PILLARS OF THE PAST

By Charles Ginenthal

HISTORY, SCIENCE, TECHNOLOGY
AS THESE RELATE TO
CHRONOLOGY
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WHAT IS HISTORICAL EVIDENCE?

Immanuel Velikovsky, in his Ages in Chaos series on the chronological reconstruction of the ancient Near East, provided evidence that the history of Egypt and Mesopotamia was at least 500 to 600 years shorter than accepted by historians. In a sense this shortening of the chronology of history brought him exile from the historical establishment. His revolutionary concept can be compared to the great achievement of Voltaire which also brought exile. This occurred, according to Will Durant, “because by telling the truth it offended everybody.”¹ In Velikovsky’s case, “everybody” was the historical establishment—except for a few prominent historians such as Pfeiffer, Garstang, and Schaeffler, who felt Velikovsky had made a respectable case for his chronological revision. Others have followed in his footsteps and have concluded that the history of that time must be shortened even more drastically, suggesting a reduction of about 1500 years. That would mean the onset of civilization began about 1500 to 1200 B.C., and not around 3000 B.C.

But how was one to determine whether or not ancient history could be so greatly shortened? In my first attempt to defend this new chronology I suggested, as do and did most other revisionists, that to a great degree the history itself—in its details—would act as a test of each area of historical investigation. But those who opposed the greatly shortened chronologies could, with justice, cite the historical record to prove that the chronology of Egypt and Mesopotamia could not be so greatly reduced. In realizing that the historical record was a highly abbreviated set of rags and tatters, with a vast number of documents missing, I sought a quite different angle of investigation, not subject to question, as the modus operandi with which to test the lowered chronology. The instruments that offered themselves for the analysis of the chronology of the ancient historical past were science and technology. That is, it was possible to call upon several scientific and technological phenomena that could be employed to test the chronology of the Near East. If it

¹ Will Durant, The Story of Philosophy (NY 1961), p. 169
could be demonstrated that each of these phenomena required a reduction of the established chronology, then the established historical record was required to agree with the science and technology and not the other way round. A historical record could not be correct if it was repeatedly in conflict with, and contradicted by, the facts of science and technology.

Velikovsky also touched on this methodology but leaned quite heavily on the historical documents as support for his own revision. The historian who actually introduced me to history in terms of technology and therefore science was John Dayton, whose 1978 tome, *Minerals Metals Glazing & Man* was a powerful stimulus that led me into several areas of science and technology that he had not surveyed for relevant evidence as fully as I believe they ought to have been. In this respect I had a tool that could not be assailed by historical documents. If the records were contradicted by the science and technology then they did not fit the conventional chronology and had to be made to fit a new, revised one. This did not in any way denigrate the historical record; it merely meant that it was not the final arbiter of chronology but was secondary.

In order to understand how this methodology works in terms of this kind of analysis, I have chosen a historical dispute which was finally resolved by scientific data and not by the solidly established documents and analysis of them. This approach has generally been anathema to historians as Marc Bloc, writing in *The Historian’s Craft*, explains:

“It is sometimes said ‘History is the science of the past.’ To me this is badly put.

“For to begin with, the very idea that the past as such can be the object of science is ridiculous. How, without preliminary distillation, can one make of phenomena, having no other common character than that of not being with us, the matter of rational knowledge?”

R.G. Collingwood, in *The Idea of History*, further explains what scientific history is:

“‘History,’ said Bury, ‘is a science; no less, and no more.’

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“Perhaps it is no less: that depends on what you mean by a science. There is a slang usage, like that for which ‘hall’ means a music-hall or ‘pictures’ moving pictures, according to which ‘science’ means natural science. Whether history is a science in that sense of the word, however, need not be asked; for in the tradition of European speech going back to the time when Latin speakers translated the Greek επωτημη by their own word scientia, and continuing unbroken down to the present day, the word ‘science’ means any organized body of knowledge. If that is what the word means Bury is so far incontestably right that history is a science, nothing less.”

This book interprets the word “science” to mean “natural science,” and suggests that no matter how well organized the historical body of knowledge is, it fails to be chronologically accurate where it is opposed to natural science and its more practical associated field, technology. For an overall picture of the historian’s craft, R.G. Collingwood’s The Idea of History, revised edition, is quite illuminating.

The difference between the historian’s approach and the one taken in this book is simply that natural science and technological evidence is the true arbiter of the issues. In this respect it is suggested that the approach of the historian is not scientific because the documents of the past are not allowed to be refuted. Douglas Futuyma outlines this dichotomy:

“[T]here is another important criterion of a scientific theory which most scientists accept. This is Karl Popper’s dictum that a theory be in principle ‘falsifiable.’ That is, a good theory [or historical chronology] doesn’t explain everything; it specifically predicts that certain observations, if made, would prove the theory wrong. . . . The nonscientific theorist lives within an impregnable fortress, safe from criticism because the hallmark of nonscientific theories is that they cannot be falsified. They are formulated vaguely, or invoke agents whose actions cannot be predicted, so that they ‘explain’ every possible outcome of a situation. Whatever your personality or history may be, a good astrologer will find some conjunction of the planets that explains why you are this way, even though as a Sagittarian you’re ‘expected’ to be the opposite. . . . there is more to a good scientific hypothesis than corroboration; it must be falsifiable.”

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Another aspect of scientific as opposed to historical data is that while scientific theory can often be tested, the historical documents cannot be tested by such clear-cut criteria. Let us, therefore, examine a dispute in both its historical and scientific contexts to illustrate the methodology largely, but not exclusively, employed in this book.

**THOMAS JEFFERSON AND SALLY HEMINGS**

According to Virginius Dabney, a highly respected historian and distant relation of Thomas Jefferson:

“Largely forgotten charges that Thomas Jefferson had a handsome light-skinned slave [Sally Hemings] as his mistress for several decades have been resurrected in a recent Jefferson biography. This book was followed by a popular novel elaborating upon the same theme. The appearance of these works has brought to public attention allegations that were first given currency a year after Jefferson became president of the United States in 1801, . . .

“The late Fawn Brodie, author of the biography *Thomas Jefferson: An Intimate History*, and Barbara Chase-Riboud, who wrote the novel *Sally Hemings*, say categorically that Jefferson had Sally for his concubine during thirty-eight years, a relationship that resulted in five children. The claim that Jefferson fathered these children is based to a considerable degree on so-called psychological evidence and the result purports to be ‘psychohistory.’ . . .

“Brodie and Chase-Riboud have described the charges as completely authentic. Their volumes were chosen by major book clubs and reprinted in paperback editions, with large sales and widespread publicity in the printed and electronic press. Furthermore, the allegations they have made have been accepted as valid by an astonishing percentage of the mass media, as well as by countless individuals. At least two national television networks have seriously considered developing miniseries based on the Chase-Riboud novel, and efforts are underway as of this writing to interest a motion picture producer. *Parade*, the huge-circulation Sunday newspaper supplement,
declared flatly that Jefferson had Sally as his mistress and begat a brood of children. *The New York Times* and the *Chicago Tribune* have tended to support the charge, a substantial number of reviewers have accepted the story as true, and publications as diverse as *Newsday*, the *Washingtonian*, and the *Unitarian World* have published categorical statements that the liaison existed. . . .

“This massive acceptance by major elements of the media—and apparently of the public—occurred despite the fact that the three internationally recognized authorities on Jefferson’s life and career have found the books to be wholly unsound. Historians reviewing the Brodie biography for such scholarly publications as the *Journal of American History* and the *Journal of Southern History* have also expressed skepticism, if not outright disbelief . . .”

Dabney, a former editor of the *Richmond Times—Dispatch* and recipient of the Pulitzer Prize, who has written several scholarly works, had decided to prove via an analysis of historical documents that Jefferson did not father children with his housekeeper and slave, Sally Hemings. In his statement he offers that despite what is presented as fact in the media, three internationally recognized authorities on Jefferson found the book to be “wholly unsound” as have such scholarly publications as the *Journal of American History* and the *Journal of Southern History*. Doesn’t this all sound like the arguments raised by some critics of Velikovsky, that he won support from the public and some of the media, scientists and academics but not from the really responsible members of these establishments?

For a scholarly analysis of the historical debate, I recommend most highly Annette Gordon-Reed’s *Thomas Jefferson and Sally Hemings—An American Controversy*. In it, the depth of the various historians’ biases and double standards is rather thoroughly outlined. But the question of scientific proof is fundamental to this debate. What is it and what does it indicate? An article in the “Science and Ideas” section of *U.S. News and World Report* succinctly reports for Nov. 9, 1998:

“This week’s issue of *Nature* presents the results of scientific tests that show a conclusive DNA match between a male descendant of Sally Hemings and another man who can trace his lineage to Thomas Jefferson’s paternal uncle. Advances in mapping of the so-called Y chromosomes, which confer maleness on embryos, allow scientists now

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to consider DNA matches of the type reported by Nature as virtual proof of genetic linkage.

“The evidence here, in other words, removes any shadow of doubt that Thomas Jefferson sired at least one son by Sally Hemings.”

