormous and extremely sudden [climatic] change at the end” in terms of snowfall and snowmelt patterns in the Himalayan Mountains. While he holds in opposition to the theses of these three volumes, this climatic change is not in evidence elsewhere; the depth of the contradiction discovered drove him to suggest “A new approach, therefore, should not be hampered by the present belief that geomorphological evidence requires a long duration of [Harappan] culture.”

An excellent indication of when the flood and subsequent climatic change occurred is related to an observation of Alexander the Great’s general Aristobulus. If the Indus civilization was destroyed and abandoned as historians suggest around 1700-1800 B.C., after 1500 years of abandonment the Indus cities of the plain would have fallen not only

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42 Shyam Singh Shashi, Encyclopedia Indica (New Delhi India 2001), pp. 242-243
43 *ibid.*, p. 243
44 *ibid.*, p. 250
into ruin but would have been covered by flood silt as pointed out above. Nevertheless, when Alexander set out to conquer India, his general, according to geographer Strabo, tells us, as cited in Saggs, *Civilization Before Greece and Rome*, p. 12:

“Aristobulus … says that when he was sent upon a certain mission [to India] he saw a country of more than a thousand cities, together with villages, that had been deserted because the Indus had abandoned its proper bed.”

Saggs concludes on the same page that “what Aristobulus must have seen was the remains of an Indus civilization which flourished in the third millennium B.C. …” These cities, towns and villages would have been covered by both flood silts and/or windblown desert sands in a few hundred or so years, but Aristobulus saw them still standing around 330 B.C., which means they had been abandoned not 1500 years earlier but only 400-450 years earlier. Cities in deserts, where sandstorms occur and where a major river floods the land, could have been buried beneath sand and silt over a millennium and a half. They do not survive to be seen.

By moving Harappan chronology by over 1300 or so years closer to the present, one can see that this civilization existed for a far shorter time than presently believed and was subject to climate change, a massive oceanic flood and even earthquakes at its end.

Related to this is the other obvious problem regarding chronology. Certain historians do accept the conclusion that there was a massive oceanic inundation that swept up the Persian Gulf to flood the southern Mesopotamian plain and covered an area from between 30,000 to 40,000 square miles. However, they cannot connect this flood with the massive deluge that had to also sweep up from the Indian Ocean along the same southern shore of the nearby Indus civilization creating comparable devastating results. When a large oceanic wall of water of this immensity occurs in one region covering an immense area, that flood would also be great enough to affect a comparable low-lying plain along the same coast. And to a certain extent historians still cannot settle on a timetable for the growth and end of Harappan society. As Shashi among others states:

“One of the most enigmatic whodunits of antiquity concerns the decline of the Indus Valley (Harappan) civilization. Remains of this vast civilization of South Asia are scattered over an area larger than either ancient Egypt or Mesopotamia …

“It is now apparent that a re-evaluation is necessary of some of the earlier theories that have come to form over the past thirty years … It is especially necessary to call for a retrial concerning the placing of guilt for the demise of the Indus civilization …

“It is still premature to talk in terms of absolute dates—the entire chronology of South Asia down to the 6th century BC is a web of pluses and minuses of hundreds of years.”

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45 *ibid.*, p. 259
Shashi goes on to say:

“The details of the story of the decline and fall of the Indus civilization are, as yet, far from clear, but a pattern of contributing factors is taking shape. This pattern does not include invasion and massacre [by Aryan warriors] as basic factors. On the contrary, it appears that a series of natural disasters occurred—possibly as swiftly, certainly more devastating than any hypothetical invasion. A sudden rise in the Arabian Sea took place … This resulted in a disastrous increase in the already serious floods in the major river valleys ... The economy [thereafter] must have decayed rapidly. There is now incontrovertible archaeological evidence that the major population shift was to the southeast.”

Along these same lines, Georg Feuerstein, Subhash Kak, and David Frawley put the case for a catastrophe in no uncertain terms:

“A growing number of geologists converge on the following explanation [for the demise of the Indus civilization] … over a comparatively short period of time, major tectonic shifts occurred possibly accompanied by volcanic eruptions, which drastically altered the flow of rivers. Prior to its final demise, the Sarasvati River [on the Indus plain] had shifted course at least four times, gradually turning the region around it into inhospitable desert. Such geological changes are endemic to northern India and result from the pressure of the Indian plate pushing into Asia and raising the Himalayas, which is occurring at this very moment… Here is our own reconstruction of that catastrophic event:

“Around four thousand years ago [2800 years ago in the short chronology], the earth buckled under a large area of northwestern India, as it is still prone to do today. But at that time, the buckling was sufficiently pronounced to have far-reaching consequences. Perhaps the ground under the cities, towns, and villages of the early Indic civilization rose imperceptibly, and the devastating effects became apparent only over a number of years. Perhaps the buckling announced itself locally in a series of devastating earthquakes, followed by long-term geographic and climatic changes.

“We favor the second explanation, because northwestern India is known to be prone to earthquakes. While we have as yet no clear-cut evidence of Harappan sites having been destroyed by earthquakes, a number of sites were destroyed or damaged by floods, and many others abandoned because of changing river courses. The event would have had to take place rather quickly to allow for sites to be abandoned altogether …”

These researchers further offer the following “possible scenario”:

“We can envision tidal waves [from the Indian Ocean] randomly flooding large areas, and broken dams and walls of water bearing down on unsuspecting

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46 ibid., p. 264
47 Feuerstein, Kak and Frawley, op. cit., pp. 91-92
townsfolk and villagers, sweeping them miles away from their homes, together with panicky livestock, uprooted trees, boats torn loose from their anchor points, and the debris of houses. ... “The cataclysm left behind desiccated river beds, empty canals, parched soil and, finally, abandoned towns and villages. Gradually, as trees and crops died for lack of water, wind-blown sand filled the gaping river beds ...”

Although Shashi, Feuerstein, Kak, and Frawley hold as much as possible to the established chronology and the uniformitarian or gradualist interpretation of the evidence, it is clear that the catastrophist picture of this event is slowly dawning on them and others. The highly heretical concept that this was not a somewhat catastrophic event but a full-blown cataclysm may yet take more decades or even centuries, if ever, to be accepted.

THE EIGHTH CENTURY B.C. FLOOD IN CHINA

As with the Mesopotamian flood and the Indus civilization flood destroying or nearly destroying the cultures in these regions, further east along the same southern coastline of Asia, there was also a third leg of this great flood that swept northward and overwhelmed Chinese civilization. And as with those of Mesopotamia and that of the Indus, it too was so immense as to be unique in the annals of Chinese geological history that no other flood in China was reckoned to have occurred subsequently over the next 5000 years comparable to it. Here, too, the flood swept into the great flood plain that encompassed the Yellow and Yangtse Rivers.

Interestingly enough, no historian or archaeologist could cogitate the parallels that were encompassed by all three of these floods. The myopic vision created by the established chronology caused not only historians and archaeologists to separate each of these cataclysmic events from one another chronologically, but the geologists, also imbued with the same established chronology, failed to notice that there were three overwhelming inundations all happening on the southern coastal plain of Asia of such a magnitude that they could not envision that these could have been the end products of one cause that happened at the same time. According to Velikovsky, who dated these events to around 1500-1400 B.C., the Chinese flood was tied to others and to the first catastrophes, not the second:

“In the days of [Chinese emperor] Yahou the event occurred which separated the almost obliterated and very dim past of China from the period that is considered historical: China was overwhelmed by an immense catastrophe ...

“... the entire land was flooded.’

\[48\] ibid., p. 92
“An immense wave ‘that reached the sky’ fell down on the land of China. ‘The water was well up on the high mountains, and the foothills could be seen by all.’…”

“The emperor ordered that all efforts be made to open outlets for the waters that were caught in the valleys between the mountains. For many years the population labored, trying to free the plains and valleys of the waters of the flood by digging channels and draining the fields … all efforts were in vain. The minister who was in charge of this urgent and immense work, Khwan, was sentenced to death because of his failure … only his son Yu succeeded in draining the land. …

“The chronicles of modern China preserve records of one million lives lost in a single overflow of the Yellow River. Another natural catastrophe—the earthquake—also caused great devastation in China at various times: it is estimated that in the year 1556 [A.D.] the quaking earth took 830,000 lives and 3,000,000 in 1662. Was not the catastrophe of the time of Yahou one of the major inundations of rivers, as modern scholars supposed it [as they do for that in ancient Mesopotamia] to have been? But the fact that this catastrophe has been vivid in traditions for thousands of years, while neither the overflow of the Yellow River, when a million people perished, nor the great earthquakes, play a conspicuous part in the recollections of the nation, is an argument against the established interpretation.

“… The overflowing rivers of China subside in a few weeks, and the water does not remain in the plains until the following spring, but flows away, and the ground dry in a few more weeks. The flood of Yahou required draining … and during all this period water covered the lower part of the country.”

Some critics may argue that this flood goes well back into Chinese prehistory to perhaps 2000 B.C. or more, but the dating of the chronology of ancient China is not at all fixed with precision. In terms of astronomy, Jean-Paul Zahn and Magda Stavinschi inform us that the date of “… the oldest [total] solar eclipse recorded in China [is] (709 BC).” Others have sought and accepted other eclipses that were partial. There are others who have dated eclipses based on estimates of how long generations were, or of the precise location and time of these events. But this is not accurately known, and they have even estimated the ΔT—slowing of the Earth rotation back to 2000 B.C. and earlier, when such estimates are inordinately shaky and unknown. Michael Love claims, for instance, that an eclipse occurred “which almost surely corresponds to an eclipse on April 21, 899 B.C.”

Terrien de Lacoperie et al. had long ago pointed out:

49 Immanuel Velikovsky, Worlds in Collision (NY 1950), pp. 101-102
50 Jean-Paul Zahn, Magda Stavinschi, Advances in Solar Research at Eclipses from Ground and from Space (Dordrecht, the Netherlands 2000), p. 8
52 Brian M. Fagan, Snapshots of the Past (Walnut Creek CA 1995), p. 74 ff
“The ancient Chinese, like other civilized nations of antiquity, had no regular system of chronology. Events were dated from the accession of the ruling sovereign, and longer periods were calculated by the addition of intervening reigns. With this want of exactness, discrepancies could not fail to arise, and the traditions disagree until 841 B.C., when we find [and assume] the various sources of information henceforth agree together. The famous sexagenary [60-year]-cycle, in use for computing days from remote times, was not applied to the years before 104 B.C., when Szema Tsien, the renowned author of the She Ki or ‘Historical Records,’ employed [this cycle not proved to have been used earlier than 104 B.C.] backwards to the said date 841. The common scheme of chronologies, beginning with 2697 B.C. as [the] first year of Hwang ti, was not established before the eleventh century of our era …, and was based on assumed recurrences of regular periods and on other processes of computation foreign to the historical method.

“Several references to chronology in ancient authors … and a certain number of astronomical statements which modern science has been able to verify, enable us to restore some ancient dates on a more trustworthy basis.”

That essentially has been the ongoing method of attempting to establish ancient Chinese chronology well back beyond the 8th century B.C. It has now reached a point where modern Chinese versions of Manethon, Berossus, and Josephus are attempting to give China an ancient pedigree equal to those of Egypt, Mesopotamia, and Israel. As reported by Peter Hessler,

“In recent years Li [Xuequin] has been director of the Xia-Shang-Zhou Chronology Project. Initiated in 1995, and funded by the central [Chinese] government, the project was designed to establish exact dates for China’s early cultures. Previously, the earliest date in Chinese history for which there was ample archaeological and textual [but not scientific] evidence was 842 B.C.; the Chronology Project came up with a new timetable. Internationally the project has been heavily criticized—many foreign scholars believe that the Chinese are attempting to fortify their history in ways that are more nationalistic than academic. Some say that the project was motivated primarily by a sense of competition with the West, which has earlier recorded dates for cultures such as ancient Egypt. During the Chronology Project, academic differences about ancient dates were sometimes resolved by voting—Chinese scholars gave their opinions, and the year with the most votes won. [As reported in the newspaper:]

“CHINA DAILY (December 16, 1998).–

“A PROJECT TO BRIDGE gaps in China’s ancient history had made remarkable progress after two years of research. China is world-famous for its 5,000-year-history as a civilized nation. Unfortunately, a 2,000-year gap in

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China’s development has concealed the country’s true ... age ... The missing 2,000 years include the Xia, Sheng, and Zhou dynasties and the time before that, dating back to well before 2100 B.C., says Li Xuequin, history researcher with the Chinese Academy of Social Sciences in Beijing ...

“The exact time marking the beginning of China’s ancient history will be published late next year, said Li.”

It should be noted that the approximate 1800 years which the short chronology removes from ancient Near Eastern history is matched by a 2000-year length for China. Thus, we contend that the unique size and duration of the floods for southern Mesopotamia, Harappan civilization, and ancient China were caused by one and the same catastrophic event, ca. 800 B.C., which tilted the Earth’s axis. This, we maintain, was followed by a final climatic change experienced across the globe and discussed in these three volumes and in The Extinction of the Mammoths.

Since we posit a pole shift at this time, there should naturally occur a change in the calendar and the various ancient cultures should have undertaken to reorganize their calendars at that time. Velikovsky sums up the chapter in Worlds in Collision that shows these ancient civilizations all amended their calendars and cardinal (east-west and north-south) points, as well as adjusting their seasons after that 800 B.C. date.

“I am very proud of these chapters of mine toward the end of Worlds in Collision, because I succeeded to quote from practically every ancient civilization from Peru, to Mexico, to Rome, to Greece, to Babylonia, to Assyria, to Persia, to India, to China, to Japan and to Egypt ... always a quotation not by myself, always a specialist expressing the same wonder ... at the beginning of the eighth century [B.C.].

“Soon after that time, in all places, in all civilization[s] one or another [calendrical] reform was done ... The reform was [carried out] almost simultaneously—at least during one and the same [8th] century.”

Velikovsky was discussing the addition of 5 or 5 ¼ days to these calendars, but there were other changes he described as well, related to the north-south, east-west positions and lengths and times of the spring, summer, fall, and winter seasons.

In terms of the floods we find that in each instance, in southern Mesopotamia, the Harappan and Chinese civilizations a flood was experienced that was so overwhelming that it never happened again for supposedly the next 5000 years, based on the established chronology, or 2800 based on the short chronology. If these were indeed local events caused by swollen rivers flowing into the adjacent countryside, then at least one of these regions would have experienced a comparable flood. But in all three cases, no such comparable event happened. We

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55 Peter Hessler, Oracle Bones: A Journey between China’s Past and Present (NY 2006), p. 387
56 Velikovsky, KRONOS vol. XI, pp. 67-68
maintain that the evidence is too strong to be ignored and passed over on the basis of a deeply flawed chronology.

In the next unit we will take up the flooding of Greece—the Deucalion flood and the flood that overwhelmed Ugarit. We further suggest that there may be other evidence of floods in the ancient world for this time of which evidence has as yet not been discovered. Perhaps some geologists who come across this evidence will bring this new evidence to the light of day. It is hoped that one day this negative view that rejects a major flood catastrophe in the eighth century B.C. will cease to prevail and historians, archaeologists, and geologists will say, as did Sir Leonard Woolley’s wife:

“… by the time I had written up my notes [I] was quite convinced of what it all meant, but I wanted to see whether others would come to the same conclusion. So I brought up two of my staff and, after pointing out the facts, asked for their explanations. They did not know what to say. My wife came along and looked and was asked the same question, and she turned away remarking casually, “Well, of course, it’s the Flood [of Noah].”

EARTHQUAKES, FLOODS, CLIMATE, AND CHRONOLOGY

In our discussion of the Harappan flood, we cited Feuerstein, Kak, and Frawley that there had been great earthquakes that accompanied or followed the deluge, though they feel there is “as yet no clear-cut evidence … of earthquakes.” The axis of the earth does not change its position in space without creating immense stresses in the crust of the Earth which will be released over centuries by earthquakes. Velikovsky understood this phenomenon and devoted portions of his book Worlds in Collision to this geological reaction to a pole shift, which caused

“… a major shock [that] convulsed the lithosphere, and the area of the earthquake was [over] the entire globe.”

Fault lines exist all across the Near East and the stresses of this pole shift would, at the beginning, have leveled whole towns and cities, with after shocks doing the same, especially in the first century after the initial event. In this respect, Velikovsky maintained that this second catastrophe did not totally destroy everything but mankind, digging out of the rubble, began to rebuild, or moved to other sites, perhaps believing the original town or city was cursed by the gods. Some sites were totally overwhelmed, others not so much, and civilization continued on after this pulse of destruction, which went on to strongly affect various centers for about a century as the stresses in the Earth dissipated. And all

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57 Woolley, Excavations . . , op.cit., p. 27
58 Velikovsky, Worlds ..., op.cit., p. 62
this, in terms of the short chronology, occurred during the 8th century B.C., correlating with the floods described above.

What must be pointed out is that the level and range of these earthquakes, as with the great floods of Mesopotamia, the Harappan and Chinese civilizations, should have been of a magnitude that cannot be explained by uniformitarian action, and thus in the annals of history, so many towns, cities, etc., over a vast region should have been so violently affected that the like of this earthquake swarm/storm was never to be repeated again in the thousands of years that followed. Yes, indeed, one can point to earthquakes in the Middle East that were highly destructive, but we suggest the numbers we will encounter in what we are about to present have no comparable equal nor extent. We will begin our excursion which covers what Velikovsky called “The Ruins of the East,” with Greece during Minoan and Mycenaean times, which we have already scientifically dated to the 8th century B.C. Of this destruction, which Velikovsky attributes to the 15th century B.C., he states:

“All the great periods in Minoan Crete terminated in natural catastrophes. The monumental work of Sir Arthur Evans, The Palace of Minos at Knossos, furnishes abundant evidence of the physical nature of the destructive agent that brought to a close the … Minoan culture … He speaks of a ‘great catastrophe that took place toward the close of Middle Minoan II. ‘A great destruction’ befell Knossos on the northern shore of the island and [the city of] Phaestos on its southern shore.’ The isle lay prostrate, overwhelmed by the elements.

“When, finally, the survivors or their descendants began the work of restoration, their labor was destroyed again in an ‘overthrow.’ Barely half a century passed [based on the established chronology] between these two catastrophes …

“In the later phase of Middle Minoan III the phenomena ‘conclusively point to a seismic cause for the great overthrow that befell the Palace and the surrounding Town.’ ‘Throughout the exposed areas of the building [palace] there is evidence of a great overthrow, burying with it a long succession of deposits …’

“At the end of the next age, Late Minoan I, the existence of the palace of Knossos ‘was cut short by some extraneous cause, though without any such signs of wholesale ruin as seem to have marked its earlier disaster.’ However, Martinatos, director of the Greek Archaeological Service, finds: ‘The catastrophe of Late Minoan I was fatal and general throughout the whole of Crete. It seems certain that it was the most terrible of all which occurred on the island.’ The palace at Knossos was destroyed. ‘The same tragedy befell all the so-called mansions … Whole cities, too, were destroyed … Even sacred caves fell in like the one at Arkalokhori.’ … In this catastrophe Crete received ‘an irreparable blow.’ The only explanation for the upheaval ‘is one of natural causes; a normal earthquake, however, is wholly insufficient to explain so great a disaster.’
“Then came the destruction of Late Minoan II. The sudden catastrophe interrupted all activity; but there are indications also that, though the upheaval was instantaneous, some preparations had been made in an effort to appease the deity for fear of the impending event. Evans writes: ‘It would seem that preparations were on foot for some anointing ceremony … But the initial task was never destined to reach its fulfilment.’ Beneath a covering mass of earth and rubble lies the ‘Room of the Throne’ with alabaster oil vessels. ‘The sudden breaking off of tasks begun—so conspicuous … surely points to an instantaneous cause.’ It was ‘another of those dread shocks that had again caused a break in the Palace history.’ The earthquake was accompanied by fire. The actual overthrow was greatly aggravated by ‘a widespread conflagration,’ and the catastrophe attained ‘special disastrous dimensions owing to a furious wind then blowing.’ …

“After this last catastrophe the palace at Knossos was never again rebuilt.

“From the topography of Knossos and its surroundings it appears that sometime in the past the site of this city was at the head of an inner harbour connected by a channel with a larger harbour the entrance to which was between two headlands to the north. ‘Some tremendous catastrophe had raised that section of the island far above the level which it occupied when the city of Cnossus [Knossos] existed [which displacement is just the action caused by an earthquake].’

“Archaeological work on Crete disclosed vast catastrophes of a physical nature. …

“The island of Crete presents excellent ground for examination of the effect of the great catastrophes of the past on an early civilization. The island was not invaded … so that the effects of a natural disaster cannot be mistaken for destruction by the hand of man.”

Here we have evidence of a geological displacement which “raised that section of the island far above the [sea] level it occupied when the city [of Knossos] existed.”

In terms of the short chronology the climate of Crete and Santorini was a great deal wetter and thus this pole shift requires that the Aegean climate changed sufficiently to cause a notable shift in the type of vegetation that grew in the region, especially trees. There are a number of citations from classical antiquity that indicate that ancient Crete was colder and wetter in the early period of the Minoans as opposed to the present hotter, dryer climate indicative of a pole shift. Theophrastus, for example, in On Winds, reports:

“The Cretans among others say that nowadays the winters are more severe [colder] and more snow falls, adducing as evidence that the mountains were settled in olden times and bore grain and [trees with] fruit as the land [then] was planted and tilled. For there are extensive plains among the mountains of Ida and in the other mountains, none of which are worked now because they do not bear. Whereas … in those days … they were settled … because the rain was generous,

59 Velikovsky, *Earth …, op.cit.*, pp. 188-191
and wintry weather did not often occur. If then what they say is true, the Efesian winds [very dry north-easterly winds that blow between May and September in the Aegean] must be more numerous.”\(^{60}\)

The point that should be stressed is that geography places present-day Crete at a latitude where rainfall is inadequate for forest growth. Angelos Chaniotis explicitly shows that

“The existence of forests in ancient Crete has been often denied by modern scholars, especially geographers (e.g., Dewolf, Postel, and van Effenterre 1963, 32; Wagstaff 1972, 282; Nixon, Moody, and Rackham 1988, 168f; Rackham and Moody 1996, 128-131) as it has been asserted [that forest flourished there] by ancient literary sources.”\(^{61}\)

R.F. Willetts encapsulates the problem thus: “As … remarked Crete [was] once … fertile and prosperous [but is now] of the rockiest and most barren [nature].”\(^{62}\) It is generally held by scholars that human behavior denuded the land and that wild goats, common on Crete, ate whatever small seedling trees grew, creating a now rocky, barren landscape, as Vasilios P. Papanastasis et al. show:

“The most notable effects have been on vegetation. It is widely believed that Crete was covered by forests in the Neolithic period, which were subsequently destroyed by humans and their activities during the Minoan period … as well as in later periods.”\(^{63}\)

A.T. Grove and Oliver Rackham, in response to this general viewpoint, argue:

“A sure route to pseudo-history lies in ignoring the [ecological] behavior of plants and animals. Historians gather ancient allusions to people cutting down trees, and assume that these add up to a record of deforestation, as if depleting a forest by cutting down trees were the same as destroying it.”\(^{64}\)

Grove and Rackham later go on to explain and outline the ecological reasons and evidence that contradict the long-held concept that ancient man destroyed the forests of Crete in Minoan and later times; they also supply some of the scientific evidence that upholds the fact that in Minoan times forests flourished there:

“There are two prehistoric pollen profiles in Crete. Ayia Galini—a hot dry area near the south coast—had deciduous oak, hazel, alder, elm, and lime. [The other


\(^{61}\) Angelos Chaniotis, *From Minoan Farmers to Roman Traders* (Stuttgart, Germany 1999), pp. 207-8


\(^{63}\) Vasilios P. Papanastasis et al., “Land-Use Changes and Landscape Dynamics in Western Crete,” *Recent Dynamics of the Mediterranean Vegetation and Landscape*, Stefano Mazzoleni et al., eds. (Chichester UK 2004), p. 81

\(^{64}\) Grove and Rackham, *op.cit.*, p. 18
site,] Tersana, on the NW coast, also now a dry site, had oaks, lime, hazel, and *ostrya*. Hazel, alder, and lime are now extinct in Crete and rare in most of Greece [to the north]. The southern limits of vegetation, in effect, moved at least 500 km [310 miles or 4.5 degrees latitude] northward since the early Holocene [10,000 years before present] …

“Why did these trees become extinct [on Crete] …

“The disappearance of north European trees [from southern Europe, Crete, etc.] is … explained by a change to a hotter, drier climate. Explanations in terms of human activities, however, need to be considered [in terms of tree ecology]. Woodcutting could not exterminate them, for they all tolerate it well [elsewhere] and are extensively coppiced further north [where the same woodcutting was going on]. If they had been grubbed out because they grew on cultivable land, they would survive as field trees, in ravines, in ancient woodlands … and on rugged ground along with cultivation, as deciduous oaks do. …

“What about browsing [by goats and other animals]? These trees are palatable and browsing is partly responsible for the decline of lime [trees] in England. However, if [browsing] exterminated them in Mediterranean Europe, they would survive on cliffs, they survive on especially cold cliffs [where neither goats nor other herbivores can go].

“The [earlier tree] vegetation, therefore, unequivocally implies a long period of less arid climate [as in Africa, Arabia, Mesopotamia, and the Harappan civilization, as described in these three volumes of *Pillars of the Past*]. To judge by the plants’ behavior, it was wetter and less seasonal than now [that is, the seasonal temperatures varied less and rainfall fell more regularly throughout the year] …

“When was the change to something like the present climate? Pollen evidence indicates a gradual change—the aridization—over the Neolithic and Bronze Age, in Crete lime and hazel diminish over a long period, finally disappearing from the pollen record after 2500 B.C. …

“In Santorini the change was apparently still incomplete by the seventh century B.C.”

These researchers make it clear that the trees that grew on Crete presently grow at least 500 km/310 miles/ca. 4.5 degrees latitude farther north, which implies some of these trees only grow even farther north, perhaps as much as 5 to 7 degrees. This is very much like we found to be the case for the Sahara Desert.

Why Crete, in geological time scales, became arid supposedly 2500 B.C. while Santorini 90 miles/150 km away lost its forest hundreds of years later, in terms of the established chronology, is not given a rational scientific explanation.

The fact of the matter is that neither of these regions in the Aegean lost their forests as early as is presented. The building materials employed to construct these
cities and used right until the time they were destroyed contradicts the established chronology. For example Walter Burkert describes “A miniature fresco from Knossos [which] shows a large crowd of people beside a group of trees…” It is assumed that no such trees grew on Crete and Santorini in large numbers.

However, there is undeniable evidence for the use of wood, particularly great beams in the construction of Minoan cities and towns, which indicates lumber was truly plentiful right up until the times these cities and towns were destroyed. Thereafter the climate/pole shift created an environment in which forests could no longer grow. Russell Meiggs in his monograph *Trees and Timber in the Ancient Mediterranean World* (Oxford UK 1982) has voluminously shown that forests were abundant in the Aegean and elsewhere. Roger Sands has nicely summarized Meiggs’s work on Crete:

“Crete was well forested prior to the Minoan civilization … and was very well developed by 1700 BC. Indeed the presence of these forests was a major factor in the development of Minoan civilization. Cypress was prominent in the forest but fir, pine, oak, olive, ash, maple, black mulberry, tamarisk and possibly cedar were also found … The Minoans had a passion for building elaborate palaces and wood was widely used in construction. Wood was used for columns, beams, doors, windows, and furniture and also inside mud-brick walls. Minoans had a very good knowledge of the different properties and uses of wood from different species. Minoan Crete became a major nautical power and an important trading post and much wood was used in shipbuilding. Most wood, however, was used as fuel in making bronze, in cooking, making sacrifices, and in preparation of lime [plaster] for the walls of the palaces. Because of the ready supply of wood, Minoan Crete was probably an exporter of both bronze and timber to the Near East.

“Eventually deforestation of Crete caused timber to become scarce. Evidence for this was in the reduced use of timber in construction and its replacement with gypsum. There was a greater amount of recycling of bronze. Relatively more charcoal and less wood was used as fuel. Minoan Crete changed from being an exporter of wood to being an importer … Timber became more and more difficult to obtain, and shipbuilding was reduced …”

As for trees on Santorini, whose fall we have dated to shortly after the fall of Minoan Crete, the evidence shows that it, too, was experiencing full aridity and had lost nearly all its trees:

“Santorini is the half submerged caldera of a great volcano … whose explosion in the seventeenth century BC failed to destroy Minoan Crete [by a tidal wave], and probably did not alter the world’s weather …”

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“Rainfall [presently] is about 380 mm [1 foot] a year, but additional moisture (and reduced evaporation) come from fog, even in summer. Soils consist mostly of volcanic ground glass, afford poor water retention but excellent root penetration.

“Santorini [today] is virtually treeless. Oaks, junipers, and even the drought-defying wild pear are absent, lentisk only just occurs. Introduced pine and cypress barely survive, olives usually die …

“Excavations have shown that the island was desert-like at the time of the ‘Minoan’ eruption. It [then] grew olive as well as vines and there are reports of tamarisk and an oak tree; we have seen the remains of large tree-roots under soils buried by the last eruption. [These s]oils were better developed … [Since t]he island was little if any higher before the eruption than now, it would therefore not have attracted more rain. We are inclined to attribute the change in vegetation to the tail-end of the Aridization.”

Rather than use the term climate shift to deal with the fact that trees that had once grown in the Aegean and in the rest of the Mediterranean basin, just as they did in the Sahara, the more neutral, less catastrophic term “aridization” is employed. But we are never told why this aridization that was permanent occurred. Climatologists and other researchers claim it just happened and the climate across the vast region of the Sahara, Mesopotamia, the Aegean, etc., changed from one that had plentiful rainfall which permitted forests to grow to one that caused nearly all the trees to die off: While some scientists have suggested that for the Sahara to bring about this dramatic effect, there was a pole shift. The researchers of the Aegean islands know that they, too, experienced a similarly dramatic climate shift but ignore even the concept of a pole shift for this region.

Nevertheless, in both cases certain trees died off and survived only in regions well to the north.

As far as change in climate and vegetation in Mycenaean mainland Greece is concerned, A.J. Toynbee reports:

“At this time Mycenaean Greece was well forested on the hills altogether. The plains had been deforested to some extent by Neolithic and early Bronze Age agriculture. Oaks were prevalent on the plains, pines along the coast, full of rich soil; and her mountains were heavily afforested—a fact of which there are still visible traces. There are mountains in Attica which can now keep nothing but bees, but which were clothed, not so very long ago, with fine trees producing timber suitable for roofing the largest buildings; the roofs hewn from the timber are still in existence. There were also many lofty cultivated trees, while the country produced boundless pasture for cattle. The annual supply of rainfall was not lost, as it is at present, though [now] being allowed to flow over the denuded

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68 Grove and Rackham, *op.cit.*, pp. 320-321
surface into the sea, but was [then] received by the country, in all its abundance, into her bosom, where she stored it in her impervious potters’ earth and so was able to discharge the drainage of the heights into the hollows in the form of springs and rivers with an abundant volume and a wide territorial distribution. The shrines that survive to the present day on the site of extinct water supplies are evidence for the correctness of my present hypothesis.”

All these changes Critias witnessed with his own eyes. He had no reason to prevaricate the evidence. No political or economic phenomenon was forcing him to misrepresent what his eyes plainly saw. And this climate change had to have occurred no more than a century or so before him which places it in the 8th century B.C. Are we to believe Theophrastus and Critias were so bereft of their wits that they both decided to make this entire climate concept up? No one has, so far as I have read, explained what was to be gained by these eminent Greeks who supposedly invented this scenario that is fully complemented by the evidence of the Sahara, and directly meant that Crete and Greece experienced a major climate/pole shift in the 8th century B.C.

Yet there is further proof that rainfall was common on Crete and Akrotiri on Santorini, as described in volume II of this series, pages 424-5, namely that these cities had extensive drainage systems that were employed to bring water to people through terra-cotta pipes and carry away waste water. Also, there were drains attached to carry off rain in the streets. These people with their water drainage innovations in Europe, according to Ernest Zebrowski whom we cited on page 424, “had running water a thousand years before any other city we know of.”

When we add to this all the scientific and technological, as well as linguistic and other evidence presented for placing Minoan and Mycenaean civilizations well inside the first millennium B.C., the evidence all corroborates and converges to prove these regions were covered by forest growth up to and somewhat beyond 750 B.C., just as the ancient Greeks claimed.

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69 Critias cited in A.J. Toynbee, *Greek Historical Thought from Homer to the age of Heraclius* (Boston MA 1950), pp. 169-170
THE EARTHQUAKE EVIDENCE

But let us return to the numerous other earthquakes that swept through the Near East, Anatolia, the Aegean, Greece, etc, at this period. Velikovsky cited Claude F.A. Schaeffer regarding these destructions which we date to the 8th century B.C.:

“The Ruins of the East

“In the ruins of excavated sites throughout all lands of the ancient East signs are seen of great destruction that only nature could have inflicted. Claude Schaeffer, in his great recent work, discerned six separate upheavals. All of these catastrophes of earthquake and fire were of such encompassing extent that Asia Minor, Mesopotamia, the Caucasus, the Iranian plateau, Syria, Palestine, Cyprus, and Egypt were simultaneously overwhelmed. …

“The enumerated countries were the subject of Schaeffer’s detailed inquiry; and recognizing the magnitude of the catastrophes that have no parallels in modern annals or in the concepts of seismology, he became convinced that these countries, the ancient sites of which he studied, represent only a fraction of the area that was gripped by the shocks.

“… It spread ruin from Troy to the valley of the Nile. … Laid waste were cities of Anatolia like Alaça Hüyük, Tarsus, Alisar; and those of Syria, like Ugarit, Byblos, Chagar Bazar, Tell Brak, Tepe Gawra; and of Palestine, like Beth-Shan and Ai; and of Persia, and of the Caucasus. Destroyed were the civilizations of Mesopotamia and Cyprus … . In all cities walls were thrown from their foundations, and the population markedly decreased. ‘It was an all encompassing catastrophe. … The initial and real causes must be looked for in some cataclysm over which man had no control.’ It was sudden and simultaneous in all places investigated.”

Velikovsky goes on to ask:

“What was the nature of the perturbations that … changed the entire aspect of the known world from Europe to Asia and Africa? Fire raged, lava flowed, tremors traveled across whole continents, and climate went through revolutions. Schaeffer wondered at the vast extent of the earthquakes, unknown in modern annals. He asked: ‘Could it be that in earlier times earthquakes were of very much greater force and wider spread than they are now because geological strata, originally out of equilibrium, were settling with the passing of time?’ This explanation of the readjustment of geological strata as time goes on is not valid if we keep in mind that geology ascribes to this planet three billion years of existence, and three thousand years is just one millionth of this period. The earth would have adjusted its strata long before, in the geological ages. Apparently the earth was thrown out of equilibrium only a few thousand years ago, which also explains the change in climate simultaneous with the upheaval.

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70 Velikovsky, *Earth . . ., op.cit.*, 194-195
“Schaeffer’s investigation reaches Persia in the East; inquiring in lands beyond Persia, we find that a rich Indus Valley civilization, with many fortified cities, came to a sudden end … the facts brought forth by Mortimer Wheeler strongly suggest to various scholars that a natural catastrophe engulfed the area in those early Vedic times. In its wake the Aryans came into the country; a Vedic Dark Age ensued, and on the ashes of the effaced world Aryans, step by step, built a new civilization.”  

In the first chapter of his book we stated that catastrophism is growing and will continue to grow in terms of Velikovskian concepts and that is just what is happening with the concept that great earthquakes devastated the ancient world. Amos Nur, professor of Earth Sciences and professor of geophysics at Stanford University and Dawn Burgess, a PhD in geophysics from Stanford have undertaken in their recent book to defend Schaeffer’s work, based on scientific grounds. Although they still adhere to the established chronology, their book and its evidence is a welcome contribution to historical catastrophism. Nur and Burgess explain the reaction to Schaeffer’s theses, citing E.B. French who wrote in a short paper in 1996 “Archaeologists of my generation … who attended university in the immediate aftermath of Schaeffer’s great work, were brought up to view earthquakes, like religion, as an explanation of archaeological phenomena to be avoided if at all possible.”

George M.A. Hanfmann in a review of Schaeffer’s work argues:

“… nowhere, as far as the written records go, does there seem to be evidence of earthquakes as far reaching and as totally destructive as those postulated by Schaeffer. We may note here that Schaeffer has not attempted to bolster his chronological scheme by a consideration of the dated earthquakes mentioned in the literary sources, with the exception of the reference in the El Amarna tablets which he interprets as indicating an earthquake in Ugarit in 1365 B.C. But even if such earthquakes had occurred at Ugarit, their effects would have been serious only in the immediate vicinity of that site. Even the strongest earthquake (‘8’ in the ‘absolute’ or ‘magnitude’ scale) can devastate cities only within a fairly narrow radius from its epicenter (ca. 50-60 miles [80-96 km]. It is true that earthquakes of ‘intermediate depth’ may be felt over a rather extensive area, but large-scale destruction of the kind that collapses walls would again be limited to a relatively small area. One might argue that the geological situation was different in the ancient Near East; but modern geologists say that it was not and that there is no reason to believe that seismic intensity was in any substantial degree greater then than now.”

71 ibid., p. 199
On the other hand, Siro Igino Trevisanato points out with respect to Thera/Santorini that Ian Wilson in *The Exodus Enigma* (1985) showed that

“... from July 1925 to June 1926 Santorini erupted with a Volcanic Explosivity Index under 4, over 1000 times weaker than the one in the Bronze Age. Yet even this weaker event was accompanied by earthquakes powerful enough to damage buildings throughout the Aegean. Iraklion on the northern coast of Crete was damaged. So were the island of Rhodes (225 kilometers/140 miles away), the neighboring islands of Karpathos and Castellorizo, as well as the Aegean coast of Turkey. The earthquake was also felt [but did not destroy buildings] outside of the Aegean area and was recorded in the city of Tripoli in Libya, in Damascus in Syria, in Jerusalem, and last but not least, in northern Egypt.”

What must now be pointed out is that the Thera eruption that was supposedly 1000 times stronger than the 1925-1926 eruption somehow failed to completely demolish walls at the edge of the caldera along with many walls in the town of Aktrotiri, one mile from the caldera, as shown in vol. II, pages 358-394 and 416-8. The view that that earlier eruption could have wreaked damage over thousands of miles away by earthquakes is unsupported by the evidence at Thera. However, it is clear that a pole shift would generate immense stresses in the crust of the Earth to create earthquake swarms for well up to a century afterward.

Here we have encapsulated a thoroughly uniformitarian [gradualist] interpretation and analysis of the evidence presented by Schaeffer. Since these numerous earthquakes do not occur and have not occurred between the present going back to that ancient time, *ergo* they could not and did not happen in that past era. Mark Rose, an apparently dyed-in-the-wool uniformitarian, Managing Editor of *Archaeology*, a publication of the Archaeological Institute of America, has turned, in the style of the old-guard critics of Velikovsky, to ridicule him and Nur/Burgess in an Internet article “Godzilla’s [sic] Attacking Babylon!” He admits volcanic eruptions and hurricanes occurred but draws the line at vast, far-reaching earthquakes across the ancient world. He claims, using uniformitarian logic:

“For all this talk of catastrophes, there may be no need for alarm. Or, at least no need to worry too much about it. On the morning of Sunday, November 1, 1755, residents of Lisbon set food on to cook, then when [sic] to mass. Offshore, there was a strong earthquake, felt from Scotland to Jamaica, which generated a tsunami that smashed the city. Those parts of Lisbon not inundated by the wave burned as kitchens [sic] fires ignited the collapsed buildings. An estimated 30,000 to 40,000 were killed, including those who perished from disease afterward.”