More than that, DNA tests were carried out on descendants of men whom the historians had accused of being the real fathers of Sally Hemings’ children and conclusively proved that they did not father Hemings’ children. As further reported,

“Until the 1960’s, hardly a white scholar in America was on record as believing that Sally Hemings bore Jefferson’s children. The pattern of denials was so strong that Annette Gordon-Reed, who recently wrote a much acclaimed book . . . [noted above] meticulously dissecting the historians’ conclusions and the way in which they were reached and articulated. . . . The DNA results, she says, barely concern her. “It’s the historiography that’s offensive.”

As one can see it took about 200 years for historians to face up to the fact—scientific fact—that all their analytical investigations of the documents that they used to prove that Jefferson could not have fathered Sally Hemings’ children were false. Science had settled a two century debate not in favor of establishment historians but against virtually all of them and their documented proofs, all their analyses of the situation and definitive pronouncements, such as this statement by the historian Douglas Wilson:

“If he [Jefferson] did take advantage of Hemings and father her children over a period of twenty years, he was acting completely out of character and violating his own standards of honor and decency. For a man who took questions of morality and honor seriously, such a hypocritical liaison would have been a constant source of shame and guilt. . . .”

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8 quoted in Annette Gordon-Reed, Thomas Jefferson and Sally Hemings—An American Controversy (Charlottesville VA 1997), p. 116
In her “Conclusion”, Gordon-Reed castigates the historians and their hypocritical, biased analyses:

“It is my belief that those who are considered Jefferson scholars have never made a serious and objective attempt to get at the truth of the matter. . . .

“The failure to look more closely . . ., the too ready acceptance and active promotion of the Carr brothers [as the possible fathers of Sally Hemings’ children] . . ., the reliance upon stereotypes in the place of investigation and analysis, all indicate that most Jefferson scholars decided from the outset that the story was not true and that if they had anything to do with it, no one would come to think otherwise. . . . The goal has been . . . to restrict [and falsify] knowledge as a way of controlling the allowable discourse on this subject.

“This attitude betrays a basic disrespect for readers of history. One of the most disheartening things about going through this material was to see the extent to which some of the authors seem to have relied on their expectations that most readers would never check, or even be able to check, assertions they made. Historians’ prejudices and individual desires to keep inviolate their particular image of Jefferson prevented a fair, hard-headed, and thorough presentation and consideration of the facts.”

Gordon-Reed then goes on to point out that in their criticisms of “the opposition” they exhibited the same “disrespect for readers [of history].”

The use of crucial scientific tests essentially removes the possibility of coming to historical conclusions based on a biased selection and interpretation of the facts. It allows for the historian’s role as evaluator of documents but also as that of a neutral tester. The aim of this approach is not to overlook documented evidence but rather to test the documented record by a stringent scientific process. And above all, let the science determine the validity of that documented record. Thus, the historians who turned to the historical record to prove that Thomas Jefferson never slept with his teenaged slave-housekeeper in Paris and fathered her children used that record in defending their position. Those who suggested otherwise had also

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9 ibid., pp. 224-225
turned to that same record for their support. However, a neutral scientific test that was carried out and, importantly, could be replicated by other laboratories, set the historical record straight.

The solution to fundamental challenges to the chronology of the Near East must also be settled, as far as possible, by all the evidence, but with the proviso that when the science contradicts the documents, etc., which historians rely on so heavily, the scientific facts dominate. The aim is not to exclude evidence but to enlarge the evaluative technique and give chronology and the history that flows from it a solid foundation. It is only through scientific testing that the proper foundations of the chronology of the ancient Near East can be erected.

However, historians are loath to accept scientific findings that oppose their historical chronology. For example, Egyptologist Kenneth Kitchen commented on the findings of X-ray examinations of “Tuthmosis III, Amenophis III and Rameses II [who] reigned 52, 38 and 66 years respectively given the available documentation.” [Kitchen claims] “such rulers cannot have died aged 35/40 [for Tuthmosis III], 30/35 [for Amenophis III] and 50/55+ [for Ramses II] respectively . . .”\(^{10}\) He goes on to argue that these “impossible cases . . . have X-ray ages that are currently useless historically . . . Thus on the central point of kings’ ages at death X-ray results fail to persuade.”\(^ {11}\)

Here it is made clear that the scientific evidence as it relates directly to the ages of certain pharaohs fully contradicts the chronology the historians have established “given the available documentation.” However, unlike the historians who relented in the face of DNA evidence related to Thomas Jefferson’s and Sally Hemings’ progeny, Kitchen and his colleagues are unwilling to relent in the face of forensic X-ray evidence related to the chronology of three Egyptian kings. But what if there were several other forms of scientific and technological evidence that pointed to the same result as that of the X-rays? Could the historical establishment reject all these as well and claim they are “useless historically” and “fail to persuade”?

This, however, is the nature of the evidence to be presented in this book with regard to the length of ancient Near Eastern chronology. What this author will demonstrate is that several lines of science and technology, such as weathering and erosion, astronomy, radiocarbon dating, pottery dating, geology of tin ore, technology for producing steel or to harden iron, sediment geology, agronomy,


\(^{11}\) *ibid.*, p. 237
linguistics, as well as historical evidence, make a fundamental contribution to the length of ancient Near Eastern chronology. But most significantly they all contradict the long chronology and support, corroborate and converge in upholding the short chronology.

In this sense history is moved closer to science. It becomes a field of research that establishes methodologies that can test, falsify, and establish replicable experiments. This allows for the philosophical concepts presented by Karl Popper to permit historical questions to be judged by basic scientific tests. It takes history from the exclusive domain of historians and opens the door to the scientist and technologist, not as secondary adjuncts or merely laboratory aids, but as fully-fledged historical researchers. It is their scientific approach, subject to testing and replication, that must take its rightful place in the vast arena of historical and chronological controversy. Nothing less will do.

This author wishes to express special thanks and appreciation for the good advice given to him by Professor Lynn E. Rose on questions dealt with in this book. His sage advice was welcome and helpful. However, the contents of this work and responsibility for what is presented rest with this author. The same applies to Birgit Liesching who generously helped in the typing and formatting of this manuscript.
CHAPTER 1

THE FOUNDATIONS OF ANCIENT HISTORY

“Even more than science, history has to be remade periodically. Remade, not merely revised, because the past is not dead. The past is gone; in this sense it lies beyond our power and need to concern us. But the past is apt to deceive us. The historical record is sparse, and it thins out the farther back one goes. Many classical writings have disappeared. Our knowledge of antiquity is grossly imperfect and intertwined with legends and fables.”

James R. Newman

*Science and Sensibility*


“The one duty we owe to history is to rewrite it.”

Oscar Wilde

*The Critic as Artist*  
(1891), p. 101

“The very ink with which all history is written is merely fluid prejudice.”

Mark Twain

*Following the Equator*  
(1897), vol. 2, chapter 3

The foundations of ancient Near Eastern chronology are assumed by nearly all researchers—historians—in the field to be properly and correctly delineated, and therefore they believe that their history is properly erected on this basis. Though it is admitted that there may be discrepancies within that framework, it is nevertheless accepted that these are relatively trivial and in no way contradict the blueprint organized over the past few centuries. To construct this chronology they have incorporated several methodological approaches employing several forms
of historical evidence and sometimes even scientific evidence to buttress the great monument handed down to us as ancient history.

This conclusion has recently been challenged by Professor Gunnar Heinsohn of the University of Bremen, Germany, Professor Lynn E. Rose, emeritus professor of the State University of New York at Buffalo, and Emmet J. Sweeney of Derry, Northern Ireland.¹

Each of these historical revisionists has acknowledged his enormous debt to the trail-blazing work of Immanuel Velikovsky, whose published books: *Ages in Chaos, Peoples of the Sea, Ramses II and his Time*, as well as his unpublished historical works: *The Dark Age of Greece* and *The Assyrian Conquest* challenged the established conventional chronology. Velikovsky’s revision not only overturned much of ancient history, but shortened the length of that chronology by at least 500 to 600 years. In this respect, his followers have gone beyond Velikovsky and have revised the history and shortened the length of the chronology by 1000 years or more. Although there is, as yet, no complete agreement among these revisionists, they have overall maintained that the length of ancient Egyptian and Mesopotamian history must be significantly reduced.

To determine whether or not this shattering alteration of ancient chronology has a basis in fact, one must examine the proper foundations upon which conventional ancient history rests. To make such an evaluation, this author maintains that the most decisive and valid data upon which this determination *must be made* is that of empirical science. While history is generally built on the accumulated records of humanity, these can be tested by scientific methods and may be found in error for good reason. As for the reliability of the documented record, Egyptologist Barbara Mertz discussed a well-known battle fought by Ramses II against a people known as the Hittites wherein the documented record in Egypt claims that Ramses II inflicted—seemingly single-handedly—such an enormous loss of life on his enemies that they, in defeat, “sued him for peace which he magnanimously granted.”