Robert Drews gives a typically uniformitarian explanation as to why earthquakes are not an acceptable thesis for these ancient destructions:

"The Catastrophe—which is to say, the destruction of palaces and cities all over the eastern Mediterranean ca. 1200 B.C.—is not invariably attributed to human agency. Although most scholars do hold that the cities were destroyed by men, a minority has explained the Catastrophe (or at least the most conspicuous instances of it) as the result of a terrible ‘act of God.’ Specifically, six archaeologists [C.F.A. Schaeffer for Ugarit and the entire Near East and beyond, Sir Arthur Evans for Knossos and Crete, Carl Blegen for Troy, Spyridon Ikovides for Mycenae, and Klaus Kilian for Tiryns] concluded that their Bronze Age sites were destroyed by earthquakes. In addition, two of the archaeologists have claimed that the quakes that destroyed their sites were also responsible for the fires that burned many other famous sites. In this view, the Catastrophe was an ‘act of God’ of proportions unparalleled in all of history."

But such a catastrophe is an isolated event according to certain authors and had never happened so closely in time all across the Near East. On the other hand, Nur and Burgess write:

"The reaction to Schaeffer’s hypothesis was immediate and sharp criticism, bordering on ridicule, and, in the end, his ‘great work’ was somewhat of a blow to his career. The main arguments against his hypothesis were that the earthquake required for such a catastrophe was un acceptably large, and that there was no evidence that such large events could happen in that region. Persistent ridicule from his peers (e.g., Hanfmann 1951, 1952; Ambraseys 1971; and Rapp 1986, 371, 37) greatly diminished Schaeffer’s reputation, and his example served as a caution to his colleagues and successors ...

"Fear of ridicule is a great shaper of scientific orthodoxy, and Schaeffer probably [like Velikovsky] suffered a great deal because of the scorn he faced. By the end of this book [Apocalypse], however, it will become clear that … Schaeffer’s proposal … was not at all a ridiculous idea, at least not for geological reasons."

As for the fall of these civilizations, especially that of the Mycenaean, there have been several theses advanced ranging from collapse because of social unrest, disruption of trade, a Dorian invasion from the north, the invasion of the Sea Peoples, etc. The major argument advanced to countervail Schaeffer and Velikovsky’s earthquake hypothesis was advanced by Robert Drews, quoted above. He adheres to the Sea Peoples as the destroyers of the western Near East, like the Huns who destroyed the Roman Empire and were responsible for destroying and burning these great cities. In this regard, he has provided a clear-cut choice to explain the

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77 Nur and Burgess, op.cit., p. 27
devastation—a uniformitarian invasion as opposed to a vast wave of catastrophic earthquakes. His book published by Princeton University has overall received very positive reviews. Carol G. Thomas of the American Historical Review writes:

“The argument is carefully constructed, solidly grounded in the evidence, and well written. … We have long known that barbarians [Sea Peoples] had a hand in the collapse [of Bronze Age civilizations] but could find no advantage great enough for them to threaten, even destroy, the wealthy, large, and well-defended kingdoms. Drews has put sword and shield in their hands to give them that advantage, returning to human causation after some decades in which scholarship has sought explanation in impersonal [catastrophic] forces. … Drews’s argument will have to be taken seriously.”

Drews agrees that the period of the destruction, which he refers to as “the Catastrophe,” occurred quickly. “Within a period of forty to fifty years at the end of the thirteenth and beginning of the twelfth century almost every significant city or palace in the eastern Mediterranean world was destroyed, many of them never to be occupied again.”

Drews argues as a uniformitarian that such a widespread catastrophe is denied because “[s]ince in plate tectonic theory the pressure generated by the shifting of plates is relatively constant, one must assume that the incidence of catastrophic quakes in the eastern Mediterranean was approximately the same in the thirteenth and twelfth centuries [B.C.] as it has been in better documented time.” By simply assuming that uniformitarian geological processes have always acted at the same rate, of course it is a foregone conclusion that these numerous catastrophic events over a short period would not be generated.

With respect to drought or aridification following these catastrophes, Drews again maintains his uniformitarian stance:

“Although it may be that drought was a precipitating factor for the Catastrophe, whatever role it played is likely to have been too early and limited to warrant the conclusion that the Catastrophe was in any way the ‘result’ of a drought [or drought the result of a pole shift catastrophe]. Despite an intense search, evidence for a radical change in weather patterns over all of the eastern Mediterranean in the late thirteenth and early twelfth century has not yet been found. Arguments have been made that in the period 1400-900 [B.C.] the entire northern hemisphere was hotter and drier than normal … Contrarily, it has been maintained that from a peak of aridity ca. 1500 B.C. the weather became colder and rainier for the next three centuries, culminating ca. 1200 [B.C.] in a ‘little ice age.’ Whichever of these diametrically opposite scenarios is preferred, the

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78 Drews, op.cit., back cover
79 ibid., p. 4
80 ibid.
81 ibid., p. 37
geographical and chronological frames of reference are here so vast [across that part of the world] that they do not help very much.”

Not only does Drews draw upon uniformity to deny a climate shift, he also employs the established chronology to interpret the climate evidence. Since that chronology is in great error, so, too, is the chronology of that climate shift. With these uniformitarian and chronological assumptions given the status of fact, Drews turns to the only thesis that makes uniformitarian and chronological sense to him: “A military explanation seems to provide all that is necessary. Shortly before 1200 [B.C.] barbarian raiders and pirates discovered a way to overcome the military forces on which the eastern kingdoms relied. With that [military] discovery they went out into the world and made their fortunes.”

After discussing the military, chariot warfare aspects of these Bronze Age societies Drews turns to the temple reliefs in Egypt which, as was shown above, depict Greeks and Persians of the first millennium using weaponry of that first millennium time and interprets it for the late second millennium:

“The Medinet Habu reliefs indicate that the weapons of Ramesses’ opponents [the Sea Peoples] were javelins and long swords … Neither the long sword nor the javelin was an invention of the late thirteenth century: a long slashing sword had been available in temperate Europe for centuries … Until shortly before 1200 BC, however, it had never occurred to anyone that infantrymen with such weapons could outmatch chariots [employed at that period]. Once that lesson had been learned, power suddenly shifted from the Great Kingdoms to motley collections of infantry warriors.”

If Drews had sought to find out, as Velikovsky had, where and when these weapons were used and by whom, he might have realized, as did Velikovsky, that instead of dealing with Sea Peoples of unknown origin he was dealing with first millennium Greeks and Persians. Yet how does Drews know this fact? He doesn’t, and frankly admits as much, and goes even further to submit that the foundation of this invasion hypothesis is based on “a guess” to explain the catastrophe:

“Before attempting to demonstrate these generalizations, I must make some apologies. Warfare in the preclassical world is a subject on which we evidently will never know very much. We have some idea what warfare was like in fifth-century Greece [which employed the same weapons seen on the Medinet Habu reliefs] … By extension, we can imagine at least the outlines of battles fought by Archaic Greeks … But beyond ca. 700 [B.C.] questions begin to multiply and about the second millennium [the supposed time of the Sea Peoples’ invasion] we are grossly ignorant … On many questions one can only guess, and since guessing

82 ibid., pp. 78-79
83 ibid., p. 93
84 ibid., p. 97
seems unprofessional, historians do as little of it as possible. The result, however, is that for lack of evidence one of the most important things about the preclassical world is largely ignored. There is good reason to think that the evolution of warfare made and unmade the world of the Late Bronze Age. Even though we cannot be certain about this evolution, and especially about its details, it is time that we begin to guess.”

Now, it is time that we see what the science says about the nature of the catastrophe. Therefore we can compare these two antithetical explanations of these destructive events. We will have to compare and contrast the historical interpretative evidence with that of the science of seismology to determine which is congruent with the physical evidence. It is thus another case where the historical approach to evidence can be tested against the scientific. Here the two theses can be tested by the scientific method. I suspect the interested reader well understands what the science will show. Before we do so, it should be pointed out that Drews’s invasion argument has no evidence to support it in Greece, as Curtis Neil Runnels and Priscilla Murray state:

“Although the Sea Peoples are intriguing protagonists in the drama of Bronze Age catastrophes, intensive research has failed to turn up any evidence that they or Drews’s other mercenaries were active on the Greek mainland, and the hypothesis that outside invaders destroyed the Mycenaean civilization remained unproven.”

What we will now present in case after case is that the seismological scientific evidence is rather clear-cut and conclusive that immense seismic events swept across the ancient Near East, even to the Harappan civilization and probably across the entire globe. In Drews’s Chapter Three, “Earthquakes,” he presents the evidence and reasoning—or more aptly the lack of evidence—which he maintains denies the validity of the earthquake hypothesis. The first criticism is that particularly the Mycenaean cities and others did not experience any displacement of the ground either horizontally or vertically.

“There is, of course, no documentary evidence for a catastrophic earthquake ca. 1200 [B.C.] … [W]hat little documentation we have indicates that the Catastrophe was wrought by men. Nor is there any archaeological evidence that any of our six sites [in Greece and Syria] was at the epicenter of a catastrophic quake. At each site buildings clearly collapsed, upper stories falling to floor level, or single-story houses subsiding into streets. At none of the sites, however, was any displacement of surface levels reported. Vertical displacement, one would suppose, should have resulted in a stratigraphic step, with all the prequake strata (at Troy, for example, from [level] VI down to I) shifted here and there to a point

85 ibid., pp. 97-98
86 Curtis Neil Runnels, Priscilla Murray, Greece Before History (Stanford CA 2003), p. 125
measurably above or below their normal altitude. Horizontal displacement would
have shown up in the lateral jamming of what had once been a continuous surface.

“It appears in fact that the destruction levels at the six problematic sites
[Knossos, Mycenae, Tiryns, Troy, Ugarit, and Midea] are very similar to
destruction levels at sites outside the seismic zone.”

Another point made by Drews related to the assumed lack of skeletons at these
various sites: Falling walls kill and trap people during earthquakes. On this point
Drews claims:

“Still more surprising is the fact that none of the six quakes, presumably the
most severe that those particular sites ever suffered, resulted in casualties. The
absence of skeletons could be explained in a city sacked by intruders. If given any
kind of warning, the occupants of an unwalled place such as Knossos or Pylos
would probably have fled long before the sackers arrived …”

A third argument raised by Drews is that earthquakes would not have generated
massive fires in those ancient days:

“Another surprising fact is that in each of the six sites the killer quake resulted
in a fire or fires that raged over much or all of the site. Over the several millennia
of ancient history thousands of villages, towns, and cities were burned, but in
almost all cases the fires were set deliberately: sackers of cities routinely burned a
place after they had looted it. Earthquakes, on the other hand, were in antiquity not
associated with devastating fires, presumably because there were no gas mains or
electrical cables, and most cities and towns consisted primarily of masonry
structures. In his discussion of earthquakes Pliny does not even mention the
danger of fire. Lamps or cooking fires could of course be disturbed; in describing
the terrible quake that in A.D. 17 shattered twelve cities in the Roman province of
Asia, Tacitus says that because it occurred at night the survivors could see ‘fires
shining out from among the ruins.’ But there is no indication that on this or any
other occasion fire devastated a whole city: of the several hundred ancient
earthquakes that W. Chapelle cataloged from literary sources, none is known to
have ignited a city-wide fire. It therefore strains credulity to suppose that a single
earthquake should have resulted in conflagration at three sites in the Argolid
[plain]–Mycenae, Tiryns, and Midea–and that similar fires should have been set
by this or other quakes at Knossos, Troy, and Ugarit.”

The fourth argument is: “Most amazing of all is that at none of the six sites
claimed to have been destroyed by earthquake ca. 1200 B.C. was anything of value
buried by the collapsing buildings.” Drews’s fifth point is that after the quakes

87 Drews, op.cit., p. 38
88 ibid., p. 39
89 ibid.
90 ibid., p. 40
the few resettlement communities located themselves in the very same earthquake-prone area. Whereas if they had experienced and survived a massive earthquake, they would have settled in some other safe area.\footnote{Bruce Lerro, \textit{From Earth Spirits to Sky Gods} (Boston MA/Lanham MD/Oxford UK 2000), p. 242}

All in all, these five criteria Drews presented would seem to indicate the earthquake thesis can have very little if any standing. The problem with Drews’s presentation is that very little of it is true. Particularly troublesome is his argument with respect to fires. He first claims that “there were no gas mains or electrical cables” to cause fires; then adds “most cities and towns consisted primarily of masonry structures” and then concludes at the same time “sackers of cities routinely burned a place after they looted it.” The contradiction obviously contained in these statements that never seems to have dawned upon Drews is: if these cities and towns “consisted primarily of masonry structures,” and could not catch fire because “there were no gas mains or electrical cables,” how could the “sackers [have] routinely burned [these] place[s] after they looted [them]”? As Nur and Burgess argue, there is a “commonness of ash layers in ancient archaeology [which] argues against this, unless every ravaging army carried its own fuel to spread evenly over every city it sacked.”\footnote{Nur and Burgess, \textit{op.cit.}, p. 139} Furthermore, in terms of climatology, since the climate, as historians claim, was the same as that of today in those parts of the ancient world, where did the sackers get the wood with which they incinerated these cities? Did they \textit{schlep} [a Yiddish word meaning arduously carry] whole forests of wood with them, then spread this material fairly evenly over the cities they sacked, or in only certain places of them, and routinely burn them? This these imaginary sackers never did, but here, then, is a very important piece of the climatology puzzle as to how these cities could have burnt leaving in their wake a layer of ash.

That this condition was common where these earthquakes occurred, is presented throughout the literature. Drews himself reports: “Åström has shown beyond doubt that Midea [on mainland Greece] was destroyed in a conflagration (ash deposits were found everywhere, with an ash layer 40 centimeters [16 inches] thick near the interior of the Cyclopean wall).”\footnote{Drews, \textit{op.cit.}, p. 44} Whence come 16 inches of ash, unless, and only unless, there was a great deal of wood in that area? These thick layers of ash are found all across these regions and will be noted in the following: Regarding Mycenae, Carol G. Thomas and Craig Conant write, “[A]side from these [three small] areas, there is hardly a section of the [Mycenaean] citadel which has not yielded a layer of ash deposited around 1200 BCE.”\footnote{Carol G. Thomas, Craig Conant, \textit{Citadel to City State} (Bloomington IN 1996), p. 16}
With respect to Palestine, Andrew Knowles tells us: “Those who have dug in many of them [Palestine’s cities, towns, and villages] have discovered that many of them were destroyed toward the end of the 13th century B.C. In some places a thick layer of ash is a sign…”95 As for Ugarit in Syria, The Cambridge Ancient History vol. II, pt. 2 (Cambridge UK 1975), p. 14) reports: “The disaster [at Ugarit] was sudden and complete. Fire swept the city covering it with a thick layer of ash.” (emphasis added) Sweeney, with regard to Ugarit, reports: “Schaeffer discovered evidence of a vast conflagration in Ugarit in northern Syria. The Ugarit catastrophe left a layer of what appeared to be calcinated or hardened ash almost four meters [12 feet] in depth.”96

As for Anatolia, Ilhan Aksit admits: “In about 1200 B.C. vast hords [sic] of tribes were migrating into Anatolia … Excavations have revealed a thick layer of ash in very many places …”97 Frank Joseph colorfully describes these conflagrations:

“In Anatolia … every important Bronze Age site and many insignificant ones, including all the important settlements belonging to the … Hittite Empire were utterly destroyed by fire sometime around 1200 BCE … At the ruins of Hattusas, the imperial capital, researchers found copious amounts of ash, charred wood, and slag from mud bricks, melted at some inconceivably high temperature—evidence of an intense fire. Identical evidence was uncovered at nearby Alaca Hoeyuek, where a thick layer of ash covered the entire metropolis. The fortress city of Alishar … had been gutted from end to end by a monstrous fire.

“About sixty miles to the east … Mashat Hoeyuek … went up like a matchstick. Far away to the west, the great city of Milawata … burst into all-consuming flames sometime around 1200 BCE. … Numerous forts, cities, and villages along the upper Euphrates River in eastern Anatolia were burned in what [Robert] Drews called ‘site-wide’ destructions. … The list of settlements great and small is a catalog of fiery exterminations, complete and unsparing.”98

The amounts of wood in these cities, towns, and villages necessary to generate so much ash clearly demands huge supplies, and therefore great forests. Given the near desert-like conditions that exist today in these areas, and assuming there was no climate change subsequent to the heyday of these sites, it would have been literally impossible to create so much ash.

The fact of the matter is that there is clear evidence that the Hittites built their cities using a great deal of wood. Just as with Knossos on Crete, excavations in

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96 Emmet Sweeney, The Genesis of Israel and Egypt (NY 2008), p. 21
97 Ilhan Aksit, Ancient Civilizations of Anatolia and Historical Treasures of Turkey (Ann Arbor MI 1982), p. 40 (emphasis added)
98 Frank Joseph, The Destruction of Atlantis (Rochester VT 2002), pp. 155-156 (emphasis added)
Anatolia found great logs and beams of wood. On this point Seton Lloyd reports that at Arzawa, the Hittite capital,

“The walls were built up to about three feet high in stone and above this came timber framing filled with mud brick. There was also an upper story which must have consisted almost entirely of wood. But in this case the stone sub-structure itself was also reinforced with wooden beams; and even the foundations, as we discovered … consisted of huge logs, laid transversely to the line of the wall itself about three feet beneath the ground. Inside the building there was a forest of columns, as well as door linings, panelling and other fittings, all of wood. We calculated that more than four thousand tree-trunks must have been used in the part of the building which we excavated. Whole mountainsides must have been deforested to provide the timber.”

Mandelkehr shows:

“The site devastation discovered in Anatolia is truly awesome. Mellaart, a key investigator in Anatolia, goes beyond simple scientific reporting and expressed his personal dismay at the destructiveness discovered …

“Excavations in western and southern Anatolia … expose a picture of utmost horror. … No theory of local wars between kingdoms could possibly account for this devastation or desertion of settlements, it was far too widespread, too intense, too violent, and too unexpected’ …[Carl Blegen states].

“The stratum of Troy IIg had an average thickness of more than one meter; it consists mainly of ashes, charred matter and burned debris. This deposit apparently extended uniformly over the great megaron and across the entire site, eloquent evidence that the settlement perished in a vast conflagration from which no building escaped ruin’ …

“Blegen conveys the nature of the unexpected disaster:

“‘In all areas examined … it was obvious that catastrophe struck suddenly without warning, giving the inhabitants little or no time to collect and save their most treasured belongings before they fled. All the houses exposed were still found to contain the fire-scarred wreckages of their furnishings, equipment, and stores of supplies. Almost every building yielded scattered bits of gold ornaments and jewelry, no doubt hastily abandoned in panic flight.’”

Drews, of course, claimed “Earthquakes … were in antiquity not associated with devastating fires, presumably because there were no gas mains or electrical cables.” To this Nur and Burgess offer this direct rebuttal:

“We … know … Drews’s argument does not hold up, as there are many examples of fires caused by earthquakes and other accidents in cities without modern utilities. In Tokyo, in 1927 [A.D.], most of the fires that led to the city’s

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99 Seton Lloyd, Mounds of the Near East (Chicago IL 1963), p. 88
100 Mandelkehr, op. cit., pp. 44-5
near total destruction came not from modern utilities but from [cooking or heating] charcoal fires that ignited straw ... In Lisbon, Portugal, after a devastating earthquake in 1755 [A.D.] ... cooking fires ignited a citywide conflagration that burned for three days ... In 1356 [A.D.], an earthquake in Basel, Switzerland, triggered weeklong fires that destroyed the city. Most of the accounts of the AD 363 earthquake in Jerusalem describe fires that killed many people.”

Drews also argued above that “In his discussion of earthquakes Pliny does not even mention the danger of fire ... of the several hundred earthquakes that W. Chapelle catalogued from literary sources, none is known to have ignited a citywide fire.” Again, Drews’s statement is false as, again, Nur and Burgess point out:

“Further, the destruction from the earthquake of AD 17, which encompassed twelve cities along the Gediz River in Turkey and was described by Pliny [in his Natural History] (1938) as the worst quake in the memory of mankind, was aggravated by the accompanying conflagration (Ambraseys 1971). Clearly, we know that fires were associated with ancient earthquakes, though sources [as Drews claims] that never mention them abound.”

For example, on Crete a town now called Gournia (its original name is unknown) was excavated by Harriet Boyd (later Harriet Boyd Hewes). Ian Wilson describes what she discovered there at that ancient Bronze Age time:

“But, as Harriet Boyd ... came to realize, it was at this very time that a thousand years of previous uninterrupted [peace] ... at Gournia came to a sudden and virtual final halt. Whatever had happened had been so swift that carpenters and copper-smiths had simply dropped their precious tools on the floor never to return to them. There was a special stone mould for casting knives and chisels which the one-time owner had valued so highly that when it broke he rebound it with bronze strips. It lay abandoned. Whoever or whatever had been responsible for the catastrophe, its most obvious accompaniment had been a fire of extraordinary intensity. As Harriet Boyd remarked in her official report on the excavations:

“‘The conflagration which destroyed the town left proof of its strength in many parts of the excavation. Wooden posts and steps were entirely burned away, leaving deposits of charcoal and marks of smoke-grime; bricks were baked bright red. In a ground-floor of the palace lay a large tree-trunk which had supported an upper-floor, completely charred through, but retaining its original shape; the central hall of the palace was choked with such timbers. Limestone was calcinated, steatite was reduced to crumbling fragments ...’

“Thereafter the ruins remained undisturbed until the coming of Harriet Boyd.”

101 Nur and Burgess, *op.cit.*, p. 139
102 ibid.
103 Ian Wilson, *op.cit.*, p. 88
A few miles from Gournia, Richard Seager began excavations on the uninhabited island of Pseira, and unearthed a Minoan harbor town with another at Mochlos 3 miles (5 km) east of Pseira, both of which were destroyed by fire:

“... but as at Gournia and Pseira, their industry had come to a sudden and violent end. ... A typical Mochlos house had in Seager’s words ‘perished in a great conflagration of so violent a character that almost all the objects found in it are badly charred and blackened.’ And while in the previous excavations no human victims had been found, that was not the case here. According to Seager, ‘In a number of places in this house were found human bones badly charred, showing that the destruction was no peaceful one, and that many of the inhabitants perished. The same fact had already been noted in other houses.’”

Wilson continues to show the level of destruction that occurred all across Crete:

“As in the course of this century more and more Minoan sites have come to light in eastern Crete, so an almost identical picture of sudden catastrophe ... has been revealed. In the early 1920s, Greek archaeologist S. Xanthudides uncovered, eight miles east of Heraklion, the once magnificent stone-built villa of Nirou Khani, with flagstoned floors and forty elegantly finished rooms on the ground floor alone. Like every other site, it had been destroyed by fire. At the time disaster struck it was crammed with valuable objects, among them, four superb bronze axes, the biggest so far found, and well over forty tripod-style tables, left neatly stacked against a wall. And herein lay a peculiarity. Like everywhere else on Crete, Nirou Khani had no defences. Therefore, if the place had been seized by raiders, why did not any surviving original inhabitants return at least to salvage items that the raiders had left behind?

“A similar mystery emerged in the 1960s, when Greek archaeologist Professor Nicholas Platon discovered the remains of Crete’s easternmost Minoan palace, with accompanying town, a short distance from the shoreline of the mountain-circled bay called Kato Zakro. Like Nirou Khani, it ended in flames. According to Platon’s account,

“'The fire was certainly of large extent and of great intensity; it reduced everything to ashes. Fed by inflammable material such as the oil in the storerooms, it turned many stones into lime, charred completely all the wooden parts of the palace and the internal timber of the walls, burned the columns, and in some areas transformed by conflagration even the clay pots, some of which were crumbled and distorted or changed into a shapeless mass. ...'

“The town suffered no less severely than the palace, yet as at Nirou Khani precious objects lay undisturbed, among them elephant tusks, bronze ingots and exquisitely made vases in the style of those depicted in the tombs of Tuthmosis III’s viziers. In Platon’s words,
“Most of the valuable objects were still in their original positions. Tools lay where they were being used on that day, and raw materials and unfinished artefacts were found in workshops. The finished products were still in the storerooms. In the kitchens and their annexes there were remnants of food, together with cooking utensils either in use at the time or in storage compartments.”

Wilson adds:

“Like all other Cretan sites of the Late Minoan IB period, Zakro had no walls with which to defend itself against marauders. But the peculiarity lay in why so many sites should have succumbed at the same time, with apparently so little resistance (bodies were found only at Mochlos), and with the hypothetical attackers apparently taking so little advantage of their marauding—failing to loot, and failing to settle in their places of conquest. Whatever had happened was so final that most of the remains were simply left for the earth to cover them. So could something other than human agency have been responsible?

“For Professor Platon such an idea was not mere speculation. As he observed at Kato Zakro, in several places around the palace something very much more powerful than human agency seemed to have been at work:

“… huge stones, some dressed, some not, had been hurled to a distance or had fallen and shattered, blocking passages and filling open spaces. Whole sections of the upper storey had been thrown down, at many points preserving their relative continuity. Sections of the walls of the façade, carefully constructed of dressed stone, fell in a block from their bases in such a manner that the stones were spread out in a series on the floor. … The steps of the stairways had subsided, and in many cases were displaced from their original position. … Many of the pithoi [large storage jars] in the deep storerooms of the West Wing, mainly those arrayed along the west wall, had been compressed and squeezed as if pressure from some great force had been applied from east to west. A similar enormous pressure was evidenced by whole walls of dressed porous stones that had fallen from their foundations in one piece. …”

Donald Kagan summarizes the destruction across Crete thus:

“The catastrophe which overtook the Cretan cities [the] LM [Late Minoan] II at Knossos, was practically universal. Knossos, Phaistos, Agia Triadha, Gournia, Mokhlos, Mallia, and Zakros all show traces of violent destruction accompanied by burning. At Pataikastro, Pseira, Nirou Khani, Tylissos, and Plate there is a distinct break in habitation, though no trace of burning was found.

“This overwhelming disaster must have taken place at one and the same time and it has been attributed to severe earthquake.”

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105 ibid.
106 ibid., pp. 89-90
Of the last moments of Knossos, Kagan writes:

“The final scene takes place in the most dramatic room ever excavated—the Throne Room. It was found in a state of complete confusion. A great oil jar lay overturned in one corner, ritual vessels were in the act of being used when the disaster came.”

The evidence of a major earthquake swarm all across Crete, accompanied by blazing fires is quite clear and direct. But the last point made by Platon that the “enormous pressure was evidenced by whole walls of dressed porous stones that had fallen from their foundations in one piece” brings us to the next criticism raised by Drews who argued above “At each site buildings clearly collapsed … At none of these sites, however, was any displacement of surface levels reported … Horizontal displacement would have shown up in the lateral jamming of what had once been a continuous surface.” In essence, Drews claims that there is no earthquake displacement that shows either vertical or horizontal movements of the ground at these sites, thus refuting the earthquake thesis. This further absolute assertion on Drews’s part is once again completely false, whereas Nur and Burgess show:

“Like fallen columns, collapsed walls—especially those made of stone or mud brick using ancient techniques—are often clear markers of past earthquakes. A single site in any given earthquake generally has one axis where the shaking is the strongest. Walls oriented perpendicularly to this maximum-shaking axis are more likely to collapse in moderate earthquakes, leaving walls that are oriented parallel to the shaking axis intact or less badly damaged. When many similarly oriented walls at a site have fallen in the same direction, particularly when they buried grain, gold, or valuables in their fall, the action of an army is an unlikely cause. We find such damage in many different sites, including some layers at Troy, Jericho, and Mycenae.”

Drews suggests that fire moved walls or the ground subsided, causing the walls to collapse. Again there is clear evidence that this statement is also false. Ione Mylonas Shear describes Panagia [Women’s] Houses at Mycenae and shows the displacements were created by an earthquake and not at all by fire, as Drews contends:

“The scattered pottery and the chimney pot found smashed on the floor of Panagia House I suggests that the house was suddenly and unexpectedly destroyed. The absence of deep layers of ash on the floors of the house indicates that the destruction was not caused by fire. … Confirmation of the earthquake may possibly be seen in the collapsed state of the doorway leading into the house and the condition of the south wall of Room 2 where the preserved portion of that wall

108 ibid., pp. 107-108
109 Nur and Burgess, op.cit., p. 107
was found leaning outward, toward the south. House I was so badly damaged that no attempt was made to rebuild it…

“Although it was with some surprise that this earthquake was first recognized in 1962 when Panagia House I was first uncovered, subsequent work in Panagia House II produced ample evidence for its occurrence. Perhaps the most striking is the state of the party [shared] wall between Rooms 11 and 12 … The south part of the wall rests firmly on its foundations. The north and central sections, however, have buckled and shifted forward off those foundations. The absence of ash on the floor of the [two] rooms again indicates that the destruction, whatever its cause, was not a result of fire.”

As for Tiryns, its excavator Klaus Kilian reported in a paper published posthumously (1996) that

“… the evidence consists of building remains with [vertically] tilted and curved walls and foundations … [A] comparative study of buildings that have been affected by earthquakes in the last 100-200 years [which also exhibit the same vertical and curved displacements] supports our conclusions that the observed deformation of excavated buildings are [sic] of seismic origin.”

Regarding Building VI at Tiryns Kilian reported:

“A high wall was transformed into a mass of rock. … [T]he walls on a terrace and on the other side of the corridor are tilted downhill (westwards) and uphill (eastwards) respectively, that is, in a direction opposite to that of a possible slope-movement. … Such antithetical tilting of nearby walls is not the result of landslides but seismic disturbances.”

At Midea, which is located between Mycenae and Tiryns, the excavator writes that they found “collapsed, distorted, curved walls …”

Summarizing the evidence in Greece, Mandelkehr states:

“Site destruction in Greece was present, analogous to the other areas. Mellaart describes the Early Hellenic settlements on the Greek mainland as ‘ending in a conflagration of catastrophic nature.’ (The Peloponnesus is the southern portion of mainland Greece, comprising approximately one-quarter of the mainland area.) The pattern description is the same as for Anatolia—destruction over many sites, gaps in occupation, shifted sites and rebuilding on a reduced scale. … The following statement is made by Weinberg …:

“…evidence of a great prosperity in the Aegean in the middle phase of the Early Bronze Age … That all this wealth and power ended in a catastrophic destruction is clearly documented at Lerna as it is at most other mainland sites of the EH II period.”

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111 Nur and Burgess, *op.cit.*, p. 158
112 *ibid.*, p. 159
113 *ibid.*
“Finley … describes the abruptness of the event in very clear terms:

“‘The word “break” should be understood in its strongest sense. Archaeological records are filled with changes of all kinds, but not often with anything so massive and abrupt, so widely dispersed, as occurred at this particular time. In Greece, nothing comparable was to happen again until the end of the Bronze Age a thousand years later. Settlements which were, for their time, rich and powerful, and which had had a long history of stability and continuity, literally came tumbling down, and what followed differed unmistakably in scale and quality.’

“Renfrew has stated that the shift in material culture was so pronounced and abrupt in the case of the EH II-III transition that it stood out as ‘more marked than any other subsequently seen in Greek prehistory, or any previously documented since the development of farming life.’ After[ward,] the region lacks the previous monumental architecture, sealing systems, ceramic uniformity, settlement hierarchies and all indications of a sophisticated and complex society. … A team of five archaeologists—Wright, Cherry, Davis, Mantzourani and Sutton—conducting a survey of sites in Greece (the Numea Valley Archaeological Project) has the following to say, referring to the Numea Valley in southern Greece…

“‘The occurrence of EH I and (especially) EH II material, in substantial quantities and at many sites, stands in stark contrast to the rarity of EH III shards and the virtual absence of recognizable MH types.’

“The great center at Lerna serves as a focal point for the destruction in Greece. In the Peloponnesus (southern Greece), the EH II settlement of Lerna III was destroyed … The great House of Tiles at Lerna was found to be burnt and reduced to ruins. The burning of this structure and of Lerna III itself has been described by Caskey as ‘the end of an era both historically and archaeologically.’ He reports that succeeding settlements are clearly separated from the debris of this construction… Vermeule … has the following to say:

“‘After the House of Tiles was burned, it took Greece over half a millennium to approach the same point of civilization again. The islands never did, except where their local arts were revitalized by Crete…’

“Repeating an earlier statement, Weinberg … refers to Lerna as typifying the general destruction:

“‘… evidence of a general prosperity in the Aegean in the middle phase of the Early Bronze Age … That all this wealth and power ended in a catastrophic destruction is clearly documented at Lerna as it is at most other mainland sites of the EH II period.’”

What then is to be made of Drews’s argument regarding this statement that “Still more surprising is the fact that none of the six quakes, presumably the most severe that those particular sites ever suffered, resulted in casualties. The absence

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114 Mandelkehr, *op. cit.*, pp. 50-51
of skeletons could be explained in a city sacked by intruders.” And, shockingly, this statement is again false. Nur and Burgess explain why skeletons beneath rubble are a clear indication that an earthquake caused the buildings to collapse down on the people:

“... [M]any archaeologists are reluctant to invoke earthquakes to explain excavated destruction. Instead, they attribute collapsed walls, uniformly fallen columns, and slipped keystones [from arches] to poor construction, soil creep, and ground water seepage, if not just aging in general. ... Crushed and broken skeletons found under rubble are a different story, however. Many archaeologists accept these as definitive evidence of earthquake destruction.

“The destruction of a massive stone building by human hand takes some time. Given the methods employed in ancient times, people had ample time to escape when an army was hammering at a building; those inside were likely to flee the structure rather than wait for the roof and walls to crash down upon them. An earthquake, on the other hand, gives no warning, and often [not always] there is no time to do anything but cower until the shaking stops. Thus, the discovery of human remains crushed beneath their dwellings creates a strong argument for an earthquake, rather than a militia, as the destructive force.”

There are few provisos related to the finding of skeletons in collapsed ancient ruins. Sometimes the first instances of quakes may not be strong and people may escape. As Jonathan Hall remarks, “earthquakes can often inflict heavy physical damage with comparatively light loss of human life depending upon the season and time of day at which they strike.” If people are away from home, working in the fields or elsewhere, an earthquake will leave few skeletons in their homes.

The second proviso has to do with fires and especially their greatness and the level of heat they produce. Where there are deep layers of ash or slag of calcinated mud brick, which Frank Joseph, above, claimed could only be “melted at some inconceivably high temperatures—evidence of an intense fire,” the bodies would have been incinerated or completely cremated. As Penny Coleman informs us:

“The crematory operator had placed [a] body in a wood coffin, slid it into the cremation chamber, and turned on a fire that could heat up to 2500 degrees [°F]. First, the coffin burned and then the body, which like every human [or animal] body, contained water, carbon-based soft tissues, and bone. The intense heat evaporated the water, burned the soft tissues, and turned the bones [to very small] fragments. The whole process took between two and a half to three hours and yielded somewhere between six and a half to nine pounds of fragments. Today most cremated remains are processed into smaller pieces.”

115 Nur and Burgess, op.cit., p. 141-142
116 Hall, op.cit., p. 54-55
117 Penny Coleman, Corpses, Coffins, and Crypts (NY 1997), pp. 60-1
Charles Ginenthal, *Pillars of the Past*, vol. III

In great conflagrations that generate high temperatures and burn for days, whatever bodies were killed or buried in the rubble and then incinerated would undergo the same cremation process. The great fire-bombing of the German city of Dresden during World War II created just such conditions throughout most of the city. Maria Ritter shows:

“While in the refugee camp, Mother had read a public notice instructing people to inform officials of the whereabouts of any victims under the rubble. She reported the location of the hallway on Wintergartenstrasse 31, mapped out where her mother and sister had been lying. She left them behind [when she left] crouched along the left side of the hallway, next to each other. Despite their efforts [in uncovering this site], no human remains could be found, only ashes and rubble. A mass cremation had taken place.”

With these provisos, let us turn to the discovery of skeletons in ruins dated in terms of the established chronology to ca. 1200 B.C., and in the short chronology to ca. 800-750 B.C. At Tel Dor, supposedly dated to 1100 B.C., we have just the conditions we had at Dresden, and therefore only one female skeleton was recovered.

“Ephraim Stern (1993), who excavated here for several seasons, discovered that one habitation layer of Dor was burned and destroyed ... with a widespread layer of debris and ashes over the entire site:

“‘From this period ..., we found massive evidence of a fierce conflagration that had oxidized the mud bricks and shattered the limestone used in the buildings, leaving great areas of ash and charcoal as much as 6 feet thick. ... The same thick [ash] destruction layer resulting from a violent conflagration appeared on the other (western) side of the mound.’”

Later a skeleton was discovered of a woman which for obvious reasons had not been cremated. Andrew Stewart in 1993 described the find thus:

“This was by no means easy archaeology. The room was small, and made smaller by the low stone screen against which she lay. ... she was both badly contorted and cruelly smashed up. A limestone wall had fallen on her and had crushed her into the earthen floor below. Numerous rocks penetrated the skeleton itself. A scatter of potsherds, stone tools, a bone needle and several small animal bones lay right beneath her, some of them also poking into her body.”

The wall which had fallen on this woman crushed her body into the earthen floor, protecting her, the bone needle, and the small animals bones from the fierce heat of the fire. The possibility that she has been killed by a raiding barbarian army was raised and that the wall later collapsed to crush her was investigated:

118 Maria Ritter, *Return to Dresden* (Jackson MS 2004), pp. 85-6
119 Nur and Burgess, *op.cit.*, p. 146
120 *ibid.*, p. 147 (emphasis added)
“Stewart consulted an expert in bone fracture analysis, Dr Patricia Smith at Hadassah Medical Center in Jerusalem. She found evidence of a particular kind of breakage, a spiral fracture that only occurs in fresh bone. Her analysis indicated sudden and massive crushing of [the] entire body while her bones were still clothed in flesh …” Stewart gave the opinion that the destruction … layer in Dor was caused by an earthquake …”¹²¹

On the other hand, Stern still assumed she had been “a casualty of battle.”¹²² The problem with this interpretation brings us to Drews’s point, above, that hardly “anything of value [was] buried by the collapsing buildings.” Nevertheless, Nur and Burgess report “numerous valuables were found under the rubble, the sort of plunder conquering armies remove rather than bury.”¹²³

At this site we have clear evidence that the city of Dor was not destroyed by an invading army because of the body instantly killed and crushed into the floor, the others having either escaped or been cremated to ash by a fierce conflagration over the entire site, and “numerous valuables … found under the rubble.” Future excavations may turn up minute pieces of bone from other victims in the ashes of Tell Dor, if such could survive a fire that left 6 feet [2 meters] of ash.