“At this point [Mertz writes] we are faced with a major problem in historiography. In other words, how much of this nonsense can we reasonably swallow? We have seen how varied and remarkable are the sources from which a student of history may derive the information he uses to make up a consistent story of what happened in the past. When written records are few the historian uses other materials, which require complicated analyses. But even when an event is well documented, even when we have a written pseudo-historical account—we must still evaluate the reliability of the source. Many questions must be asked. Is the tale written by an eye-witness or does the author rely on second-hand information? If the former, was he a good observer? If the latter, has he examined his witnesses and tried to test their eyesight and credibility? What is the bias of the author—is he for or against the people he is writing about? Even if he professes to be moved solely by a desire to record the ‘truth,’ is he sufficiently detached from the scene and the players of the drama to write about them dispassionately? Does he have a conscious or unconscious purpose—vilification or glorification of a man or a belief, self-aggrandizement, propaganda? In some cases we must pry into the entire life history of a chronicler or writer of history in order to discover his prejudices and the bearing they may have on his interpretation of the events of his time.

“Our task of evaluating the written records of ancient Egypt is relatively easy since we can start with the assumption that every scribe has an ax or two to grind. The annals of the various kings are not a dispassionate record of events; they are intended to glorify the kings, on earth and in the Hereafter. Hence we can and must take every statement made in such annals with a good-sized chunk of salt.”

“Fortunately [Mertz adds] we do not have to rely on logic to prove that the Egyptians lost that fight. By one of those almost miraculous coincidences which do occur, we have at our disposal the Hittite version of the same battle, from the royal archives of the capital of Boghazköi. According to it, Ramses was defeated and had to retreat . . .”

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2 Barbara Mertz, *Temples, Tombs and Hieroglyphics*, (revised and updated) (NY 1978), pp. 269-270
3 *ibid.*, p. 271
Here we encounter two historical records, the Egyptian giving victory to Ramses II, the Hittite claiming he was defeated. Which are we to trust, especially since Mertz claimed these annals are deeply biased? On the other hand, what if no counter-documentary evidence was found to throw into question Ramses II’s victory, how would one ever have been able to establish with certainty the truth about this historical episode? As Newman pointed out above, “the past is apt to deceive us. The historical record is sparse, and it thins out the farther back one goes.” With respect to Egyptian history Alan Gardiner was emphatic about this problem of sparse documentation:

“It must never be forgotten that we are dealing with a civilisation thousands of years old and of which only tiny remnants have survived. What is proudly advertised as Egyptian history is merely a collection of rags and tatters.”

In this respect, C.W. Ceram is even more emphatic:

“Anyone approaching the study of ancient history for the first time must be impressed by the positive way modern historians date events which took place thousands of years ago. . . . For as we examine the sources of ancient history we see how scanty, inaccurate, or downright false, the records were even at the time they were first written. And poor as they originally were, they are poorer still as they have come down to us: half destroyed by the tooth of time or by the carelessness and rough usage of men.

“As a matter of fact, the more we pursue our studies the less are we impressed by the dates which initially filled us with respect. We begin to recognize the framework of chronological history for what it is—a purely hypothetical structure and one which threatens to come apart at every joint. Crooked and tottering, it gives us a picture of a strangely arbitrary history, while at the same time our instinct tells us that ancient civilizations must have had some sort of reasonable and organic growth. When we reach this point in our studies we begin to be doubtful of every single date!”

In spite of these powerful reservations, Mertz, Gardiner, and Ceram, and the rest of the historians of the establishment, consider that they do indeed possess a rather accurate record of the history of the ancient world built on a solid chronological foundation. They cannot conceive that they may be involved in a mistakenly biased interpretation of that chronological history, nor that they hold to erroneous preconceptions which exclude the possibility of examining that framework from a totally different blueprint. G.R. Elton in his book *The Practice of History* warned the historians that

“Preconceived notions are a much greater danger to historical truth than either deficiency of evidence or error in detail. . . . Sociologists establish ‘models’ which they test by supposedly empirical evidence. To the historian this seems a very dangerous procedure: far too often the model seems to dictate the selection of facts used to confirm it.”

On this point, L. Laudan, cited in W.R. Bird, describes the reasons for the level of resistance by researchers to evidence or reinterpretations of evidence that falsifies their paradigms:

“Historical and sociological researches strongly suggest that the scientists [and, I add, historians] of any epoch . . . regard some of their beliefs as so fundamental as not to be open to repudiation or negotiation. . . . Numerous historians and philosophers . . . have documented the existence of a certain degree of dogmatism about core commitments in scientific [and, I add, historical] research and have argued that such dogmatism plays a role in promoting the aims of science [or history] . . . one does not even begin to get at those differences by pretending that science [and history are] . . . characterized by an uncompromising open-mindedness.”

To paraphrase R. Brady: when a chronological theory becomes part and parcel of the basic working knowledge of an entire community of historians, as in all other fields, it becomes the only context within which these historians understand the world. Doubt of the correctness of the chronology comes to be regarded as something less than legitimate, and critics of the chronology find themselves talking

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6 G.R. Elton, *The Practice of History* (NY 1967), pp. 36-38n
only to each other. The establishment historians rarely listen to these critics because they know that they are right and the critic must be wrong. Thus, the critic is, in a certain sense, self-exiled, for he or she is trying to question what the common language of the field takes for granted. The establishment cannot deal with a condition that appears to be wholly intolerable, namely that their theory and the conceptual system upon which it is built may be in error. It is therefore nearly impossible to get around or overcome this opposition. Historians, like researchers in other fields, through their education and acculturation over time become so imbued with the paradigm that has always guided their studies that it becomes unthinkable to question the raison d’être of their Weltanschauung.

The unfolding chapters of this book will make this view manifest. It will expose numerous areas of historical fact as false based on scientific evidence. Scientific evidence will be exhibited again and again that undermines and directly contradicts the documented history and the established chronology. But most importantly it will be shown that the scientific empirical evidence speaks with one voice; the evidence not only contradicts the historians’ chronology but rather all the various facets of it corroborate one another and converge on an entirely new paradigm—a new chronology.

This book will mainly deal with scientific evidence as it touches upon and reflects that history. It will be taken that science is the correct basis for making any determination of what the historical chronology is. Historical interpretations based on sparse, slanted, crooked, and arbitrary documentation cannot be valid if these are in direct conflict with empirical evidence, i.e. facts.

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8 *ibid.*, p. 47
MANETHON

“History will bear me out, particularly as I shall write that history myself.”


The individual to whom historians turn to bear out their chronology did actually write that history himself. He was named Manethon, a chauvinist priest-scholar. His aim, as is well known, is described by Velikovsky:

“In composing his history of Egypt and putting together a register of its dynasties, Manetho was guided by the desire to prove to the Greeks, the masters of his land [conquered by Alexander the Great] that the Egyptian people and culture were much older than theirs or than the Babylonian nation and civilization. . . . It is to his reckoning that we owe the still much-accepted date[s of Egyptian history] . . .”

Manethon, as Mertz pointed out respecting compilers of the past, has an ax to grind. It was to show that the Greek upstarts who had conquered his homeland were inferior because his country had a longer, more venerable historical tradition. In order to prove how very long and firm that history was (Freudian pun fully intended), Manethon organized and listed 31 dynasties, each containing numerous pharaohs in a series—seriatim—which gave great depth in time to Egyptian history. Others have suggested that there were 30 dynasties in Manethon’s list. For this work the author will use the more commonly employed number of 31 dynasties.

Gardiner describes Manethon’s compendium thus:

“Manetho undertook a chronicle of the Egyptian kings of which apart from some much edited extracts preserved by the Jewish historian

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9 Immanuel Velikovsky, Peoples of the Sea (NY 1977), pp. 207-208
Josephus (fl. [who flourished] A.D. 70), there only remains a garbled abridgement in the [later] works of the Christian chronographer Sextus Julius Africanus (early 3rd century A.D.) and Eusebius (early 4th century A.D.), and a much later compiler named George the Monk, known as Syncellus (c. A.D. 800), contributing greatly to the transmission. In Manetho’s work the entire history of Egypt, after the reign of the gods and demi-gods, was divided up into thirty-one dynasties of royal families beginning with Menes and ending with Alexander the Great’s conquest in 332 BC. In spite of all the defects this division into [31] dynasties [running one after the other] has taken such firm root in the literature of Egyptology that there is but little chance of it ever being abandoned. In most [of the later copied compilations of Manethon’s list] the form in which the book has reached us there are inaccuracies of the most glaring kind, these finding their climax on Dyn. XVIII, where the names and true sequences [of the pharaohs] are now known from indisputable monumental [written] sources. Africanus and Eusebius [kings lists] often do not agree; for example, Africanus assigns nine kings to Dyn. XVIII, while Eusebius has only three kings. Sometimes all that is vouchsafed to us is the number of kings in a dynasty ( . . . [as] in Dyn. VII-X [and] XX) and their city of origin. The royal names are apt to be incredibly distorted, that of Senwosre I of Dyn. XII, for instance, being assimilated in the form Sesonchosis to that of [the pharaoh] Shoshenk of a thousand years later. The lengths of reigns frequently differ in the two versions, as well as often showing wide departures from the definitely ascertained figures. When textual and other critics have done their best or worst, the reconstructed Manetho remains full of imperfections. What is even more serious, the story of Amenophis and the lepers quoted from Josephus, as well as the fantastic happenings ascribed to some of the kings, shows that he made use, not only of authentic records, but also of popular romances devoid of historical value. None the less, his book [though garbled] still dominates our studies.”