As for Greece, Drews’s charge regarding the absence of skeletons is again false. In our discussion, above, of the Panagia Houses of Mycenae we were able to show that there were vertical and horizontal wall displacements in House II, in House I: “The absence of deep layers of ash on the floors of the house indicated an earthquake as Shear discusses this house with little ash but there was “The discovery of a skeleton covered with fallen stones in the doorway of Room 5 … not only confirmed the suddenness of the destruction but suggested the destruction had been caused by an earthquake.”¹²⁴ Beyond this single skeleton, Nur and Burgess tell of “A house destroyed … 200 meters [660 feet] north of the Citadel, the basement was filled with stone rubble, as well as the skeletons of three adults and a child crushed beneath them …”¹²⁵ At Tiryns they cite Kilian who found “skeletons crushed beneath the fallen walls’ … [and i]n 1956 [N.] Verdelis excavated fill near the fortification wall … He discovered ‘two skeletons, evidently not burned but killed and covered by the fallen debris.”¹²⁶

At Midea, not only were the “collapsed, distorted, curved walls” clear signs of an earthquake, but “a skeleton [was] found under collapsed debris,” and “In one of

¹²¹ ibid., pp. 147-148
¹²² ibid., p. 148
¹²³ ibid., p. 149
¹²⁴ Shear, loc.cit.
¹²⁵ Nur and Burgess, op.cit., p. 156
¹²⁶ ibid., p. 158-159
the rooms in the area of East Gate’ … ‘the skeleton of a young girl was found, whose skull and backbone were smashed under fallen stones’.”\textsuperscript{127} The very same evidence of skeletons and displaced walls was also found at the Greek cities of Thebes and Menelaion.\textsuperscript{128}

We have already shown that on Crete at Mochlos Richard Seager “found human bones badly charred.” Before moving on let us briefly examine the flood evidence at Ugarit near the Syrian coast and Tiryns in Greece. If a great underwater seismic event occurred in the Aegean between these cities it would have sent a wall of water from the sea upon these sites. With respect to Ugarit, Amélie Kuhrt informs us that “Ugarit suffered an earthquake and tidal wave, followed by fires which ruined its port and destroyed almost half the city, including part of the palace. But recovery from the [first] disaster seems to have been swift.”\textsuperscript{129}

Sergei Leonidovich Soloviev \textit{et al.} in their book \textit{Tsunamis in the Mediterranean Sea 2000 BC–2000 AD} point out:

“More reliable evidence that the tsunami … did reach the shores of the Near East, actually retaining its destructive force, was obtained during the geological exploration carried out in the region of the Syrian city of Ugarit which used to be the capital of the ancient North Phoenician state. … A library of clay boards filled up with cuneiform was excavated. Scientists deciphered one of the poems with an account of how the harbour and half of the city of \textit{Ugarit} were washed out by a strong wave approximately in 1380 BC. …

“As to the height of the tsunami on the shore of the Near East, a sea-carried pumice [volcanic rock] layer with the chemical composition corresponding to the eruption of Santorini has been found at several points on the shore of Israel, and also, in postglacial sediment to the north of Jaffa (now Tel Aviv) on a terrace 7 \text{meters, 22 feet} high … It is this value that one can take as the height to which the water rose on the shore of the Near East.”\textsuperscript{130}

It is not generally known that Thera erupted about 25,000 to 27,000 years ago and left a great deal of volcanic rock on the bed of the Mediterranean Sea. An earthquake on that sea bed would therefore carry this ancient material across to Israel while also inundating Ugarit.

It must be pointed out that Ugarit was not completely destroyed in the 8th century B.C. because there are 12th Dynasty statues there that prove it existed

\textsuperscript{127} \textit{ibid.}, p. 160
\textsuperscript{128} \textit{ibid.}, pp. 160-161
\textsuperscript{129} Kuhrt, \textit{op.cit.}, p. 306
\textsuperscript{130} Sergei Leonidovich Soloviev \textit{et al.}, \textit{Tsunamis in the Mediterranean Sea 2000 B.C.—2000 A.D.} (Dordrecht, the Netherlands/Boston MA), pp. 20-21
down into Hellenic times, i.e. the 4th century B.C. Schaeffer maintained that Ugarit survived long after the earlier catastrophes:

“It is nevertheless premature to conclude … that the Tell [Ras Shamra] was more or less completely abandoned after the disappearance of the town of Mycenaean date [1200 B.C.] … During the seventh and sixth centuries [B.C.] the highest part of the hill south of the site of present excavations was inhabited. A small cemetery attests this fact …

“During the sixth century the port of Ugarit was visited by Greek sailors who called it Leucos Limen. A small hoard of archaic Greek staters [coins] belonging to this period has come down to us. … Barring a few Cypriot coins, the majority of these were struck in the neighbourhood of the Thracian-Macedonian silver mines. … The hoard was buried in the second half of the sixth century …

“[There are also] Hellenic sarcophagi … There were still a few dwellings in existence on the Tell during the fifth and fourth centuries containing fairly rich grave-goods including Persian obols [coins] …”

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The fact that Greek sailors were trading with Ugarit in the 500s B.C. indicates it was still a thriving port or they would not have sailed to it. The further fact that during the fifth and fourth centuries there were burials “containing fairly rich grave-goods” also indicates that great prosperity existed at Ugarit to allow such interments. It is thus tentatively suggested that Ugarit’s final demise came when it may have resisted Alexander the Great. Not every city that experienced the 8th century catastrophe was completely destroyed and/or abandoned.

Sweeney shows the correlation between the Ugarit fire and the flood at Ur:

“Neither Schaeffer himself, nor any one else for that matter, could have expected to see the fire of Ugarit as contemporary with the flood of Ur. Apart from the fact that the agents of destruction were apparently very different, the chronology also disagreed. …

“Clearly then, the vast destruction observed by Schaeffer and others throughout Syria/Palestine and Anatolia could not be made to tie in with the great flood observed by Woolley in Mesopotamia. Such has remained the accepted wisdom for many years … in spite of the divergent terminologies, the pre-flood culture of Ugarit … matches very closely the pre-flood culture of Ur …, whilst the post-catastrophe cultures of the two cities also match in detail. Most illuminating is the change in pottery styles. The pre-conflagration town of Ugarit employed ‘Ubaid-type pottery of almost exactly the same kind as that used in pre-flood Ur. Similarly the immediate aftermath of the Ugarit fire saw the introduction of a new culture employing distinctive wheel-made pottery named Khirbet-Kerak. But the Khirbet-Kerak culture closely parallels the Jamdat Nasr culture of post-flood Mesopotamia. Again, pre-conflagration Ugarit was entirely illiterate, with early

hieroglyphs only appearing afterwards—a situation precisely reflecting that of pre- and post-Flood Mesopotamia.”\textsuperscript{132}

Soloviev et al. add:

“According to the writings of Demokles, Strabo and others, strong earthquakes in Asia Minor covered the Greek regions Ionia, Lydia up to Troy. Many cities were wiped out in the north. Mt Sypilos … was destroyed. Marshes became lakes. The tidal wave flooded Troy [Ambraseys 1962]. (Probably these were the same events as in 1380 BC [at Ugarit and in Israel]).’\textsuperscript{133}

There is clear evidence of a great flood at Ugarit and along the shore of Israel and possibly at other eastern Mediterranean sites that correlate with the period of earthquake swarms. What then of the flood of Deukalion on the opposite Greek shore? The evidence for a flood in Greece was discovered by Eberhard Zangger who shows that “About 200 m[eters, 220 yards] east of Tiryns is an old tree … The first auger core [into the ground there] revealed an almost 5 m[eters, 15 foot] thick unstratified layer [of flood deposits].”\textsuperscript{134}

Zangger elsewhere points out that

“Studying numerous earth cores taken by hand augers and power drills, I had discovered that parts of the lower town of Tiryns … had been buried under several meters of mud deposited by a flash flood that had occurred around 1200 BC. This catastrophe coincided with an earthquake, for which evidence was found in the archaeological record of the Tiryns citadel. Both of these events occurred shortly after 1200 BC precisely at the time when the Mycenaean civilization suddenly collapsed.”\textsuperscript{135}

That is, the amount of sediment laid down at Tiryns was 5 meters or over 15 feet in depth, which is one meter or 40 inches deeper than the level of sediment left at Ur. As we were told by Woolley, at least 25 feet/8 meters of water moving slowly is necessary to deposit 8 to 11 feet/3 meters of sediment. This means that the level of water at Tiryns had to be of a depth greater than 25 feet. Here then is the problem: the dam that held back the waters above Tiryns was too low to produce this level of water. Since 25 feet is necessary to deposit about 11 feet, to deposit an additional 3 feet/1 meter of sediment requires that the depth of the water had to be over 30 feet at Tiryns. Yet that was the height of the dam at Tiryns at some distance from the town. Stanley Wayne Trimble describes the nature of this situation:

\textsuperscript{132} Sweeney, The Genesis . . ., op.cit., pp. 21-22
\textsuperscript{133} Soloviev et al., op.cit., p. 21 (emphasis added)
\textsuperscript{134} Eberhard Zangger, The Flood from Heaven: Deciphering the Atlantis Legend (Ann Arbor MI 1992), p. 122
\textsuperscript{135} Eberhard Zangger, “Who Were the Sea People?”, Saudi Aramco World, vol. 46, no. 3 (May/June 1995), pp. 20-31; also available on the Internet: http://www.saudiaramcoworld.com/issue/199503/who.were.the.sea.people..htm
“… (ca. 1250–1200 BC) is the Tiryns dam. It seems that during a flood, a stream south of Tiryns abandoned its bed and shifted to the north of Tiryns. To protect the lower town from further floods, the inhabitants installed an artificial river diversion consisting of 10 m[eter/33 foot]-high and 300 m[eter/330 yards]-long dam and a 1.5 km [almost a mile]-long canal [to drain away excess water]. The dam is a huge earthen embankment with Cyclopean [massive stone] masonry.”\textsuperscript{136}

This dam, at a distance of over four miles from Tiryns, at best held a level of water of 33 feet/10 meters deep.\textsuperscript{137} When it broke, it would pour out its waters in a rampage onto the plain surrounding the town of Tiryns and have to fill it to the depth of 33 feet/10 meters downstream at the site of the town. Since it was at best that height when it left the dam, it would fall to a much lower level as it spread out and flooded the plain. Therefore it could not leave 15 feet/5 meters of sediment at Tiryns.

Another reason that denies the dam flooding hypothesis at Tiryns is that we were told earlier that water that is moving swiftly scours, rather than drops sediment. When a dam breaks or collapses, the water behind it under pressure charges forward and scours the land over which it flows. To leave 15 feet/5 meters of sediment, this water had to slow to a point where it stood relatively still, and that does not happen when a torrent descends from a height of 33 feet/10 meters above a plain. It cannot flood the plain over its breadth at Tiryns to 33 foot/10 meter depth nor will it stand sufficiently still for long enough to deposit deep sediment. The only way to accomplish both phenomena is if a great tidal wave flowed over the plain and then gradually receded. That being the case, the only source capable of doing both actions is the Aegean Sea. Until hydrologists explain away these basic contradictions to this dam-flood disaster, the tidal wave theory stands!

Thus, not only is there strong geomorphological evidence for tsunami floods over southern Mesopotamia, the Indus Valley, the plain of the Yellow and Yangtse Rivers in China during the 8th century B.C., but also for similar but smaller tsunami flooding of Ugarit, Israel, and the plain around Tiryns at this same period.

Getting back to skeletons, as evidence of earthquakes in the ancient Near East, Nur and Burgess conclude:

“… evidence supporting the theory that earthquakes were involved in the … Late Bronze Age comes from skeletons found crushed beneath collapsed buildings in that period, evidence that Drews inexplicably dismisses as nonexistent. Reports from site after site vividly illustrate abundant evidence of such remains, with crushed skeletons found in Knossos, [Mochlos,] Troy, Mycenae, Tiryns, Thebes, Pylos, Gla, Midea, Kynos, Jericho, Ugarit, and Megiddo, all dated (with more or less controversy) to the end of the Bronze Age … . However, in nearly every case,

\textsuperscript{137} Zangger, \textit{The Flood from Heaven, op.cit.,} p. 86
some scholars [historians] reject the notion that earthquakes buried these individuals. In some instances, they argue that in large earthquakes loss of life should have been more widespread. However, except when earthquakes strike at night, one may reasonably assume that most inhabitants manage to escape the collapse of buildings. Further, earthquake survivors generally go to extraordinary measures to sift through the rubble in search of dead relatives [in order to give them proper burial so they can go to the land of the dead] or [seek] buried valuables. Possibly, then, most of those killed beneath the rubble were extracted and given decent burials, whereas those who could not be recovered remained beneath the rubble.”

Furthermore in cities which experienced tremendously widespread, extremely hot fires, the bodies and bones would have been cremated.

What we must explain at this point is: How was it possible that virtually every criticism Drews argued was totally in error, and that the evidence was readily available to him in the very literature with which he, as a historian, should have been familiar? The point is germane to the thrust and concepts presented in these three volumes, which also use science and technology to explicate history/chronology. It seems rather obvious that historians, dealing mostly with documents and interpretations of them, hold the findings of science to be of less value than what they think they know. When scientific or technological evidence is there in front of them, they do not see it, as Drews did, or ignore it, or, as Drews also did, explain it away with ad-hoc excuses, or look for exceptions, no matter how improbable these may be, to justify their belief system. On the other hand, whenever the science supports their belief system, they never question it, in fact, they hold that science up as the highest form of proof of what they are proposing. In these cases it is solid, direct proof or, to put it bluntly, science is only science if it agrees with what they want and is suspect if it does not.

Since Nur and Burgess did not deal with the Indus civilization and the evidence that it, too, experienced earthquakes at the same time as those just discussed, let us examine this material. Feuerstein, Kak, and Frawley, who are ardent proponents of a massive historical catastrophe to explain the fall of the Indus culture, in their chapter “The Great Catastrophe and Reconstruction of the Early Indic Civilization,” write:

“Upon the discovery of the Indus cities, Aryan invasion theorists [like Sea Peoples or Dorian invasion theorists] promptly scouted around for signs of battles between the invading Aryans and the settlers of the Indus River. In a late layer at Mohenjo-Daro, archaeologists discovered the skeletal remains of thirty-eight individuals in contexts suggesting violence. Four individuals were found in a room

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138 Nur and Burgess, op.cit., pp. 243-244
containing a well. Two of them were apparently trying to climb the stairs to the street when they were killed.

“In another house, thirteen adults and a single child were found. One of the skulls bore a long cut that could have been made by a sword. Another skull showed similar marks of violence. Five [actually, six] more skeletons—three men, two women, and a child—were found sprawling amidst the bricks of a collapsed wall, ash, and broken pottery. In a lane, six more skeletons—including that of a child—were found, but archaeologists have yet to provide a description [of them]. The same is true of a single skeleton dug up in another street.

“A group of nine skeletal remains—including those of five children—was found under a mound of debris. Archaeologists have suggested that they were hastily buried. …

“Hypnotized by the Aryan invasion theory, archaeologists summarily attributed the death of these urbanites to a ‘final massacre’ at the hands of marauding bands of Aryan nomads. In the words of Sir Mortimer Wheeler:

“‘The general inference from the thirty-eight derelict corpses at Mohenjo-daro is that from the moment of death the place was uninhabited. The absence of skeletons (so far) from the citadel may imply that the raiders, whoever they were, occupied and cleared this commanding position for their own momentary use. For the rest, it may be suspected that sporadic fires in the sacked city kept predatory animals at bay.

“‘Looking back on the macabre scene we may perhaps conclude that, since seventeen of these skeletons seem definitely to belong to the latest [final] occupation and the remainder present the same aspect and have been found in inconsistent circumstance, we have here in fact the vestiges of a final massacre, after Mohenjo-daro ceased to exist. Who were the destroyers? We shall not know. It may be that some hill-tribe fell upon the enfeebled city and put it to the sword.’”

Today the concept of an Aryan invasion is largely discredited and thus these researchers argue:

“Clearly, something happened to bring this great civilization to its knees, but it was not any brutal invasion by nomads from the northwest. The catastrophe that befell the people of the Indus cities was of an entirely different order and far more devastating than any pillaging tribe could ever be. As we will show, the cities and their inhabitants succumbed to unpredictable Nature herself in a catastrophe of gargantuan proportions.”

Before proceeding, we wish to present how widespread the climate change and aridification ranged from the Sahara, to Egypt, to the Eastern Desert of Egypt, to the Sinai, the Aegean, Anatolia, Syria, Palestine, to Arabia, Mesopotamia, and the Harappan civilization. Thus, we will now go only slightly further eastward to the

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139 Feuerstein, Kak, and Frawley, op.cit., pp. 77-78
140 ibid., p. 79
Deccan region of northern India to show that this aridification affected that region as well. According to M.K. Dhavalikar:

“It was generally thought by early historians that in the development of mankind, the hunting-gathering stage was followed by pastoral nomadism after which man began to produce his own food by domesticating plants and animals. This was the accepted view until the middle of the 19th century when Hahn suggested that pastoral nomadism was an offshoot of sedentary agriculture, and it is now generally agreed that pastoral nomadism came after crop cultivation (Khazanov 1984, p. 85). One reason for this view is that nowhere in Eurasia have there been pastoral nomads without the knowledge of agriculture. An excellent illustration of this view is provided by the archaeological evidence in the Deccan … Exploration followed by selective excavation in the Krishna valley has provided convincing evidence of flourishing agricultural communities in the latter half of the 2nd millennium BC, whose successors had to resort gradually to pastoral nomadism because of the drastic change in climate. This was confirmed in the course of our intensive excavations at [various sites] (lat 18°35’N, long. 74°32’E). Thirteen seasons’ work has enabled us to study the culture process and culture change throughout the nine centuries of occupation at the site, from 1600 BC to 900 BC. … [There was] a shift from farming to pastoralism.

“The first quarter of the 2nd millennium BC marks the appearance of early farming communities in the Deccan … This distribution, however, was sporadic, but by the middle of the 2nd millennium BC the region was dotted with several self-sufficient villages, some of which … developed rapidly and became large regional centres … They appear to have been organized into chiefdoms … This prosperity was in a large measure due to the congenial environment during 1500-1000 BC, which was a relatively wet phase (Krishnamurthy et al. 1981). The prosperity, however, did not last long and came to an abrupt end by the close of the 2nd millennium B.C.”

The reason this is so baffling is that historians and archaeologists refuse to accept such a massive relatively sudden climatic shift in this region and all the others discussed in these volumes could have occurred at the same time. Dhavalikar continues:

“The opening of the 1st millennium BC witnessed large-scale desertion of the settlements by the early farming communities in the northern Deccan. Everywhere in [certain regions] human activity came to a halt (Dhavalikar 1984). We have, as yet, no satisfactory explanation of this abandonment of habitations, but a possible clue is furnished by the soil analysis of the sterile layer encountered at Nevasa …

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which ... belongs to the brown soil group and suggests a less humid climate than when the [original] virgin black soil was formed. They observe:

"Our pedological [soil] investigations have revealed that the so-called weathered black layer shows definite soil characteristics and it has been formed by the weathering of the habitational deposits of [the] chalcolithic period. This indicates that the place was deserted by the chalcolithic people due to some calamity and [a] considerable period must have elapsed to allow for the growth of vegetation and the formation of soil on the top portion of the habitational deposit. The chemical and physical analysis indicate that the [new] soil formed here belongs to the brown soil group and as such comparatively drier climatic conditions must have prevailed with annual rainfall of about 50 cm [20 inches] accompanied by hot summers and corresponding scrub forest type of vegetation growing in the locality."

"On the basis of further studies, Mujumdar (pers. comm.) thinks that the rainfall might have decreased to about 300 mm [12 inches], indicating that the climate was becoming more and more arid. ... Thus, the early farmers of the northern Deccan seem to have deserted their settlements at the close of the 2nd millennium BC, [but to the south] it appears that the climate ... was slightly more congenial ..."142

Across the ancient world from west and east Africa to Arabia, Harappa and the Deccan in India the same climatic scenario plays out as that found in Mesopotamia, Anatolia, and Greece of a drastic drying out of these regions.

The final argument against the concept of this earthquake swarm theory is that it cannot be encapsulated in a uniformitarian paradigm. The catastrophe is of such gigantic proportions that historians and even scientists cannot subscribe to what is for them unthinkable and inconceivable, namely a global event. Yet even the one that Nur and Burgess advocate is anathema simply because of its size and destructiveness. Nur and Burgess explain:

"Robert Drews (1993) compiled a map showing the sites that have been found in the eastern Mediterranean that were destroyed at the end of the Bronze Age ... Drews names forty-seven Aegean and eastern Mediterranean sites where archaeological evidence suggests some level of catastrophic collapse between 1225 BC and 1175 BC [in the established chronology, and 800 B.C. and 750 B.C. in the short chronology], a period of about fifty years. Despite the ongoing debate about what caused the collapse, most scholars agree that severe destruction was widespread. Drews (1993) notes, ‘Within a period of forty or fifty years at the end of the thirteenth and beginning of the twelfth century [B.C.] almost every significant city or palace in the eastern Mediterranean world was destroyed, many of them never to be occupied again.’ ..."

142 ibid., pp. 157-158
“The most persistent argument against the earthquake hypothesis is the sheer size of the quake required to cause such damage or the unlikely coincidence of so many earthquakes occurring in such quick succession … But given this skepticism about how widespread the effects of an earthquake could be, some archaeologists seem determined to prove earthquakes could not even have happened during the time in question.”

In essence, we have no evidence that the Sea People existed to inflict these widespread destructions and, as these volumes clearly show, we have no scientific or technological evidence that advanced civilizations existed as far back as 1200 B.C. People who do not exist in an historical period that does not exist cannot destroy anything! Furthermore, we have about 50 major and hundreds of minor centers of civilizations of the vast swath of the Earth that were destroyed by earthquakes, floods, and the aftermath of these catastrophes in a few generations. The cause for these earthquakes and flooding events is not to be found in present-day uniformitarian actions. It is to be found in extraordinary catastrophic actions on an enormous scale. In the first chapter of this book I stated that catastrophic interpretations of ancient historical events would inexorably “grow and grow and grow.” That is indeed the case with the burgeoning earthquake, fire, and flood concepts we have been discussing. E. Guidoboni reports:

“There are several reasons why the study of earthquakes in archaeology could be important and should be incorporated into the interpretation of archaeological sites. First, it provided a much better understanding of the impact of natural disasters on history, where there is no consensus (see Fainter 1988). For example Ambraseys (1971) [in his attack on Claude F. Schaeffer’s thesis] adamantly states ‘Earthquakes in the past twenty-five centuries have had little of any influence on historical developments in the Middle and Near East … but they have never caused the ruin of a culturally advanced civilization…’ under usual circumstances. Ambraseys is correct. Earthquakes are instantaneous events that cause sudden damage, but societies generally recover even from the severe damage a large earthquake causes. However, when the effect of a series of earthquakes is compounded with wars, or economic difficulties, the end can be cataclysmic.

“While Ambraseys questions the impact of earthquakes on societies, Rapp (1986) questions the evidence itself. ‘Without direct evidence of seismic destructions, earthquakes should be ‘explanations of the last resort in archaeology’. However, if an area is known to be susceptible to earthquakes (from geological and geophysical evidence) [as is the Near East], the earthquake damage

143 Nur and Burgess, op.cit., pp. 225-227
is as respectable a hypothesis for destruction unless historical or archaeological evidence suggests otherwise.”

Feuerstein, Kak, and Frawley also maintain that these climate changes were generated by celestial catastrophes—citing Victor Clube and Bill Napier who envision cometery Tunguska-type explosions. That is, as stated above, historical catastrophism will continue to grow.

“Even though catastrophes have in modern times been far less severe in magnitude than the cataclysms that spasmed the Earth and terrorized humankind in prehistory, they can still have a planetwide effect altering climate and vegetation. …

“The Earth is a living, breathing organism, and the thin skin of land that protects it from its hot interior is rather flexible and porous. There are constant earthquakes and earthquakes around the world, which cause tidal waves and floods [etc.]. …

“Nevertheless, according to [most] orthodox scientific [and historical] opinion, the progress of human civilization has been ensured by a relatively stable environment: All the great geological upheavals belong to the time of pre-sapiens history. Since the ice sheet receded from Eurasia …, Nature has shown humankind a rather benign face. The story goes that, apart from occasional hiccups in the form of rather localized earthquakes and volcanic eruptions, Nature has thrown no big tantrums in civilized history. This scientific [and historical] assumption is just that—an assumption, a belief that is not borne out by the facts.

“In pointing out this flimsy scientific [and historical] credo, we must remember that, before anything else, scientists [and historians] are human beings and therefore they are liable to making the same errors and entertaining the same prejudices as any other human beings. This comes as a surprise only to those who think that the scientific [and historical] ideal of objectivity is a reality rather than an ideal to be aspired to … In practice, scientific [and historical] research frequently falls short of the demanding standards of objectivity…

“According to a minority opinion, human civilization has not been spared large-scale natural catastrophes. On the contrary, this small dissenting voice asserts, Nature’s cataclysmic changes have shaped much of early culture and thought ….”

And certain historians are beginning to approach these issues from a catastrophic viewpoint, albeit as close as possible to unformitarian concepts such as that of a prolonged drought. Like the arguments over the extinction of the mammoth, they can only conceive that man is responsible, or climate was the cause or some great pandemic of diseases worked its will. In connection with the

145 Feuerstein, Kak, and Frawley, op.cit., pp. 80-82
concept of a drought which explains none of the earthquake evidence, nor the major and minor floods etc. discussed above, Amihai Mazar states:

“Various theories explain the destruction and influx of new populations. The destruction of the Mycenaean civilization was once seen as the result of Dorian invasions from the north; today it is generally thought that economic difficulties, particularly an extended series of droughts, brought on the collapse of the Aegean culture. Responsibility for the destruction of the Hittite empire was pinned on the invasion of the so-called Sea Peoples; it now appears that the empire also suffered from extended droughts and famine …”146

By trial and error, the historians are groping in the darkness to explain what has eluded them for centuries. They still cannot and will not see that the evidence—the catastrophic evidence—that links these earthquakes, fires, tidal waves, and climate shifts are all interconnected events. They cannot, as yet, broach these interconnections with a uniform/uniformitarian explanation. These difficulties will continue to elude them.

While there were tidal waves that affected Tiryns and Ugarit, these were not the kind of waves that inundated southern Mesopotamia, the Indus valley plain nor that of the southern river plain of China. Tsunamis originate from one point and therefore the distance from southern Mesopotamia to China is too great to have been caused by a tsunami. Although I have occasionally used the word “tsunami” to describe these floods, I have only used that term to describe what these inundations would appear to be, not their ultimate cause.

There are two further points that must be presented before going on that relate directly to Velikovsky’s 8th century B.C. Martian catastrophe. One is the great noises emanating from the Earth after that brief encounter between Mars and the Earth, which Velikovsky attributed to the ca. 1500 B.C. Venus event, namely “Theophany” or great noise:

“Earthquakes are often accompanied by a roaring noise that comes from the bowels of the earth. This phenomenon was known to early geographers. Pliny wrote that earthquakes are ‘preceded or accompanied by a terrible sound.’ Vaults supporting the ground give way and it seems as though the earth heaves deep sighs. The sound was attributed to the gods and called theophany. …

“In the days of the Exodus … the world was shaken and rocked …

“It was a perfect setting for hearing words in the voice of nature in an uproar. … The earth groaned: for weeks [at the beginning] all its strata had been disarranged …

“The din caused by the groaning earth repeated itself again and again, but not so loud, as subterranean strata readjusted themselves after being dislocated; earthquakes incessantly shook the ground for years.”\textsuperscript{147}

Since the earlier cataclysm of 1500 B.C. did not take place in historical times, even though it, too, was clearly accompanied by shriller and more sustained noise over many years, it would have been largely forgotten or only vaguely remembered. The 8th century catastrophe did take place in historical times and the quaking of the Earth would have been, and was, noted in ancient texts, as Velikovsky described.

The other phenomena Velikovsky associated with the 8th century Martian-Earth near collisions are delineated by what Velikovsky calls “The Terrible Ones.” Mars in its interactions with proto-planet Venus had been moved out of its orbit and had captured an enormous number of the comets and meteors that accompanied Venus in orbit around it. These Velikovsky points out as the “Maruts.”\textsuperscript{148} “[T]hese comets, traveling in bands with Mars … are called Maruts ‘shining like snakes,’ ‘blazing in their strength,’ ‘brilliant like fires’.”\textsuperscript{149} They were great in number.\textsuperscript{150} Not only did they threaten the Earth but they were so numerous that great numbers encountered the Earth and exploded in fireballs of stupendous energy comparable to that which leveled the Siberian forest at Tunguska.

These cometary/meteorite showers were not those that ordinarily encounter the Earth. There were so many that, when they did enter the Earth’s atmosphere and subsequently heated up by friction with the air and exploded, they would have produced great devastation in the areas where they came down by the thousands or even tens of thousands. Most, of course, would have done so over the oceans or uninhabited or sparsely inhabited parts of the Earth. Others, which came down over highly populated centers of civilization, would have done so with devastating results.

The next unit will describe one of these great events and relate it to the short chronology. It was not a minor episode to those who experienced it and survived. An entire civilization was devastated, thousands upon thousands of people, and many animals both domestic and wild, died. The culture for a time was disrupted and it led to one of the major events in Hebrew history.

\textsuperscript{147} Velikovsky, \textit{Worlds ...}, \textit{op.cit.}, pp. 96-97
\textsuperscript{148} \textit{ibid.}, pp. 281-289
\textsuperscript{149} \textit{ibid.}, p. 282
\textsuperscript{150} \textit{ibid.}, p. 288
THE HYKSOS EXPULSION AND THE EXODUS CATASTROPHE

One of Velikovsky’s major themes was that of the Venus near-collision with Earth around 1500-1400 B.C. in which the Hebrew slaves in Egypt left that land after a series of catastrophes. Since the short chronology only allows high civilization to arise about 1200-1100 B.C., Velikovsky’s chronology cannot be historically or scientifically correct. Heinsohn, on the other hand, sets the Exodus in Hyksos/Akkadian/Assyrian times in the 8th century B.C. with the Hebrew host as allies to the Hyksos. Thus, the catastrophe, related not only to the ones described above ranging across the ancient world, which brought down a great number of cities, towns, and villages via earthquakes, floods, and fires, had to have also been the same one related in the chronicle of the biblical Exodus. The intent of this unit is to place the Exodus in 8th century Hyksos Egyptian history and explain by the scientific method the plagues and other events as these occurred. It is our contention that, because the established chronology was employed to date these events alone with the desire of scholars to take the biblical account as the only template against which to interpret the evidence, these have failed to explicate what most needs to be explained.

In the following pages we will show that the 8th century B.C. catastrophe not only explains the Exodus plagues, but with this chronological marker Hebrew/biblical history correlates with the history of the biblical kings outlined earlier. In this chronology the Exodus occurs at a relatively short period prior to the emergence of Israel as an ancient state in the 8th-7th century B.C. More than that, it further explains other problems such as the slavery in Egypt; who were the Hyksos’ allied kings in Egypt, why indeed did the Hyksos and their Palestinian, Syrian and other allies leave Egypt; did they actually cross the Red Sea; and much else. The short chronology thus gives ample evidence of the Hebrews in Egypt and their return to Palestine. It gives the Hebrews their rightful place in early history.

This thesis does not, however, preclude the Venus catastrophe centuries earlier and the mythic evidence that Velikovsky presented from cultures around the world, which is well supported by scientific evidence discussed by him and in The Velikovskian, and elsewhere.

According to Heinsohn, since the Hyksos were actually the Assyrians/Akkadians, they dominated Egypt for about 108 years, almost four Hebrew generations (a generation being reckoned as 30 years). However, the Bible seems to indicate the Hebrews were there for 430 years. The problem deserves to be resolved. Ahmed Osman deals with this question:

“The contradictory accounts given in the Old Testament about the length of time the Israelites spent in Egypt are one of the reasons that have misled scholars
into accepting the Hyksos period as the right time for Joseph’s appearance in the country. The relevant texts are:

“‘Know of a surety that thy seed shall be a stranger in a land that is not theirs [Egypt], and shall serve them; and they shall afflict them four hundred years. … But in the fourth generation they shall come hither [back to Israel] again (Genesis 15:13,16).

“Now the sojourning of the children of Israel, who dwelt in Egypt, was four hundred and thirty years. (Exodus 12:40)’

“To turn to the actual Hebrew, Abraham was warned that his descendents would dwell in a foreign land for arba’ m3wt shana (four hundred years) and Exodus confirms this figure by stating that the sojourn of the children of Israel was shalsheem shana wa arb’m3wt shana (thirty years and four hundred years). On the other hand, Genesis also says wa dor rabi’i yashwbw hena (and the fourth generation return they here [to Israel]). More than a century ago, in his book Historical and Critical Commentary on the Old Testament, the biblical scholar M.M. Kalisch tried to explain this discrepancy [between Genesis which had the children of Israel in Egypt four generations (120 years) and Exodus which claimed 430 years] by saying that the age of those generations was to be looked upon as one hundred and twenty years. ‘The “fourth generation” after the settlement of Jacob in Egypt was promised to be led back to Palestine. Unless we suppose these words contain an obvious discrepancy, the “four generations” must embrace a period of more than four hundred years; and no alternative is left but to understand here that term dor as the duration of life which, as a general rule, was allotted to man after the deluge.’

“More recently, K.A. Kitchen of Liverpool University tried to use the evidence of the Ugaritic tablets found in northern Syria … to give a different meaning to the Hebrew word dor. ‘Abraham is told that his descendents will re-enter Canaan “in the fourth generation,”’ he [Kitchen] argued in his book The Ancient Orient and the Old Testament. ‘The simplest explanation is that the four dor correspond to the four hundred years, not to “generations” [of 30 years] in the modern sense. This is suggested not by a mere wish for harmonisation, but by perfectly clear evidence from Ugaritic and early Assyrian sources that dor or darn can mean a “span” or “cycle of time” of eighty years or more.’

“This argument [by Kitchen] is quite unsound for a variety of reasons. For example, the Hebrew verse did not say, as Dr. Kitchen did, that Abraham’s descendents would re-enter Canaan in the fourth (dor); the translation is: ‘And the fourth (dor) return they here.’ It is therefore ‘the fourth dor’ that is the subject of the verb [return]. In this case, dor can only mean ‘generation’ [not eighty years or more], which, in fact, is also confirmed by the Ugaritic texts to which Dr. Kitchen
Charles Ginenthal, Pillars of the Past, vol. III

refers. There is not one example in them, or in Hebrew or Assyrian texts, where the word *dor* was used to mean a hundred year cycle …”\(^{151}\)

That is, there is a contradiction in the Bible between the text in Genesis which claims the Hebrews were in Egypt four generations of 40, or perhaps 30, years, a little over 100 years [which fits their staying there about the same period of time as the Hyksos/Akkadians/Assyrians] or 430 years, as in Exodus. Here the biblical text must align itself with the scientific evidence. The Hebrews could not have been in Egypt for 430 years because there is no scientific evidence for this period, prior to Exodus, even in the 8th century B.C. without disrupting all the forensic chronological evidence presented in these volumes.

There is a further reason for not accepting this 430-year number which places the building of the Temple of Solomon 480 years or 12 generations after the Hebrews entered Israel: 12 times 40 equals 480. The number 40 in the Bible is a symbolic number: Moses is on the holy mountain 40 days and nights; the Hebrews wander in the wilderness 40 years, etc. John H. Walton *et al.* state that:

“40 as approximation? The number forty appears in the Bible many times as a number of completion, signifying the passage of the appropriate amount of time: a generation (Gen. 25:20), the age of a mature man (2:11), the period in the wilderness (16:35; Num 14:33), the rule of a judge or chief (Judg 3:11; 13:1). The regularity with which this symbolic number is used suggests it has both cultural and literary significance and is therefore not to be taken precisely in most instances.”\(^{152}\)

On this, Finkelstein and Silberman comment:

“… [I]n Kings II:42 Solomon is given a suspiciously round figure of forty years of kingship, recalling the traditional biblical typological expression of forty years for ‘a generation,’ as in the length of the Israelites’ wandering in the wilderness or just for ‘a very long time.’ David’s reign … is likewise recorded as forty years …

“Unfortunately, scholars have generally taken these round numbers as precise indications for the dates of the early kings…

“The truth is that we can take these symbolic biblical [numerical] descriptions only as a general indication …”\(^{153}\)

Therefore, citing the Bible without science to support any of its statements leads to chronological problems. There is only one mass Exodus from Egypt historically—that of the Hyksos—and that is the only Exodus possible for the Hebrews. The plagues of Egypt have likewise been taken to have occurred exactly in the numerical order given, namely one through ten. But here, too, we challenge such a reading and interpretation of the evidence. For example, there are two times

\(^{151}\) Osman, *op.cit.*, pp. 104-105

\(^{152}\) Walton *et al.*, *op.cit.*, p. 121

\(^{153}\) Finkelstein and Silberman, *David and Solomon*, *op.cit.*, pp. 19-20
when the Israelites escaping Egypt must cross a large body of water which—just at the right moment—parts to allow them to pass over on dry land. The first, of course, is either the Sea of Reeds or the Red Sea between Egypt and the Sinai, the second occurs when they cross the Jordan river into Palestine. It is here that an extremely important distinction exists which must be taken into account in determining the reality of any such event which we will discuss below. With respect to the Jordan, Colin J. Humphreys, a Cambridge University physicist, who explained all the plagues in terms of scientific evidence, wrote, citing Joshua:

“Now the Jordan is in flood all during harvest. Yet as soon as the priests who carried the ark reached the Jordan and their feet touched the water’s edge, the water from upstream stopped flowing. It piled up in a heap a great distance away, at a town called Adam in the vicinity of Zarethan, while the water flowing down to the sea of the Arabah [the Dead Sea] was completely cut off. So the people crossed over opposite Jericho. Joshua 3:15, 16 …

“Most people, even most biblical scholars, believe the story of the crossing of the Jordan to be a legend. But … looking at this story through the lenses of science can prove, beyond reasonable doubt, that the Jordan stopped flowing … as described in the Old Testament …”¹⁵⁴

Humphreys goes on to locate the town of Adam:

“In the original Hebrew text, the word the Masoretes represented as Adam was simply the consonants ‘dm …

“I therefore looked on a map to see if there is a suitable town on the river Jordan containing the consonants dm, and indeed there is! On modern maps of the state of Jordan, a town appears on the eastern side of the river Jordan called Damiya, about seventeen miles north of where the river Jordan passes closest to Jericho. Not only that, but on the 1989 Bartholomew World Travel Map of Israel with Jordan, the same town is actually marked Damiya (Adamah). This strongly suggests that the ancient town of Adam is modern Damiya. I was later to find that many scholars agreed on this, which is why the Bartholomew map states it.”¹⁵⁵

He goes on to explain the scientific/geological reason why the Jordan River stopped flowing:

“On July 11, 1927, a well-documented earthquake shook the town of Jericho [which] was detected in seismological stations as far apart as Europe, South Africa, North America, and Russia and was measured at a magnitude of about 6.5 on the Richter Scale … Amos Nur … at Stanford University [and others] have made a detailed study of the 1927 Jericho earthquake and found that it was due to slippage along a geological fault called the Jericho fault, which runs approximately north-south under the Jordan River. The Jericho fault forms the

¹⁵⁵ ibid., p. 19
boundary between two tectonic plates, with the Arabia plate to the east and the African-Sinai plate to the west. …

“Professor Nur has written to me about this earthquake:

‘‘During the 1927 earthquake, several ground cracks appeared, together with an outpouring of ground water. This soil liquefaction phenomenon has been well observed in earthquakes elsewhere. During the earthquake, mud slides occurred along the Jordan near Damia, about 30 kilometers [about eighteen miles] north of Jericho; these temporarily stopped the river’s flow.’’  

Of great significance: this was not the only time that the Jordan River was temporarily dammed by an earthquake mud slide for a few days. Humphreys further shows:

“Amos Nur then searched historical records and found that the river Jordan had been temporarily stopped on a number of occasions, all because of mud slides induced by earthquakes. The earliest historical record of this that Nur found occurred in 1160. Nur recognized the relevance of these earthquakes to the passage in the book of Joshua quoted above. He writes:

“Adam is now Damia, the site of the 1927 mud slides which cut off the flow of the Jordan. Such cut-offs, lasting typically one to two days, have also been recorded in 1906, 1834, 1546, 1534, 1267, and 1160. The stoppage of the Jordan is so typical of earthquakes in this region that little doubt can be left as to the reality of such events in Joshua’s time.”  

What I wish to emphasize is that we are discussing the period of earthquake swarms described above in the 8th century B.C., which would have been more prevalent at that time and would have acted perhaps more strongly to block the flow of the Jordan. Humphreys also shows:

“The main sources of the river are on the slope of Mount Hermon, a snowcapped mountain on the Lebanon-Syria border and close to the border with modern Israel. Each spring the snow melts and the sudden volume of extra water turns the Jordan into a fast-flowing river with treacherous currents. Because of irrigation, the effect is not nearly as great now as it used to be. However, a British expedition sailed down the river Jordan in the nineteenth century and estimated that the Jordan in flood was half a mile [2600 feet, about 800 meters] wide.”  

All in all, Humphreys concludes:

“I have suggested that ancient Adam is now modern Damiya. Various arguments support this conclusion. First, both Adam and Damiya have the same core consonants dm. Second, modern Damiya is on the Jordan River and north of Jericho, and the Old Testament records that ancient Adam was on the Jordan and
north of Jericho. Third, and most important, historical records show that
earthquakes at Damiya have stopped the flow of the river Jordan on a number of
occasions, and the Old Testament states that in the time of Joshua the flow of the
Jordan stopped at ancient Adam. As a scientist looking at this evidence, I am
happy to agree beyond reasonable doubt that ancient Adam is modern Damiya.”
He further concludes:

“We have used a combination of science (in particular earth science and
geophysics), historical records, geography, ancient Hebrew, and the Old Testament
writings to reconstruct what happened in the final stage of the Exodus from Egypt at
the crossing of the river Jordan. I believe the evidence fits together so well that we
can say, beyond reasonable doubt, how and where the crossing occurred …

“Only one main point is missing from our reconstruction of the crossing of the
river Jordan. How many years ago did it occur?”

James et al. add the following to this: “In ten out of thirty recorded incidents,
including one in 1927, the mud-slides caused blocked the River Jordan and stopped
it flowing for one to two days. This knowledge could throw a different light on the
Jericho story: the fall of its walls [because it lies on a major geological fault-line]
and the damming of the Jordan may both have resulted from a disturbance along
the fault-line.”

The evidence here is historic and scientific and both correlate with each other
exactly in all respects except as to time. This is an acceptable interpretation of the
miracle of the crossing of the Jordan River by the ancient Hebrews. On the other
hand, the miracle of the crossing over of either the Sea of Reeds, the yam suph or
the Red Sea exhibits none of these connections. It would seem that the biblical
editors moved the miracle for the crossing of the Jordan to the one presented in
Exodus. It is extraordinarily difficult to accept that two water-crossing miracles of
a highly similar nature should have happened to these people each at a unique time
for unique, fortuitous natural reasons. And as we will see, the scientific evidence
for this Red/Reed Sea crossing has far too many problems related to the event to be
taken seriously.

The first problem is: where is the yam suph located? Like astronomical dating
to be precise we must know the site where an observation was made. There is a
second aspect of the evidence that comes into play. Does the scientific explanation
of the miracle hold water? In this regard, if there is a terrestrial explanation and
not an astronomical or a fictional one, that phenomenon will, like the mud slide
damming of the Jordan, have occurred again and again over the millennia

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159 ibid., pp. 24-5
160 ibid., pp. 26-7
161 James et al., op.cit., p. 164
separating the original one from these recurring others. That is the greatest problem facing these biblical miracles—historical replications of a natural event.

For example, the flood we described at Ugarit and Tiryns is far from a unique event in that region. Craig B. Smith describes one such wave in the Levant:

“On July 9, 551 [A.D.], in Lebanon, a very strong earthquake—its epicenter offshore from Byblos—devastated Beirut and caused the collapse of many buildings. It was followed by a tsunami in which the sea at first receded around 2 miles and then came roaring back to rush inland. A total of 30,000 persons died in the earthquake and tsunami.”

B.C. Papazachos and P.P. Dimitriu tell us with regard to tsunamis in Greece:

“About 70 major tsunamis, some of them disastrous, are known to have occurred in and near Greece since 479 BC, the year a big sea wave (the oldest reliably known tsunami) reportedly destroyed the Persian fleet at Potidaea, northern Greece. … The most devastating of all known tsunamis in the region, the sea wave of AD 365, caused the loss of thousands of lives and extensive damage to the whole eastern Mediterranean and is probably one of the world’s largest.”

Tsunami events are generally separated in time by many hundreds or even a thousand years and have been attributed to the gods as with the Deucalion or Ogyges events. In fact, Julius Africanus ties the flood of Ogyges with the Exodus. “We affirm that Ogyges from whom the first flood [in Attica, i.e. mainland Greece] derived its name, and who was saved when many people perished, lived at the time of the Exodus of the people from Egypt with Moses.”

With regard to the location of the *yam suph*, which in Hebrew means Sea of Reeds, this rather contradicts the Red Sea interpretation because “reeds grow only in freshwater rivers and lakes and not in saltwater seas … although salt-tolerant reeds and rushes exist that grow in slightly salty water, they cannot grow in very salty water like seawater.” In point of fact, Humphreys admits “the Red Sea … is in fact one of the saltiest seas in the world …” Rachel Carson shows: “The saltiest ocean water in the world is the Red Sea …” The translation of *yam suph* from Sea of Reeds to Red Sea occurred

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165 Humphreys, *op.cit.*, p. 173
166 *ibid.*, p. 174
167 *ibid.*, p. 182
“… when … Jewish scholars in Egypt in the third century B.C. translated into Greek
the Hebrew words … *yam suph* as *eruthra thalassa* which is Greek for ‘Red Sea’.”  
Humphreys assumes that these later Jewish scholars knew the true location of
the *yam suph* but chose the name “Red Sea”, and thereafter this interpretation
stuck. Bruce Feiler in his book *Walking the Bible: A Journey by Land through the
Five Books of Moses* (2001), calls this “the most famous mistranslation in history”:
“Inevitably, efforts to decipher where this event [the Re(e)d Sea crossing] took
place have preoccupied biblical readers for centuries. … It was the Greek
Septuagint, translated by the Jews in Alexandria in the third century B.C.E. that
introduced the most famous mistranslation in history, ‘Eruthra Thalassa,’ Red
Sea, for what should have been Sea of Reeds. This mistake was picked up by the
Latin Vulgate and embedded into English with the King James Bible in 1611. …
There are five main candidates for the Sea of Reeds: (1) the Mediterranean; (2) the
marshy region just south of the Mediterranean; (3) Lake Timsah; (4) the Bitter
Lakes [of the Sinai Desert]; (5) the Red Sea itself … Papyrus grows only in fresh
water which would seem to rule out the Mediterranean and the Red Sea.”

The fact is that reeds tend to grow just offshore around many seas and oceans, if
there is sufficient fresh water available to support such growth. Humphreys does show
that reeds grow in and around the Red Sea especially at the Gulf of Aqaba and
therefore accepts that the 3rd century B.C. translation by the Jewish scholars
supposedly removed by about 1000 years from the event, actually knew this to be a
fact. But the Bible, as was shown earlier, was written in Persian and Hellenistic times,
and these editors could not have known any such thing. The discrepancy between the
earlier words *yam suph* or Sea of Reeds may have been right while the later Hebrew
writers in Hellenistic times may have been influenced by the Greeks and wrote this as
*Eruthra Thalassa* or Red Sea. At this stage this cannot be known. Therefore, the exact
location of the *yam suph* is and remains unknown and speculative.

This being the case, any natural explanation for this event will vary with the
location. Humphreys has rather persuasively argued that the Hebrews’ passage
across the Red Sea occurred at the Gulf of Aqaba, and presents as his interpretation
that “All that night the Lord drove the sea back with a strong East wind (Exodus
14:21)” which blew the waters away from the shore, geologically and
oceanographically known as “setdown.” This would have caused the waters in the
Gulf of Aqaba to be pushed away from shore by about 800 yards, or 2400 feet. If
there was a ridge of submerged land closer to the shore the winds would have

169 Humphreys, *op.cit.*, p. 175
170 *ibid.*, p. 187
171 *ibid.*, pp. 191-4
created a second setdown heap of water that fits the biblical description of the Hebrews walking on dry land between two walls of water.

The problem with Humphreys’ natural explanation is that if this happened, say, 3500 years ago, it would have happened again and again and again, just like the earthquake mud slide that dammed the Jordan river. People would have observed this and told others, and scholars or oceanographers would have written about it. Biblical scholars would have gathered this and reported it as well. Humphreys’ analysis, though extremely well documented, fails to deal with this fact. Humphreys’ thesis thus does not hold water. His wind has only worked once at Aqaba but fails to do so again and again at Aqaba as he admits.

The second theory that explains this event would have the Hebrews crossing the Sea of Reeds somewhere in the Sinai adjacent to the Mediterranean. A.G. Galanopoulos and Edward Bacon offer this and it has been picked up by others to explain the crossing of the yam suph. It is based on the established chronology and also on the supposed great eruption of the volcano of Thera/Santorini. They directly state:

“An inscription found at El Arish, referring to a great Egyptian disaster, supports the views that the Sea of Reeds was the Sirbonis Lake—the lagoon between the towns of Romani and El Arish to the east of the Nile Delta. … Strabo gave its maximum length as 200 stades (22 miles) and its breadth 50 stades (5 ½ miles), and said that it was separated from the Mediterranean by a narrow sandy strip and that the lake communicated with the sea through a passage named Ekregma (gap)…

“When the huge caldera central part of Santorini [collapsed] the sea rushed in to fill the cauldron-like cavity … As a result the water ebbed away from all the East Mediterranean coasts. As the sea withdrew, the strip dividing the lagoon from the sea would certainly widen, the ‘gap’ would cease to exist and the lagoon for a while became completely separated from the sea. The Israelites, taking advantage of this opportunity, would then have been able to cross the lagoon, or, more precisely, pass over the new piece of dry land suddenly created at the gap mentioned by Strabo.”

To this Barbara J. Sivertsen answers: “Galanopoulos suggested that the … eruption of the Santorini (Thera) volcano … was responsible for the plagues … and the destruction of the Egyptian army in the Sirbonis lagoon on the northern coast of Egypt. … [A]rchaeological remains indicate that the land spit [sandy strip] over which the Israelites were said to have passed did not exist before the mid-first millennium B.C.E., well after any possible Exodus.”

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Dr. Hans Goedicke, Chairman of the Department of Near Eastern Studies at Johns Hopkins University, Baltimore, has also explained this event by way of a tsunami which withdrew the waters from these marshes or lakes (covered by reeds) to allow the Hebrews to pass, and then returned to drown Pharaoh and his army. Ian Wilson, in his book *The Exodus Enigma*, describes the Goedicke thesis:

"The Israelites were faced with a considerable dilemma. Having somehow become aware that an Egyptian army was in pursuit, they would have known that, with all their accompanying families, flocks, and baggage, they would never clear the frontier post ... before the fast-moving chariots with their deadly archers had overtaken them. With the Mediterranean to the north of them and the 'Reed Sea' and canal to the east, the only manoeuver to which their leaders could resort in the time available would have been to head for the nearest high ground, to give them an advantage in defense. As Goedicke noted, amidst the monotonous flatness of the [Nile] Delta, there is in this eastern area only one elevation that might have been suitable for this purpose, a hill called Tell el-Hazzob immediately to the south of the Israelites' route. Here, Goedicke suggested, they stationed themselves for what seemed certain annihilation when there occurred the totally unexpected: the 'Miracle of the Sea' of Exodus 14, in which a massive wave swept the Egyptians away before their eyes."

Goedicke held that this event was caused by the eruption of the island of Thera. But the problem with this scenario is that the eruption was quite small, as pointed out in volume II, pages 388-393 and 416-8, where we showed that the town of Akrotiri, one mile from the crater's rim, had many of its three-storey buildings still standing, in spite of the blast wave that should have knocked them down, or the earthquakes associated with violently erupting volcanoes should have toppled them. There were also Minoan buildings and graves at the very edge of the caldera which showed no sign of having been moved by this catastrophe, while geologists found petrified blue-green algae which must have lived inside the waters of the caldera, below sea-level, more than 10,000 years ago, meaning there was no mountain to blast away in Minoan times. And lastly, the rock face inside the caldera clearly had deeply eroded gullies caused by rainfall which would have taken several thousand years at least to erode, meaning the caldera was not formed only a few thousand years ago.

As for the supposed tidal wave, we showed that the sediments on Crete exhibited no evidence that they were laid down by a massive tsunami. All this scientific evidence being the case, there was no great tsunami that would have swept over the Reed Sea region. No one has shown these scientific facts that fully contradict all that has been attributed to the Thera eruption are in error. In fact,
certain theorists have attributed to this eruption both the great plagues of Exodus as well as the crossing of the Sea of Reeds. But to do so overlooks another fundamental scientific fact: tsunamis travel rather quickly, as Wilson points out: “A Japanese scientist, I. Yokoyama, has deduced that a tsunami from Thera could have reached Egypt in between sixty to ninety minutes.”175 The Israelites could not have crossed the Sea of Reeds in 90 minutes after the eruption and have, prior to that 90 minutes, experienced a whole series of plagues caused by that same eruption.

Then there is the unique coincidence that the fleeing Hebrews had two distinct miraculous water crossing events, one across the river Jordan, and another across the Sea of Reeds or the Red Sea; nature conveniently created an earthquake mud slide for one, and a tsunami for the other. Two such events are not probable or to be believed. The one that does have standing is the crossing of the Jordan river because the number of elements presented in the Bible all correlate with, and corroborate, one another. The crossing of the Red or Reed Sea can have no standing because specificity of place, time, and scientific evidence are missing and speculation is given too much room to scientifically explain this event.

What is being suggested is that the editors of the Bible writing in Persian and Hellenistic times fabricated this Passover event and may have had another such event as their model which they took for their own. They may have known of a truly similar event in Persian times that clearly is like a part of the scenario in which pharaoh’s army is drowned in the Red or Reed Sea. Sergei L. Soloviev et al. point to a tsunami that drowned a Persian army in Greece in the spring of 479 B.C.:

“After a series of defeats of the Persians in Greece, the Greek city of Potidaea, located at the base of the Pallene (Kassandra) Peninsula in the lowest place and blocking the entrance to the peninsula, rejected Persian rule. The Persian commander Artabaz besieged Potidaea which was surrounded by sea. After three months of siege, a strong and persistent drop in the sea level set in. The Persians saw that the coastal sand-bank had become a marsh and advanced along the Pallene coast. When they had covered two-fifth of the distance to Pallene through the marsh, [a] huge sea flood began, such as had never occurred here, according to [the] inhabitants, although high floods of tide had been observed quite often. Those who could not swim drowned, and [the] others were killed by inhabitants of Potidaea, who rowed up to them. The tsunami wave devastated the city.”176

This story was probably part of the news current in that part of the Near East and could have been the model upon which the Red or Reed Sea crossing was created.

In terms of Heinsohn and Sweeney’s revisions, the Hyksos who left Egypt en masse were composed of many different peoples and were not all slaves. John

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175 ibid., p. 135
176 Soloviev et al., op.cit., p. 22
Albert Wilson in this respect writes: “By the time the Hyksos entered Egypt, they must have included few of the original newcomers, but many Hurrians and Semites and other displaced persons from Syria and Palestine. The result was that their culture and those Hyksos names known to us show considerable mixture of various ethnic elements.”\(^{177}\) It does appear that toward the end of the Hyksos/Akkadian/Assyrian epoch a very large number of Palestinians and Syrians were moved by their overlords, or for economic reasons came to live in northern Egypt. Gösta Werner Ahlström, Gary O. Rollefson and Diane Vikander Edelman describe the demographic make-up of the Hyksos at the end:

“The Kamose stela reporting the expulsion of the Hyksos and the end of their rule uses the terms ‘Asiatics’ and the ‘ruler of Retenu.’ This clearly identifies the Hyksos as Palestinians. Because Palestine had a somewhat mixed population, this may also have been the case among the Asiatics in the Delta. A study of the names shows, however, that the majority are West-Semitic. The Palestinian origin of the Hyksos has partly been supported by archaeological finds, even though the so-called Tell el-Yehudiyyeh ceramics can be questioned as being of Syro-Palestinian origin. However, pottery and objects from the graves at Tell el-Dab’a … are typologically identical with those found in M[iddle] B[ronze] II sites in Palestine and Syria.”\(^{178}\)

Historians have tended to be in either of two camps with regard to the Hyksos and the Exodus. In one camp, the Hyksos are in no way related to the Hebrew Exodus; the other camp concludes the Hyksos were the Hebrews and the Hyksos departure from Egypt was really that of the Israelites. Only Heinsohn and Sweeney, so far as I know, have maintained the Hyksos were the Akkadians/Assyrians with their various allies and conquered peoples, whom they sometimes deported en masse. Among these peoples were Palestinians and Syrians. Ann Rosalie David describes the situation:

“Josephus mistakenly interpreted the hieroglyphic word for the Hyksos to mean ‘shepherd kings’ or ‘captive shepherds,’ although it is now known that this word should be translated as ‘chieftains of foreign lands.’ He concluded that the Hyksos invasion represented the descent of the Hebrews into Egypt, and that the period of Hyksos rule should be equated with the Hebrews’ sojourn there; finally he identified the biblical Exodus with the final expulsion of the Hyksos by the local princes of Thebes … However, there is no other evidence to support this theory, and apart from the chronological problems it poses, there is also a major discrepancy in that the Hyksos were driven out of Egypt, while the Exodus represented the flight by people who desperately wished to leave Egypt …”\(^{179}\)

\(^{177}\) John Albert Wilson, *The Culture of Ancient Egypt* (Chicago IL 1956), p. 161

\(^{178}\) Gösta Werner Ahlström, Gary Orin Rollefson, Diane Vikander Edelman, *The History of Ancient Palestine from the Paleolithic Period to Alexander’s Conquest* (Minneapolis MS 1993), p. 192

Note that with both possibilities regarding the Hyksos there is no knowledge of who they were nor where their homeland was, and in the case of the Hebrews exactly when they arrived in Egypt nor how they actually obtained their emancipation. By putting these two different peoples together, that is Hyksos/Akkadians/Assyrians with Syrians, Palestinians, and whatever others came to Egypt under their overlordship, we can resolve not only the chronological problem that Ann Rosalie David just mentioned, but also the history, and give to each its proper, rightful place in that history and chronology. Ralph Ellis, a critic of Velikovsky and researcher, has come to the conclusion that the Hyksos were the Israelites and that the close affinities of their histories prove this connection:

“A summary of the events leading up to the exodus is perhaps required at this point. We know, from both the historical and biblical records, that the people of Egypt thought that the gods were angry during this period [and caused a catastrophe]; both the *Tempest Stele* [of Ahmose I] and the Bible appear to talk of great storms deluging the otherwise arid lands of Egypt.

“We also know that there were tensions between the Theban pharaohs and the Hyksos pharaohs, and likewise between the Egyptian pharaoh and the Israelites … Furthermore, we know that both the Hyksos and the Israelites were thrown out of Egypt, and that both these events involved a battle with the Egyptian army. Finally, both the entire Hyksos and the entire Israelite populations embarked on an exodus towards [Palestine]—the Egyptian historian Manetho even indicates that the destination of the Hyksos refugees was Jerusalem.

“The similarity between these two historical events is perfectly obvious and so it should not be surprising that someone [including Heinsohn and Sweeney] should propose that they are, in reality, one and the same event. But even if they were the same event, what is not quite so certain is whether this exodus was initiated by a simple pitched battle followed by a hasty retreat, or whether there was some kind of a treaty signed and a more orderly withdrawal initiated.”

To this Siro Igino Trevisanato further adds: “Many ancient writers had claimed that the two flights [Hyksos and Exodus] coincided. The Jewish historian Josephus (1st century AD) equated Avaris with the city from which the exodus took place (*Against Apion* 1.14). This view was shared by Clement of Alexandria (2nd century AD), who stated that the exodus took place at the time Ahmose ruled Egypt (*Stromata*, 21). The same thought is found in Tatian (2nd century AD), who adds that Ahmose was a contemporary of the Greek ruler Inachos (*To the Greeks*, 38).

“Lending support to these ancient statements is recent archaeological evidence from Avaris [from where the Hyksos left their Egyptian capital *en masse* for their fortified towns in Canaan.

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180 Richard Ellis, *Tempest and Exodus*, op.cit., p. 62
“In his book *Avaris, The Capital of the Hyksos* (1996) … Prof. Manfred Bietak … argues that the Hebrew exodus may preserve the memory of thousands of refugees leaving Avaris as Ahmose entered the city. In fact, the biblical text itself states that at the time the Hebrews left, many non-Hebrews also left with them (Ex. 12:38).”

If the Hebrews were part of the Hyksos contingent in Egypt and not an impoverished, enslaved people, then their departure should be that of a capable, militarily developed nation and not a rag-tag group of militarily ignorant slaves. On this, Richard Gabriel puts forth:

“One of the difficulties confronting the historian concerned with the military history of ancient Israel is where to begin. When the subject of Israel’s military tradition is addressed, it usually begins with Joshua and the Israelite conquest of Canaan. Just why this should be the case is unclear since even a cursory examination of Exodus reveals more than a few examples of the military art practiced with an expertise quite sufficient to hold the attention of any serious student of strategy and tactics. What is the crossing of the Reed Sea if not a textbook example of how to conduct a night water crossing? Or Joshua’s skirmish with the Amalek[ites] if not an example of how to fight a rear guard action while protecting the withdrawal of a column? And the desert trek? One can only marvel at the Israelite logistics officers who managed to move large numbers of people through a desert wilderness while keeping them alive until they reached their destination …

“For the most part Exodus has been presented to the world not by military historians but by theologians and cultural historians. It has been the theological writers who have taken the lead in portraying Exodus in a manner sometimes ignorant of military matters. …

“… as expected slaves do not develop military traditions or great warriors … General Sir Richard Gale in his Great Battles of Biblical History captures fully the image of Israelites as runaway slaves making their way inexpertly across the desert.

“'The long trek over the arid desert was a grim undertaking for a tired and undernourished people. Thousands of them, old and young, men and women, with their children, their sick and their lame, with their goats and their donkeys, their cooking pots and rough black tents must have looked a sorry sight.’

“Under these circumstances, who else, one wonders, but God himself could have rescued such a disorganized lot from themselves.

“These images passed easily into the common understanding of the Israelite saga with the consequence that Exodus has been over-looked as a genuine source of military history.”

Were there slaves in Egypt? is the question. On this, Gabriel writes:

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181 Trevisanato, *op.cit.*, p. 108
“Three factors worked against the development of slavery in Egypt. First, with a population of 7 to 9 million at the time of the Exodus there was never a shortage of manpower for any military or governmental project ... Second, Egypt was a highly developed administrative state and possessed the ability to organize and deploy manpower for social tasks on a colossal scale ... Third, Egyptian religion and law forbade slavery, just as it extended equal legal and religious standing to women and children. As Rostovtzeff notes, Egyptian peasants had legal rights under the law which, when violated, often led to labor strikes. If working conditions on the land or construction projects became overly harsh, workers would strike and take refuge in religious temples ... It is somewhat curious, then, that the Israelites should have remembered being enslaved in a country that did not practice slavery on any scale.”

However, the Hebrews in Hyksos Egypt were not under Egyptian rule but under Assyrian rule, and Assyrians were well know to enslave conquered peoples and move them en masse to Mesopotamia. The Assyrians brought both allies and slaves to Egypt to maintain their hold on the land. As we have seen earlier, in the Sinai at a turquoise mine for this period, in the short chronology the 8th century B.C., an enslaved Hebrew wrote: “El save me from this mine.” This was indeed slavery. On the other hand, many Hebrews held positions of power, just as the Scythians did at Ur. The placing of the Hyksos/Akkadians/Assyrians in Egypt explains how some Israelites returning as members of the Hyksos/Israelite Exodus had experienced hard labor. Saul Friedman explains:

“Slavery remained [even] when the Chaldeans ruled Babylon. As A.T. Olmstead writes, ‘Slave sales form the largest single group of our documents...’ Armed struggles among peoples attempting to prove which of them was the fittest provided a continuous supply of slaves. Victorious warlords showed little mercy to the vanquished. Monumental steles and orthostats of Assyrian monarchs reveal a general policy of annihilation and [slave] deportation. Tukulti-Ninurta I (1242-1206), for example, boasts how he deported 10,000 captives from Syria and dragged the king of Babylon to Ashur in chains.”

On the other hand, some Palestinians rose to positions of power. Kurinsky reports on artifacts of the Hyksos that:

“The Metropolitan Museum of Art in New York has, for example, no less than eight scarabs of one official bearing the distinctly Semitic name Hur, which means ‘the Noble’ or the ‘freeborn,’ as in the familiar Hebrew name Ben Hur, ‘the son of Hur.’ Some of these administrators or ‘viziers’ are entitled ‘Sole Companion of the King,’ others are entitled ‘Overseer of the Treasury.’ These scarabs are found in association, for example, with those of King Sheshi, the purported founder of

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183 ibid., p. 61
184 Saul S. Friedman, Jews and the American Slave Trade (New Brunswick NJ 1998), p. 25
the Fifteenth [Hyksos] Dynasty, and have been recovered from sites throughout Egypt ranging from Canaanite Gaza to Kermen in Nubia. Numerous other Scarabs and seals of both Egyptian and Asiatic officials with similar titles testify to the importance of the office during this period.

“Many of the [Hyksos] kings bore distinctly Semitic names: Anath-her, for example, was the Egyptian rendering of the Semitic goddess Astarte. Some have recognizable Hebrew names like Yaakov, Hiyan, Hamudi. The king with the unmistakable Hebrew name Yaakov, as Jacob is pronounced in Hebrew, provides a parallel to the biblical name of the father of Joseph. The name Yaakov appears on numerous scarabs of the period found in Egypt, in Nubia (biblical Kush) and in Canaan, in the form of the Egyptian transliteration $Y'qb-HR$.\[185\]

Thus, the Hyksos employed both Egyptians and Palestinians, Syrians as well as others loyal to them to conduct their affairs in Egypt. They were employed throughout that society from slaves to pharaohs.

This ultimately brings us to the catastrophe connected to the Exodus which we equate with the time of the earthquake swarms that devastated the ancient Near East and the floods of Mesopotamia, the Harappan civilization, the great Chinese flood, the floods at Ugarit and Tiryns and possibly along the shores of Israel. Velikovsky employed the Ipuwer Papyrus as the connecting link of Egyptian documentation with the plagues of the Exodus. In many respects I am in agreement with that connection. However, if there truly was a great catastrophe that occurred prior to the Hyksos and their various allies leaving Egypt, one would expect to find the pharaoh ruling in Thebes to have experienced it as well and, having found it so overwhelming, to have commemorated it for posterity. The pharaoh who did drive the Hyksos out of Egypt was Ahmose I and he did commemorate this horrifying event with a stela. Ellis describes the event, speaking of

“The Tempest Stele was erected by the Pharaoh Ahmose I at the beginning of the eighteenth dynasty of Egypt ... The stele derives its dramatic title from the great storms that it details, which apparently struck Egypt during this pharaoh’s reign. ... Ahmose I was [an] Upper Egyptian (Theban) pharaoh ...

“Climatically speaking, southern or Upper Egypt can be thought of as being in the midst of the Sahara desert. Although the occasional [monsoon] desert storm may create a flash flood in this region every few decades or so, the area is otherwise bone dry. Pharaoh Ahmose’s account of a raging nationwide tempest of [supposed] rain, continuing without cessation and being louder than a waterfall at Aswan, can therefore be considered highly unusual in this region. ...

“The Tempest Stele says of this storm:

\[185\] Kurinsky, op. cit., p. 67
“now then … the gods declared their discontent. The gods (caused) the sky to come in a tempest of rain, with darkness in the western region and the sky being unleashed without (cessation, louder than) the cries of the masses, more powerful than (…), (while the rain raged) on the mountains louder than the noise of the cataract which is at Elephantine. Every house, every quarter that they reached (…) floating on the water like skiffs of papyrus opposite the royal residence for a period of (…) days, while a torch could not be lit in the Two Lands (of Egypt). Then his majesty said: “How much greater this is than the wrath of the great god, than the plans of the gods!”

“[There was, according to Ellis, H]eavy rain for days on end, darkness, loud noises and all the lamps extinguished in Egypt …”

Ellis assumes that the tempest was a massive rain storm, but the stele does not use the word rain. Historians have filled in these breaks in the writing with their own interpretation as can be seen where brackets or parentheses are placed.

Trevisanato adds the following information about the Tempest or Storm Stela:

“The storm stela text was published and commented on by Karen Polinger Foster and Robert K. Ritner as part of ‘Texts, Storms, and the Thera Eruptions’ in the Journal of Near Eastern Studies 55 (1996): 1-14. The two authors state that the storm mentioned on the stela could be linked back to the aftermath of the Santorini eruption, and thus ought to be part of the biblical plagues, albeit in an area where the Hebrews did not live. Other scholars such as Malcolm H. Wiener and James P. Allen disagree. In ‘Separate Lives: The Ahmose Stela and the Thera Eruption,’ also published in the Journal of Near Eastern Studies 57 (1998): 1-28, it is pointed out that the storm took place in the southern part of the country and can be linked to standard monsoons, which at times also hit southern Egypt. …

“Thus, here we do have a real and huge disaster, which took place in Egypt and is attested in Egyptian records. More precisely, we have here a huge storm which came from the west and devastated southern Egypt around Dendera and Thebes …”

The nature of the storm is taken to be that of a deluge of rain. The Tempest Stela, however, does not use that word, the first reference in it to the material is, as Ellis cited the text, only the letter “r”. The second reference is in brackets which means the entire determination of what fell from the sky was assumed—and only assumed—to be rain. Associated with what fell is noise “louder than the cataract which is at Elephantine.” I ask the reader to consider the roar of a mighty waterfall, and try to conjure up a downpour being louder. This is hardly possible. But the second phenomenon associated with what fell from the sky is “darkness,” and also thereafter “the torch could not be lit.” Although rainfall showers are accompanied by darkened skies, this darkness was so much greater than any known that the

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186 Ellis, op.cit., pp. 47-48
187 Trevisanato, op.cit., pp. 108-109
pharaoh believed this event was “much greater … than the wrath of the great god, than the plans of the gods.” Furthermore, the darkness lasted for a long enough time to leave this great impression on the people.

The question is: what will produce noises or blasts so loud and at the same time engender darkness for a considerable time? According to Trevisanato and others it is the eruption of Thera/Santorini, between 500 and 750 miles to the north. However, the blast of a volcanic eruption lasts only a short time and not for a prolonged period. The other possible thesis is that a vast meteor shower fell on Egypt, creating sonic blasts that knocked down houses and killed people, etc. In order to address this question, we must deal with the plagues associated with this event.

The turning of the Nile red is directly and naturally associated with many of the plagues that followed, as reported by Greta Hort’s “The Plagues of Egypt,” ZAIO, vol. 69 (1957), pages 84-303. Barbara J. Sivertsen summarizes her thesis:

“In 1957 one ecologically minded scholar, Greta Hort, saw the plagues as disturbances in the ecology of the Nile, triggered by exceptionally strong July and August Nile flooding that brought down blood-red flagellates from the mountain lakes of Ethiopia, along with larger than normal quantities of the reddish sediments from the Abyssinian Plateau. These flagellates, Euglena sanguinea, took oxygen from the river water, which killed the fish and brought on flies [that fed on their floating bodies]. This drove the frogs from the river not long before the high flood levels [which permitted mosquitoes to lay lots of eggs and] produced a lot of mosquitoes. Unfortunately, the frogs had contracted anthrax and spread it to animals and people, producing more of the plagues. Hail, coming in early February … destroyed the flax and barley, locusts blew in from Arabia, and a dust storm produced the exceptional darkness of the ninth plague. Hort didn’t explain the pillar of cloud and fire, however. In fact, large amounts of sediment from Ethiopia show up during low Nile floods, not high ones. More importantly, the vicissitudes of the Nile floods and their effects would have occurred in other years [from the same sediments] and would thus have been regarded as ordinary events, whereas the Exodus portrays the water turning to blood as an extraordinary, one-time-only event.”\(^{188}\)

Here, then, Sivertsen has placed her finger on the problems related to all accounts of the Exodus, just as we have seen with the Israelites’ crossing of the Jordan River because of earthquake mud slides. If the phenomena of the Exodus were the result of some terrestrial natural event, it would have happened again, not necessarily in Egypt, but in other regions of the Earth subject to the same conditions. This is the overwhelming problem and contradiction facing any other terrestrial natural explanations of the Exodus.

\(^{188}\) Sivertsen, *op.cit.*, p. 6
For example, if Theran volcanic ash was responsible for the plagues, then other volcanic eruptions on Earth over the past 3000 or more years would have deposited in other regions acidic or other types of ash in rivers that not only turned the waters there red, and killed off the fish, but would have driven the frogs onto the land to escape the poisonous waters, to die and be eaten by all manner of insects. If, as some researchers suggest, some sort of algal bloom such as Dinoflagellates, which become red tides, were responsible, or other toxic algae did so, then these would have done the very same thing somewhere in rivers on Earth over the past 3000 or more years, leading to dead fish, frogs on land eaten by swarms of insects that lay eggs on their carcasses to create a plague of gnats, flies, or whatever. Again, those who advocate such terrestrial hypotheses, never point to another place on Earth that experienced all these same phenomena. To repeat, if the cause of the plagues was a natural terrestrial event, it would have happened at other times, not only in Egypt but elsewhere. Why didn’t the Tigris or Euphrates rivers suffer from the supposedly immense Theran eruption, or the rivers in Anatolia, or Greece itself? The obvious logical, scientific and evidential answer is that these regions were not affected because no such long-chain effects arise from volcanic eruptions. The entire list of terrestrial causes fails to meet this criterion of repeatability. And to the best of my knowledge, no one has ever taken it up or attempted to explain this away on scientific grounds. Their hypotheses work wonderfully in the laboratory of the various researchers’ minds, but fail to work in the laboratory of the natural world. As Wallace Thornhill, a physicist from Australia, once sharply remarked about Einstein’s “Gedankenexperiment” or “thought experiment,” “A thought experiment is not an experiment!” A scientific experiment to be valid must be replicable, and thus all these terrestrial concepts have failed to pass the most basic scientific test of replicability. They are, therefore, not scientific.

And here is the tragedy. Eminent historians have parroted these concepts and spoken of them as if they were scientific fact. For example Kitchen writes:

“Of all the modern treatments of the phenomena in the text, the most straightforward was given by G. Hort some time ago. From known geographical, climatic, ecological, microbiological, and medical phenomena, she was able to demonstrate a clear sequence of events through the ancient Egyptian year ...”\(^{189}\)

Invoking geography, climatology, ecology, microbiology, and medicine, all scientific disciplines, without replication proves nothing, but makes a strong impression on the reader that science upholds these biblical events, when in point of fact it denies them. There is, however, a fundamental difference between the report in the Tempest Stela and those earthquakes discussed earlier, namely that

the Egyptian catastrophe was one of total darkness and deafeningly loud, explosive noise. These are not reported anywhere else in the ancient world of that same time. Therefore, the catastrophe was of a totally different nature. The Tempest Stela specifically states “The gods [caused] the sky to come in a tempest …” That is, the cataclysm was not an earthquake but a phenomenon that came out of the sky. It could not be a rain storm which would cleanse the air and thus not create darkness, but something else that would produce a shockingly loud, frightening noise and darken Egypt with a material for a fairly long time. It could not be a sand storm, since such an event would not create deafening and frightening noises. Furthermore, it produced events which will be discussed below that were unique in the annals of history. The event can well be explained by an immense meteor or cometary stream or both, that showered down across Egypt, generating immense air blasts in the sky and creating with it billions of tons of fine dust that shrouded Egypt in darkness. That is, thus far, what we can gather from the Tempest Stela.

Evidence of the debris of these meteorite falls has not been presented but has been found in Mesopotamia as described by W. Bruce Masse:

“Archaeologists are sometimes confronted with evidence for what appears to be rapid destruction within individual archaeological sites and occasionally across large regions. The typical default conclusion is that this represents the destructive forces of a conquering army and/or some other concurrent destructive natural forces such as large-scale earthquakes and massive volcanic eruptions or perhaps rapid climate change. A prime example of such an abrupt event is associated with the end of the Akkadian [Hyksos/Assyrian] empire … A number of large urban cities contain evidence of widespread and apparently synchronous social collapse and destruction at around this time … This had been modeled as abrupt climate change (aridification) associated with volcanic ash fall as represented in a thin but widespread dust layer (Weiss et al. 1993). However, a number of researchers and archaeologists have raised serious doubts about the suggested physical causes and [sic] as well as the timing of the event (Peiser 2003).

“More recent microstratigraphic examination of this dust layer and its context suggest[s] an impact [cosmic] origin together with a significant revision of chronology (Courty 1998). As reconstructed by Courty at Tell Leilan (Syria), the dust layer sits on top of an occupational surface possibly deformed by a shock wave. This surface exhibits evidence of the rapid propagation of wildfire … The dust layer contains tiny rock fragments from various contexts (sandstones, basalts, marine limestone, gabbros) along with numerous glassy microspherules of varying mineralogical compositions and glassy grains derived from vaporized rocks. The shocked and burned occupational layer and overlying dust layer are themselves sealed with mud from a heavy rainfall. Thus what had been originally considered a [volcanic] tephra fall now appears to be impact ejecta … Courty (2001) has also
examined soils at Tell Brak (Syria), and has modeled a similar sequence that took place very rapidly. Although most scholars remain skeptical of Courty’s impact interpretation, the model fits well with the data from surrounding regions (Masse 1998).

“A problem when dealing with the study of microspherules (Raukas 2000) is that many different sources exist for such material including terrestrial (diagenic, biogenic, industrial, volcanic), extraterrestrial (interstellar and interplanetary dust, METEORITIC AIRBURSTS) …

“The message from this study and from the two examples [Tell Leilan and Tell Brak] are (1) airbursts and tektite strewn fields are poorly known [or recognized] and [therefore not] documented … (2) few people, including those in the Quaternary geosciences are trained to recognize and deal with potential tektites impact glass melts, and microspherules–archaeologists are woefully lacking in this regard; and (3) the use of microstratigraphic methods and distributional studies are vital for determining the nature and context of impact glasses and other impact products.”

The implication is that Egypt may not have been the only area that experienced a great meteor stream and airbursts at this time. What we do suggest is that this type of event was also directly associated with earthquakes, fires, floods, and climate change.

Let us first examine the evidence of the various phenomena discussed in the Tempest Stela as these relate to the Hyksos/biblical Exodus. The first is that there was extraordinarily loud noise unlike any others that caused the stela to be commemorated. Velikovsky dealt with this when he answered Carl Sagan’s critique of his book Worlds in Collision, wherein Velikovsky wrote:

“‘Meteorites when entering the earth’s atmosphere, make a frightful din’ [to which Sagan wrote] ‘when they are generally observed to be silent;’ … [to which Velikovsky responded] The Smithsonian Institution published in 1929 a volume on Minerals from Earth and Sky. George P. Merill, Head Curator, Department of Geology, U.S. National Museum, contributed ‘The Story of Meteorites’, in which he gives a long series of reports of loud explosions accompanying the fall of meteorites. Meteorites are a subject that belongs to Sagan’s own field, but he does not know they can make noise. For example, in Emmet County, Iowa, on May 10, 1879: ‘The sounds produced by the explosions incidental to its [the meteor’s] breaking up were referred to as terrible and indescribable. … The first explosion, for there were several, was louder than the loudest artillery.’ This is only one of a number of illustrative cases described by the Smithsonian Institution. So silent when entering the atmosphere they are not, Sagan notwithstanding.”