Rose quotes Gardiner as declaring that “no Egyptologist has yet been able to free himself from the shackles imposed by the native annalist’s [Manethon’s] thirty Dynasties, and these are likely always to remain the essential framework of our modern expositions.”

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10 Gardiner, op.cit., pp. 46-47
11 Rose, op.cit., p. 283
“Expressions like Gardiner’s ‘None the less’ are constant refrains in the Egyptological literature. No matter how unreliable Manethon may be, he still manages to keep everyone in ‘shackles’; no matter how often he has been proven wrong, ‘his book still dominates our studies’.

“Gardiner has a similar ambivalence about the Turin King-List. Thus on page 67 [Gardiner writes]: ‘we clutch at a straw . . . when we assume that the Turin Canon is to be implicitly trusted.’ But Gardiner remains consistently of two minds on all Manethonian materials like the Turin King-List, and in the very next sentence he is compelled to say: ‘On the other hand, its evidential value has often been underestimated, for the sober figures which it offers should inspire confidence rather than the reverse. . .

“It would be only reasonable to expect that some of the numbers in the Turin King-List or in Manethon MIGHT BE ACCURATE. But even that does not mean that scholars have to wear ‘the shackles imposed by the native annalist’.

“THE WAY IN WHICH THESE SHACKLED EGYPTOLOGISTS HAVE CHOSEN TO ARRANGE THEIR REMNANTS IS NOT THE ONLY WAY. A radical restructuring of the dynasties and kingdoms is long overdue. But this means taking a closer look at Manethon himself, that rabid nationalist who put Egyptology in ‘shackles’ in the first place! It will of course be equally important to see just how Manethon has been viewed by various authorities.”\(^{12}\)

Rose goes on to cite elements of Velikovsky’s analysis of these twentieth century Egyptologists. For example, Breasted, in *A History of Egypt* (London 1905), page 23, describes Manethon’s work as

“. . . a late, careless and uncritical compilation which can be proven wrong from the contemporary monuments in the vast majority of cases, where such monuments have survived.”

\(^{12}\) *ibid.*, p. 284
R.H. Hall in the *Cambridge Ancient History* (1923) vol. I, page 167, is also cited:

“As for Manetho, originally his dates were probably trustworthy; but his text has been so terribly mangled by copyists that it would be most unsafe to trust its data unless they are confirmed by the Turin Papyrus or by monumental evidence.”

Mertz concedes:

“In view of the fact that Manetho is damned with such faint praise, one might ask why we rely on him . . . The answer, as most Egyptologists sourly admit, is because Manetho’s concept has been used for so long that it would be inconvenient to discard it.”

Inconvenient is hardly the appropriate word to describe what would happen to Egyptological chronology and history if Manethon were discarded. But this is precisely what is required.

Rose, with justice, comments on such statements:

“Thus Hall seems to be saying that we are not to accept any of the details from Manethon unless they are already known to us [and corroborated] from some other source anyway. This causes a bit of a problem: if one is to accept only what has been checked and confirmed, and if ‘the vast majority’ of what has been checked has indeed been ‘proven wrong’, then there is not very much left that can be accepted!”

Velikovsky writes on page 209 of *Peoples of the Sea*:

“Despite the fact that Manetho’s lists were discredited by the documentary evidence of the Eighteenth and Nineteenth Dynasties, the best known of all and rich in documents, the dynasties for which there is no documentary evidence were preserved in accordance with Manetho’s scheme, since there were no extant monuments to refute those parts of the lists. The fact that in many cases no documents were

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13 Mertz, *op.cit.*, p. 40
14 Rose, *op.cit.*, p. 285
found to substantiate the existence of such dynasties was not always regarded as an obstacle sufficient by itself. There are almost no tangible clues even to the existence of Manetho’s Seventh to Tenth Dynasties or some other, later dynasties.”

First, we are told that corroboration via some form of documentation must exist before the Egyptologist ought to accept Manethon’s organization of the dynasties. Second, we are informed that extremely little, in the way of documentation, exists and in the vast majority of cases proves Manethon’s construction wrong. Then we are told that there are almost no tangible clues for the existence of Manethon’s 7th, 8th, 9th, and 10th Dynasties or some other, later ones, but that these have actually been incorporated into the historical chronology based on almost nothing. However, when one presents a revision of that Egyptian history, the panoply of materials that have been arranged to fit into it are called upon as refutation. A “circular” argument says that the revision is not possible because the evidence has already been incorporated to prop it up so it contradicts the revision.

But as Claudius James Rich explains, one need not be shackled by documents lacking in truth. In his discussion of Diodorus as an authority on the ancient world he writes:

“Those who have investigated the antiquities of Babylon have laid much stress on the authority of Diodorus [as those who have investigated Egypt have laid much stress on the authority of Manethon], probably adverting more to the quantity than to the quality of the information he supplies. He never was on the spot: he lived in an age when, as he himself tells us, its area was ploughed over: he has therefore recourse to Ctesias [as a source]; and it must be owned that the want of discrimination in the ancients, and the credulity of Diodorus himself, were never more strongly exemplified than in his choice of a writer [as his source] who confounds the Euphrates [River] with the Tigris, and tells us that Semiramis erected a monument to her husband, which from the dimensions he specifies must have been of superior elevation to Mount Vesuvius, and nearly equal to Mount Hecla. If these are not ‘fairy tales,’ I certainly know not to what the term can be applied. When an author [such as Ctesias, Diodorus, or Manethon] can in so many instances be closely convicted of ignorance and exaggeration, we
are certainly not justified in altering what is already before our eyes, to suit it to his description.”

The same applies to the chronology of Manethon as it has been interpreted to generate the present chronology of Egypt. But this the present-day historians and archaeologists have refused to consider and have built their chronological framework on materials that often have the status of fairy tales.

David Hackett Fischer, the great critic of historians, in his acclaimed book *Historians’ Fallacies*, offers a basic, logical primer of the improper and fallacious logic historians often present in order to support their hypotheses. As Robin W. Winks, in *The New York Times Book Review*, writes,

“If one laughs when David Hackett Fischer sits down to play, one will stay to cheer. His book must be read three times: the first in anger, the second in laughter, the third in respect... The wisdom is expressed with a certain ruthlessness, scarcely a major historian escapes unscathed...

“A dose of the book is salutary...”

Fischer points out innumerable failures of historians because they base their theses on illogical foundations of thought. His book provides one with a fairly complete elaboration of these forms of illogical thinking that pervade their writings. His salvoes are right on target and have provided a wealth of material which this writer will draw upon to examine and analyze the logic of those who uphold the established chronology. In the case of Manethon, just discussed, Fischer, in his chapter “Fallacies of Factual Verification,” points out that when a paradigm is established it paralyzes and stultifies innovation or revision. This he terms

“The fallacy of the hypostatized proof as identified and defined by Perrell F. Payne, [which] ‘consists in identifying the received theory about X... [in our case, Manethon’s chronology] with X itself, and hence rejecting some variant theory of X on the grounds that it does not do justice to the nature of X.’ In historical scholarship, this form of error commonly occurs when a historian reifies [makes his theory the

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16 David Hackett Fischer, *Historians’ Fallacies* (NY 1970), back cover
only absolute reality] a historiographical interpretation and substitutes it for the actual historical event it allegedly represents, and then rejects contradictory interpretations or affirms compatible ones.”

The fact of the matter is that the hypothetical history of the ancient world had become a hypostatized proof in 1901: Rose cites H.R. Hall on page 286 of *Sun, Moon, and Sothis*. Hall states that the chronology developed by the establishment is not an interpretation open to question but a fact of nature.

“The main scheme of the history of ancient Egypt is now a certainty, not a mere hypothesis; but it is very doubtful if it would ever have become a certainty if its construction had depended entirely on the archaeologists. The complete skeleton of the scheme was provided by the continuous literary tradition preserved by the Egyptian priest Manetho; this has been clothed with flesh by the archaeologists, and in the course of this process it has become clear that in the main Manetho had articulated his skeleton correctly.”

Walter B. Emery, in 1987, in a similar vein claims that the chronological organization produced by Manethon is of “immense importance and forms the framework on which Egyptian history has been built.” But in spite of these pronouncements Labib Habachi, then the Chief Inspector of Antiquities of Egypt, clearly cautioned his colleagues, as late as 1988, that

“Egyptology is a field in which chance discovery may disprove an established theory . . . [and further warned his Egyptologist colleagues that] they should stop offering “unqualified statements” and present their comments with “probably” and “perhaps.”