The frightfulness of these explosions is described by Fritz Heide and Frank Wlotzka thus:

“Still more impressive and more terrifying than the remarkable light [in the sky] phenomenon are the sound phenomena that are associated with meteorite falls. They are so terrifying that people have fallen down from fright or have immediately panicked and sought cover in buildings or under trees. Depending on the location of observers, there is a whole scale of sounds that may be experienced, from thunder-like claps, that rattle windows, to cannon detonations and small weapons fire, or the sounds of roaring trains.”\(^{192}\)

The same applies to cometary debris which explodes before striking the ground, which is called “airbursts.” The Tunguska event is believed to have been created by the airburst of a small comet which produced a great explosive noise over a vast area. John S. Lewis gives an eyewitness account of the noise of the Tunguska event: “The noise deafened his brother and the shock caused him to suffer a long illness.”\(^{193}\) According to Reed Wicander and James Monroe, “The noise from the explosion was heard up to 1000 km [620 miles] away…”\(^{194}\) The same can apply to a meteorite fall. As Lewis also shows:

“A fireball the brightness of the full Moon was seen in Quebec City, Montreal, Boston, and in parts of New Hampshire, Rhode Island, Pennsylvania, Vermont, and Maine. Two powerful explosions were heard from Portland Maine to Albany, New York.”\(^{195}\)

Given that these explosions of light and noise are very similar to lightning and thunder and were described in the Tempest Stela as coming from the sky, historians concluded this was a thunderstorm and not a meteor or cometary shower. But the “darkness” that was so great, which followed these phenomena, would not and could not be produced by such a downpour. It could be and is directly associated with meteor or cometary airbursts. Vitaly Adushkin and Ivan Nemchinov present evidence of just such an event in Brazil for 13 August 1930:

“… according to eyewitnesses, the event began from dustiness and smoke generation in the atmosphere and the fall of [what is assumed to be a forest fire] ash (the sun turned red, darkness covered everything, reddish dust appeared) … Only some time after this were sounds and explosions heard … Ash continued to fall and darkness remained for several hours …”\(^{196}\)

At this point we have evidence for the lights in the sky, the thunderous noise and the encroachment of darkness, aligned with the Tempest Stela and with the


\(^{193}\) John S. Lewis, *Rain of Iron and Ice* (Reading MA 1997), p. 51

\(^{194}\) Reed Wicander, James S. Monroe, *Historical Geology* (Pacific Grove CA 2000), p. 172

\(^{195}\) Lewis, *op.cit.*, p. 126

\(^{196}\) Vitaly Adushkin, Ivan Nemchinov, *Catastrophic Events Caused by Cosmic Objects* (London 2008), p. 228
Hyksos and Israelite Exodus. It is also at this point that we wish to remind the reader that the dustiness of the atmosphere was such that from the “ash (the sun turned red)…” which indicates that meteoric or cometary ash and/or dust can be of a reddish hue. Velikovsky suggested it was a kind of rust which fell. Critics have, again like the historians, interpreted this rust colored material to be terrestrial rust which would not color and poison the Nile to turn the water red, nor, more importantly, kill the fish. But this was not a terrestrial mineral or chemical which fell; it was of extraterrestrial origin and its chemistry could well have been of a mildly poisonous or acidic nature. This we will further examine below.

It is to the Ipuwer Papyrus we turn now to examine the nature of the plagues of the Exodus. The first question is to determine when it was written. In terms of the established chronology it was written in the 1300s B.C. and copied around 1200 to 1075 B.C. But in terms of the short chronology it was originally written in the Hyksos/Akkadian/Assyrian times ca. in the 8th century B.C. There is, however, a significant statement in the Ipuwer Papyrus that proves it was written when the Minoans of Crete existed with the people of Byblos, as well. According to the established chronology it was in Hyksos times that trade was cut off between southern Egypt and Crete and Egypt and Byblos, and Ipuwer specifically states this. Charles Pellegrino cites Ipuwer thus: “No one really sails north to Byblos [Lebanon] today, What shall we do for cedar for our mummies? … Men of Keftiu [Minoans from Crete] come no longer.”

The Minoan civilization according to the established chronology was gone when the original document was supposedly written in the 1300s B.C., as Trevisanato explains:

“One thing we can be sure of is the fact that the text is not original, but a copy of an older text. The text as it exists was written by a scribe around 1200-1075 B.C. The scribe, however, left several spaces blank as if he could not read the original words from the manuscript [which] he was copying [or these were mutilated]. Had the scribe been the original writer of the text he would not have left the blanks, but would have written out whole sentences. Additionally, the text mentions Keftiu, a word known to designate the Minoan world [which ended 1600-1525 B.C.]. However, Greece at the time was going through a so-called dark age. To make things worse, earlier mentions of Keftiu in Egyptian documents stop with the city list of the [18th Dynasty] funerary temple of Amenhotep III, that is, around 1350 BC. If the scribe had composed an original text [of the original

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Ipuwer document] he would have used an anachronism, for Egyptian records had not used the word *Keftjw* [around 1350 B.C.] for over 150 years."**198**

If Minoan Crete fell around 1600-1525**199** as Trevisanato and other historians suggest, how could trade with it be cut off supposedly about 1350 B.C.? The only way Ipuwer would have known that trade was stopped is to have lived in Hyksos times. *Keftiu/Keftjw* means Minoan Crete and nothing else. It is basically impossible, in terms of the established chronology, for this to have been known when the original Ipuwer document was written. Nor could it have happened at all based on that same chronology.

When we compare the established chronology with that of the short one, we have the Minoans of Crete living up to around 1600-1525 B.C.: moving them into the Hyksos/Akkadian/Assyrian epoch’s near end means that they were actually living almost 800 years closer to the present. As Trevisanato reports:

“The *Ipuwer Papyrus* which … described … the red waters of the first plague, sadly notes that the Aegean no longer traded with Egypt … Ipuwer’s statement is confirmed by archaeological digs: while Aegean/Minoan artifacts had been numerous [supposedly] from 1750 BC onwards, the number plummets close to nihil by 1600/1525 BC as detailed by Peter Warren (“Minoan Crete and Pharaonic Egypt,” in *Egypt, the Aegean and the Levant* (1995) …”**200**

Therefore, the Ipuwer Papyrus lines up directly with the time of the Tempest Stela catastrophe in the 8th century B.C. and the expulsion/Exodus of the Hyksos and their Palestinian, Syrian, and other allies from Egypt in that same century. At this point we give leave for Velikovsky to present the evidence of the direct correlations between the Hebrew Bible’s Exodus and that presented in the Ipuwer Papyrus; evidence that Charles Pellegrino admits “The similarities between Ipuwer’s lament and the story of Exodus (as told by Josephus and by ancient Hebrew scribes) are nothing short of striking.”**201** Of course, though he has made a similar statement in his book *Unearthing Atlantis*, Pellegrino never mentions Velikovsky first bringing these similarities to the public.

“The Papyrus Ipuwer is not a collection of proverbs (Lauth. Chabas) or riddles (Brugsch); no more is it a literary prophecy (Lange) or an admonition concerning profound social changes (Gardiner, Sethe). It is the Egyptian version of a great catastrophe.

“The papyrus is a script of lamentations, a description of ruin and horror.

“*PAPYRUS 2:8* Forsooth, the land turns round as does a potter’s wheel.

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**198** Trevisanato, *op. cit.*, p. 136. See also p. 130

**199** *ibid.*, p. 130

**200** *ibid.*

**201** Pellegrino, *op. cit.*, p. 134
“2:11 The towns are destroyed. Upper Egypt has become dry (wastes?).
“3:13 All is ruin!
“7:4 The residence is overturned in a minute.
“4:2 … Years of noise. There is no end to noise.’

“What do ‘noise’ and ‘years of noise’ denote? The translator wrote: ‘There is
clearly some play on the word hrw (noise) here, the point of which is to us
obscure.’ Does it mean ‘earthquake’ and ‘years of earthquake’? In Hebrew the
word rash signifies ‘noise,’ ‘commotion,’ as well as ‘earthquake.’ Earthquakes are
often accompanied by loud sounds, subterranean rumbling and roaring, and this
acoustic phenomenon gives the name to the upheaval itself.

Apparently the shaking itself returned again and again, and the country was
reduced to ruins, the state went into a sudden decline, and life became unbearable.

“Ipuwer says:

‘PAPYRUS 6:1 Oh, that the earth would cease from noise, and tumult (uproar)
be no more.’

“The noise and the tumult were produced by the earth. The royal residence
could be overthrown ‘in a minute’ and left in ruins only by a mighty earthquake.
The upheaval seems to have wrought havoc on the high seas, where ships were
thrown into whirlpools; in the passage where ‘the towns are destroyed,’ it is also
said that ships were set adrift.

“The papyrus of Ipuwer contains evidence of some natural catastrophe
accompanied by earthquakes and bears witness to the appearance of things as they
happened at the time.

“I shall compare some passages from the Book of Exodus and from the
papyrus. As, prior to the publication of Worlds in Collision and Ages in Chaos, no
parallels had been drawn between the Bible and the text of the Papyrus Ipuwer, the
translator of the papyrus could not have been influenced by a desire to make his
translation resemble the biblical text.

‘PAPYRUS 2:5-6 Plague is throughout the land. Blood is everywhere.’
‘EXODUS 7:21 … there was blood throughout the land of Egypt.’
“This was the first plague.
‘PAPYRUS 2:10 The river is blood.’
‘EXODUS 7:20 … all the waters that were in the river were turned to blood.’
“This water was loathsome, and the people could not drink it.
‘Papyrus 2:10 Men shrink from tasting–human beings, and thirst after water.’
‘EXODUS 7:24 And all the Egyptians digged round about the river for water to
drink; for they could not drink of the water of the river.’
“The fish in the lakes and rivers died, and worms, insects, and reptiles bred
prolifically.
‘EXODUS 7:21 … and the river stank.
“‘PAPYRUS 3:10-13 That is our water! That is our happiness! What shall we do in respect thereof? All is ruin!’

‘The destruction in the fields is related in these words:

‘EXODUS 9:25 … and the hail smote every herb of the field, and brake every tree of the field.’

‘PAPYRUS 4:14 Trees are destroyed

‘EXODUS 9:23-24 … the fire ran along upon the ground.
… there was hail, and fire mingled with hail, very grievous.’

‘PAPYRUS 2:10 Forsooth, gates, columns and walls are consumed by fire.’

‘The fire which consumed the land was not spread by human hand but fell from the skies.

‘By this torrent of destruction, according to Exodus,

‘EXODUS 9:31-32 … the flax and the barley was smitten: for the barley was in the ear, and the flax was boiled.

‘But the wheat and the rye were not smitten: for they were not grown up.’

‘It was after the next plague that the fields became utterly barren. Like the Book of Exodus (9:31-32 and 10:15), the papyrus relates that no duty could be rendered to the crown for wheat and barley; and as in Exodus 7:21 (‘And the fish that was in the river died’), there was no fish for the royal storehouse.

‘PAPYRUS 10:3-6 Lower Egypt weeps. … The entire palace is without its revenues. To it belong (by right) wheat and barley, geese and fish.’

‘The fields were entirely devastated.

‘EXODUS 10:15 … there remained not any green thing in the trees, or in the herbs of the fields, through all the land of Egypt.’

‘PAPYRUS 6:3 Forsooth, grain has perished on every side.

‘5:12 Forsooth, that has perished which yesterday was seen. The land is left over to its weariness like the cutting of flax.’

‘The statement that the crops of the fields were destroyed in a single day (‘which yesterday was seen’) excludes that drought, the usual cause of a bad harvest; only hail, fire, or locusts could have left the fields as though after ‘the cutting of flax.’ The plague is described in Psalms 105:34-35 in these words: ‘… the locusts came, and caterpillars, and that without number. And did eat up all the herbs in their land, and devoured the fruit of their ground.’

‘PAPYRUS 6:1 No fruit nor herbs are found … hunger.’

‘The cattle were in a pitiful condition.

‘EXODUS 9:3 … the hand of the Lord is upon thy cattle which is in the field … there shall be a very grievous murrain.’

‘PAPYRUS 5:5 All animals, their hearts weep. Cattle moan. …’

‘Hail and fire made the frightened cattle flee. …
“EXODUS 9:19 … gather thy cattle, and all that thou hast in the field …
“21 And he that regarded not the word of the Lord left his servants and his
cattle in the field.’

“PAPYRUS 9:2-3 Behold, cattle are left to stray, and there is none to gather
them together. Each man fetches for himself those that are branded with his name.’

“The ninth plague, according to the Book of Exodus, covered Egypt with
profound darkness.

“EXODUS 10:22 … and there was a thick darkness in all the land of Egypt.’

“PAPYRUS 9:11 The land is not light. …’

“‘Not light’ is in Egyptian equivalent to ‘without light’ or ‘dark’. But there is
some question as to whether the two sentences are entirely parallel.’202

The question remains: can a meteor and/or cometary shower account for these
phenomena, in addition to noise, darkness, and lights in the sky? In this respect a
meteor or comet airburst would be powerful. In a huge shower of these bodies the
pressure outward from the airburst would have produced these effects. Here Lewis
reports of the Council Bluffs, Iowa, meteor fall on November 28, 1894: “The most
strange phenomenon connected with it was that about two minutes after the meteor
fell there was a terrific shock, scarcely less severe than an earthquake, which shook
nearly every building in the city … Buildings in the north part of the town fully
one mile away from where the meteor fell were violently shaken.”203 Of the
Meadville, Pennsylvania, August 12, 1904 meteor fall, he cites The New York
Times as for the one above: “A terrific explosion accompanied the compact [sic,
for impact] with the earth accompanied by a huge wind lasting fifteen seconds …
The explosion shook buildings in Titusville twenty miles away.”204

Consider then a shower of such airburst shock waves all across Egypt. Their
effects would not be uniform but could kill people by thee thousands, shake and
topple buildings, kill the animals in the fields, and knock over trees, or defoliate
them, flatten the fields of grain; those hot enough on striking thatched roofs would
set them on fire, or in grain fields set fires or in orchards the same. In fact, the small
meteor shower in L’Aigle, France, in 1803 is described by John G. Burke thus:

“[Jean-Baptiste Biot] described … in overwhelming detail his interviews with
scores of eyewitnesses: children, aged men and women, peasants, laborers, war
veterans, estate managers, and ecclesiastics. He told of the broken tree branches
…205

202 Velikovsky, Ages. . ., op.cit., pp. 25-28
203 Lewis, op.cit., p. 126
204 ibid.
205 John G. Burke, Cosmic Debris: Meteorites in History (Berkeley/Los Angeles CA/London
1986), p. 55
The meteor shower had about 3000 fragments and had knocked branches off trees. Consider a few hundred thousands or millions of such fragments and the defoliation with all the rest of the damage, death etc. in Egypt which must have been tremendous in scope.

We are not discussing individual meteor or cometary debris falls but something much, much greater, where the bodies falling to Earth burst apart into thousands of pieces and showered down with dust into a whole series of air bursts for perhaps hours. For example, Theo Koupelis reports:

“The first confirmation by modern science that rocks do indeed fall from the heavens occurred on April 26, 1803. Citizens of the small town of L’Aigle, France, saw an exceedingly bright meteor that exploded and formed a shower of 2000 to 3000 fragments that fell to Earth. Reportedly, some fragments were still warm when found.”

A great meteorite/cometary shower would have hundreds of thousands or millions of fragments raining down on Egypt creating havoc wherever they exploded close to the surface.

The argument generally raised against this concept of physical catastrophe in the Ipuwer Papyrus is by critics who depict these descriptions as symbolic and in reality reflecting the breakdown of Egyptian society. But this overlooks the basic historical fact, namely that after a catastrophe looting takes place. Given the level of devastation throughout Egypt, many mansions of the wealthy would have been destroyed to some extent and their inhabitants killed. Poor people would have taken advantage of the situation to take what they wanted, or even move into those of the homes that were still in a decent state, whose owners had died. The concept of societal collapse does not preclude that a catastrophe preceded it.

In fact, historians suggest that stones that fell from the skies were worshipped in Egypt, and one in particular known as the Ben-Ben stone. Frank Joseph and Laura Beaudoin describe it thus:

“The Ben-Ben [stone] was not only the Nile Civilization’s most holy article, but also [perhaps] its oldest … the stone rested atop a column at the very center of its own temple in Heliopolis, the ‘City of the Sun,’ the principal seat of solar worship.

“The Ben-Ben was erected at the midpoint of a square court surrounded on all by contiguous buildings and numerous columns. …

“After his [the god Atum’s] sacred stone was removed to the new completed Great Pyramid, other, single pillars were set up in imitation of the original. These

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206 Theo Koupelis, *In Quest of the Universe* (Sudsbury MA 2007), p. 314
became obelisks, memorial columns topped with a pyramidal cap known as the benbenet, a word that demonstrated their exemplar …”207

Although the connection between the worship of stones that fell from the sky with the Hyksos/Exodus/Ahmos I Tempest Stela/Ipuwer Papyrus event is not made, it clearly indicates that the Egyptians connected stones falling from the heavens to their god of the Sun, and perhaps with the cataclysmic 8th century event.

One of the major obstacles in putting this material together into a cohesive, logical series is that historians have taken the order of these plagues at face value from Exodus. Few if any who have taken these events seriously have considered the fact that they are numbered to be just ten. But why ten, no one seems to have asked, or why they occurred in the assumed order in which the Bible presents them. The biblical writer or writers gave the pharaoh ten chances to see the error of his ways and this supposition is clearly the viewpoint of a religious individual presenting a theological description of what actually occurred. The fact of the matter is that there are numerous problems with the entire presentation of the Exodus.

Barbara J. Sivertsen deals with the sources that are taken to have written of these plagues and have done so at perhaps somewhat different times:

“Scholars differ, however, on which plagues go with which Documentary [J.E.D.P.] Hypothesis source. One, John Van Seters, assigns all the plagues either to the Yahwist (J) or to the Priestly (P) source. He identifies as belonging to the Yahwist (1) the Nile turned to blood; (2) frogs; (3) flies [this word is translated as insects or gadflies by other authors]; (4) the pestilence of the livestock; (5) hail; (6) locusts; (7) the death of the firstborn. The other plagues he ascribes to the P source. Van Seters considers that ‘the whole [J] plague narrative is so consistent in its pattern and so uniform in its outlook that it must be the literary artistry of a single author, the Yahwist.’ An earlier twentieth century scholar, Martin Noth, similarly assigned the plagues to the J and P sources but came to a vastly different conclusion: ‘the set of plague stories is not a well considered literary product but is derived from living oral tradition. …’ Other scholars such as Georg Fohrer and Brevard Childs attributed the plagues to J, E (the Elohist) and P.

“In a different vein, Moshe Greenberg looked at the symmetry in the narrative units and saw three sets of three plague episodes each, ending with the plague of darkness but not including the deaths of the firstborn [others offer additional, different sources] … With all these differences of opinion, it is no wonder Roland de Vaux wrote that ‘an examination of the first nine plagues, without taking the tenth into account, reveals a very careful literary composition which in fact defies analysis by the methods of literary criticism.”208

207 Frank Joseph, Laura Beaudoin, Opening the Ark of the Covenant (Franklin Lakes NJ 2007), p. 76; see also Martin Isler, Sticks, Stones, and Shadow (Norman OK 2001, pp. 107-108)

208 Sivertsen, op.cit., pp. 35-36
While the plague of darkness is the ninth plague, Psalm 105 places it as the first:

“He sent Moses his servant, and Aaron whom he had chosen. They wrought his signs among them and miracles in the land of Ham. He sent darkness, and made the land dark; they rebelled against his word. He turned their waters into blood, and caused their fish to die …”

Beyond this there are internal contradictions with the organization of the plagues, as Sivertsen further reports:

“… the first nine plagues, while having a definite pattern and repeated motifs … also contain logical inconsistencies and repetitions. If all the water was changed to blood by Aaron’s rod (Exodus 7:20) how could there be water left for the Egyptian magicians to do the same thing (Exodus 7:22)? Did the blood or the dead fish (Exodus 7:18) poison the water? The cattle that died by pestilence in the fifth plague were resurrected to die of boils in the sixth plague and resurrected to die once more by hail in the seventh plague. Are the biting mosquitoes (or gnats or lice) in the third plague the same as the flies or gadflies in the fourth plague? Certainly the murrain of the cattle in the fifth plague is nearly duplicated by the afflictions to cattle and people in the sixth.

“But logical inconsistencies and duplications are exactly what oral historians would expect to find within stories that have been transmitted orally for great lengths of time, particularly when they have been passed down through different groups and then combined. The common oral characteristic of exaggeration (sharpening) accounts for many of the most obvious inconsistencies: all the cattle, all the crops, all of Egypt. The various anachronisms in the stories are also to be expected in orally transmitted tradition.”

And that is precisely the problem: historians and scientists who have engaged in explicating the plagues of the Exodus have unwittingly and naively assumed that the order of the plagues was accurately recorded, rather than being a distant interpretation organized to teach a religious lesson. For example, included in the Exodus record are the names of two sites where the Hebrews worked or interacted with the Egyptians, namely Pithom and Rameses. However, Sivertsen shows that the “Noted Canadian Egyptologist Donald Redford was even more pessimistic [about trusting the Exodus account]. Thirty years before he had pointed out that the biblical names [of the sites] Pithom (pr-‘Itm in Egyptian) and Rameses or Raamses were only known in the Saite period, that is, during the seventh and sixth centuries B.C.E. Other concrete aspects of the [Hebrew] Sojourn in Egypt and Exodus stories were likewise recent.”

\[209\] Sivertsen, op.cit., pp. 36-37
\[210\] ibid., p. 2
In this regard, Gary Greenberg, President of the Biblical Archaeology Society of New York, points out:

“For most scholars the reference in Exodus to the Hebrew slaves working on the Egyptian cities of Raamses and Pithom provides the chief evidence of the identity of the pharaoh of the Exodus …

“Since the city was named by Ramesses II, scholars argue that this proves Ramesses II must have been the pharaoh of the Exodus. His is the only reign before Merneptah in which that city had that name … Merneptah had to have come to the throne after Exodus.

“Despite the wide support for this argument, it is flawed. The city of Raamses existed prior to Ramesses II, but under a different name. MOST EGYPTOLOGISTS BELIEVE THAT IT WAS ORIGINALLY THE CITY OF AVARIS, THE HYKSOS CAPITAL…”

But if Pi-Ramesses was Avaris, based on the analysis of most historians, this places the Hebrews in Egypt in Hyksos/Assyrian/Akkadian times in the very city which we equate with the Exodus. As Ian Wilson has shown in Chapter 3, “The Real Biblical ‘Ramesses’” citing numerous forms of evidence: “But what is also quite obvious from Dr. Bietak’s findings is not only was this site [Avaris] the true Biblical Ramesses … [it] was in fact none other than the Hyksos capital Avaris.” He adds: “The effect of this new information is profound. Suddenly the Biblical Israelites may geographically be set not in some backwater ‘store city,’ but in the environs of a vigorous metropolis which at times occupied the very centre stage of some momentous times in Egypt’s history.”

These cities were used to date the Exodus by historians, as Sivertsen goes on to show:

“Frank Yurco (who died in 2003) was among a minority of Egyptologists who hold to the view that the Exodus actually occurred. Like many biblical scholars for the past several centuries, he cited what he believed was the most reliable part of the scriptural narrative: the names of the storecities Pithom and Ramesses in Exodus 1:11. This, Yurco asserted, pointed to the pharaoh Ramesses II, who reigned from 1279 to 1209 B.C.E.”

As can be seen, the dating of the Exodus, in terms of the established chronology which places it in the times of Ramses II, is baseless.

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212 Wilson, *The Exodus Enigma, op.cit.*, pp. 44-45
213 *ibid.*, p. 55
214 *ibid.*
215 Sivertsen, *op.cit.*, p. 1
Here again, we have the writers of the Bible writing names that, according to the short chronology, only existed in Egypt in the 600s and 500s B.C. But these names must, in terms of the established chronology, have been employed 700 to 800 years earlier. In the short chronology the remembrance of the plague events requires a living memory and oral tradition of 100 to 200 years. What we have of the Exodus account in the Bible cannot be, and is not, a precise record but an old and distorted picture that became so through retelling and injecting religious themes into the narrative. For example why would the biblical writers place the turning of the Nile to blood first, say, instead of the thunderous noise? The probable answer was the importance of the Nile to Egyptians. It was in fact one of their gods, Sobek the crocodile, whose feast was an annual religious event. Here Philip Graham Ryken explains:

“To understand how distressing this [Nile turning to blood] was for the Egyptians, one has to appreciate how dependent they were on the Nile. The river was their lifeblood, the basis for their entire civilization. The Egyptians used the Nile for almost everything, and without it their land would have become a desert. The river provided the transportation system that helped them move goods from place to place. It formed the irrigation system that enabled them to grow their crops. It was their water supply, and also their food supply because fish [which died in the second plague] was one of the staples of the Egyptian diet. The river’s annual floods set their calendar and gave them fertile topsoil. In short, the land of Egypt was the gift of the Nile …

“Since the Egyptians practically owed their existence to the Nile, it is not surprising that they worshipped the great river as their creator and sustainer …”

It seems evident that the Biblical writer(s) could prove the might of their god over that of the Egyptian god by having him destroy the heart of Egypt’s life even for a few days. Furthermore, doing so threatened their ability to grow crops to eat or catch fish, a major staple of their diet. But none of these theological considerations can have anything to do with the order and length of these plague events, if one considers the science as this relates to these phenomena.

During a meteor or cometary debris shower, the airbursts would not first turn the Nile red. The explosive bursts would kill some people and some animals, destroy and damage some buildings, defoliate some trees, flatten some fields of grain and set some of them on fire, cause fire to run along areas of the ground, create explosive noises all at the same time, followed by prolonged darkness. Other people, animals and buildings, trees, fields of grain would remain untouched. The extraterrestrial dust and ash could poison the Nile and turn it a reddish color, which would kill fish by the millions that were eaten by insects, drive frogs onto the land where they would die, to be eaten by all kinds of insects—not just flies or gnats or

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crawling vermin—but insects that could attack other people and animals and sting them, creating boils, or the people allergic to the dust might have had allergic skin reactions like boils.

Lastly, to presume that neither the Hyksos nor their Palestinian, Syrian, or other allies were affected by such a disaster is not scientifically or logically feasible. The disaster befell all those in the path of the meteor or cometary debris swarm. The killing of only the first-born Egyptians, Velikovsky claimed, was “the last plague [which] has a distinctly supernatural quality in that all the firstborn and only the firstborn were killed … An earthquake [or other catastrophe] that destroys only the firstborn is inconceivable, because events can never attain that degree of coincidence. No credit should be given to such a record.” Therefore, the interpretation of the biblical writers of these plagues is also inconceivable because events during a major catastrophe can never attain that degree of ordered, unique coincidence and no credit should be given to such a record. The catastrophe should be seen through the lens of science that orders such events in the way they actually happen in nature and reality. In the words of Leopold von Ranke, what historians need in order to attain reality, “wie es eigentlich gewesen,” or as it really happened, is scientific evidence. The number of plagues will never be known of a certainty without one being in Egypt to see and accurately report them. The view of historians, scientists, or anyone else that this can be properly known consists of assuming everything that needs to first be proved. They are stating as a fact what must first be established as fact.

Further proof that Egypt had undergone a great catastrophe in the reign of Ahmose I is that he actually began to rebuild and refurbish the country as stated in the Tempest Stela and explained by Trevisanato:

“The king [Ahmose I] then went to Thebes to pay homage to the god [Amon] and [then] proceeded to fix the country. Not only did he fix the damage from the rain, but he also fixed earlier damage done to temples which had been caused either by neglect or by voluntary vandalism (or both).”

Ellis presents the Tempest Stela thus:

“Continuing with the text of the Tempest Stele, it would appear that there was still a great deal of devastation across the country that had to be cleared up by Ahmose I … The text reads:

“Then his Majesty was informed that the tombs had been entered … with the tomb chambers collapsed, the funerary mansions undermined and the pyramids

217 Velikovsky, *Ages. . .*, op.cit., p. 32
219 Trevisanato, op.cit., p. 109
fallen, having been made into rubble. Then his Majesty commanded to restore the
temples which had fallen into ruin in this entire land; to refurbish objects in the
noble chamber, to mask the secret places, to introduce into their shrines the cult
statues which were cast to the ground, to set up the braziers, to erect the offering
tables, to establish their bread offerings, to double the income of the personnel, to
put the land into its former state.'

"The text seems to be obvious in its meaning: the damage caused by the great
storm had to be repaired. But nevertheless, on reading the passage a second time,
not all of it makes sense. All the texts that mention the storm [supposedly] speak
of heavy rain and darkness, but significantly none of them mention wind of any
kind. So how did heavy rain on its own collapse a mighty pyramid? How did rain
ruin temples? How did heavy rain open up the secret places? … How did heavy
rain cast the cult statues and offering tables to the ground, or end the custom of
bread offerings?" 220

Ellis suggests "the entire concept of a ‘storm’ [is] false … actually a part of the
literary subterfuge of the scribes due in part to the Egyptians’ preoccupation with
the pun." 221 He further suggests "But what if the subsequent damage to the temples
was caused not by the storm, but by the political and religious instability that the
sudden ‘storm’ [or dispute] had generated?" 222 I suggest both are possible and
probable but that the sudden storm was greatly destructive. Ellis believes the word
“rain” was a pun meaning the “Hyksos” who did the damage. 223 It is suggested
here that the word storm means a physical storm, and the fact of the matter is that
the Hyksos did not act as barbarians destroying what they conquered. As Assyrians
they would have respected the customs and traditions of their conquered subject
peoples. Why would the Hyksos plunder and destroy cities and temples over which
they intended to rule and from whom they expected to derive tribute, especially
after they had reigned there for a very long time? Ahmose I comes at the end of the
Hyksos era, and if the Hyksos had razed cities, pyramids, and temples, Ahmose
would have attributed this destruction on the Tempest Stela directly to them.
Surely, this would have been a powerful piece of propaganda to rally the people of
Egypt to his cause. In point of fact, the Egyptians who ruled alongside the Hyksos
were loathe to take up arms against their overlords because they were treated
extremely well. The pharaoh Kamose who preceded Ahmose I, tried to rally these
subjugated Egyptian rulers to his side to overthrow and remove this foreign

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220 Ellis, op.cit., p. 65
221 ibid.
222 ibid., p. 66
223 ibid.
domination; actually they could not be swayed against their foreign Hyksos sovereigns, as outlined by Kurinsky:

“‘They treated all the natives with cruel hostility …’ [Manetho] was contradicted by Kamose … who himself dedicated an inscription on a stone stele … The inscription details the proceedings of a council meeting attended by the Egyptian hierarchy of Upper Egypt. It states that fellow Egyptian princes rejected out of hand an invasion of the [Hyksos] north proposed by Kamose to avenge his father’s demise and revitalize Egyptian pride. The inadvisability of the adventure was argued by all the attendant Egyptian leaders because it was said that the Egyptians favored the rule of the Asiatics. The Upper [southern] Egyptian nomarchs [governors] attendant at the meeting hosted by Kamose insisted that the treatment they received from the Asiatics was so congenial that the southerners were welcome to pasture their cattle in the grassy north seasonally; that they were generously granted corn for their swine when drought conditions in the south created the need. ‘All are loyal [to the Hyksos] as far as Cusae,’ they pointed out their refusal to join Kamose’ …

“The text of the Kamose inscription is given by Gardiner as follows [where Kamose says] …

“‘I will grapple him [the Hyksos] and slit open his belly. My desire is to deliver Egypt and to smite the Asiatics.’ Then spoke the grandees of his council: ‘See, all are loyal to the Asiatics as far as Cusae. We are tranquil in our part of Egypt. Elephantine is strong, and the middle part is with us as far as Cusae. Men till for us the finest of their lands. Our cattle pasture in the Papyrus marshes. Corn is sent for our swine. Our cattle are not taken away’.”

These Egyptians had not been treated with brutality but with respect. If their profound religious beliefs and practices had been stopped, their temples left in ruins, their statues toppled, their alters destroyed, they would have joined Kamose, and their people would be ready to follow him to oust the Asiatic foreigners. No, the Tempest Stela describes a catastrophe of nature, accompanied by looting, as does the Ipuwer Papyrus.

Lastly, we come to the expulsion of the Hyksos and its likeness to that of the Hebrew Exodus. In both cases we would expect to find that the departure of both from Egypt was negotiated and led to the Egyptians giving both safe leave to return to their former homelands. According to Kurinsky:

“‘Their departure from Egypt was occasioned by a revolt of the Upper Egyptian [Theban] kings who [according to Manetho]:

“‘made an attempt to take them by force and by siege, with four hundred and eighty thousand men to lie round about them; but that, upon despair of taking the place [Avaris] by the siege, they came to an agreement with them that they should

224 Kurinsky, *op. cit.*, pp. 76-77
leave Egypt and go, without any harm to be done to them whithersoever they
would; and that after this agreement was made, they went away with their whole
family and effects, not fewer in number than two hundred and forty thousand, and
took their journey from Egypt, through the wilderness for Syria … they built a city
in that country which is now called Judea and that was large enough to contain this
great number of men and called in Jerusalem’.  

Manetho further claimed that at the time of this Hyksos exodus, “The Asiatics …
had then the dominion over Asia,” which places this evacuation of Egypt in both
Hyksos and Assyrian times as Heinsohn and Sweeney contend. How do we confirm
this statement of Manetho as true, so far removed in time from this event?
Hatshepsut does this because she presents the same report. Ian Wilson discusses this:

“At Speos Artemidos, just south of Beni Hassan in Middle Egypt, there is a
rock temple of Hatshepsut with a rather enigmatic inscription carefully translated
by the great Egyptologist Sir Alan Gardiner in 1946. Gardiner acknowledged it as
a ‘difficult’ text and Goedicke has subsequently furnished his own, somewhat
different translation, the two versions meriting comparison…:

“‘I have banished the abomination of the gods and the earth has removed their
foot-[prints]’ [Gardiner]

“‘And when I allowed the abomination of the gods [i.e. the immigrants] to
depart, the earth swallowed their footsteps!’ [Goedicke]  
The banished abomination of the gods, in Gardiner’s translation and in
Goedicke’s, in the time of Hatshepsut are unknown. But this does refer to the
expulsion/Exodus of the Hyksos and their Syrian, Palestinian, and other allies from
Egypt which is explained by Egyptologist Joyce Tyldesley:

“Here Hatshepsut is deliberately invoking the legend of the Dreadful maat-less
[soul-less] Second Intermediate Period [of Hyksos rule]–a much exaggerated
version of real events–in order to underline the peace and stability of her own reign.
Indeed, she is the first of the post-Ahmose [I] pharaohs to express a loathing of the
Hyksos which many later rulers were to copy. Hatshepsut was not a woman to allow
a few factual occurrences to hinder her from writing a revised version of [Hyksos]
history, and she claims credit for both ridding the land of the detested foreigners and
for restoring the monuments and indeed the religion of her ancestors … There can
be no truth at all in her boast that she rid Egypt of the Asiatics; Hyksos rule had
ended many years before Hatshepsut came to the throne. Similarly, her claim that
the Hyksos heathens ruled ‘without Re’ is also untrue; as we have already seen, the
Hyksos rulers adapted their own religion to that of their adopted country and several
Hyksos kings actually bore names compounded with that of [the god] Re. However,

225 ibid., p. 65
226 ibid.
227 Wilson, op.cit., pp. 135-136
in Hatshepsut’s eyes, these exaggerations would not have been lies. The role of pharaoh was a permanent one which passed from individual to individual and, as the current office holder, Hatshepsut was quite entitled to use the achievements of the previous pharaohs when and as she saw fit.”

Here, then, we have confirmation of the statement by Manetho that after years of siege of the Hyksos stronghold of Avaris, the Egyptians negotiated for them to leave Egypt. Arthur Cotterell also claims “[Ahmose I made] a peace treaty which allowed the Hyksos ‘with all their possessions and households complete’ to leave Egypt and journey through the desert to Syria.” All this, of course, happened years after the catastrophe.

Having moved the Hebrew Exodus/Hyksos expulsion into the 8th century B.C., we are still at a time when the climate had not completed its first aridification and thus the deserts were beginning to lose their vegetation, which would die off in a decade or two. Whatever lakes, rivers, ponds, small streams existed would dry up, leaving sand or saltflats, and the final cover of soil would be blown away. Yet the masses of people leaving Egypt would not find the Sinai Desert as it is today but rather a relatively savannah-like environment dotted with trees, bushes, and grasses. What flocks they brought or commandeered from the Egyptians before they began this journey would thus have found reasonably ample grazing possible. Nevertheless, the biblical claim of the people staying there for 40 years is simply not provable.

In terms of science, one of the miracles in the desert during that transitional emigration can be known. Their ability to find manna may be a reality. That one can explain a mythical story by scientific means is but a hypothesis in need of verification. It is not historical nor scientific truth unless verification establishes such a connection. To show how this applies to manna, Humphreys has presented a reasonable and persuasive scientific analysis of the source of manna which is verifiable. We must therefore consider the Sinai not as a desert with few trees or as arid as it is today. People actually gather and eat this material. Here, then, is Humphreys’ analysis:

“It was in the Desert of Sin that the Israelites first ate Manna … Here are the key points as recorded in the book of Exodus:

“‘When the dew was gone, thin flakes like frost on the ground appeared on the desert floor … Moses said to them [the Israelites], ‘It is the bread the Lord has given you to eat…’ Each morning everyone gathered as much as he needed, and when the sun grew hot it melted away. … The people of Israel called the bread manna. It was white like coriander seed and tasted like wafers made with honey …

228 Tyldesley, *op.cit.*, p. 157
the Israelites ate manna for forty years until they … reached the borders of Canaan.’ (Exodus 16:14-16, 21, 31-35) …

“A botanist named [Fritz] Bodenheimer organized a manna expedition to the Sinai Peninsula … and … published his findings in 1947. He saw for himself tamarisk trees being bitten by … insects … and then exuding a resinous secretion that was about the same shape and size as a coriander seed. When it fell to the ground it was white. Bodenheimer ate some and wrote, ‘The taste of these crystallised grains of manna is particularly sweet. It is most of all like honey when it has been left a long time to solidify.’ So the biblical description of manna as being white, like coriander seed, and tasting like honey is exactly what Bodenheimer found. … I believe we can say, beyond reasonable doubt, that the manna the Israelites ate in the desert was a natural substance produced by suitable trees.”

The product still grows there today as pointed out by Hoffmeier speaking of a visitor to who at

“St. Catherine’s Monastery in 1838 learned that the monks gathered what they called manna and that it was highly valued. In fact, the abbot of the monastery promised to give [the visitor] some.

“… the explanation of Ehrenberg and Robinson [was] accepted [by Bodenheimer], namely that manna is produced by secretions of certain insects. He noted that this phenomenon occurs annually in June, and he observed it during a visit to Sinai in June 1927. He also noted that it was during the period May-June [in the Bible] that the Israelites would have reached the area where tamarisk manna occurs. This concurs with the dating observed … based upon chronological data provided in the Torah. Bodenheimer discovered that in northern Iraq the Kurds collect ‘thousands of kilograms every year in June and July.’ This observation demonstrated that the volume of manna required to feed a large group of people can indeed be produced, leading him to conclude: ‘We have seen that all the eye-witness reports of the Bible can be taken as literal descriptions of tamarisk manna in Sinai’.”

Like the Jordan River crossing, the people crossing the Sinai would have arrived there around June, after three-months–three new moon periods–which brings us to the pillar of light in the sky by day and of fire by night which was probably a comet, Halley’s Comet, another somewhat common event dated to 763 B.C. Duncan Steel dates the apparition of Halley’s Comet tentatively to August of 763 B.C. If this is correct, then the Israelites arrived in the Sinai at the very time that the tamarisk manna would have ripened and could have been harvested.

230 Humphreys, op.cit., pp. 288-290
232 Duncan Steel, Marking Time (NY 2000), p. 146
The Israelites were not returning as foreigners to their homeland. They were indeed coming to a land with which they shared trade, language, a religious outlook, and customs. Although in Egypt for about a century, they nevertheless had been aliens in Egypt, were hated for their dominance and therefore, somewhat like the early Norman French conquerors of England, remained ethnically separate from their Egyptian subjects. They most probably returned to Palestine periodically to visit family, to trade or carry on governmental business. Therefore, upon their return they were integrated into that society to enlarge it and enrich it. What is being suggested here is that there was no conquest by Joshua nor a period of Judges. As is well known, no-one has found archaeological evidence of the conquest. Jonathan Michael Golden put the concept simply down as follows: “The myth of the Israeliite conquest leading to the complete displacement of the Canaanites is probably exactly that.”

Golden adds:

“The biblical story of the Israeliite conquest describes an army led by Joshua that moved throughout the land devastating Canaanite cities in its path. The story as described in the Hebrew Bible specifically mentions a number of cities by name, and in searching for evidence relating to this story, some archaeologists have pointed to destruction levels identified at these sites. More recently, a number of scholars (Dever 2001; Finkelstein 1999) have rejected this approach, questioning the essential premise—a renegade group’s ability to vanquish successfully and with relative ease into [sic] a well-established population—as well as the facts. William Dever (1992a), for instance, has argued that there is not a single destruction layer dated around 1200 B.C.E. that can be attributed with certainty to the Israelites …

“In addition to the problem of datable destruction layers, a number of scholars have pointed to the evidence for cultural continuity, with little in the way of dramatic changes in the material culture that might be expected with the sudden incursion of a new people. For instance, the Bull Site in northern Manasseh reflects an ongoing Canaanite influence on religious practices, and the same may be said for the linguistic evidence (Smith 2000). As an alternative to the tale of violent conquest, models suggesting a more peaceful infiltration (Alt 1925) … have been advanced.

“Thus, considering the evidence for cultural continuity during the Late Bronze–Iron I transition, and the lack of evidence for securely dated destruction layers, the literal truth of the biblical narrative concerning the conquest becomes increasingly difficult to support. Indeed, Israel Finkelstein and Neil Asher Silberman have reviewed a good part of the archaeological evidence from this period and concluded that ‘the process we describe … is the opposite of what we have in the Bible: the emergence of early Israel was the outcome of the collapse of the Canaanite

233 Jonathan Michael Golden, *Ancient Canaan and Israel* (Santa Barbara CA 2004), p. 60
culture, not its cause … There was no violent conquest of Canaan. The early
Israelites were—irony of ironies—they themselves originally Canaanites!’ (2001, 118).”

As is obvious to the reader, the short chronology of Heinsohn and Sweeney
fully explains another archaeological/historical problem, enigma, contradiction that
is inexplicable with the established chronology. Because the Israelites were
Canaanites in Egypt, when they returned home there was no need for conquest, no
need for cultural or linguistic or other change. The archaeological evidence fits the
short chronology—hand in glove.

The implications of this fact are as follows: (1) Since there was no conquest of
Palestine, the material on Joshua is only a biblical story meant to explain, by those
who had no evidence of him, the period following the Exodus. In other words, the
Book of Joshua is a creation by the writers of the Bible to show how the Israelites
took the land of Israel and, therefore, that period never existed. (2) Since Joshua
did not exist because there was no need to destroy the Canaanites, the integration
of the Egyptian Canaanites/Palestinians into their homelands was rapid. This also
means the Book of Judges is superfluous as well and a creation of these same
writers. Provan, Long, and Longman state of the Book of Judges:

“A common response to these features of the book is to assume that they
somehow diminish the historical value of texts. In their discussion of the book of
Judges, Finkelstein and Silberman, for instance, state simply that ‘theology, not
history, is central [to it].’ Similarly, Miller and Hayes conclude that ‘the Book of
Judges can hardly be accepted at face value for purposes of historical
reconstruction.’ Not only are they bothered by ‘matters of detail in the individual
stories which strain credulity,’ but they are particularly vexed by the ‘editorial
scheme which is artificial and unconvincing.’”

Marc Zvi Brettler discussed the Judges broadly thus:

“In The Early History of Israel, the great French scholar Roland de Vaux noted
that the ‘age of the judges’ is an artificial construct and he is skeptical of the
veracity of many of these stories, which were written much later than the events
they describe. (de Vaux 1978:751-63) …

“Like de Vaux, Miller and Hayes are not comfortable with the typical
designation of this period and speak instead of ‘what is often called “the period of
the Judges”’ (Miller and Hayes 1986:87). They note that the stories are often
schematic or typological and ‘can hardly be accepted at face value for purposes of
historical construction’ (Miller and Hayes 1986:87), though they tentatively put
more confidence in particular stories … Even the typically iconoclastic Gösta W.
Ahlström … for example, … finds the story where Ehud confronts Eglon the

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234 ibid., pp. 60-61
(Louisville KY 2003), p. 161
Moabite king problematic, since there is no evidence for Moabites in this period (Ahlström 1993:377).

“In sum, most historians of Israel now recognize the problematic nature of the Judges as a source for ancient Israelite history. …

“The most articulate, carefully reasoned expression of the problem of using Judges as a straightforward narrative recounting the past comes from … Niels Peter Lemche in his Early Israel. Based on his analysis of oral literature and careful investigation of how traditions grow, especially in the pre-literary stage, he notes:

“‘This means that the nature of the sources dealing with the period of the Judges prevent [sic] our being able to write a history of this period. We cannot even permit ourselves to use the various traditions of the Judges as historical references to individual events which actually occurred during the pre-monarchic period.’ (Lemche 1985:379) …

“‘… Thus we would no longer automatically be able to assign a particular milieu to this narrative of the period of the Judges. It might just as easily be pre-Israelite, but assimilated into Israelite tradition. It might just as easily derive from the period of the monarchy, although the oral tradition has assigned it to the period of the Judges. In short, we have no way whatsoever to determine whether any historical tradition at all underlies the narrative … as long as we lack other sources. Precisely the same judgment applies to most of the other traditions in the Book of Judges.’” (Lemche 1985:383)

“Once stated so clearly, and put into a broader perspective of how traditions develop, this position is quite obvious, and it is not surprising that it has gained general assent.”236

Brettler’s The Book of Judges is an eye-opener for criticisms of the Bible and is a “must read” for biblical scholars.

Provan, Long, and Longman, however, see the Book of Judges not as a theological, didactic narrative, but as history, and give their reason for so doing:

“Our approach is to acknowledge the schematic, patterned character of the depiction of the judges period, but not to set this in opposition to the potential historical import of the picture painted. That testimony about the past can comfortably combine compositional technique, didactic intent, and historical information should come as no surprise. In order rightly to judge the nature of the historical information, one must, of course, take account of the nature of the presentation. The book of Judges presents a portrait of an age. When viewing a portrait painting, we instinctively take account of selectivity of detail, simplification, coloration, patterned composition, some artificiality in arrangement, and so forth—and we do not assume that these features detract from the historical likeness. Indeed, in the hands of an accomplished artist, they further

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236 Marc Zvi Brettler, The Book of Judges (London/NY 2002), pp. 5-7
the referential intent of the piece. Our approach to the book of Judges is similar.\textsuperscript{237}

What Provan, Long, and Longman are doing is assuming that the historical basis of the Book of Judges is a fact. They have not proven it to be so, only assumed. It is simply circular reasoning. Once these scholars have assumed that the Book of Judges is a historical reality, interpreting it as historical reality must be correct and valid. Niels Peter Lemche staunchly rejects this entire approach:

“When this procedure of historical-critical scholarship is dissected in this logical manner, it crumbles like a house of cards. Circular argumentation is false, and always will remain false. Nothing can change that. A scholarly assumption [such as the one about the Book of Judges] may look like a legitimate argument, but contrary to genuine argumentation it cannot be falsified—\textsuperscript{238} to use this concept as coined by the philosopher Karl Popper … It is characteristic of such cases that there is no \textit{tertium comparationis}, no external evidence that may prove the argument to be correct and not a baseless assumption. …

“However, if it is impossible to provide any evidence supporting a certain hypothesis, it is impossible to decide whether it is correct or false, and so it is a false argument.

“For these reasons it is Alpha and Omega for traditional historical-critical analysis of the Old Testament that whatever thesis it proposes can be the subject of a falsification process. If this is not the case, the thesis is false and of no consequence for subsequent scholarship …”\textsuperscript{238}

The question never answered by Provan, Long, and Longman: What evidence outside the biblical narrative proves that there was a person sitting for the portrait and that the artist did not invent a character in his mind and then embellished it in oils? Thompson is scathing in his denunciation of this approach to biblical history:

“Such thinking, posing as an historical and critical scholarly discipline, has been a great embarrassment to modern research. Rather than being historical, it broke the first rule of history by failing to distinguish it from myth. Rather than being critical, it used logic entirely circular. Rather than being a self-correcting, self-critical science, it took for granted its own assumptions.”\textsuperscript{239}

There is no external evidence by which to evaluate the book of Judges as valid, therefore, the only evidence of its historical validity is the Bible itself and the capacity of scholars to assume its validity—not a shred of this evidence is presented because it does not exist! What we have is a lack of any archaeological evidence of the Conquest or for the Book of Joshua, and historians looking at the biblical literature for the Book of Judges are saying it is a theological, didactic tract

\textsuperscript{237} Provan, Long, Longman, \textit{ibid.}
\textsuperscript{238} Lemche, \textit{op.cit.}, p. 111-112
\textsuperscript{239} Thompson, \textit{The Mythic Past, op.cit.}, pp. 4-5
unrelated to any historical period. This means that the centuries between Joshua, the Judges, and the period of the monarchy never existed. Therefore, monarchical Israel comes only about a century or less after Exodus. Both the science and technology for dating the Hyksos and their Syrian/Palestinian and other allies places them in Egypt in the 9th-8th centuries B.C. and about 100 years later or less, the Kingdom of Judah is established. As with the rest of the documents of ancient history, when we remove centuries from biblical history it becomes a coherent whole with both Mesopotamian and Egyptian history.

Let us connect the events in terms of the chronology for Mesopotamia, Palestine, and Egypt to delineate the coherent integrated chronology.

The Hyksos being the Akkadians/Assyrians who conquered Egypt with their Syrian, Palestinian, and other allies reign there for some 108 years from the 9th into the 8th century B.C. They were expelled during the reign of Ahmose I who must therefore be dated to the 8th century B.C. During his reign there was a catastrophe which came from the sky creating explosive noise and darkness, as noted in the Tempest Stela. The Ipuwer Papyrus which speaks of trade between Minoan Crete and Byblos being cut off, places the catastrophic and social events it describes in Hyksos times when this trade was halted. The Ipuwer Papyrus and the Tempest Stela of Ahmose I describe the events experienced by the Hebrews while they were in Egypt as allies of the Hyksos. Thereafter Ahmose I began to rebuild the country after the devastation of what was a massive cometary/meteor shower.

Ahmose I then turned his attention to the Hyksos in the north and besieged Avaris perhaps for three years. Unable to win a final victory, he negotiated with the Hyksos and their allies there to leave the land. This can be seen in the inscription of Hatshepsut at Speos Artemidos wherein she took the false credit for allowing “the abomination of the gods [the Hyksos] to depart” which was actually done by Ahmose I. The Hyksos and their allies departed from Egypt and arrived in the Sinai in June to harvest the tamarisk tree droppings, which they may even have been familiar with, and around that time a comet, or even Halley’s Comet appeared for a few weeks that appeared as a miraculous omen that led them along on their journey. When they came to the River Jordan, they probably were camped there for some time when an earthquake mudslide permitted them to cross it on dry land.

Having returned to their homeland, they did not need to conquer it and became integrated into the society around them. Thus, this explains why there was no archaeological evidence of destruction. Once home and integrated into society they began to build through trade, during the ancient Industrial Revolution, a small but significant nation in that part of the world. This culminated in the early to late 7th and early 6th century in a monarchy.
All these events took place during the 18th Dynasty, which places it, as Lynn Rose claimed, prior to the 12th Dynasty. Tied to this is the unimpeachable astronomical evidence presented by Rose in volume II, showing that the Hebrew kings of Judah and Israel reigned in Neo-Assyrian/Persian and Hellenistic times. This means the historical writings of this period in the Bible were being committed to parchment at the time these events were happening. This further connects with the massive amounts of written material archaeologically uncovered in Palestine after the 6th century B.C. It further shows that alphabetic writing became dominant around 700-500 B.C. throughout this region and was thus a further extension of the ancient Industrial Revolution.

Therefore, as promised earlier, we have shown that the short chronology correlates with and integrates all this material evidence and more from Mesopotamian through Palestinian through Egyptian history and chronology into a coherent whole wherein all the pieces fit together. In this respect, we also see that much of Velikovsky’s thesis also fits into this history/chronology and hence his revisionist contributions are still valid. Most significantly, biblical history and chronology now has a place in the arena of historical reality. There is undoubtedly still a great deal more to be done to fine tune this work. I cannot follow in detail the evidence and arguments of Gunnar Heinsohn and Emmet Sweeney who may have done just that, but I maintain that by and large the overall chronology and history of the ancient Near East is now firmly established along the lines they have developed and also by the historical hypotheses and astronomical evidence of Lynn Rose. No other revision of this history incorporates the depth and breadth of evidence that we have presented in these three volumes of Pillars of the Past!

There is, however, one final area of historical evidence that we turn to now to place the beginnings of north Mesopotamia and Anatolia into the framework we have thus far established.
CHAPTER 11

THE INDO-EUROPEAN LANGUAGE, CLIMATE CHANGE, AND CHRONOLOGY IN PREHISTORY

At the beginning of this book, we presented evidence that the Afro-Asiatic languages spoken from western Africa into Mesopotamia were related and stemmed from an original language. With the aridification of the lands in these regions, the peoples speaking this language became permanently separated and thus the closely related dialects evolved into distinctly different forms unintelligible to each other and to speakers of the original. Nevertheless, because the time span separating the original language from the later evolved forms was not long, linguists could still connect the various languages to each other and could consider the original form. But because, as we have shown, various forms of scientific information require that all the speakers of these later languages lived about 1800 or more years closer to the present, and because scientific and other evidence requires the same for speakers of the original, in tandem, this evidence directly supports the short chronologies of Heinsohn, Rose, Sweeney, and in part Velikovsky.

With the Indo-European family of languages we observe an identical situation, in which a group of people in prehistoric times spread an original language or highly similar languages across another vast region, running from India northwest across to Anatolia, Greece, Roman Italy, Germany, Britain, Scandinavia, and east via Russia into Chinese Turkestan where Tocharian, a daughter language of the Indo-European stock, was spoken. In this case we will show that the beginnings of civilization in India, Anatolia, Greece, etc.\textsuperscript{3}n, like those across Africa into Mesopotamia were spread by a pastoralist people; the scientific evidence we presented, which shortens the chronology in this part of the ancient world, applies to the Indo-European speakers and shortens the time span as well by 1800 or more years. In a real sense, linguistic evidence in both cases speaks with one voice that requires, yet again, that the chronology of the ancient world must be greatly shortened. Charles Darwin, in fact, expressed the concept of the genealogy of languages as a possible method of understanding the development of humanity in his \textit{On the Origin of Species}:

“It may be worth while to illustrate this view of classification, by taking the case of languages. If we possessed a perfect pedigree of mankind, a genealogical arrangement of the races of man would afford the best classification of the various languages now spoken throughout the world; and if all extinct languages, and all intermediate and slowly changing dialects, had to be included, such an arrangement would, I think, be
the only possible one. Yet it might be that some very ancient language had altered little, and had given rise to few new languages, whilst others (owing to the spreading and subsequent isolation and states of civilisation of the several races, descended from a common race) had altered much, and had given rise to many new languages and dialects. The various degrees of difference in the languages from the same stock would have to be expressed by groups subordinate to groups; but the proper or even only possible arrangement would still be genealogical; and this would be strictly natural, as it would connect together all languages, extinct and modern, by the closes affinities, and would give the filiation and origin of each tongue.”

Understanding that the origin and spread of these Indo-Europeans is a major problem for prehistorians, Sir Colin Renfrew introduced “The Indo-European Problem in Outline”:

“In the year 1786, an English judge, serving in India at the High Court in Calcutta, made a quite extraordinary discovery. He was Sir William Jones, who had trained as an oriental scholar before reading law. On arrival in Calcutta, three years earlier, he had taken up the study of Sanskrit, the language in which the earliest literary and religious texts in India were written, many of them from the fourth to the sixth centuries AD, by which time Sanskrit was no longer spoken but served as the language of scholarship and literature; much as Latin was used in the west in Renaissance times. In his ‘Third Anniversary Discourse’ to the Asiatic Society of Bengal he briefly mentioned an observation he had made which can be taken as a starting point for the whole study of historical linguistics, and certainly for the field of Indo-European studies:

“‘The Sanskrit language, whatever may be its antiquity, is of a wonderful structure; more perfect than the Greek, more copious than the Latin, and more exquisitely refined than either, yet bearing to both of them a stronger affinity, both in the roots of verbs and in the forms of grammar, than could possibly have been produced by accident; so strong indeed that no philologer could examine them all three, without believing them to have sprung from some common source, which, perhaps, no longer exists; there is a similar reason, though not quite so forcible, for supposing that both the Gothic and the Celtic, though blended with a very different idiom, had the same origin with the Sanskrit; and the old Persian might be added to the same family, if this were the place for discussing any question concerning the antiquities of Persia.’

“This brilliant observation has been further developed and analysed by generations of scholars in many major works, and there is little doubt that Sir William Jones was right. He saw that in comparing two languages, points of resemblance in the grammatical structure are as important as similarity between the words of the vocabularies. He appropriately compared Sanskrit, Latin and Ancient Greek, all by then dead languages, which had flourished at about the same time, and he drew into

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1 Charles Darwin, On the Origin of Species by Means of Natural Selection (NY 1883), p. 370
the discussion two of the [ancient] languages of northern Europe – Gothic (the ancestor of German) and Celtic – and rightly compared these with the Old Iranian (Persian) language in which the hymns of the Avesta, the ancient Iranian scriptures, are written.

“Sir William Jones saw that these resemblances were so striking as to be more than fortuitous. These different languages are all related in some way to one another. The most obvious explanation (although, as we shall see, not the only possible one) is that they are all descended from some common source.”

Various attempts have been made to correlate these migrations of language speakers with genetic markers, but to date these movements have been fraught with serious problems.

The first problem we shall undertake to resolve is the age and date of the Proto-Indo-European language before it was passed along and evolved into the daughter languages described above. This is particularly hampered by the methods employed, of a historical nature rather than of a scientific one, which have stymied understanding, as W.F. Albright and T.O. Lambdin show:

“It is erroneous to say that during the third millennium [B.C.] Indo-Europeans were nomad horsemen–or [lived] in Central Asia … Both assumptions are based on false a priori hypotheses and misdated stratigraphic finds. Even today the chronology of central Europe and even more of central Asia in the Neolithic, Early and Middle Bronze periods is almost hopelessly confused. Until archaeological chronology has been clarified, and the exact level in stratified sites, at which remains of domesticated horses and [Asian] camels are found, has been fixed, it is idle to speculate about equine nomads as bearers of early Indo-European migratory movements.”

We have repeatedly shown that the civilizations of the Hittites, Greeks, and all the others discussed in these volumes, did not exist in the second millennium B.C. based on scientific, technological, and other evidence. This undeniably poses a major chronological obstacle to the established chronology for the evolution of the Indo-European languages. David W. Anthony in a recent book specifically attempts to explain how, why, and when these developments occurred. He succinctly designates a point emphasized again and again by this author, namely, that over a period of a thousand years, languages change so greatly as to be unintelligible to later speakers, and that it is extraordinarily difficult or impossible to show clear lineages from one to the other.

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“Time changes everything. Reading to my young children, I found that in mid-sentence I began to edit and replace words that suddenly looked archaic to me, in stories I had loved when I was young. The language of Robert Louis Stevenson and Jules Verne now seems surprisingly stiff and distant, and as for Shakespeare’s English—we all need the glossary. What is true for modern languages was true for prehistoric languages … So what do we mean by Proto-Indo-European? If it changed over time, [h]owever we define it, for how long was Proto-Indo-European spoken? Most important, when was it spoken? How do we assign a date to a language that left no inscriptions, that died without ever being written down? It helps to divide any problem into parts, and this one can easily be divided into two: the birth date and the death date.

“This chapter concentrates on the death date, the date after which Proto-Indo-European ceased to exist [and evolved into other languages unintelligible to each other and their later speakers]. But it helps to begin by considering how long a period probably preceded that. Given that the time between the birth and death dates of Proto-Indo-European could not have been infinite, precisely how long a time was it? Do languages, which are living, changing things, have life expectancies?

“The Size of the Chronological Window: How Long Do Languages Last?

“If we were magically able to converse with an English speaker living a thousand years ago … we would not understand each other. Very few … languages … remain sufficiently unchanged after a thousand years to be considered the ‘same language’.”

That is, Indo-European cannot have been born, say, 5000 B.C. and by 3000 B.C. still be the same language passed along to the Hittites, Greeks, etc., because over a period of 2000 years it would have evolved into a greatly different language or a few greatly different languages. As J.P. Mallory explains:

“Where we find great similarity of speech over a large area we can normally assume a recent expansion since the factors of time and distance will normally reduce a single language into a continuum of mutually related but increasingly different languages. This being so, the similarity of the Indo-European languages when we first encounter them historically, stretching from the Atlantic to India, all speak for their relatively recent spread from a more confined area. To ascribe to such a dispersion a very great antiquity would be to attribute to the Proto-Indo-European language [stability] properties wholly contrary not only to all the evidence from the world’s other languages but also to human behaviour itself.”

That is, the time from the birth of the Indo-European language to its death must be much less than 1000 years. In this application to Old Indic and Greek,

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conventionally thought to have been spoken daughter languages of Proto-Indo-European. Anthony sets the death or terminal date for it at ca. 2500 B.C.:

“… the terminal [or death] date for Proto-Indo-European—the date after which our reconstructed form of the language becomes an anachronism—can be set around 2500 BCE, more or less, from the perspective of Greek and Old Indic. It might be extended a century or two later [i.e. closer to the present], but as far as these two languages are concerned, a terminal date much later than 2500 BCE—say, as late as 2000 BCE—is impossible.”7

Anthony also admits: “archaeologists cannot agree about the antiquity of Proto-Indo-European. Some say it was spoken in 8000 BCE, others say as late as 2000 BCE, and still others regard it as an abstract idea that exists only in linguists’ heads and therefore cannot be assigned to any one time.”8 Nevertheless, he importantly points out:

“One simple point can be extracted from these debates: if the Proto-Indo-European core vocabulary changed at a rate $\leq$ [less than or equal to] 10% per millennium, or at the lower end of the expected range, Proto-Indo-European did not exist as a single language with a single grammar and vocabulary for as long as a thousand years. Yet the grammar of Proto-Indo-European, as reconstructed by linguists, is remarkably homogeneous both in morphology and phonology [the sound of its words]. Proto-Indo-European nouns and pronouns shared a set of cases, genders and declensions that intersect with dozens of cognate phonological endings. Verbs had a shared system of tenses and aspects, again tagged by a shared set of phonological vowel changes (run-ran) and endings. This shared system of grammatical structures and phonological ways of labeling them looks like a single language. It suggests that reconstructed Proto-Indo-European [having been spoken for a relatively short period of time] probably refers to less than a thousand years of language change.”9

If we choose this approximate 2000-2500 B.C. date or one near to these for the death of Proto-Indo-European, the Mycenaean Greeks who supposedly existed around 1900 B.C. and the Indians who supposedly existed somewhat earlier, 2200 B.C., both can speak languages that are closely related to Proto-Indo-European and to each other. Therefore, the separation of these languages from the parent language is considerably less than 1000 years; for the archaic Mycenaean Greeks it is about 500 to 600 years at most and for the Indians 800 to 400 years. The same condition and dating then also applies to the other Indo-European daughter languages.

The attentive reader may have noticed that the assumed dates for the birth and death of Proto-Indo-European were determined by calibrating them to and with the established chronology. Since the established chronology puts the Greeks, Hittites,
Indians, etc., after around 2000 B.C., the Proto-Indo-European language from which they evolved could be no older, that is, go no farther back in time than 2500 B.C. If one were to move Proto-Indo-European farther back in time to, say, 3000 or 4000 B.C., then in the ensuing 1000 or 2000 years it would have evolved into a completely different language or set of languages and could not be retraced back to the same parent language. But since we have shown these civilizations only existed in and around the first millennium B.C., Proto-Indo-European must be moved closer to the present, to around 1500 B.C., so that it could be recognized in the daughter languages of these divergent regions. Placing the Proto-Indo-Europeans around 1500 B.C. has significant implications for Velikovskian catastrophism as well as for chronology.

Since Velikovsky suggested that there had been an immense cataclysm around 1500 B.C., it follows that not only were most of the megafauna genres extirpated, that there were very few survivors of some species and these, too, later went extinct, but that the human race experienced the same destruction within certain regions where populations were totally wiped out and/or left with some survivors while other regions such as the northeast or northwest of the Black Sea area and central Africa were not as greatly devastated. That is, in certain regions of the Earth the human population was largely exterminated by that catastrophe, leaving very few survivors, while others were hardly touched, leaving much larger populations. It is these larger populations that were pastoralists or became pastoralists, that spread across Africa or Eurasia, into which the small pockets of survivors were then integrated who adopted the Afro-Asiatic and Proto-Indo-European languages. These spread across Afro-Asia and Eurasia with their flocks, establishing their languages throughout these areas. In some pockets with larger numbers of survivors, this integration and language adoption did not take place and thus these isolated languages exhibit no relation to any others, such as the Basque language on the Iberian Peninsula and in south-western France. The migrating groups, we suggest, were mainly pastoralists and not primarily agriculturalists.

Renfrew, however, has suggested that agriculturalist Proto-Indo-Europeans originated somewhere in north-eastern Anatolia and spread because these farmers gradually expanded outward to cultivate new lands on the great Eurasian steppe prior to 6000 B.C.:

“I have indeed argued that before about 6000 BC there were, in the eastern part of Anatolia, and perhaps in some adjacent lands to the east and south-east, and probably nowhere else, people speaking languages ancestral to all the Indo-European languages of today. So that is indeed a kind of ‘homeland’ model, but it is certainly not a [pastoralist/military] migrationist model of the old-fashioned and traditional kind. It does not assume a sudden and unexplained eruption from some rather ill-defined nuclear area, linked perhaps in some way to warlike nomad pastoralists. On the contrary, it links the spread of early Indo-European languages to a well-defined
demographic process itself closely correlated with the adoption of a farming economy.” Anthony explains the chronological problem that negates Renfrew’s gradual agriculturalist spreading model:

“By linking the dispersal of the Indo-European languages with the diffusion of the first farming economy, Renfrew achieved an appealingly elegant solution to the problem of Indo-European origins. Since 1987 he and others have shown convincingly that the migrations of pioneer farmers were one of the principal vectors for the spread of many ancient languages around the world. The ‘first farming/language-dispersal’ hypothesis, therefore, was embraced by many archaeologists. But it required that the first split between parental Indo-Hittite and Proto-Indo-European [or Proto-Greek or Proto-Indian] began about 6700-6500 BCE, when Anatolian farmers first migrated to Greece. By 3500 BCE … the Indo-European language family should have [greatly changed and] been bushy, multi-branched and three thousand years old, well past the period of sharing a common vocabulary for anything.”

This is the very same problem that confronts Pitman and Ryan’s thesis that the Indo-Europeans fled after the flooding of the Black Sea about 7000 B.C., comparable in time to that of Renfrew. They suggest that:

“The Semites and Ubaids fled southward to the Levant and Mesopotamia; the Kartvellons retreated to the Caucasus; the LBK [the linear pottery farmers] dashed across Europe, leapfrogging from one site to the next, pushing ahead their frontier for reasons never adequately explained; Vinča retreated upstream to the enclosed valley of the Hungarian plain. Others went to the Adriatic and the islands of the Aegean. Some refugees migrated into the heartland of Eurasia via the Don. Still others used the Volga as access to the distant steppes of the southern Ural Mountains. In due course the Indo-Europeans [from the Black Sea basin] occupied an arc extending from the Adriatic, western Europe, and the Balkans across Ukraine to the Caspian Sea. From somewhere in this strip the Tocharians struck out east to settle one day in the Tarim Basin at the edge of what was to become the Old Silk Route.”

As we will now show, the 2500 B.C. date for the spread of the Proto-Indo-Europeans to Anatolia, Greece, the Indus and elsewhere is still too old and fraught with linguistic problems and contradictions. Let us begin this modest survey with the mainland Mycenaean Greeks who are assumed to have lived from around 1900-1200 B.C. when they then supposedly entered a 500-year Dark Age. Anthony reports that “Mycenaean Greek, … as recorded in the Linear B tablets, was clearly Greek, not Proto-Greek, by 1450 BCE, the date of the oldest preserved

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10 Renfrew, op.cit., p. 266
11 Anthony, op.cit., p. 75
Now here is the linguistic problem/contradiction: this early form of a clearly recognizable Greek being spoken around 1450 B.C. would also, by 700 to 600 B.C., some 700 to 800 years later, have become fairly “bushy and multi-branched,” and largely beyond the period that languages would be sharing so many close affinities and particularly words in common.

John Chadwick presents a broad, though not exhaustive, list of words in Linear B compared with words in Greek. He asks:

“Is Linear B Greek? Are the spelling rules [that Ventris employed in deciphering these texts] merely a convenient device to enable us to equate foreign words with Greek ones? There are many ways of answering this; perhaps the simplest is to compile a list of some of the words which are accompanied by self-evident ideograms:

<table>
<thead>
<tr>
<th>[Linear B]</th>
<th>[Ideogram]</th>
<th>[Greek]</th>
<th>[Meaning]</th>
</tr>
</thead>
<tbody>
<tr>
<td>ti-ri-po-(de)</td>
<td><img src="image.png" alt="Image" /></td>
<td>tripous (tripode)</td>
<td>‘tripod cauldron’</td>
</tr>
<tr>
<td>di-pa</td>
<td><img src="image.png" alt="Image" /></td>
<td>depas</td>
<td>‘vessel of some kind’</td>
</tr>
<tr>
<td>pl-a-ra, pl-je-ra</td>
<td><img src="image.png" alt="Image" /></td>
<td>phiale, phielai</td>
<td>‘dish’</td>
</tr>
<tr>
<td>a-pi-po-re-we, a-po-re-we</td>
<td><img src="image.png" alt="Image" /></td>
<td>amphipheous, amphipheous (dual in origin-rewe)</td>
<td>‘amphora’</td>
</tr>
<tr>
<td>pa-ka-na</td>
<td><img src="image.png" alt="Image" /></td>
<td>phasgana</td>
<td>‘swords’</td>
</tr>
<tr>
<td>to-ra-ke</td>
<td><img src="image.png" alt="Image" /></td>
<td>thoration</td>
<td>‘corslets’</td>
</tr>
<tr>
<td>ko-ru</td>
<td><img src="image.png" alt="Image" /></td>
<td>korus</td>
<td>‘helmet’ [with strap]</td>
</tr>
<tr>
<td>pa-we-a, pa-we-a</td>
<td><img src="image.png" alt="Image" /></td>
<td>pharea (originally pharwea)</td>
<td>‘cloths’</td>
</tr>
<tr>
<td>i-qo</td>
<td><img src="image.png" alt="Image" /></td>
<td>hippos</td>
<td>‘horse’</td>
</tr>
<tr>
<td>o-no</td>
<td><img src="image.png" alt="Image" /></td>
<td>onos</td>
<td>‘ass’</td>
</tr>
<tr>
<td>po-ro</td>
<td><img src="image.png" alt="Image" /></td>
<td>poleos</td>
<td>‘foal’</td>
</tr>
<tr>
<td>to-ra-nu, to-ra-nu-we</td>
<td><img src="image.png" alt="Image" /></td>
<td>threnus, threnues</td>
<td>‘footstool’</td>
</tr>
</tbody>
</table>

“(... Others less certain could be added; but the close correspondence with Greek words can be seen at a glance, and it is even closer if we substitute for the classical forms the older reconstructed ones. We must conclude that Linear B is either Greek or a language so much like Greek as to be indistinguishable from it.”

How can a language spoken around 1500 to 1200 B.C. be “Greek or a language so much like Greek as to be indistinguishable from it” that was spoken 600 to 800 years later? That would be like expecting to find the English spoken by Chaucer to

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13 Ibid., p. 49
be so like modern-day English as to be recognized as English or a language so much like English as to be indistinguishable from it. Yet historians and philologists never mention this chasm in the linguistic history of the Greek language.

Victor Ehrenberg fully admits that “The records in Linear B … were written in an early Greek otherwise unknown but [which is] clearly a precursor of Homeric Greek.”

That is, the Mycenaeans were living around the same time as the 12th Dynasty that Lynn Rose dated to the mid-to-later first millennium B.C. And in fact, Sir Arthur Evans has shown that Cretan artifacts that are dated to the Mycenaean epoch were found associated with those of the 12th Dynasty, that Middle Mycenaean II polychrome fabric vessels from Crete were found on a few Egyptian sites; particularly “it was found together with Twelfth Dynasty objects [which] included two glazed steatite cylinders of Sesostris [II] and Amenemhat III …”

Based on the same type of evidence historians use for dating pottery, Linear B had to have been written in the first millennium B.C.

In other words, linguistically historians assume that Greek stayed unchanged enough to be a direct link with Homeric Greek after about 800 years! This defies basic linguistics. In a period of about 800 years, the connections between Linear B and Homeric Greek should be barely detectable, and in no way so closely linked. Again, this ineluctably points to the placement of the Minoans and Mycenaeans well inside the first millennium B.C. One reviewer of Michael Ventris and James Chadwick’s *Documents in Mycenaean Greek* (1956), seeing this linguistic and chronological problem, said that the “Greeks in Knossos before 1400 [B.C.] are still something of an embarrassment,” but sloughs it off by assuming they should be there. These linguistic problems are endemic, as we have repeatedly shown in these volumes, with the established chronology.

When it comes to Sanskrit, another daughter of Indo-European, and the Mitanni, who spoke a Hurrian, non-Indo-European tongue, we encounter an identical problem/contradiction to the established chronology. Anthony reports:

“Old Indic, the language of the *Rig Veda*, was recorded in inscriptions not long after 1500 BCE but in a puzzling place. Most Vedic specialists agree that the 1,028 hymns of the *Rig Veda* were compiled into what became the sacred form of writing in the Punjab, in northwestern India and Pakistan, probably between about 1500 and 1300 BCE.”

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17 Ibid., p. 108
18 Anthony, op.cit., p. 49
The problem is, as Mallory explains, “These were initially preserved only in oral form, but there is abundant circumstantial evidence to indicate that they were written down by the sixth century B.C.”\(^\text{19}\)

That is, the linguists assume, based on the established chronology, that although the earliest possible time for writing down Old Indic in the Vedas is 500 B.C., they claim these go back perhaps as far as 1500 B.C. and were memorized and transmitted orally with little change for about 1000 years. There is no proof of this, only the assumption that this occurred. Even over 1000 years the original Old Indic must have changed immensely. But the fact of the matter is that it did not. Anthony goes on to show:

“But the deities, moral concepts, and [most significantly the] Old Indic language of the *Rig Veda* [of the 7th century B.C.] first appeared in written documents not in India but in northern Syria.

“The Mitanni dynasty ruled over what is today northern Syria between 1500 and 1350 BCE. The Mitanni kings regularly spoke a non-Indo-European language, Hurrian … But all the Mitanni kings, first to last, took Old Indic throne names, even if they had Hurrian names before being crowned. Tus’ratta I was Old Indic *Tvesa-ratha* ‘having an attacking chariot’, Artatama I was *Rta-dhaaman* ‘having the abode of r’ta’, Arts’s’umara was *Rta-smara* ‘remembering r’ta’, and S’attuara I was *Satvar* ‘warrior’. The name of the Mitanni capital city, Waššukanni, was Old Indic *vasu-khani*, literally meaning ‘wealth-mine.’ The Mitanni were famous as charioteers, and, in the oldest surviving horse training manual in the world, a Mitanni horse trainer named Kikkuli (a Hurrian name) used many Old Indic terms for technical details, including horse colors and numbers of laps. The Mitanni military aristocracy was composed of chariot warriors called *maryanna*, probably from an Indic term *mārya* meaning ‘young man’ … Several royal Mitanni names contained the Old Indic term *r’ta*, which meant ‘cosmic order and truth,’ the central moral concept of the *Rig Veda*. The Mitanni king Kurtiwaza explicitly named four Old Indic gods (Indra, Varuna, Mithra, and the Nāsatyas), among many native Hurrian deities … And these were not just any Old Indic gods. Three of them—Indra, Varuna, and the Nāsatyas or Divine Twins—were the three most important deities in the *Rig Veda*.\(^\text{20}\)

Is it probable that the language of the Mitanni, conventionally dated from 1500-1350 B.C., used Old Indic words that were written down in the 600s B.C.? Let us list them again:

1. “having an attacking chariot”
2. “remembering the cosmic order or truth”
3. “warrior”

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\(^{19}\) Mallory, *op.cit.*, pp. 36-37

\(^{20}\) Anthony, *op.cit.*, pp. 49-50
4. “wealth-mine”
5. probably “young man”
2. and 6. “cosmic order [and] truth”
7. [and] having the Old Indic deities “Indra, Varuna, Mithra, and the Nāsatyas (Divine Twins)”

Over a period of 1000 years, many of these words would hardly have stayed so alike that they could be easily translated. Instead, over a period of 1000 years, these words should have changed so greatly as to be nearly impossible to recognize, except the names of the gods.

In both the case of the Mycenaean Greeks and the Mitanni who, as we have shown in volume II, are the Medes, living in the first millennium, these linguistic problems vanish. Since the Mycenaees are dated to the 8th-7th century B.C. or possibly somewhat closer to the present, the Linear B true Greek language would naturally evolve into the classical Greek of Homer. Since the Mitanni are the Medes of the first millennium B.C. dated to the 7th-6th century B.C., or possibly somewhat closer to the present, their use of 7th century Old Indic words and gods is only natural. That is, in addition to all the scientific, technological, and other evidence employed in these volumes that show the Mycenaees and Mitanni/Medes are first millennium civilizations, the Indo-European linguistic evidence correlates with these and tells the same story, making eminently good historical linguistic, and chronological sense.

Lastly, let us deal with the Hittites. The scientific, technological, and other evidence for Hittite chronology, which places them well into the first millennium B.C. as alter egos of the Lydians, was presented in chapter 16 of volume I. The dendrochronological evidence for placing the Hittites in the second millennium B.C. was shown, in volume II, pages 353-378, to be substanceless. Therefore, having established these facts as evidence, we will deal primarily with linguistic evidence.

According to Renfrew, the language we term “Hittite represents the earliest instance which we have of a preserved Indo-European language: our earliest Indo-European inscription.”

But, as we have shown in volume I, pages 506-507, the Hittite language and the Lydian language, Arzawan, according to J.C. Macqueen, are the same language. To repeat Macqueen:

“And so we reach the final position that the language [of Lydia] known as Arzawan is in fact the language of the Hittites, while the language written in ‘Hittite hieroglyphics’ is a dialect of the language of Arzawa.”