But is the articulation of the skeleton created by Manethon and clothed by the archeologists—who accepted this priest’s framework as being correct from the very beginning—truly correct? What is left of Manethon’s scheme of history “really amounts to little more than the dynastic numbers [1 through 31]. What else is there—about the general scheme of Egyptian history—that comes from Manethon

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17 ibid., pp. 55-56
20 Labib Habachi, *The Obelisks of Egypt* (Cairo 1988), p. 40
but not from any other source?”21 Listing the Egyptian dynasties in a series, where Dynasty 1, the earliest, is followed by 2, 3, 4, to 31, is only one way of organizing that history. But there is another way! In fact, there are statements in the literature that directly claim that the Egyptian dynasties may be arranged in a totally different manner in which all 31 dynasties do not follow one after the other. Eusebius himself, one of the copyists of Manethon, stated:

“But if the number of years is still in excess, it must be supposed that perhaps several Egyptian Kings ruled at one and the same time; for they say that the rulers were kings of Thebes, of Memphis, of Sais, of Ethiopia, and other places at the same time. It seems, moreover, that different kings held sway in different regions, and that each dynasty was confined to its own nome [district]: thus it was not a succession of kings occupying the throne one after the other, but several kings reigning at the same time in different regions. Hence arose the great total of years [in Manethon’s chronology].”22 [emphasis added]

Here it is suggested that there may be a completely different arrangement of the 31 dynasties prior to Alexander the Great, organized by Manethon but not clothed with flesh by the archaeologists. Eusebius lived far closer to the time of Manethon and may actually have read these words in a copy of Manethon or read commentaries that indicated the pharaohs ruled not in succession occupying the throne one after the other, but that various pharaohs reigned at the same time in different areas of Egypt. This interpretation of Manethon was therefore available to modern historians from the start. That they chose not to follow this hypothesis does not mean it is incorrect. If this other interpretation, that kings ruled at the same time in different regions of Egypt, is correct, then not only is the great length of Egyptian history fundamentally flawed: the history based on it is false. Isaac Newton, on the basis of his own chronological reconstruction, is cited by Frank E. Manuel. Newton disliked the priestly trick of extending the length of successive dynasties for all of Egypt, saying “they were contemporaneous local kings whose domain did not extend further than a single city,” and adding:

“‘Whereby Egypt was made one monarch older than ye world’ ‘which makes it probable that a great part of Manetho’s kings reigned in several parts [of Egypt] before the day of Ammon + Sesac [Shishak] when Egypt was divided into several small kingdoms + that the priests

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21 Rose, op.cit., p. 286
22 William G. Waddell, Manetho (London 1940), p. 9
of Egypt out of the Records of their several cities collected the kings of all these kingdoms into one continual succession to make the ages of their gods look ancient’.“23

Georges Cuvier, in his great work, The Revolution of the Globe, also looked into this question, citing historian Joseph Christoph Gatterer’s 1786 work:

“In Egypt there is indeed talk of hundreds of centuries; but they are filled with gods and demigods and before the invasion of the pastoral people [the Hyksos], what have been taken to be successive kings should be interpreted as the chieftains of several contemporary little states.” 24

Further, Waddell cites T. Nicklin’s Studies in Egyptian Chronology, offering the same concept:

“The Manethonian Dynasties are not lists of rulers over all Egypt, but lists of partly or more or less independent princes, partly of princely lines from which sprang rulers of all Egypt. (cf. the Scottish Stuarts, or the Electors of Hanover.) Some were mere Mayors of the Palace or princelets maintaining a precarious independence, or even more subordinate Governors of nomes from whom, however, descended subsequent monarchies.” 25

With respect to the concept of co-reigning pharaohs, Gardiner also suggests this solution. Since the Turin King-List has a hundred pharaohs ruling in Egypt over a 200-year span of time, giving each an average reign of 2 years, his solution was to have several kings reign at the same time in different regions:

“No insuperable objection stands in the way of the hypothesis that many kings were contemporary and ruling in different parts of the country.” 26

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25 Waddell, loc.cit.
26 Gardiner, op.cit., p. 66
The consequence of Gardiner’s solution which he raised in adopting this idea seems to have eluded him. If during a 200-year period of Egyptian history numerous pharaohs ruled at the same time in various districts, why should one assume that this same condition did not extend to all or almost all of Egyptian history? For example, James et al. report that


It is taken by the historians that these periods of co-reigning monarchs in Egypt were an anomaly compared to the orderly succession of pharaohs given by Manethon. But this may not have been the case. Clearly, we now have direct evidence from the ancients themselves that many pharaohs co-reigned in different districts of Egypt. We have statements by a copyist of Manethon, Eusebius, and by others who lived in the ancient world that concur with statements of modern historians such as Gardiner, Nicklin, etc., that the foundation stone of Egyptian history may be in error.

Velikovsky’s approach to Manethon’s chronology is explained by Rose:

“Radical as he was, Velikovsky did not greatly change the two remaining stretches [the periods before and after his own revision] of dynastic history. That is, even after he had displaced Dynasties Nineteen, Twenty, and Twenty-One, he allowed the Dynasties from earlier and from later to remain in their traditional sequence.

“Velikovsky was suspicious about Dynasties One to Seventeen, and wondered if some of those Dynasties even existed; he also wondered if some of them might be contemporary with others, and

27 James et al., Centuries of Darkness, (New Brunswick NJ) p. 233
whether there was not a considerable garbling here and there. But he did not attempt to refute, or really even to deal with, that earlier stretch of Egyptian history, except to say that the division of the Middle Kingdom into the Eleventh and Twelfth Dynasties was unnecessary, that Dynasties Seven to Ten might not even have existed, and that some ‘other, later’ Dynasties might not have existed either. (He could have had the Fourteenth, Sixteenth, or Seventeenth Dynasties in mind here.) [emphasis added]

“... In other words, Velikovsky did not substantially challenge the overall chronological pattern [of Manethon]...

“Thus the extent to which Velikovsky rejects the overall Manethonian sequence is not what one might think. In no way does Velikovsky’s work imply that Manethon’s overall scheme is entirely wrong. Much of that scheme is left unchallenged and intact.”

The importance of Velikovsky’s chronology, therefore, is not that it is necessarily incorrect, but that if the historical chronology of the ancient Near East is far shorter than the 500 years he proposed, then much of his chronology may fit within the new chronology offered by Rose, Heinsohn, and Sweeney.

Sweeney, in the “Epilogue” of his latest book, is quite clear that Velikovsky’s reconstruction still has great merit in terms of the shortened chronology:

“Indeed, now that the reconstruction of Near Eastern history has been completed, we can only look in wonder and admiration at Velikovsky’s achievement. Not one of the published volumes is wrong completely; and one at least, Peoples of the Sea, is completely right—both with regard to dates and character identification. Ages in Chaos is also completely right in terms of character identifications and synchronisms: only the overall timescale, as we have said, is at fault. Even the most “wrong” of his historical books, Ramses II and His Time, is basically correct with regard to dates, for Ramses II really did reign in the first half of the 7th century, and only the attempted

28 Rose, op.cit., p. 289
synchronisations with the Jewish and Babylonian histories were mistaken.

“Thus for a pioneering effort Velikovsky’s historical work was astonishingly accurate, and all of us, who over the years have enjoyed countless pleasant hours poring over and discussing his ideas, owe him an immense debt of gratitude. We can only hope that the wider academic world will eventually also come to realise this.”

Rose’s chronology of ancient Egypt appears in a chart (p. 293) presented below as Table 1.

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<td>Thirty-One [Macedonians]</td>
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Rose claims “It seems no accident that Dynasties One to Thirteen are in the [Nile] Valley and that Dynasties Twenty-Two to Thirty-One are in the Delta.” Rose’s revision clearly follows the concept outlined by Eusebius, Newton, Cuvier, Nicklin, and to some extent Gardiner, and even Velikovsky who, Rose suggested, “wondered if some of them [i.e. dynasties] might be contemporary.”

Jens Lieblein’s comments are appropriate to the situation at hand:

“I have never understood the obstinacy with which scholars have hung on to the regular succession of the thirty dynasties of Manetho.

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29 Sweeney, *op. cit.*, pp. 106-107
30 Rose, *op. cit.*, p. 294
However many voices of incontestable authority have protested, the error seems to be the fashion in our days.”

Some critics may claim that since neither Rose’s nor Heinsohn’s nor Sweeney’s chronologies agree in every aspect, one need not investigate the outlines of the history they present. However, it took a considerable time before the Egyptologists came to a general consensus of the present-day conventional chronology. Duncan MacNaughton, in 1932, described the situation that existed vis-à-vis the chronological disagreements among the historians:

“There is no judge to say who [i.e. which historian of ancient Egyptian chronology] is right and though fully twenty eminent Egyptologists have each proposed a series of dates, none of the resulting chronologies is such as to command general acceptance.”

If the earlier Egyptologists had chronological disagreements based on Manethon’s outline, it is only to be expected that revising the orthodox chronology by a thousand or more years will also lead to disagreements among the revisionists before a new chronology can be established.

In a certain sense, the earlier debate among the Egyptologists is somewhat similar to that which the revisionists present today. In the early part of the 20th century, Flinders Petrie, one of the founding fathers of Egyptian chronology, claimed that Egyptian history began between 5000 and 6000 B.C. In his 1909 *The Arts and Crafts of Ancient Egypt*, reprinted in London in 1996, he dates the “Prehistoric work [of Egypt dating from] (8000-5500 B.C.)” on p. 12. On page 14 he dates “The first dynasty (5500 B.C.)” On page 15, Petrie dates “The Pyramid age [to] (4700-4000 B.C.)”.

While Petrie and his followers claimed that Egyptian civilization began around 5500 B.C., present-day Egyptologists suggest that its onset should be dated to around 3000 B.C., a difference of 2500 years. Budge, in defending the longer 5500 B.C. chronology, argued:

[For ancient Egypt] “the [chronological] system which will have the best chance of survival, and at the same time be most correct, seems,

31 quoted in James et al., *op. cit.*, p. 17
32 Duncan MacNaughton, *A Scheme of Egyptian Chronology* (London 1932), p. 1
judging by the evidence before us, to be that which will take into due consideration the extreme antiquity of civilization . . . in the Valley of the Nile, and which will not be fettered by views based upon the opinion of those who would limit the existence of civilization of ancient Egypt to a period of about 3000 years.”

The date of 5500 B.C. for the First Egyptian Dynasty did not carry the day or the consensus reached by the historians. Budge’s view of the debate over the long vs. shorter chronology is, in this author’s view, to some extent à propos to the present debate over the further lowering of the 3000 B.C. date for the First Egyptian Dynasty by an additional 1000 to 1500 years or more.

“The various theories [of ancient Egyptian chronology] put forward by competent men were based upon:—(1) The scientific examination of the mummified remains of the historical Egyptians; (2) historical and geographical information derived from their hieroglyphic inscriptions [i.e. the documents], (3) the philological [written] peculiarities of the language exhibited by the hieroglyphic texts; and (4) statements made by ancient chronographers and historians [namely Manethon].

“The evidence derived from the statements referred to under No. 4 [Manethon’s chronology] was, of course, only of scientific value when supported by evidence derived from any or all of the classes of information summarized in Nos. 1, 2 and 3. The researches which have been made since the times when the main theories were propounded show that in each of them the many details were correct, and that their authors would have arrived at right conclusions had their deductions been based upon a larger number of facts, and upon a wider field of examination and information. Unfortunately, however, the field available for examination was limited and all the necessary facts were not forthcoming, and the pity is that the early writers of Egyptology assumed that they had solved a number of far-reaching problems in Egyptology when it was evident to all unbiased observers and honest enquirers that they still lacked the information which could only be obtained from data that were then non-available. Speaking broadly, the propounders of theories were hampered by their own preconceived views, and also by ideas derived from the works of scriptural and

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classical writers, and their difficulties were increased greatly by their own efforts to make the evidence derived from the ancient Egyptian native’s [i.e. Manethon’s] writing ‘square’ with what they obtained from foreign sources.’’

In a nutshell, Budge politely accuses his colleagues of forcing the chronology of Egypt into a procrustean bed because of their “preconceived views.” This author suggests that present-day Egyptologists have not arrived at the right conclusions because their deductions are based on the wrong methodology and therefore the wrong facts. None of the foundations outlined by Budge in numbers 1, 2, 3, or 4 can be of “historic value” unless they are supported by “scientific evidence.” The solution to a number of far-reaching problems in Egyptology will become evident to all unbiased observers and honest enquirers only when it is obtained from empirical data. Speaking broadly, the propounders and advocates of the present established chronology are hampered by their own preconceived views which, as we will see, do not ‘square’ with the scientific evidence. As Fischer succinctly states, “A historian must not merely get the facts right. He must get the right facts right.”

Fischer argues:

“[John Henry] Wigmore has exhaustively argued, sound evidence consists in the satisfactory relationship between the factum probandum of the proposition [thesis] to be proved and the factum probans, or the material which is offered as proof. That is sufficiently obvious. But it is not so obvious to many scholars that the criteria for a satisfactory factum probans [evidence for proof] depend in large degree upon the nature of the factum probandum [thesis being pursued]. This is a pedantical way of saying that every fact in history is an answer to a question and that the evidence which is useful and true and sufficient to question B [say the established chronology of ancient Egypt] may be false and useless in answer to question A [the revisionist chronology of ancient Egypt] . . .

“. . . an historian must not merely provide good relevant evidence but the best relevant evidence . . .

“. . . evidence must always be affirmative. Negative [or missing] evidence is a contradiction in terms—it is no evidence at all [such as

34 ibid., pp. 2-3
35 Fischer, op.cit., p. 62
the non-existence of evidence for dynasties that are still maintained in the literature].

While Fischer properly argues that “the burden of proof, for any historical assertion, always rests upon its author,” and “all inferences from empirical evidence are probabilistic”, etc., this author suggests, and will demonstrate, that the empirical scientific facts repeatedly contradict the established chronology but affirmatively corroborate the revisionists’ chronology, but most importantly converge in their support for that revision.

Many historians and archeologists will no doubt argue that the objective archaeological work carried out over the past two centuries makes it abundantly clear that there is no room for any major revision such as that proposed by Rose, Heinsohn, Sweeney, and in large part by Velikovsky. This was the objectivist concept offered by Fekri Hassan in Antiquity for December 1997, pp. 1020-25. Ian Hodder of the Department of Archaeology at the University of Cambridge, England, in his response reflects much of what this writer holds regarding that archeology:

“In the context of discussing the need to bring ‘unity and harmony to a world afflicted by ethnic sectarian and nationalistic conflicts,’ Hassan . . . talks of ‘upholding the mandate of reason’: Whose reason? It is clear that Hassan refers to the reason of a world community of trained archaeological experts . . . Trained by whom? He talks . . . of archaeology as a ‘scientific discipline within the academy.’ Which academy and defined by whom? The academy Hassan refers to guards against any usurpation of the concept of ‘fact’ . . . and upholds canons of knowledge against the abandonment of reason. . . . Again, concepts of ‘fact’ defined by whom, and whose reason?

“How . . . can authority derive from self-appointed ‘experts in the academy’? . . .

“Thus reason [according to Hassan] is opposed to beliefs based on fear, self-importance, authority, prejudices, superstitions. . . . The competence of trained scholars using standards and canons of description is opposed to superstition, dogmatism, obscurantism.

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36 ibid.
37 ibid., p. 63
Hassan . . . is incorrect to argue that archaeological science, or any science, is not derived from belief. It is belief, often passionately held, that generates questions, hypotheses, research pathways. It is belief, often dogmatic in tone, that leads to innovative lines of inquiry. It is belief, in the guise of theory, through which we make sense of the data so that they are, to some degree, theory-laden, as even procedural archaeologists now accept.”38 [emphasis added]

Whose chronology indeed, based on whose approach to evidence? What will be presented in the following chapters will indicate that the serial chronology of Manethon is inconsistent with the scientific evidence which this writer will present but consistent with the highly shortened chronologies of Rose, Heinsohn, Sweeney, and to a great degree Velikovsky. As Ludmilla Jordanova stressed in History in Practice,

“How do you decide whether someone or something IS reliable? Consistency is clearly vital; if a person [a historian] makes ten claims all of which turn out to be corroborated by other evidence, this is a high level of consistency and reliability. Let us suppose another person [a historian] has a much lower success rate and that it is necessary to choose between conflicting accounts by these two. . . , the former will seem a much better bet. There is, of course, no guarantee that this time they are still being reliable, but it seems an inherently more plausible assumption, the more independent [in our approach, the scientific] witnesses corroborate that account, the more reliable it will be deemed.”39

Science as it can be applied to history is largely the methodology of this book. In retrospect this author will apply empirical methods to history in order to resolve the conflict between the two antithetical chronologies. In a sense the argument goes back to that of Marx and Engels who argued, as did Velikovsky, for one science, one history:

“We know only a single science, the science of history. One can look at history from two sides and divide it into the history of nature and the history of men. However, the two sides are not divided off; as

long as men exist the history of nature and the history of men are mutually conditioned.”

This book is an attempt to tie history to science as far as is possible, and to determine what science tells us about history. In a sense, it offers a new methodology to unravel the past and set history upon a solid foundation. As one of the greatest analysts of the conceptual foundations of historiography, R.G. Collingwood, wrote in *The Idea of History*,

“History constructed by excerpting and combining the testimony of different authorities I call scissors-and-paste history. I repeat it is not really history at all, because it does not satisfy the conditions of science . . . history is not science, which is a fact that everybody knows in spite of the groundless claims by professional historians magnifying their office.”

In 1973 Kent R. Weeks, an Egyptologist with the American University in Cairo, along with James E. Harris, made it clear that the cut-and-paste history of ancient Egypt is in need of revision along scientific lines of analysis. What they suggest is à propos to the thesis of this book:

“Most people believe that since the hieroglyphs of ancient Egypt can be read, its art analyzed, its buildings excavated, a vivid clear picture of Egyptian life has been reconstructed. Unfortunately, this is not true. Egyptology is a young discipline—hieroglyphs could not be read accurately until well after Jean-François Champollion published his decipherment in 1822—and most of the texts from ancient Egypt are still in need of careful translation. Scientific archaeology is even younger, and of the thousands of archaeological sites known in Egypt, the vast majority is still undug, or only partially dug, or destroyed by thieves and untrained amateurs . . . Papyri, paintings, and texts simply do not tell enough.”

With all this in mind, let us begin.