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21 Renfrew, op.cit., p. 54
One of the languages of the Hittites is in fact the language of the Lydians, while another Hittite language is a dialect of Lydian. According to Albright and Lambdin, one of the Hittite languages was Luwian, but they directly state "We now know that Lydian was closer in some respects to Hittite than to Luwian."\(^{23}\)
That is, the Hittite languages supposedly spoken from around 1900 to 1200 included one language that was the same as the language used in Lydia of the 7th-6th century B.C. while still another Hittite language was a dialect of Lydian. How can Hittite stay static for about 700 to 800 years and still be the same language as the one used by the Lydians? This is again a linguistic impossibility. In this case the population of Hittite Anatolia retained an entire language of words meaning the same thing for perhaps 800 years or more as a gesture of benevolence to future linguists. As Mallory and Adams have said, this is “impossible.”
But there is still other linguistic evidence that leads to the same conclusion as pointed out by I.-J. Adiego:
“Lycia is located along the southwestern edge of Anatolia [an area supposedly destroyed by the Sea Peoples]. There have been a number of attempts to supplement the meager archaeological information available to us concerning the Bronze-Age peoples of this region by searching in the Hittite sources for place and ethnic names comparable to those of Lycia. But scholars are divided in their opinion on certain possible correspondences … The reason for disagreement is that phonetic similarity alone is not sufficient evidence for identification. It must be combined with a convincing geographical location for the places mentioned in the Hittite sources, something usually extremely difficult to obtain.
“Nevertheless, in a recently published inscription written in hieroglyphic Luwian recounting the campaign of the Hittite king Tudhaliya IV (second half of the thirteenth century B.C.), there are attested cities described in the land of Lukka [Lydia or Lycia], with names identical to those attested much later in the Greek and Lycian sources.”\(^{24}\)
That is, separated by about 700 years from the Hittites, the Lycians are naming cities with identical Hittite names. But supposedly the Hittite Empire was totally destroyed in Anatolia by 1200 B.C. by the invading Sea Peoples. How was it then that the Hittite/Lydian language is the same as two other Hittite languages, namely Luwian and Palaic, which somehow still continued to be written in various forms that have survived some 800 years later? Along similar lines, Drews shows:

\(^{22}\) J.C. Macqueen, *The Hittites and their Contemporaries in Asia Minor* (London 1986), pp. 25 and 179
\(^{23}\) Albright, Lambdin, *op.cit.*, p. 141
“A recent discovery has added to our direct information, proving that a Lycian held a post at the court of the prince of Byblos about 1800 B.C. [or over 1000 years before the Lycians existed in Anatolia]. The name Kuk (k) un (is) and national origin [from Lycia] are both given in a mortuary stele mentioning the name of the prince of Byblos in question, Abi-shemu. Since the same name appears in Lycia in the Hittite period (fourteenth century B.C.) and is also found in Lycian inscriptions of the sixth to fourth century B.C. we have a particularly clear example of the existence of daughter dialects of [Hittite] Luwian at about the time when Luwians are supposed by some scholars to have entered Asia Minor.”

On all linguistic levels the Hittites were Lycians using the same language and dialects and they named several cities with Lycian names, all of which places them in the first millennium B.C. In fact, because the Lycian heritage and history was largely given to the Hittites, we know nothing of their prehistory, as Ekrem Akurgal and Leo Hilber admit:

“Nothing remains of the pre-Lydian period. In all probability its first phase goes back to the period of the Hittite Empire, as shown by Hittite-sounding royal names …”

Finally, since the Hittites/Lydians lived in Greek times, they would employ aspects of Greek culture that helped them. To repeat the words of John Garstang yet again:


Kurt Bittel describes a Hittite depiction that “is painted with a procession of warriors in an awkward style yet unmistakably related to East Greek work of the 6th century B.C.” Mary G. Houston reports: “The Greek and Asiatic influences are clearly to be seen … If the tunic and small shoulder-wrap be compared with those of the Archaic Greek goddess from Attica … the similarity is evident; on the other hand the high cap and the boots resemble those seen on the costumes of figures in Hittite sculptured reliefs.”

So not only did the Hittite language not change for 800 to 900 years, but the Hittites used weapons and armor “almost exactly [like] the Athenian panoply of the fifth century [B.C.]” and dressed in a “high cap and boots” like those worn in Attica. Did they do this as a gesture of benevolence to future historians to enable them to make these connections?

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25 Drews, The End of the Bronze Age, op.cit., p. 141
26 Ekrem Akurgal, Leo Hilber, The Art and Architecture of Turkey (NY 1980), p. 25
27 Garstang, op.cit., p. 294 (capitalization added)
29 Mary G. Houston, Ancient Greek, Roman & Byzantine Costume (Mineola NY 2003), pp. 85-86
As we have seen, the closest language to Proto-Indo-European, Hittite, was spoken in the first millennium B.C. and it follows that the Proto-Indo-European language could not have died around 2500 B.C. because over the almost 2000 years separating Hittite/Lydean and Proto-Indo-European the linguistic connection would have disappeared. Proto-Indo-European must be placed at least over 1500 years closer to the present, just as with the Afro-Asiatic linguistic evidence. The evidence says the same for the Mycenaean Greek language which is directly related to Homeric Greek of the 6th-5th century. The evidence says the same for Sanskrit. All three Indo-European languages, Hittite, Sanskrit, and Greek, show either they are identical to a language of the 6th-5th century, as Hittite is to Lydian, or so closely related to the 6th-5th century languages that they are of the first millennium B.C., not the second. In all these cases linguistics support the short chronology and contradict the established chronology!

CLIMATE CHANGE: ARIDIFICATION ACROSS ASIA

Since the evidence in these three volumes places ancient Near Eastern civilization in the first millennium B.C. and the Near Eastern countries experienced simultaneously great climate aridification, then this very same change should be found all across Asia and also with the Indo-Europeans in Central Asia. In this way the climate shift that we described in the Sahara, Arabia, Mesopotamia, Anatolia, Greece, Crete, Palestine, and the Indus Civilization should be reflected across the vast region of Central Asia and even into China. Let us begin with China.

H.H. Lamb concluded that in early civilized times, before it became a desert, forests grew in the Gobi Desert.\(^\text{30}\) Stephen Schneider and Randi Lander tell us that the Tarim Basin, located in the Sinkiang province of China in Central Asia, “was once dotted with cities, settlements, and forests … Today the area is mostly desert.”\(^\text{31}\) That is, prior to the 8th century B.C. drying up of all these deserts, this region could support cities, settlements, and forests. This would simply be impossible if the climate ca. 6200 years ago or 4200 B.C. was the same as it is today. How could cities and forests exist prior to 4200 B.C. when civilization, according to the established chronology, did not even exist then? The only way to have cities,


\(^{31}\) Stephen Schneider, Randi Lander, *The Coevolution of Climate and History* (San Francisco CA 1984), p. 102
settlements, and forests thriving in this region during historical times, is that there was much more rainfall to support cities and settlements, and permit forests to grow. Lamb further reports that “Archaeological evidence from China, in particular a Neolithic village of Yangshao culture of 5000-6000 years ago [3000-4000 B.C.], apparently supported by many inscriptions on oracle bones from as late as the period 1,400-1,100 BC indicates … that through the warmest postglacial times, the winter climate was generally about 5°C [9°F] warmer than now … the whole year 2°C [3.6°F] warmer than now, rain was commoner than snow in winter, the various species of the natural fauna and flora ranged farther north than now, and crops like rice could be sown about a month earlier.”

Chu Ke-Chen, in analyzing the evidence of the Yangshao, points out that the fauna that existed in Ban Po near Sian found that in excavations were subtropical animals that do not thrive in that northern area presently, clearly showing that the climate was much warmer and much more pluvial. These subtropical animals include water deer, tapirs, and bamboo rats. He further shows that the ancient Chinese people grew millet and vegetables, but not rice. Chu Ke-Chen further suggested that bamboo also migrated south after Yangshao times by some three degrees or over 200 miles. K.A. Wittfogel, in analyzing oracle bones from the Shang Dynasty, concluded that the climate of China was notably warmer during Shang times and noticeably cooler thereafter.

With respect to the early Europeans living at that time, William Ryan and Walter Pitman show that the forests in the Tarim Basin lasted well into the historical period: “Mark Aurel Stein’s fabulous journey to the Tarim Basin of Chinese Turkestan [where the Indo-European language Tocharian was spoken] was preceded by a lesser-known but equally daring and perilous adventure of twenty-nine-year-old Sven Anders Hedin. On January 14, 1896, he and four porters left the desert outpost of Khotan in search of the ancient city of Makan, hidden beneath desert sands since antiquity. The explorer struck out into no-man’s-land, a sea of monstrous [sand] dunes rising to heights of three hundred feet. For weeks his party penetrated the lifeless world until they literally stumbled upon ‘a dead forest of sun-bleached, wind-scoured tree stumps protruding through the sand.’ At the edge of the forest were structures crafted not of stone or mud-brick, but of hand-hewn posts and walls of reeds attached by twine to stakes and plastered over with clay. The polished interior walls were painted with colorful murals depicting both women in flowing garments kneeling in

34 K.A. Wittfogel, “Meteorological Records from Divination Inscriptions of Shang,” *The Geological Review*, vol. 30 (1940), pp. 110-113
prayer and men with black beards and mustaches that were clearly not Chinese. The pictures included nautical scenes of boats sailing on a vast inland lake. Further digging into the ruins revealed docks for the boats and wood for their keels. Hedin wrote that this lost world

“was one of the most unexpected discoveries that I made throughout the whole of my travels in Asia … who could have imagined that in the interior of the dreaded Desert of Gobi, and precisely in that part of it which in dreariness and desolation exceeds all other deserts on the face of the earth, actual cities slumbered under the sand, cities wind-driven for thousands of years, the ruins of a once flourishing civilization? And yet there stood I amid the wreck and devastation of an ancient people.’

“The mummies found in recent years are from other settlements where the obscuring sand has been cleared away. One woman with long auburn hair and distinctive Caucasian facial features [preserved in this highly desiccated area] lived [there] during the Middle Bronze Age…”

Of these Indo-European Tocharians, Ryan and Pitman writes:

“The Tocharians … had deserted their adopted land when Marco Polo passed close to their remains. They had either been driven away by or succumbed to drought once the lakeside garden [there] turned into desert [like that of the Sahara] and the lake withered into seasonal pools. Their [Tocharian] language and mummified corpses announce their proto-Indo-European origin. Their fate in the Tarim Basin, resulting from too little water [from a major climate shift] …”

What we have here are Indo-Europeans living in the Tarim Basin just as the Afro-Asiatics lived in the Sahara, in the desert strip across Africa, Arabia, and Mesopotamia in climatic conditions that allowed for forests and flora to thrive much farther to the north. There is a parallel climatic history all across the vast continental regions of Saharan Africa to the Gobi Desert. But as we know from Egyptian reliefs this rainy climate lasted well into the Old Kingdom and elsewhere as noted above. Because the work of Lynn Rose lowers the length of Egyptian history by at the very least 1477 years, and with other scientific, technological, and other data, the Indo-Europeans also, in terms of this as well as the climatic evidence, lived about 2000 years closer to the present than historians and philologists have assumed.

The very same chronological climatic shifts must also apply to the vast Eurasian steppe running from the Ukraine across to Siberia, and that is just what transpired. David Anthony has outlined the vegetational shifts based on radiocarbon-dated pollen cores from the Don River in Europe to the middle Volga River in Russia to the upper Irtys River in Siberia at latitudes over the great Eurasian steppelands.

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35 Ryan and Pitman, *op.cit.*, pp. 213-214
As with the Sahara, there were two major shifts. During the first period he reported that on the Don the flora was “[b]irch-pine forest on sandy river terraces. On [the] floodplain, elm and linden forest with hazelnut & black alder. Oak and hornbeam [are] present …” In the middle Volga, “[o]ak trees appear, join elm, hazel, black alder forests …” As for the upper Irtysh, there was “[b]irch-pine forest evolving to open pine forest in forest-steppe, with willow near waterways. In [the] steppe, Artemisia and Chenopodia.”

In essence, there were forests of various types growing on the vast Eurasian steppe at the earlier period. Therefore, in line with the Sahara, this environment should have first become less wooded for a period of time which we date to around 1500 B.C. and then a second climate shift should have caused the steppes to lose most of these woodlands, to be replaced by grasslands, ca. 800 B.C. That, too, is the case.

As for the first climate shift, wherein the forest land should be diminished, Anthony reports for the Don in Europe “[s]light reduction in deciduous trees, increase in Ephedra, hazel, lime, and pine on [the] floodplain.” For the middle Volga, “[the] lake [there] slowly converts to sedge-moss. Swamp Typha reeds peak. Pine and lime trees peak. Probably warmer.” Oddly enough for the upper Irtysh, this did not happen, and the “forest expanded.” However, with the second climate shift in all cases the woodlands greatly decline. For the Don it is “Very dry [and there is s]harp forest decline. Ceralia [grasses] appear. Chenopodia [show a] sharp rise. Maximum aridity.” For the Volga region there is a “[r]eduction in overall forest. In forest, pine [is] down, birch up [and] Artemisia, an arid herb indicator, increases sharply. [The l]ake is [then] covered by alder shrubs …” For the upper Irtysh, “[f]orest retreats, broadleaf [type of tree] declines. [The] Mokhove bog on the Tobol dries up [and] steppe [vegetation] grows.”

The conclusion is overwhelming that all the regions discussed in these volumes which experienced a rich pluvial flora and fauna before 1500 B.C. then experience a two step climate shift that ends with the entirety of these regions becoming far more arid than previously, the last occurring around 800-750 B.C. As Elena Efimovna Kuz’mina, speaking of the Indo-Europeans, states: “Steppe migrations were brought about apparently by … the climatic crisis of the increasing aridity of the climate, recorded simultaneously from Central Europe to China.” And, in terms of dating this aridity Morris Berman reports: “Some historians believe that the period from 1200 through 500 BC was characterized by great aridity …”

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37 all citations in this paragraph, Anthony, *op.cit.*, p. 301
38 all citations in this paragraph, *ibid.*
have forcefully argued that these date to the period from ca. 1500 through 800 B.C., which is close to these dates and clearly in line with all the rest of the evidence. Thus, from the Sahara, Arabia, Mesopotamia, Anatolia, the Aegean, Greece, the Indus, to Chinese Turkestan, the Gobi Desert, and the great Eurasian steppe, the climatic shifts all coincide, correlate, and converge in support of the short chronology. No other historical revision exhibits this, nor does the established chronology fit this evidence. It clearly fits Velikovsky’s chronology based on astronomical catastrophic pole shifts in that his first Venus catastrophe destroyed much of the human race across the face of the Earth, so that the survivors could spread into these areas, bringing their language with them, as explained by Nicholas Wade:

“If a large land area is wiped clean of people, those who recolonize the empty lands will create a spread zone of their own language. The last Glacial Maximum depopulated the northern part of the Eurasian continent between 20,000 and 15,000 years ago [the catastrophe ca. 1500 B.C. did the same]. Those who returned could have been the speakers of the ancient language that preceded Proto-Indo-European and other large language families. This postulated [but unproven] ancient superfamily is called Nostratic by some scholars and Proto-Euroasiatic by the linguist Joseph Greenberg …

“Another major perturbation of Mosaicism may have been agriculture [or pasto-ralism] … from each center where agriculture [and pastoralism] was first developed, populations may have expanded outward, spreading their languages with them …

“In some cases a single center spawned several different language families …

“The center in the Near East was the source of at least two major language families. One was the Indo-European family of languages. Another was the Afroasiatic.”

This does not mean all areas were completely depopulated. Small pockets of survivors as well as major pockets survived this first cataclysm to spread into and across the lands around them that could support their flocks. The thesis of the short chronology requires not only that the history of the human race is much shorter, but that prehistory is much shorter as well and must be reduced by 2000 to 3000 years, or possibly more. At this point, however, with our work on historical chronology based on scientific and technological foundations nearly completed, we end this effort for the ancient Near East. In volume IV of Pillars of the Past, we will examine the evidence of Stonehenge and the Megalithic North as this relates to Velikovsky’s chronology. In time, it is believed, historians will gradually be forced to come to the same conclusions as outlined in these volumes in spite of their incapacity to accept the forensic evidence that so clearly and directly contradicts all the false foundations they have built on. All major reforms/revolutions in science take time in which to take hold and grow. The revolution

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41 Wade, *op. cit.*, p. 106
called for by the work of Heinsohn, Rose, Sweeney, and Velikovsky may require centuries to be recognized and may even be dishonestly attributed to others yet to come. Hopefully, by that time, historians, archaeologists, and others involved in such research will have developed the capacity to give credit to these pioneers rather than acting as an inbred, xenophobic group that only allows insiders’ concepts to be disseminated and accepted. This will mean that the historians’ endeavors will have matured. That at least is the hope. Until that time, as Velikovsky presciently and correctly pointed out, the ancient history along with the prehistory of the human race will remain *Ages in Chaos*.

**LAST WORDS**

“Fundamental challenges to disciplines tend to come from outside. It is customary for students to be introduced to their fields of study gradually, as slowly unfolding mysteries, so that by the time they can see the subject as a whole they have been so thoroughly imbued with conventional preconceptions and patterns of thought that they are extremely unlikely to question its basic premises.”

Martin Bernal
*Black Athena* vol. I
(New Brunswick NJ 1987), p. 3

“The scientific spirit requires a man [and woman] to at all times readily dump his whole cartload of beliefs, the moment experience is against them.”

Charles Sanders Peirce
*Collected Papers of Charles Sanders Peirce* vol. I
Charles Hartshorne, Paul Weiss, eds.

Scientific and technological evidence always will override historical interpretations of documents and what is dug up from the earth. The great revision of Heinsohn, Rose, Sweeney, and Velikovsky championed by me in these volumes is based on this first sentence above.

The most difficult and persistent problem faced by revolutionary ideas is the capacity of insiders in the establishment to publish and spread malicious dis-information that appears to their colleagues and the public to be solidly based in fact when it is not. The best example for this readiness to accept these mis-representations based on the deep-seated contempt historians feel, in the case
presented in this work, of a massive chronological revision of ancient Near-Eastern history, is that of Jacques Berlinerblau in his analysis of Martin Bernal’s revision which actually lengthens Egyptian chronology by about 300 years:

“One of [the] most daring acts [is] the challenge [anyone] poses to the standard chronologies used by historians … This means that [one] redates historical events which a community of scholars have already dated to their satisfaction. Tinkering with dates is dangerous business, for scholars often unquestioningly accept these chronologies and base their research on the dates they provide. Subsequent generations of researchers predicate their findings on those who have unquestioningly accepted the reigning system and so on. Accordingly, when a heretic correctly redates these events, an academic domino effect occurs whereby one theory collapses only to discredit a subsequent theory, and so on. As far as antiquity studies go, an attack on … chronology is an unparalleled act of aggression.”

Berlinerblau’s use of the words “Tinkering with dates is dangerous business” contains the emotionally loaded term “dangerous.” Its inference is that tinkering with dates is seen as a threat. Yet how could it be a threat or a danger if historians truly knew, as well as felt, that their chronology was based on solid foundations? One plus one equals two, and, should anyone suggest otherwise, that would not be seen as a danger or a threat. It is only when one does not have a chronology based on solid foundations—forensic scientific foundations—that tinkering with dates becomes frightening and is seen as a threat. And this, too, Berlinerblau admits, stating that those who tinker with dates pose an “unparalleled act of aggression”—again the emotionally loaded term “unparalleled act of aggression.” The revision is not seen from the perspective of logic, reason, and evidence, but as an act of aggression to be dealt with by counter-aggression.

This response can be readily observed in the comments of historian James D. Muhly who poses an argument that is loaded with emotional baggage and counter-aggression:

“Extremist [revisionist] scholars usually maintain that their position is so obviously correct, so clearly superior, that only a grand conspiracy has kept it from gaining acceptance. The feeling of neglect and rejection that fuels the drive and determination of extremist scholars gives their work an unmistakable aura of paranoia.”

We are paranoid and this can be easily seen by historians imbued with and educated or, as I suggest, indoctrinated into the established chronology. That is, Muhly suggests, without any credentials in the field of psychiatry, that he knows a

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42 Jacques Berlinerblau, Heresy in the University: The Black Athena Controversy (Piscataway NJ 1999), p. 49 (emphasis added)
thesis that undermines/attacks the one he accepts, is that of a paranoid personality. The same can even be found in the deliberations of philosophers of science. For example Paul Feyerabend, who presented the concept that to understand science there is no hard and fast rule but that objective truth/knowledge can be obtained by all methods of inquiry. Therefore, he held that Velikovsky’s approach to knowledge of the past by psychoanalyzing humanity’s myths was a valid method by which to discover such knowledge. As we have already seen in chapter 1 of this book, scientists are doing just that. In correspondence with I. Lakatos, Feyerabend was chided by Lakatos in a letter stating:

“… I do not know whether I told you that Peter Clark wrote a paper two years ago on the Velikovsky programme [method] and … I told him he should first get his Ph.D. in a more reputable subject and now of course he writes on the phenomenological theory over the kinetic one up to 1905. After that he will finish, with Urbach’s help, his demonstration that Velikovsky’s research programme is ludicrously degenerating; or, rather, that it does not even add up to a research programme.”

According to Lakatos, a degenerating programme is one that does not lead to further research and new knowledge. Therefore, all the continuing research and evidence/new knowledge presented in these volumes and others that enlarges on Velikovsky’s original work is not true research and cannot have discovered new knowledge. All this is, of course, stated without one scintilla of scientific evidence as support. The outlook of Muhly and Clark presumes that established historical knowledge/chronology is solidly founded, but I suggest it is the insider’s vision which is not supported by scientific evidence. Lloyd S. Kramer points out what surely underpins Muhly’s own emotional frame of mind: “Professionalization has contributed immensely to the understanding of the past. It has also often taken the form of a certain arrogance when historians claim to speak with the authority of a scientist.”

Muhly’s assertion is not backed up by scientific evidence but relies on the emotional expressions “extremist scholars” whose work exhibits “an unmistakable aura of paranoia.” This lack of rational response to any form of major chronological revision of the past is ubiquitous and has been the norm rather than the exception, as has been demonstrated in these volumes and elsewhere. Consider, for example, the manner in which Egyptologist Barbara Mertz dealt with Velikovsky’s thesis presented in Oedipus and Akhnaton:

“One psychologist [Immanuel Velikovsky] has gone Freud one better: he not only supplied the missing details of Akhnaton’s childhood, and pronounced him to be

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44 Imre Lakatos, Paul Feyerabend, Mateo Motterlini, For or Against Method: The Lakatos-Feyerabend Correspondence 1968-1974 (Chicago IL 1999), p. 355
suffering from an Oedipus complex, but proposed the novel theory that Akhnaton was, in fact, Oedipus.

“I am doing historians who employ psychological techniques a grave injustice by mentioning the Oedipus-Akhnaton theory for it cannot be taken seriously, either as psychology or as history. It is representative of one of the lunatic schools which flourish around the fringes of many fields of scholarly discipline, and it differs from the outpourings of the Pyramidiots only in the air of verisimilitude it creates. You need not know anything about Egyptian archaeology to realize that the writings of the Pyramid mystics are nonsense; it is the reason that Egyptologists seldom bother to refute them. Admittedly, Egyptologists do not often argue in public with people like the author of *Oedipus and Akhnaton* either; but they ought to do so. Certainly, the Oedipus-Akhnaton equation sounds ridiculous. It is ridiculous; but it should not be dismissed without investigation. We cannot afford to dismiss any theory just because it flatly contradicts all our preconceived notions of actuality. The Oedipus-Akhnaton theory is invalid, not because it is new and startling, but because it is based upon a series of misstatements and misinterpretations, presented with considerable skill and with a respectable imitation of scholarly style, whose errors can only be perceived by a reader who knows a good deal about Egyptian culture. Yet its basic sin against true scholarship, the same that mars the books of the Pyramid mystics, is that the author is not working with an open mind. He is not using facts to construct a theory, but is selecting facts to support a preconceived and unshakable belief. Whatever the techniques a historian chooses to work with, he must use them without prejudice and be prepared to revise, or dismiss, his theory when he runs up against a fact his tools cannot handle.”

In this brief citation we can see the emotional baggage Mertz brings to the discussion. While she wrestles with her rational side to deal with Velikovsky and his thesis on a balanced historical level, one cannot help noticing the loaded emotional statements pouring forth which she cannot restrain. She feels she is doing her colleagues a “grave injustice by mentioning the Oedipus-Akhnaton theory,” which underlies her fear of even mentioning Velikovsky by name whom, indeed, she does not name, as in so doing she will contaminate them. But her colleagues are supposed to be adults and do not need protection; they can certainly judge for themselves. Saying the theory “cannot be taken seriously” and relating it through guilt by association to “Pyramid mystics” is clearly an attempt on her part to disparage the work in the eyes and minds of both her professional and non-professional readers. Mertz first says “Certainly the Oedipus-Akhnaton equation sounds ridiculous.” But then her emotional side cannot contain itself when she states: “It is ridiculous; but it should not be dismissed without investigation.” Her investigation gives us a list of her generalities, none of which are backed up by a single citation from Velikovsky

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46 Barbara Mertz, *Temples, Tombs, and Hieroglyphics* (NY 1978), p. 312
nor by a single authority. Her proof for her rejection of the theory is simply by making accusations without a shred of evidence to support them and assuming no one will question her authority regarding these accusations. This is often the way Velikovsky’s books have been dealt with. No evidence is given, only ex cathedra judgements. Judgements are not, and never have been, evidence.

Martin Bernal, while rejecting Velikovsky’s chronology, claims that his rendering of Oedipus as Akhnaton appears to be completely valid:

“Velikovsky pointed out many similarities between the stories [of Oedipus and Akhnaton]: Oedipus’ ‘swollen foot/leg’ and the grossly enlarged legs in all representations of Akhnaton; Oedipus’ incest with his mother and the complex patterns of incest in the court of Akhnaton; the oracular role of the blind seer Amenhotep, son of Hapu, and Teiresias, both of whom were associated with serpents, the ‘fratricidal’ conflict between Oedipus’ sons Eteocles and Polyneikes and Akhna-ton’s heirs [and sons] Smenkare and Tutankhamun; and the brothers’ tight relations with their sisters.

The royal uncle, Ay in Egypt and Kreon in Boiotia, played remarkably parallel roles. Furthermore, the Greek legends have other Egyptian associations, such as the name Thebes and the Theban sphinx, whose riddle has striking solar associations that fit the role of the great Egyptian sphinx…”

Egyptologist Bob Briar of Long Island University in his book The Murder of Tutankhamen (NY/Berkeley CA 1999) maintains, just as Velikovsky does, that the son of Akhnaton on coming to the throne was murdered by his uncle Ay in like manner as Oedipus’ son was murdered by Kreon. But sadly, Briar fails to mention that he took his theme directly from Velikovsky. However, by using part of Velikovsky’s thesis, Briar shows that he found merit in Velikovsky’s concept. Interestingly, the German writer Philipp Vandenberg the same year did just what Mertz advised. According to John Richardson:

“But what of the relationship between Akhnaton and his mother? … Velikovsky’s assertion that Akhnaton, like Oedipus, was engaged over an extended period of time in a sexual relationship with his own mother is likely the strongest one in the literature, and he has managed to convince a number of archaeologists.”

In this case, unlike Mertz, we have a citation for “Philipp Vandenberg, Nefertiti, An Archaeological Biography (Philadelphia PA 1978).” Vandenberg, an internationally acclaimed and respected researcher and author, himself states, without any of the emotional qualms or baggage of Mertz:

“Strange as it may seem, there is a distinct possibility that during the first few years of her marriage, Nefertiti had to share her husband [Akhnaton] with her mother-in-law—

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47 Martin Bernal, Black Athena Writes Back (Durham NC 2001), p. 336
48 John Richardson, Singing Archaeology: Philip Glass’s Akhnaton (Hanover NH/London 1999), p. 161
49 Ibid., p. 276
that Tiy’s relationship with her son may have gone far beyond the bounds of a normal mother-son relationship. Evidence for such a hypothesis has been found in the tomb of Yuya, the ‘Manager of the House of the Great Dual Treasure House, and the Harem of the Great Royal Wife Tiy,’ with its unusually numerous and detailed wall pictures.

“Yuya’s tomb can be dated fairly precisely to year 12 of the reign of Amunhotep IV [Akhnaton]. But although Amunhotep III had been dead for those twelve years, the tomb inscriptions still refer to [Akhnaton’s mother] Tiy, not only as ‘the mother of the king,’ and ‘the queen mother,’ but also as ‘great royal wife’ and ‘great queen Tiy.’ In one depiction of the nightly enjoyment of the last beverage, Tiy sits across from her son. Nefertiti [Akhnaton’s wife] must be content with a place behind him.

“The American psychologist Immanuel Velikovsky carefully examined Yuya’s tomb and arrived at a provocative theory about Beketaten, ‘the king’s own daughter.’ To do so, he used an astonishingly accurate rule of thumb of Egyptologists, which says that, in depictions of children, all who are the same size are about the same age. In Yuya’s tomb Beketaten is depicted the same size as Nefertiti’s third daughter, who is probably eight years old in the twelfth regnal year of Amunhotep IV [Akhnaton]. But how can Beketaten, ‘the king’s own daughter’ by Tiy, be only eight if her father, Amunhotep III, had been dead for twelve years? There is one convincing possible explanation–Beketaten is not the daughter of Tiy and Amunhotep III, but the issue of an incestuous union between Tiy and her son [Akhnaton].

“Finally, it is interesting to note that when Tiy died, Amunhotep IV had her laid to rest [for eternity] far from the tomb of her husband in the western royal mortuary valley near Thebes. In fact, he had her placed in his own tomb.

“Sexual relations between mother and son were fairly rare in ancient Egypt–they were considered perverse.”

Here, then, we have a respected researcher into archaeology, Vandenberg, doing precisely what Mertz stressed, namely the theory “cannot be dismissed without investigation” which he undertook. Vandenberg did not come to the conclusion that Velikovsky’s work belonged to the “lunatic schools” of the “Pyramid mystics.” He certainly knew a great deal about “Egyptian archaeology,” though not an Egyptologist or archaeologist, enough to understand Velikovsky’s thesis had historical merit, saying Velikovsky “used an astonishingly accurate rule of thumb of Egyptologists” to reach his conclusions. He saw nothing “ridiculous” in Velikovsky’s work; knowing “a good deal about Egyptian culture,” he found that Velikovsky was working “with an open mind” using facts appropriately and without “prejudice.” He did not feel he had to protect historians and the public from knowledge of Velikovsky, and cites evidence which Mertz never cited. His judgement is backed up by evidence which not one Egyptologist has, so far as we know, answered!

At this point, I wish to do a tentative analysis of Mertz’s psychology. As a teacher of handicapped children, I often had to deal with many who were emotionally challenged or physically very debilitated. Some of my supervisors, when they observed me working with a highly sensitive, difficult case, often became agitated and removed themselves hastily from the home teaching site because they could not handle the highly charged environment in which I worked. I believe that Mertz may have had her own Oedipal or other emotional problems and reading Velikovsky’s work on Akhnaton triggered her outburst; her switching back and forth from the rational to the emotive statements suggests she was dealing with emotional issues beyond her grasp and that is a pity because, I believe, she vented her emotive fears on a decent, honest scholar.

Cyril Dean Darlington reports on

“the cause of Akhnaton’s failure … An important factor, perhaps the decisive factor, was the disastrous results of his marriages. His incestuous marriage gave only daughters; his other marriages gave sons who died young …

“There is one respect in which this influence, reflecting the character of Akhenaton himself, stretches across the seas and across the ages. The life and death of the king have been held to make him the prototype of Oedipus. The detailed comparison of Velikovsky shows us [this] …

“These similarities indicate that it was a true Egyptian story which was taken to Greece …”

Does Muhly believe Mertz did not act in an almost paranoid manner when she was discussing Velikovsky’s historical work? To this evidence I will add yet again that Velikovsky’s historical research was commended by Claude F.A. Schaeffer. Etienne Drioton, a historian and world authority on Egyptology, who commended Velikovsky’s book *Ages in Chaos*, wrote in a letter:

“Dear Doctor [Velikovsky],

“You have so kindly sent me a copy of your fine book, *Ages in Chaos*, which I received this morning, and which I have already read almost in its entirety, so stirring and fascinating is it.

“You certainly overturn, and with what zest! many of our historical assumptions, which we have considered established. But you do it with a total absence of prejudice and with impartial and complete documentation, all of which is most gratifying. One


52 see Immanuel Velikovsky, *Stargazers & Gravediggers* (NY 1983), p. 318
might dispute point by point your conclusions: whether one admits them or not, they will have posed the problems anew and made it necessary to discuss them in depth in the light of your new hypotheses. Your fine book will have been in every way of great use to science …

“Etienne Drioton”\(^53\)

Robert H. Pieffer, Chairman of the Department of Semitic language at Harvard University, and an authority on the Bible, wrote about Velikovsky’s book on biblical history, *Ages in Chaos*, on the back of the inside cover the following: “Dr Velikovsky discloses immense erudition and extraordinary ingenuity. He … documents all his statements with original sources … His conclusions are amazing, unheard of, revolutionary, sensational … If Dr Velikovsky is right, this volume is the greatest contribution to the investigation of ancient times ever written.”

William F. Albright, professor emeritus of Semitic languages at Johns Hopkins University, stated about Velikovsky’s *Oedipus and Akhnaton* in the *New York Herald Tribune*: “Only one contemporary writer can blend … Greek legend, Egyptian archaeology and Freudian psychoanalysis … the author’s achievement must be rated first class in its genre. Nothing is wanting: well-written [with] erudite Egyptological documentation.”\(^54\)

Gertrude Smith, Chairperson of the Classics Department of the University of Chicago, wrote of *Oedipus and Akhnaton* that the book which “solves problems in Egyptian history … is excellent reading.”\(^55\) In essence, we have researchers and ancient historians lauding Velikovsky’s book, *Oedipus and Akhnaton*, or agreeing with some of his conclusions. Some of these were giants in these fields. But Mertz kept this information from her readers in order to attempt to destroy Velikovsky’s reputation and work. Her behavior can best be described by how E.I. Carlyle analyzed William Corbette who

“… possessed the supreme qualification for successful controversy, an unfailing belief in the entire truth of [her] own views and opinion. It severed [her] in two ways. In the first place, [s]he was so entirely convinced that [s]he was right and that those who opposed [her] were mentally incapable of reasoning justly, that [s]he frequently persuaded [her] readers. And in the second place having infallible certainty on [her] side [s]he had no need to make too careful an inquiry into evidence.”\(^56\)

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\(^53\) Ibid., p. 263-264


\(^55\) Ibid., p. 354

\(^56\) E.I. Carlyle in Ringo, *op.cit.*, p. 285
The tragedy is that Mertz is not alone in this characteristic. Far too many historians often act with the same bellicose approach to new concepts. One of these, in particular, Kenneth Kitchen, an Egyptologist, will be exposed below. Were all these historians incompetent, as Mertz’s remark that Velikovsky’s work “is ridiculous” would suggest, or is it Mertz who stands before us as the one who appears ridiculous? And of course, those in the historical community can withhold these commendations from their readers following in Mertz’s footsteps.

We have shown in these volumes and elsewhere that critics of Heinsohn, Sweeney, and Rose’s short chronologies have fabricated or falsified evidence in order to disprove this thesis. They are the very same types of fabrication and falsified evidence that has also been employed to disprove the short chronology, in this case that of Peter James et al.

Egyptologist Frank Yurco in “An Egyptological Response to ‘Centuries of Darkness’,”57 accused James et al. of overlooking “an important synchronism.” There he claimed that according to James et al., in Syria at the battle of Karkar in 853 B.C., the Egyptian pharaoh Osorkon II sent 1,000 troops to go up against the Assyrian king Shalmaneser III. Therefore, James et al. would have dated this pharaoh to the 8th century, which in terms of their own chronology would be impossible. What Yurco withheld from his readers is that, although the Assyrian records of Shalmaneser III say an Egyptian military group fought at that battle, they do not give the name of the pharaoh who sent them. What Yurco did was simply apply the established chronology from tables in Kenneth Kitchen’s book to link these two kings. As James states, “Yurco’s ‘critique’ was so shoddily researched that it would shame an undergraduate.”58

Another example is that James et al. claimed Ramesses III used the abbreviation “Sesi” as his name. In this way they equate Ramesses III with the Egyptian king “Shishak” who supposedly invaded Palestine around 925 B.C. in the established chronology. The Hebrew name for Shishak could also be read as either “Shyshk” or “Sysk.” In 1991, Kenneth Kitchen, outraged by this proposed revision, claimed59 Ramesses III did not use “Sesi” as an abbreviated formed of his name. Tragically, the problem for Kitchen is that he simply lied about this: Kitchen was cited by James

et al.\textsuperscript{60}, showing Kitchen had actually published material to prove that Ramesses III indeed used the name “Sesi” on an inscription from Medinet Habu. So, after Kitchen claimed “Sesi” was an abbreviated name of Ramesses III, he then claimed James et al. were in error for saying exactly that. Frank Yurco in his own attack, page 11, repeated Kitchen’s misrepresentation, yet neither was taken to task by their colleagues for their clear-cut presentation of outrageous lies.

Kitchen, a defender of the status quo chronology, has gone after James et al. with what can only be described as undistilled belligerence. As James shows:


“For his example he chose the 21st Dynasty, claiming that the successor of Siamun, penultimate ruler of this dynasty, was the brother-in-law of Shoshenq I, founder of the next (22nd) Dynasty. Therefore, according to Kitchen, this rules out any overlap between the 21st and 22nd Dynasties, as we proposed.

“We responded in a letter [P. James and R. Morkot: ‘Letter, reply to Kitchen,’ Times Literary Supplement (June 7, 1991), p. 15], towards the end of which we focussed on his ‘single example’. We agreed it is known that a 21st-dynasty Pharaoh called Psusennes was the contemporary of Shoshenq I. (Kitchen opted to call him his ‘brother-in-law’.) This in itself shows that there was some overlap between the two dynasties. Further, there is no evidence that this Psusennes was the successor of Siamun and hence the last ruler of the 21st Dynasty, and thus nothing to rule out our proposed overlap between the two dynasties. We concluded that we ‘were confident that he [Kitchen] cannot demonstrate their successive nature without recourse to circular argument or reliance on Manetho [a late source from Hellenistic Egypt].’

“In his response Kitchen … failed to take up our challenge. So, in a final rejoinder …

“We noted:

“Kenneth Kitchen appears to have conceded the major point of his initial review. In our reply we challenged Professor Kitchen to produce hard evidence that the 21st and 22nd Egyptian Dynasties were successive rather than overlapping. Since he failed to respond, we can only assume he was unable to do so, replying on different matters entirely.

“To this date Kitchen has not replied to back up his ‘single point’ with any evidence, although he has never lost opportunities to make critical remarks about our work. …

“Kitchen also claimed that his case regarding the relationship between dynasties 21 and 22 was ‘backed by other evidence (the Neseramun family tree, etc)’. What the Neseramun genealogy says is actually rather surprising. The family trees of Egyptian

\textsuperscript{60} Peter James et al., “Centuries of Darkness: A Reply to Critics,” Cambridge Archaeological Journal, vol. 2, no. 1, p. 127
officials often mention under which Pharaoh a given individual held office. In this case the Neseramun genealogy specifically states that Siamun was the contemporary of two individuals. If Kitchen is right, one would expect from the rest of the genealogy that these individuals lived before the end of the 21st Dynasty. As it happens they did not. In genealogical terms they lived one to two generations after Shoshenq I, founder of the 22nd Dynasty [that is, there was overlap between the 21st and 22nd Dynasties]. … The evidence from the Neseramun family tree thus shows completely the opposite of what Kitchen claimed. …

“Kitchen's attempt to debunk our restructuring of chronology should now be a matter of increasing embarrassment.”

The last case, I will let James describe for himself:

“The worst case, evidently one of sheer fabrication, appeared in a [“Review of Centuries of Darkness,” Bulletin of the Anglo-Israel Archaeological Society, vol. II (1991/2)] by James Mellaart, a famous archaeologist and, until recently, a lecturer at University College London. While he made some favourable comments, he claimed to have access to an unpublished cuneiform text which gives a list of synchronisms between Lydia … and Assyria, running back 21 generations from the 7th century BC through to the Late Bronze Age. According to Mellaart it confirmed the conventional chronology and made ‘short shrift’ of our model. Apparently some scholars were taken in and rejoiced at our defeat. Alan Millard of Liverpool University, a noted expert on Near Eastern languages, praised Mellaart’s review as ‘appropriately negative’ [see A. Millard, “Review of Mellaart 1991/2,” Archaeology and Epigraphy ??, (1994) p. 27 – question marks in original]. Quite incredibly, Mellaart has never produced any evidence that such a unique text exists, outside his imagination. Despite his best efforts, Professor David Lewis, an eminent epigraphist at Oxford, could find no trace of such a tablet. Other scholars, such as cuneiform expert Professor David Hawkins of the School of Oriental and African Studies, are confident that the text is simply not real. With evident embarrassment, the editor of the Bulletin of the Anglo-Israel Archaeological Society, which had carried Mellaart's review, published a note, alongside letters from ourselves (James & Kokkinos 1992/3) and Lewis, stating that Mellaart’s “alleged documents … should not be cited as valid source material.” [S. Gibson, “Editorial Comment,” Bulletin of the Anglo-Israel Archaeological Society 12 (1992/3), p. 82]. And there this extraordinary episode ended. Mellaart does not appear to have mentioned his tablet since.”

On top of all this, there has been direct and individual suppression of evidence for chronological revision in some journals. As James further informs us:

“It is quite clear, for example, that no-one would be encouraged to publish articles agreeing with our [short chronological] model – they would probably be

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61 Ibid., pp. 6-7
62 Ibid., p. 14
automatically rejected. We have naturally encountered this problem ourselves. With respect to academic journals, we have been frustrated by a ‘lack of airtime’ in which we could defend our case against critics. For example, when the *Cambridge Archaeological Journal* published lengthy, and sometimes rambling and unjustified, criticisms from several scholars, the editor declined to publish our reply in the same issue and allowed only limited space in the next. In the case of *Antiquity*, our reply to Manning and Weninger’s ill-considered treatment of the radiocarbon evidence from the Aegean, was flatly rejected.”63

David Rohl also had to suffer such indignities with regard to his revision, as have Velikovsky, Rose, Heinsohn, Sweeney, and James *et al.* Ralph Ellis describes yet another assault on this revisionist:

“While most classical historians will stick rigidly to [the] pronunciation [of words in and between two languages] a few do recognize that pronunciation can change especially when crossing from one language to another. One such author, B.S. Isserlin, indicates that the Canaanite consonants ‘s’ and ‘th’ were amalgamated and adapted into the ‘sh’ consonant. The more radical author, David Rohl, similarly showed that the Egyptian ‘s’ could evolve into the Hebrew ‘sh’.

In fact, in his book *Legend*, Rohl supplies a long list of consonants that have been subtly altered when words were adopted into other cultures and languages. But the Egyptologist, Kenneth Kitchen, strongly disputes Rohl’s hypothesis of changing consonants [from Egyptian to Hebrew] saying:

“Rohl’s attempt[ed] equation … is totally false and ignores what is known of the linguistic facts. These are that between Egyptian and Hebrew, s is always reproduced as s, never sh – and sh as sh and never s.”

“Academics are normally so cautious, couching each and every postulation in terms of its degree of possibility; until, that is, they are challenged on a topic. Suddenly, all caution is thrown to the wind and the challenge is deemed to be ‘totally false and ignores what is known of the linguistic facts’. The problem is that such intemperate assertions are bound to fall flat on their face, for these consonants do indeed change as they are adopted by other languages. Here are a few examples:

<table>
<thead>
<tr>
<th>Egyptian</th>
<th>Hebrew</th>
<th>Meaning</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sar</td>
<td>Shar</td>
<td>barley (hair of)</td>
</tr>
<tr>
<td>Suara</td>
<td>Shul</td>
<td>chariot equipment</td>
</tr>
<tr>
<td>Suabebe</td>
<td>Shuabebe</td>
<td>go back</td>
</tr>
<tr>
<td>Subi</td>
<td>Shub</td>
<td>return …</td>
</tr>
</tbody>
</table>

“In addition to these ‘exceptions,’ discovered in a brief perusal of the Egyptian and Hebrew dictionaries, there are any number of words that transpose the ‘sh’ and ‘s’

63 Ibid., p. 16
within their own language; and this is especially so with both Egyptian and Hebrew.”

This author does not accept the revisions of James et al., or Rohl, and has explained their failures in terms of forensic historical evidence. Nevertheless, I have shown how even their more modest revisions have been greeted by the community of historians that James D. Muhly knows as his colleagues. If revisionists like James et al. and Rohl are to be seen as “extremist scholars” and their arguments as “paranoid,” what does Muhly consider the behavior of all his colleagues, outlined above and throughout these volumes to be? It is anything but objective and I dare say Muhly, if he ever reads these books, will never condemn them by name nor discuss their deeds in detail. Scholarly crimes by one’s colleagues are to be hushed up, buried, and hopefully forgotten. Heaven forbid the public should ever learn about such behavior.

Any major revision in this field has been answered by Kitchen with the same coarse behavior. Historian Alessandra Nibbi has summed up Kitchen’s reaction to concepts with which he disagrees:

“Kitchen never justifies his own view which is basically the traditional one and founded on a great number of assumptions … Kitchen casts it aside with some disdain and attempts to push all the evidence into its old shape, without stopping to consider whether it fits or not. Kitchen fills in all the lacunae in our knowledge with his own preconceived explanations and assumptions and is fully satisfied with the results he gets from this exercise.”

A typical example by Kitchen, unrelated to revisionist theories, is presented by Steven L. McKenzie when discussing a relief of Shoshenq’s raid into Palestine which contains a long list of place names:

“The British Egyptologist Kenneth Kitchen has very recently suggested that [King] David’s name is in that list. The name occurs in an expression that Kitchen translates ‘highland/heights of David’ … The immediate context, he says, is a set of places in southern Judah and the Negev (the southern part of Palestine) where the Bible reports David was active when he was fleeing Saul (Samuel 21-30). The area, Kitchen concluded, must have been known by David’s name.

“This occurrence of David’s name is less certain [because] much of the relief is damaged and illegible. Of the names that can be read, many cannot be identified for certain with any known sites in Palestine. Not all scholars agree with Kitchen that the names on the relief reflect any consistent geographical order. In addition, the Egyptian word translated ‘highland/heights’ is rare and its exact meaning is uncertain. In an earlier publication Kitchen himself calls the reading of these words ‘obscure.’

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64 Ralph Ellis, *Solomon: Falcon of Sheba* (Cheshire UK/Kempton IL 2002), pp. 33-34
65 Nibbi, *Ancient Egypt..., op.cit.*, p. 101
“Kitchen’s reasoning is curious. It is highly unlikely that the highlands of southern Judah and the Negev bore David’s name simply because he spent time there. The term ‘the highlands of David’ for this region does not occur in the Bible or anywhere else. If this interpretation were correct, it would indicate the opposite of what Kitchen intends. The ‘highlands of David’ would most naturally refer to an area within the territory of a clan or tribe. ‘David’ in this expression would then be a clan or its land—like Benjamin, Ephraim, or Judah—not an individual at all. If ‘David’ could refer to a clan or region, as Kitchen’s reading suggests, then he may never have existed as a historical figure.”

Here we have Kitchen acting just the way Nibbi described. Kitchen never justifies his own view that “highland/heights of David” is a reality except by a “great number of assumptions.” He admits the names on the relief are “obscure,” the term “highland/heights” is rarely employed, but he assumes that because David might have spent some time there, the place must have been so named. This in spite of the fact that nowhere in the Bible is the term “highland/heights of David” ever written; moreover, areas were named for clans, and David’s clan was not David, but Benjamin. “Kitchen fills in all the lacunae in our knowledge [of this identification] with his own preconceived explanations and assumptions and is fully satisfied with the results he gets from this exercise.” But in spite of all the many errors in his criticisms of the revisionists, James et al., and Rohl, he knows he is right and stands by his criticism.

In case after case, Kitchen fabricated evidence to demean and demolish revisionists along with their hypotheses. On top of this, Kitchen vents his spleen by attacking “[Frank] Clancy [who] alleges that the lives and dates of the pharaohs of the Third Intermediate are not secure,” and has the audacity (and ignorance!) to cite “[David] Rohl—like Velikovsky of old, an absolute crank! In trying to date Shoshenq [I] to 800 BCE! Nothing could be further from the truth.”

Yet look at all of Kitchen’s mistakes, ignorance, pomposity, and audacity, given his misrepresentations of the facts of history. Who then is paranoid, who is the absolute crank? What we see Kitchen doing when the bedrock of history he believes in is threatened by anyone is deplorable. Do any of his rantings and ravings against James et al., or Rohl, or Velikovsky, sound like a scholar, or rather like a man whose stability is threatened, and who strikes out with naked aggression and overt hostility? Kitchen, nevertheless, as cited by David I. Block, actually sees himself as an unbiased “factualist.” “According to Kitchen, the agenda of a ‘factualist’ is neither to prove nor disprove any theory concerning the early history of Israel, but simply to

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66 McKenzie, op.cit., pp. 15-16
examine the facts and let the data speak for themselves. But as we have seen repeatedly, Kitchen is a “factualist” only when this scholarly approach serves him, an “assumptionalist” when that approach serves him, and an “abuser” when that approach serves him. In the last case Kitchen employs his anger as a weapon to try to cower those by whom he is threatened into submission by truculent and degrading language. Who in his right mind would want to become involved with such an arrogant person? But because this ugly behavior plays such an important role in historic and academic warfare, it must be exposed for what it is. And we have tragically and repeatedly seen this low nastiness by historians throughout these volumes. None of this crass, brutal and callow behavior is ever dealt with honestly in professional historical literature. And, of course, Muhly will think me paranoid for drawing this to everyone’s attention. Much more probable, he and his colleagues will ignore such behavior on the part of his colleagues, thereby allowing these deeds to continue to be disseminated to historians and the public. What is indeed the worst aspect of this is that they present a psychological and public relations façade that they are sincere scholars, seekers after truth, who will follow the facts where they lead, when in reality, beneath that façade, shameful acts are perpetrated. In this regard, they are following in the self-same footsteps of the Church as it dealt with the great revisionist book *Dialogo* by Galileo Galilei, which was placed on the Index of prohibited books, as described by J.L. Heilbron:

“The removal of Galileo’s *Dialogo* from the Index canceled a black mark against the book but not against its author. The official rehabilitation of Galileo took another century and a half. It began around 1940 in connection with the three hundredth anniversary of Galileo’s death. That was not a good time for a party. Nevertheless Pope Pius XII approved a campaign to demonstrate the Church’s openness to science. As an indication of this openness and on the recommendation of his Pontifical Academy of Sciences, he commissioned an unrestricted biography of Galileo. The assignment went to Monsignore Pio Paschini, rector of the Pontifical Lateran University, a historian known for his balanced account of the Church during the Reformation. It was a bold choice, since Paschini tended to be liberal and judicious. These virtues worried some of the Pope’s senior advisors, who had the satisfaction of being proved right. Paschini took Galileo’s part, admitted that the condemnation had been an error, and lost no opportunity for criticizing the Jesuits, on whom he blamed the entire affair.

“The Jesuits objected. Paschini’s two-volume work disappeared in the review mechanism, much as academic articles submitted to scholarly journals do today. The

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anniversary date, 1942, was long past when the Vatican journal, *Civiltà cattolica* got around to the subject. It admitted that the Church, or, rather, intemperate and ill-informed churchmen, had erred in condemning Galileo; and it recommended that the best use that could be made of this fact was to forget it. Paschini understood and fell silent. As a reward he was made a bishop two months before he died and an honorary member of the Pontificale Academy of Sciences which had opposed the publication of his work. With this unimaginative solution, the Church shut up a work that it had commissioned to demonstrate its openness. No good administrator could have wished to leave the matter there. What to do? Wait. The celebration of the tercentenary of Galileo’s death had not worked out; the four hundredth anniversary of his birth might do as well, or, as it turned out, better. It fell during the Vatican Council, ten years after Paschini’s death had removed the party most interested in seeing his work appear as he wrote it.

“Paschini’s biography of Galileo appeared in 1968, heralded as an indication of the Pope’s program for the peaceful coexistence of religion and science. The general scholarly press reviewed the book favorably. Lay and clerical critics commended its balance. Very few knew that, to use the old expression of censorship, the book had in fact been indexed *donec corrigatur* [forbidden until corrected], and then corrected, before publication, by the keeper of the Jesuits’ archives. Since the publication of this collaborative work, efforts at rapprochement between science and religion have intensified, particularly under the auspices of John Paul II. In 1979, on the occasion of the centenary of Einstein’s birth, the Pope told his Pontificale Academy of Sciences that he wanted theologians, scientists, and historians to work together on a reassessment of the Galileo affair. He endorsed Galileo’s principles of biblical exegesis. He gave as an earnest of the project the Church’s sponsorship of Paschini’s biography, without knowing (let us hope) that it had been censored more crudely than the old books on heliocentric astronomy.

“Thirteen years later, in 1992, having received the reports of the study committees he had appointed, John Paul announced that the theologians who had condemned Galileo had erred. By not recognizing the proper distinction between the Bible and its interpretation, they had ‘transpose[d] into the realm of doctrine of faith, a question which in fact pertained to scientific investigation.’ The Pope then exonerated Galileo and confirmed the historical framework in which he had placed the problem of reconciliation. This framework identified the theologians of 1633 with the operations as well as with the edicts of the Inquisition, and the scientists of our time with a correct and timeless epistemology. The administrators, including some of John Paul’s predecessors, who had found ways to render the edicts of 1633 dead letters long before Galileo was removed from the Index dropped from sight. That is the fate of good bureaucrats.”

69 J.L. Heilbron, *The Sun in the Church: Cathedrals as Solar Observatories* (Cambridge MA/London 1999), pp. 210-211
In their book *Telling the Truth about History*, Joyce Appleby, Lynn Hunter, and Margaret Jacobs (NY/London 1994), page 27, explain how the Church attempted to keep the public misinformed about the evidence of Kepler, Galileo, and Newton for a long time. “The French schools and colleges, especially the ones controlled by the Jesuits, resisted teaching Newtonian science until the 1750s. As one historian put it, ‘If Newton finally triumphed in France, it was probably over the corpse of the Jesuit order.’” In like terms, if Heinsohn, Rose, Sweeney, and in part Velikovsky’s revisions finally triumph in academia, particularly in its historical circles, it will probably be over the corpse of the historians’ order. I tend to think that future generations will see these castigated revisionists, especially Heinsohn, Rose, Sweeney, and Velikovsky, like the French philosophers of the Enlightenment, as “trampling underfoot prejudice, tradition, venerability, universal assent, authority—in a word everything that overawes the crown—dares to think for himself, to ascend the clearest general principles, to examine them [through science], to admit nothing save on the testimony of his own reason and experience.”

Kitchen has argued that ancient documents must be considered reliable, and his position has a very long history. For example James Karl Hoffmeier reports: “If the historian thinks there is a problem with a text’s trustworthiness, I maintain, the burden of proof lies with the historian, not the ancient writer, who cannot explain himself to the historian … [Those] who insist that a statement be proven by an external source in order to be accepted as reflecting reality are committing the historical fallacy of presumptive proof, which according to [David Hackett] Fischer, ‘consists in advancing a proposition and shifting the burden of proof to others.’ “Kenneth A. Kitchen writes “It is normal practice to assume the general reliability of statements in our [ancient] sources, unless there is good, explicit evidence to the contrary. Unreliability, secondary origins, dishonesty of a writer, or tendentious traits—all must be clearly proved by adduction [that is, by citing an example to prove the case one way or the other] of tangible evidence and not merely inferred to support a theory.”

Here Kitchen “assumes”, not proves, “the general reliability of statements” in ancient sources. He brings forth reasons to reject these sources; “unreliability” is one. How does one determine this if we already have “assumed” the sources are “generally” reliable? “Secondary origins” makes sense, but “dishonesty of a writer” may be, in certain instances, impossible to prove, since a historian can only rarely find other statements on that particular topic by other authors. “Tendentious traits” may be well hidden by the author of the ancient source and his devious

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70 Ibid., p. 39
71 Hoffmeier, *Ancient Israel in Sinai, op.cit.*, p. 21
motivations may be impossible to know. Kitchen “claims” he can clearly adduce these, when in fact all that he is doing is “assuming” that he can. In this respect Bietak contradicts Kitchen and those who accept his dictum of the obvious reliability of ancient sources:

“In contradiction to archaeology, the philological disciplines dealing with ancient civilizations depend on sources which already at the outset are burdened with the biases and tendentious opinions of their ancient writers. The written source material is therefore by no means objective to begin with.”

Statements such as these and others scattered throughout these volumes by authorities of ancient history seem never to bother or influence Kitchen nor those who think like him. For example Baruch Halpern claims that the documentation and archaeological evidence well corroborate each other and revisionists like Velikovsky and Peter James et al. have simply misunderstood and misinterpreted this evidence to draw totally erroneous conclusions, when in fact we have shown that Kitchen has done just that repeatedly:

“Major revisionists, such as Immanuel Velikovsky or Peter James pick and choose among the interpretations furnished on individual details by specialists in order to justify elaborate reorganizations of knowledge. Their concern is not with a consistent interpretation of data in a subfield, but with the convenience of some particular observation, divorced from its context, to their point.”

Having made this accusation, Halpern would naturally be expected to support this allegation with examples and evidence, but he does no such thing. He has arrogantly made the claim and assumes that because he is an authority he is under no obligation to support this scholarly attack with evidence. What he has done is substitute his authority for evidence. In this sense, like so many other proponents of the established chronology, he attacked the revisionists with ex cathedra statements. Rather than following through on their scholarly duty, they have attacked the scholars providing the revisions but failed to address their evidence. He knows he is correct because he again presents his ex cathedra statement that the established chronology is correct: “What is important is that the premise is correct—namely that our historical reconstruction based on the text is basically accurate—not as to pedantic detail, necessarily, but as to [its] overall structure; [and] that our archaeological slice of time is in fact synchronous, and the interpretation accurate.”

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73 Baruch Halpern, “Text and Artifact,” in Silberman and Small, The Archaeology of Israel, op. cit. p.333
74 Ibid.
This is an entirely circular argument which in effect states Velikovsky and Peter James et al.’s revisions are in error because our chronologies, based on documentation and archaeological evidence, corroborate one another. They are wrong because we are right. With such logic, Halpern can prove anything. He then goes on to justify this circular reasoning logically by concluding: “Now, despite what logicians might claim, there is nothing intrinsically wrong with circular argument; a slight touch of paranoia never hurt anyone …”75 Why is circular reasoning not intrinsically wrong? The reason is because Halpern says it isn’t. That is, Halpern uses a circular argument to prove there is nothing intrinsically wrong in doing this. His total lack of logic is astonishing. Above all, he contradicts James Muhly who called revisionists paranoid by saying his approach to the evidence, and that of his colleagues who argue as he does, have only “a slight touch of paranoia.” It is like logically saying a woman is only slightly pregnant. It is with such arguments and authorities who appear to be fools and scoundrels that revisionists must contend, such as Kitchen and all the others exposed in these volumes who eschew scientific evidence for historical evidence.

This is the same Kenneth Kitchen we cited above who adduced that the Greek-like alphabetic script found in Arabia had to be dated to “about the 13th/12th centuries BC” in order to salvage the established chronology. The French epigrapher Jacqueline Pirenne claimed this alphabetic script “could not possibly date before 700 B.C., since its letters were clearly derived from the Greek alphabet[,] while t]he Académie française backed her view, as did academics in Britain, Italy, and Germany.” Obviously, we should heed Kitchen and ignore the authorities in this field. This is also the same Kenneth Kitchen who argued below for camel transportation “a long way back beyond the 9th century B.C. Suffice it to mention Old Babylonian attestation [of the camel] (early 2nd millennium B.C.), and the late 19th Dynasty model [of a camel] from the Ramessides … in Egypt.” This occurred without the invention of the camel saddle, and there is no evidence to explain why the Egyptians failed to depict the camel, which supposedly brought incense to Egypt prior to the first millennium B.C. And this is the same Kenneth Kitchen who was shown to be wrong on all his attacks on James et al. and Rohl, cited above. Yet he still argues he can adduce the historical and chronological truth from documents. He could not see in himself this “general reliability” as a source of historical evidence which was proved explicitly contrary to the evidence nor his own “unreliability” as a “secondary source,” nor see his own “dishonesty” as “a writer,” nor notice his own “tendentious traits.” In fact, Kitchen argues that historians don’t need science to determine historical or chronological knowledge,

75 Ibid.
because this knowledge can be adduced from documents. In a paper that Lynn E. Rose sent to me titled “Egyptian and Related Chronologies–Look, No Science, No Pots!”, Kitchen presents the position that one can generally and more accurately determine “Egyptian and Related Chronologies” with little or no recourse to science – “No Science” – and even with little or no recourse to archaeology – “No Pots.”

“At the end of the day, the chronology of the Ancient Near East/Ancient East Mediterranean has to depend on written sources for the period when and where they do exist. In their absence, [interpretative, not scientific/geological] archaeological sequences of assemblages using stratigraphy give us sequence, but not absolute dates. During the last century, highly ingenious ‘scientific’ procedures have been developed to try to overcome the problem of fixing absolute dates, especially when explicit written records are lacking, including use of astronomy, radio-carbon, tree-rings, ice-cores, and so on. However, each of these is subject to various flaws that prevent attainment of absolutely reliable results so far. Therefore, it may here be a service to indicate where we stand, if one makes careful use of written historical evidence on its own, until other methods can be freed of the problem of inherent sources of error.”

For astronomical dating, Kitchen cited Ronald A. Wells who misrepresented Lynn E. Rose’s book Sun, Moon, and Sothis, which we outlined in volume I, pages 111-117. He has further argued that Rose’s 39 lunar dates, nearly all of which accorded with the astronomical documents of the 12th Egyptian Dynasty, and the precise retrocalculations of the position of the Moon that fit these in the first millennium, fit there by chance; that the 23 of 27 lunar festival dates which followed the New Crescent Moon by precise intervals of days, namely 0, 2, 3, 8, 16 or 19 days, scattered over a fairly long period of time, all coincidentally matched the documents, and the retrocalculations also occurred by chance; and that nearly all the New Crescent Moons for these lunar dates being moved 1477 years closer to the present matched the documents and the retrocalculations strictly by chance, which allegedly invalidates Lynn Rose’s Sothic dating of the 12th Dynasty. Wells is arguing for impossibilities to discredit Sothic dating and Rose’s application of it. Wells has exhibited only indifference to Rose’s Sothic dating, and Kitchen’s relying on him to dismiss it is irresponsible. Kitchen argues of “Sothic and lunar date,” “The most those could ever do was to provide ‘fine tuning,’ but clearly we can well survive without this, even though it [large scale chronological dating by these dates] might be a desirable aim if really reliable means were established to permit it. But not yet!”

Notice that Kitchen accepts scientific Sothic and lunar

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77 Ibid., p. 170
astronomical data to “fine tune” a chronology only after documentary evidence has established it in general terms. Science, for Kitchen, is the hand maiden to documents. Notice also that Kitchen did not question Wells’s work but assumes its “general reliability” as a source was proved explicitly, contrary to the evidence, nor Wells’s own “dishonesty” as “a writer”, nor did he notice Wells’s own “tendentiousness” in all his misdeeds with Rose’s work.

Anatol Fomenko explains that historians cannot accept the facts of astronomical science when it comes to chronology if it refutes their system because:

“According to them [historians], there is no point whatsoever in questioning the consensual chronology of ancient history, since all the dates in question can be easily verified in any textbook on the subject and have been proved veracious a long time ago. … Moreover, it would perhaps be better for the [astronomical] mathematicians to occupy themselves with [astronomy and] mathematics and leave history to historians.”

Fomenko’s answer deserves citing:

“Firstly, … chronology, being a problem of calculating dates, bears immediate relevance to mathematics. This includes astronomical calculations; the verification of their precision, calendrical problems.”

Kitchen’s logic and attitude toward scientific evidence is evidently mirrored by that of his colleagues, who did not challenge his thesis that science is secondary. Kitchen’s comportment and demeanor is so like that of a character in science fiction literature that I do not hesitate to draw the comparison. Isaac Asimov, in his great series of novels, _Foundation_ etc., has created a character, the ambassador of the galactic empire which is at its height, yet is dying, unbeknownst to any but a few scientists. Lord Dorwin personifies and epitomizes the behavior of Kitchen as a foppish but learned man, who speaks with an affectation of his own importance and also like the cartoon character Elmer Fudd.

“… Lord Dorwin said: ‘Mahvelous. Twuly mahvelous. You ah not, by chance, intewested in ahchaeology, ah you, [Mr] Hahdin?’

“… Hardin [replied] ‘No milord, can’t say I am …’

“‘I myself [Dorwin said] –dabble in ahchaeology…’

“‘Indeed?’

“‘His lordship,’ interrupted [someone], ‘… is most thoroughly acquainted with the field.’

“‘Well p’haps I am,’ said his lordship complacently. ‘I have done an awful amount of wuwk in the science [of archaeology]. Extremely well-wead, in fact. I’ve gone

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79 Ibid.
thwough all of [the literature:] Jawdun, Obljasi, Kwonwill … oh, all of them, y’know.’

‘I’ve heard of them, of course,’ said Hardin, ‘but I’ve never read them.’

‘You should some day, my deah fellow. It would amply wepay you…

‘[The great writer/researcher] Lameth, you must know,’ continued the chancellor [Dorwin], pontifically, ‘presents a new and most intewesting addition to my pwevious knowledge of the “Owigin Question.”’

‘Which question?’ asked Hardin.

‘The Owigin Question.’ The place of owigin of the human species, y’know. Suahly you must know that it is thought that owiginally the human wace occupied only one planetawy system.’

‘Well, yes, I know that.’

‘Of cohse, no one knows exactly which system it is–lost in the mists of antiquity. Theah ah theawies, howevah. Siwius, some say. Othahs insist on Alpha Centauwi, oah on Sol, oah on 61 Cygni–all in the Siwius sectuh [of the galaxy], you see.’

‘And what does Lameth say?’

‘Well he goes off along a new twail completely. He twies to show that ahchaeological wemains on the thuhd planet of the Ahctuwian System show that humanity existed theah befoah theah weah any indications of space-twavel.’

‘And that means it was humanity’s birth planet?’

‘P’haps. I must wead it closely and weigh the evidence befoah I can say foah suhtain. One must see just how weliable his obsuhvations ah.’

‘Hardin remained silent for a short while. Then he said, ‘When did Lameth write his book?’

‘Oh–I should say about eight hundwed yeahs ago. Of cohse, he has based it lahgely on the pwevious wuhk …’

‘Then why rely on him? Why not go to Arcturus and study the remains for yourself?’

‘Lord Dorwin raised his eyebrows … ‘Why, whatevah foah, my deah fellow?’

‘To get the [scientific and technological] information first hand, of course.’

‘But wheah’s the necessity? It seems an uncommonly woundabout and hopelessly rigmarolish method of getting anywheres. Look heah, now, I’ve got the wuhks of all the old mastahs–the gweat ahchaeologists of the past. I weigh them against each othah–balance the disagweements–analyze the conflicting statements–decide which is pwobably cowwect–and come to a conclusion. THAT IS THE SCIENTIFIC METHOD. At least–patronizingly–‘as I see it. How insuffewably cwude…’[to actually check the authorities’ work].’

In reality, all that Kitchen and all historians have done with the ancient sources is go through them with an uncommonly roundabout and hopelessly rigmarolish method of getting anywheres. They weigh the old documents against each other–

80 Isaac Asimov, *Foundation* vol. I (NY/London et al. 2004), pp. 74-76 (capitalization added)
balance disagreements to their satisfaction—analyze the conflicting statements and
decide which is probably correct—and come to a conclusion. But that is “NOT” the
scientific method! It is a rather crude method for discovering truth.
All that Kitchen, and those who think as he does, have done is deny science and the
facts that we have presented in these three volumes. Such an attitude toward science
is infantile, not to say totalitarian. Kurt Vonnegut, who studied anthropology at the
University of Chicago, in his novel *Mother Night*, states of such people:
“I have never seen a more sublime demonstration of the totalitarian mind which might
be likened unto a system of gears whose teeth have been filed off at random. Such a
snaggle-toothed machine, driven by a standard or even substandard libido, whirls with
the jerky, noisy, gaudy pointlessness of a cuckoo clock in Hell … [that measures
ancient historical chronology].
 “[Kitchen and those who think and act as he] wasn’t completely crazy. The dismaying
thing about the classical totalitarian mind [of such historians] is that any given gear,
though mutilated, will have at its circumference unbroken sequences of teeth that are
immaculately maintained, that are exquisitely machined …
 “Hence the cuckoo clock in Hell [will] keep … perfect time for eight minutes and thirty-
three seconds jumping ahead [and then after centuries] keeping perfect time…
 “The missing teeth, of course, are simple, obvious [scientific and technologi-cal]
truths, truths available and comprehensible even to ten-year-olds, in most cases.
“The willful filing of gear teeth [is] the willful doing without certain obvious pieces of
information.”81
Yet Kitchen and most historians will argue “I have never willfully destroyed a tooth
on a gear of [their historical machine thought] … Never have I said to myself, “This
[scientific] fact I can do without.”82 In reality, however, that is all Kitchen has done
and this is being fully admitted by certain historians, as we will shortly see.
With all his efforts, Kitchen has discovered no new knowledge; he and those who
follow in his footsteps have only rediscovered essentially the status quo—the
established chronology as it was organized long ago. And this is now fully
admitted: Paul Courbin, in speaking about the hopes of the new archaeology
inaugurated in the 1970s to employ science to resolve the old unresolved historical
problems that had long defied solution, explains and outlines the expectations of
this new scientific approach, showing where it ultimately led. He exemplifies the
problems outlined in these volumes. The New Archaeology, he says
“is the form of this improved knowledge (or understanding [of history]) of the past
which is new and original: archaeology [and history] must become a science. The great
word [SCIENCE] has been uttered, and it is a true leitmotiv. As early as 1968 [Lewis]

81 Kurt Vonnegut, *Mother Night* (NY 2006), pp. 223-224
82 Ibid., p. 225
Binford was stressing that henceforth they [the archaeologists and thus also the historians] were going to explain variability [of conflicting forms of evidence] ‘scientifically’; the subtitle of a well-known book *Explanation in Archaeology ... is An Explicitly Scientific Approach*; at a conference in 1972, Binford was acting the prophet yet again: ‘Insofar as we agree that our goals in ... archaeology are understanding of events and the people ... such understanding will not be forthcoming until a science of archaeology [and thus also a science of history] is developed.’ Flannery’s famous article is entitled ‘Archaeology with a Capital S [for Science]’; in his preface to South’s book, Binford ended by bidding historical archaeology welcome to the ‘science of archaeology’: ‘Explicitly scientific archaeology [and thus scientific history] is a science examining data from the past.’ One could find innumerable such quotations, because this determination not to let archaeology [and thus also history] lag behind when the sciences are progressing in such a spectacular fashion, this will raise it to the status of a real ‘science.’ And this does constitute the least debatable of its innovations, for even though previous archaeology had for a long time been calling on the assistance of the sciences and imagining itself to have ‘scientific’ procedures, it apparently had never had the pretension of placing itself among them. It confined itself instead to the subordinate role of an auxiliary to history—which is not even a science. ... “But first, what is a science? Or rather, what is the New Archaeologists’ idea of a science? A science, apparently, can be defined as a ‘body of theories,’ organizing an ensemble of ‘laws,’ imagined or ‘created’ by scholars whose logical consequences have been ‘tested’—or rather, have not been disproved; these allow one ‘to explain,’ and thus understand, the observed data. Traditional archaeology never accomplished these ends, as we all know, even though it occasionally deluded itself that it had.”

However, the New Archaeology had ensconced itself thoroughly in the chronology which preceded it for the ancient Near East and this failure to question this underlying chronological foundation has created a “failure” for the hopes, expectations, and procedures of this application of science. As Courbin shows: “The strange thing is that we did not see very much—at any rate, not what we were expecting. After the initial taking up of positions, that song of glory, that hymn to the future, and of course the associated curses aimed at the (few) contradictors [in archaeological and historical research], we expected that after a reasonable interval (though certainly soon, in view of the invocation of these powerful new concepts and modern [scientific] methods) we would see results which would be spectacular, to say the least—not in the traditional sense of the New Archaeology itself. We could legitimately hope that old problems would at last find the solution which had been deferred for so long; that questions which had never been asked before would now be

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asked and solved in their turn; in short, that our understanding of the human past would have new and lasting light thrown upon it. All it needed was a little patience. “Almost twenty [now almost forty] years have now gone by; and if at the beginning it was perfectly justifiable to give credit, today—with the perspective of time—the most one can say is that far from fulfilling the promise of the blossoms, the fruits have remained very dry. The disappointment is bitter. Certainly, the New Archaeologists have published (and republished) a great deal, especially theoretical works (theory has evolved considerably) but also case studies, which are of great value, though nothing out of the ordinary, so to speak. New hypotheses have been put forward about important problems new and old, but they are in no way different from those put forward before, and they don’t seem much more solidly proved than their predecessors. The revolution of the New Archaeology has not been followed by a revolution in knowledge.”

Any serious person who has diligently read these three volumes of *Pillars of the Past* must by now be well aware of the great number of unresolved problems, in every period, in every region of the ancient Near East, in every discipline that we have examined. These problems are, on the whole, not merely enormous in number but enormous in scope. To rehash them now, yet again, would require dozens upon dozens of pages. To wit, the established chronology, in the light of these problems, contradictions, etc., presents a history that is completely dysfunctional, on level, after level, after level. This level of dysfunctionality has not been, and will never be, rehabilitated if that established chronological straight-jacket, to which nearly all historians have given their assent and allegiance, is allowed to inform their research. A revolution in knowledge can only be accomplished by burying that chronology since it is already a corpse. The attempt to breathe life into this lifeless thing is simply to no avail.

The very same applies to the 200-year shortening of chronologies of James *et al.* and the 300-year shortening of chronology of David Rohl. Their revisions have the same unresolved problems on the same scale, and thus are similarly dysfunctional. This dysfunctionality, however, does not apply to the short chronology of Heinsohn, Rose, Sweeney, and to Velikovsky when his work is integrated into their revisions. In the words of Polybius:

“He indeed who believes that by studying isolated histories he can acquire a fairly just view of history as a whole, is, it seems to me, much like one who, after having looked at the dissevered limbs of an animal once alive and beautiful, fancies he has been as good as an eyewitness of the creature itself in all its action and grace. For could any one put the creature together on the spot restoring its form and the comeliness of life and

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84 Ibid., p. XXIII
then show it to the same man, I think he would quickly avow that he was formerly very far from truth and more like one in a dream.”  

Putting together the chronology and history of the ancient world is much like what Polybius just described: this is the problem for historians. It is also an apt citation as it relates to forensic history. An anthropological forensic researcher understands the nature of anatomy, as a scientific discipline, and thus can reassemble that animal based on valid criteria of science. A historian or archaeologist, however, has no such scientific tools and thus his/her reconstruction is based on an invalid discipline, no matter how well intentioned that historian is. All the facets of these three volumes of *Pillars of the Past* are built on the first approach to evidence and therefore stand as scientifically valid criteria for reconstructing the history of the ancient Near East.

With the wrong tools and the wrong methodology, the chronology is a kind of Frankenstein’s monster into which no amount of historical labor will breathe life. That historical monster is fictional rather than the stuff of reality. Sir Colin Renfrew nicely sums up the problem for historians and archaeologists of this old school of thought:

“The first step, however, is to recognize the depths of our [scientific and technological] ignorance. To realize how the existing ‘chronologies’ in different parts of the Mediterranean are bolstered up by circular arguments, where specialists in one area believe that those in other areas must know what they are talking about, and blindly use dating systems which are no better than their own [which are deeply flawed] … a chronological revolution is on its way.”  

To achieve this chronological revolution a new methodology—science—must become the preeminent tool of history and chronology. Doing things the same old way will not work, but that is just what historians have been doing over and over again with little or nothing in the way of having found new knowledge. Albert Einstein, in an adage attributed to him, calls this kind of behavior insane: “Insanity consists of doing the same thing over and over again hoping for a different result.”  

The Black-American comedienne Jackie “Moms” Mabley put it in more down-to-earth terms: “If you always do what you always did you will always get what you always got.”  

Jared Diamond put the case advocated in these volumes tersely: “The challenge now is to develop human history as a science, on a par with acknowledged

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85 Polybius, Book I, 4  
86 Sir Colin Renfrew, “Foreword,” in James et al., *op.cit.*, p. XV  
historical sciences such as astronomy, geology, and … biology.” 89 This is the only sure approach to history that will develop new knowledge. Historians, imbued with their conditioned understanding of discovering historical reality, will be disturbed by the findings in these volumes of Pillars of the Past, as Claude Allègre explains: “The more novel an idea is, the more its power to shock, and the more it upsets those whose reputations have been established elsewhere, and those whose intellectual comfort has been troubled by its emergence. Originality is a prized virtue, provided that it is not too disturbing. Beyond a certain threshold, any bold innovation will be met with marginalisation, or even sacrificial reaction.” 90

The responses by historians to the various forms of evidence presented by the modern revisionists, Heinsohn, Rose, Sweeney, and Velikovsky, described in part in these volumes as their problem, are well characterized by Samuel Ichiyé Hayakawa and Alan R. Hayakawa as what occurs when one smashes into reality. They tell of a report in the New York Post about a motorist, Samuel Rios, 30, who “Driving at 2:30 A.M. through Williamsburg [Brooklyn, New York] … swung around a corner and accidentally sideswiped a sedan parked at the curb … Furious, police charged, Rios stopped, took the jack handle from his car trunk, and slammed the offending obstacle from windshield to tail lights.” 91 Like that driver, driving in the dark, historians are building a chronology in the dark because it is not supported by the light of science and technology. Therefore, when they run into scientific historical reality, they attack it.

Those of us involved in this revisionist work are not responsible for the chronological fix historians have created for themselves. They have merely run headlong into the wall of scientific and technological reality, and continuing to attack it, despite their errors, will change nothing. To paraphrase Graeme D. Snooks:

To construct a realistic historical/chronological model of the ancient Near East it is clearly necessary to go back to the drawing board and start all over again on the foundations laid down in these volumes. We have to recognize that the established chronology is untenable, upside down, backward, twisted, and arbitrary. By employing deductive evidence rather than inductive evidence the historians have built nothing …

It is time to cut through this incoherence by developing a realistic scientific/technological alternative to the deductive/interpretative model that presently prevails. 92

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89 Jared Diamond, Guns, Germs, Steel (NY 1997), p. 408
90 Claude Allègre, quoted in Marcel Leroux, Global Warming: Myth or Reality: The Erring Way of Climatology (Berlin/Heidelberg Germany 2005), p. 145
Although I believe all these efforts and words, all this evidence, will fall on historians’ deaf ears, blind eyes and closed minds, some may actually read all this material and perhaps may have open minds. This long and difficult undertaking is therefore addressed to the few hearty souls not encumbered by doctrine. The great American novelist John Steinbeck in his novel *East of Eden*, a tale about evil in the world, tells us to whom to turn:

“That’s why I’m talking to you. You are one of the rare people who can separate your observations from your preconceptions. You see what is, where most people see what they expect.”

To those who have read all this material but still refuse to accept science and technology as the profound underpinning—the foundations—the *Pillars of the Past* that uphold the short chronology of Heinsohn, Rose, Sweeney, Velikovsky and this author, Arna Bontemps’ remarks are cited:

“In any debate … the omission of evidence is unforgivable. This remains partly true when the evidence is not immediately at hand and must be sought, but the sin is compounded after it is found and treated with disdain.”

All in all, Oscar Handlin said it best:

“Truth is absolute, it is as absolute as the world is real. … Truth is knowable and will out if earnestly pursued; and science is the procedure or set of procedures for approximating it.”

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95 Oscar Handlin, *Truth in History* (Cambridge MA/London 1979), p. 405
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