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40 Karl Marx, Friedrich Engels, *The German Ideology* (1846), p. 96
42 James E. Harris, Kent R. Weeks, *X-Raying the Pharaohs* (NY 1973), p. 58
CHAPTER 2
THE SPHINX

All ancient Near Eastern history is based solely on that of ancient Egypt. Velikovsky explained this long ago:

“The student of ancient history . . . is accustomed to relate the chronology of the entire ancient East to Egyptian reckoning. ‘A system of relative chronology can be established by excavation in any country that has been long inhabited, but it is left hanging in the air until linked up with Egypt, whether directly or indirectly through a third region.’ [O.G.S. Crawford, *Man and His Past* (London 1921), p. 72] Kings and dynasties, lawgiving and building activity, wars and peace treaties of empires and kingdoms are allocated to centuries according to the rule of Egyptian chronology. When a document is unearthed which records the relations of some king with a pharaoh of a certain dynasty, the time of the king becomes fixed because the date of the pharaoh is known. The succession of the Assyrian and Babylonian kings with the dates of their reigns is studied with the help of so-called king-lists but is constantly being adjusted to comply with Egyptian dates wherever a synchronism is assumed. Thus the lawmaker king Hammurabi of the First Babylonian Dynasty, who for a long time was placed in about –2100, in recent decades has been transferred to about –1700, in order to synchronize the Egyptian Middle Kingdom with the First Dynasty of Babylon, on the basis of material from both places found in a common deposit on Crete. The past of the Minoan culture on Crete and the past of Mycenae on the mainland of Hellas are likewise divided and apportioned among the centuries, with Egypt playing the defining role.

“Egyptian chronology must be a mighty trunk to support branches of the history of so many kingdoms and cultures of the past. Is Egyptian chronology itself really rooted in strong evidence? It would seem that it is now too late to raise the question: not only the entire scientific literature in Egyptology but also complete libraries dealing
with man’s past have been composed according to the scheme set up by Egyptologists for all other branches of ancient history.

“Everyone is agreed that Egyptian chronology is so well devised, century by century, decade by decade, and often year by year, that no new evidence could break down this massive growth.”

In dealing with this notion that Egyptian history is without enormous problems the question of the great monuments of Egypt is there to test that established chronology.

The monuments of Egypt are extremely large and impressive. According to the historians they date back to around 3000 B.C. Temples, pyramids, and various other monuments still survive in various states of decay along the Nile River and inland as testimony to the great construction activity carried on by these people. Monuments built around the same time in the same region, however, are subject to the erosive effects of wind-blown sand, rainfall, heat, moisture, and any other element that will corrode and erode stone. Therefore, monuments built in the same region, made of the same materials, subject to these same elements of decay, should generally exhibit the same level of decay. For example, the Sphinx of Giza, according to conventional chronology, was built during the reign of the 4th king of the 4th dynasty, around 2550 B.C. The Valley Temple associated with it was also built around this time. There are a great many monuments and temples of the same material that were supposedly built prior to, and contemporary with, the Sphinx and the Valley Temple, and many others built during the six to eight centuries that followed their construction. And all of these prior, contemporary and somewhat later monuments were all exposed to the same temperatures, climatic conditions, and erosive conditions as these two. They should therefore exhibit about the same level of deterioration and erosion because, being composed of the same material—limestone—and given the same conditions, they should show about the same form and degree of erosion.

According to the historians those early conditions in Egypt were quite similar to the conditions of today that have existed from that time to this. Egypt is a highly arid region with little and sporadic rainfall. Therefore, the major elements of erosion available to do damage to limestone are wind and sand. When a layered rock face is weathered by wind-blown sand, its softer layers are more eroded than its hard ones which stick out of the profile, giving the appearance of a stack of

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1 Immanuel Velikovsky, *Peoples of the Sea* (NY 1977), pp. 205-206
different-sized books with various larger ones jutting out more than the smaller ones. However, when a rock wall or monument is worn by rain, both the soft and hard layers are also eroded, but there is a significant difference: the rain cuts channels or runnels, starting at the top which is eroded more greatly than the lower level. As this gullying continues the rain also rounds off the edges of the harder layers, forming an outward rounded surface but an inward concave curve on the softer layers. The distinction between these two different forms of weathering is relatively easy to identify by any trained geologist. The Sphinx and the Valley Temple associated with it, standing on the Giza Plateau, subject to an arid wind-blown environment, should certainly not exhibit clear evidence of rainfall erosion, but in 1992, Professor of Geology Robert M. Schoch, of Boston University, stated that the evidence of erosion, which he observed, clearly indicated that the Sphinx and Valley Temple had been sculpted by rainfall.\(^2\) The rounded surfaces and gullies were quite evident.

Before going further, however, it must be pointed out that I do not believe, as do Schoch and John Anthony West, who is associated with him, that the Sphinx or Valley Temple were built prior to Egyptian civilization, several thousand years earlier than 2550 B.C. What I do suggest is that the Sphinx and Temple were constructed during the latter stages of the hipparthermal when it is well understood that there was much greater rainfall across all the Afro-Asian deserts. Rather than having this pluvial period end 2200 B.C., as is generally presented, I date the end of this rainy era to around 1500 B.C.\(^3\) Since none of the temples, monuments and, in one case, a pyramid with limestone facing stones near its apex, which are now assumed to have been built prior to, or contemporary with, or only five to six centuries after the Sphinx was constructed, show this erosion, they cannot have been built until after 1500 B.C. If this is the case, then there is a momentous discrepancy in Egyptian historical chronology, as they should all exhibit the same form and level of erosion.

Therefore, all the temples and monuments made of limestone assigned to the First through Sixth Dynasties, could not have been erected at that time as the historians claim. All these impressive constructions had to be built at a much later time to escape rainfall erosion, and any documentation found on these monuments has to be dated much closer to the present. The pharaohs whom historians associate with these monuments, if they truly existed, also had to have reigned much closer to


\(^3\) Charles Ginenthal, *The Extinction of the Mammoth* (Forest Hills NY 1997)
the present than assumed. The contradiction to the established chronology could not be greater. Such an interpretation would destroy that chronology and with it the lifetime works of thousands of Egyptologists, archeologists, and others. To make matters worse, “For most of the past five thousand years it [the Sphinx] has been buried in its enclosure up to its neck in sand, and therefore completely protected from the [erosional] effects of wind-blown sand.”

The original proposal that the Sphinx was weathered by rain came from a mystic, René Schwaller de Lubicz. Schwaller de Lubicz wrote in Sacred Science (Rochester VT 1988), page 96, that “[the] Sphinx whose leonine body, except for the head, shows indisputable signs of aquatic erosion.” Schoch, who examined the Sphinx at the behest of John Anthony West, concurred and presented at a meeting of scientists in Washington, the AAAS Annual Meeting, his findings which he had published earlier with West. He has also described the rain erosion in an NBC television documentary in 1993, Mystery of the Sphinx, and in the BBC’s Timewatch documentary, “Age of the Sphinx,” broadcast November 27, 1994. In no uncertain terms Schoch calls the erosion “a classic textbook example of what happens to a limestone structure when you have rain beating down on it for thousands of years.”

It had been taken as fact that there had been extremely little rainfall during the period in which the Sphinx was built, certainly not enough to create such glaring deterioration. In fact, the Sphinx had been repaired three times to cover over with new stones these obvious rents in its body. Here then appeared to be rather clear-cut evidence that the Sphinx was subjected to at least several centuries of almost tropical rainfall. To this challenge to their chronology, the Egyptologists, resolute in their conviction of the validity of their climatic chronological system, responded that the claims of Schoch and West were thoroughly outrageous and that their established dating of the building of the Sphinx and Valley Temple blocks was correct, and all the rest of the Egyptian monuments and especially those on the Giza Plateau surrounding these monuments had not been eroded by rain. This they based strictly on the historical record, meaning their interpretation of that record. Typical of the outcries reported are these responses:

“‘That’s ridiculous,’ scoffed Peter Lecovara, assistant curator of the Egyptian Department in Boston’s Museum of Fine Arts (Boston

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4 John Anthony West, Serpent in the Sky (Wheaton IL, 1993), p. 189
5 ibid., pp. 186-187
‘Thousands of scholars working for hundreds of years have studied this topic and the chronology is pretty much worked out. There are no big surprises in store for us.’ Ptolemaic astronomers confronted by Galileo’s evidence for a heliocentric solar system had once raised exactly that [kind of] objection.”

Mimi Mann, archeological correspondent, in an AP dispatch from Cairo portrayed those who maintained that rainfall damage had eroded the Sphinx, requiring a chronological overhaul of Egyptian history, as a bunch of loony Atlantis hunters. Zahi Hawass, the Director-General of the Egyptian Antiquities Organization in charge of the Giza Pyramids, when he learned about Schoch’s geological work, decided to halt the work. Schoch’s group had been granted access to the Sphinx by Dr Ibrahim Bakr, at the time President of the Egyptian Antiquities Organization. Hawass, angry that his supervisor had given the Americans access to the Sphinx without his permission, accused them of damaging these structures, stating:

“I have found out that their work is carried out by installing endoscopes in[side] the Sphinx’s body and shooting films for all phases of the work . . . [as] propaganda . . . but not in a scientific manner. I therefore suspended the work of this unscientific mission and made a report which was presented to the permanent commission who rejected the mission’s work in [the] future . . .”

Ever since that time, the Egyptian government has refused to allow Schoch additional geological scientific access to the Sphinx. Not only did they bar Schoch’s team from the area, but they decided that such scientific work would not be permitted by other interested groups, except perhaps by those who may support the establishment’s claims regarding the Sphinx.

Nevertheless, this attempt to disrupt Schoch’s research came fortunately after that team had assembled sufficient data and film to establish their case. Hawass had been unable to suppress this material from reaching millions of viewers of their film in the United States and England and also by other millions who read accounts of the research in newspapers and news journals. The cat was

7 West, op.cit., p. 230
8 ibid., p. 229
9 An Akhbar El Yom (Jan. 8, 1994)
well out of the bag and something had to be done to negate the influence of Schoch and West’s evidence.

Of course this sort of outright rejection of scientific evidence made the Egyptologists appear not only unscholarly, but dogmatically rigid. The proper way to debate the issue was on scientific grounds and not from accepted historical dates. Some among them realized this, and various scientific attempts were made to make the deterioration on the Sphinx match the established chronology. K. Lal Gauri had two articles published to explain the damage.\textsuperscript{10} Thereafter, Gauri presented further evidence to dispute Schoch and West’s conclusions.\textsuperscript{11}

West was among the first researchers to respond to Gauri’s first two articles, so far as I am aware. Peter James and Nick Thorpe describe West as follows:

“Credit—or blame, depending on one’s point of view—for the present controversy about the Sphinx largely goes to one man, John Anthony West, an amateur Egyptologist and guide book writer who has spent many years exploring the mysteries of ancient Egypt. West has written enthusiastically about astrology, argued for the reality of the lost continent of Atlantis, and believes that a past civilization on Mars influenced the development of our ancient civilizations . . . None of these ideas, of course, endear him to professional Egyptologists, who dismiss West as a complete crank. Crank or not, West scores full marks for persistence. . .”\textsuperscript{12}

Under the disguise of reporting about West lies a not at all subtle attempt to tarnish him with “guilt by association.” But West himself has presented exactly what he means regarding a civilization far older than those known to history, as James Harrell, a geology professor of the University of Toledo, Ohio, explains:

“‘If you pin these guys [Schoch and West] down, they really think these people were refugees from Atlantis, the Lost Continent’,


\textsuperscript{12} Peter James, Nick Thorpe, \textit{Ancient Mysteries} (NY 1999), p. 216
Harrell scoffs. West calls Atlantis the “A word,” and claims to use the name only as shorthand for the mystery civilization.”

Schoch also disclaimed these associations:

“I’m not saying that the Sphinx was built by Atlanteans or people from Mars, or extra-terrestrials. I’m just following the science where it leads me.”

Undoubtedly, James and Thorpe are pursuing what historian David Hackett Fischer terms

“The fallacy of argument ad hominem [which] occurs in many different forms, all of which serve to shift the attention from the argument to the arguer.

“[For example] there are associative ad hominem arguments which attempt to undercut an opponent by reference to the company he keeps.”

Once James and Thorpe have introduced the idea that West is associated with the sunken continent of Atlantis and past civilizations on Mars having influenced the development of ancient civilizations on Earth, one would be quite prone to believe that since he, and by connection and association also Schoch, may hold such highly dubious fringe views, which for Schoch is false, why take anything they say seriously? That Peter James, long associated with Velikovsky who suffered from similar forms of ad hominem, should employ the “guilt by association” method, is simply miserable.

This is particularly so since James has also written an excellent book titled provocatively Sunken Kingdom: The Atlantis Mystery Solved. Though James does not subscribe to Atlantis being situated in the Atlantic Ocean or dated to about 9000 to 11000 B.C., he does offer evidence that this city of myth is the echo of the destruction and burial of an ancient Hittite city. It is an extremely valuable book,

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14 Extracts from the BBC’s Mystery of the Sphinx and AAAS meeting in Graham Hancock, Fingerprints of the Gods (NY 1995), p. 422
15 Fischer, op.cit., pp. 290-291
highly documented and well written, an attempt to place the Atlantis of myth in historical times in the Near East. Were one to use the same *ad hominem* argument to discredit his work, one could say James had been a follower of Velikovsky who believed in Atlantis, and that Venus is a new-born planet which altered the position of the poles of the Earth in historical times, which might well cause numerous investigators to reject his fine efforts before attempting to read his work. Yet this is the approach he has chosen to apply to Schoch and especially to West.

His description of Schwaller de Lubicz is no less revealing, with respect to tarnishing the reputation of those who propose rainfall erosion on the Sphinx by this *ad hominem* argument of associative guilt.

“The inspiration for West’s theory came in the late 1970s when he was studying the work of the French mathematician and occultist Schwaller de Lubicz. De Lubicz argued that Egyptian art and architecture encoded symbols that were both mathematical and mystical at the same time.”

These attempts to denigrate West and Schoch have continued since the debate’s inception. For example, in 1995 in an “Editorial Report” in *KMT*, Zahi Hawass raised the same issues of Atlantis and civilizations on Mars to tarnish their reputations. In the “Reader’s Forum” that same year, West discussed Aidan Dodson’s comments on his and Schoch’s work, first citing Dodson who wrote:

“. . . ‘the long-standing failure of Egyptology to both effectively present the real facts to the general public and address the lunatic fringe head-on has led to many intelligent individuals being taken in by the Cayces, von Dänikens—and John Anthony Wests—of this world. . . . Until they do, we will continue to be bombarded by the Wests, Bauvals (Orion) and so on.’

[West then writes] “I need not elaborate on the illegitimacy of lumping Robert Bauval and myself together with von Däniken and then adding Cayce to produce a blanket pejorative, a kind of all inclusive Egyptian bogeyman.”

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16 ibid.
17 “Editorial Report,” *KMT*, vol. 6, no. 1 (Spring 1995), pp. 2-3
18 “Readers’ Forum,” *KMT*, vol. 6, no. 2 (Summer 1995), p. 5
In the full “Readers’ Forum”, F. Yurco of the University of Chicago wrote an attack aimed at West and Schoch, overflowing with denigrating remarks. Fortunately S.S. Mehle’s response in the following issue of KMT exposed what lay beneath the surface.

“But it is the last section of Yurco’s letter that disturbs me the most, with statements as ‘note the damage (?) that the claims of West, Schoch and others have caused,’ ‘fantastic assertions,’ ‘their wild notions . . . have a certain flashy appeal.’ Are these the arguments of an objective, rational scientist? Or do they sound like the ravings of a prominent member of the office of the Inquisition . . . attacking Galileo and others who attempted to disagree with Church doctrine?”

Not only has the historical establishment condemned Schoch’s finding but the press joined the chorus:

“The conclusion ‘flies in the face of everything we know about ancient Egypt,’ the Los Angeles Times added in its announcement. ‘Other Egyptologists who have looked at Mr. [note the “Mr.” rather than “Dr.”] Schoch’s work cannot explain the geological evidence but they insist that the idea . . . ‘does not match up’ with what has been known. The newspaper quoted archaeologist Carol Redmont of the University of California at Berkeley. ‘There’s just no way that could be true. . .’”

Now that their erosional view, not their historical view, is being defended in a Velikovskian organ, in spite of the fact that neither Schoch nor West subscribe in any way to Velikovsky’s ideas, it will give their opponents a new pejorative with which to tarnish them. West, however, is no shrinking violet when it comes to critics. He sees critics the way cannibals saw boat loads of missionaries coming to convert them.

Thus those who would support Schoch and West’s contention that rain caused the erosion on the Sphinx are vilified as enthusiasts of “astrology,” argue for the reality of “Atlantis” and propose that “past civilizations existed on Mars,” subscribe to “mystical ideas,” and are “occultists.” This, of course, tells us nothing about the evidence that Schoch and West presented, though it does devalue their

20 Zecharia Sitchin, When Time Began The First New Age (NY 1993), pp. 256-257
scientific and rational standing in the eyes of those predisposed to make such assumptions. Let us remember that Carl Sagan chose to tar Velikovsky as one who upheld “astrology,” believed in “Atlantis,” and suggested “intelligent life” existed on Mars!\(^2\) Describing West and de Lubicz as they do clearly informs us which side of the debate James, Thorpe and these others support. However, what James and Thorpe have failed to report to their readers regarding West is his clear exposition of the failure of Gauri’s early criticisms to come to grips with some of the arguments he raised, which to this writer are rather fundamental. For example, West raises the following point:

> “An archeological and geological survey of the Sphinx . . . was carried out by Mark Lehner, Field Director for the American Research Center in Egypt, and Dr K. Lal Gauri, Director of the Stone Conservation Laboratory at the University of Louisville, Kentucky . . .

> “Alternative explanations for the weathering of the Sphinx advanced by Dr Gauri in order to preserve the accepted chronology are in my view utterly unconvincing and are contradicted by the very facts he himself revealed. . .

> “Though the published reports are somewhat technical, the issues at stake are easily explained and perfectly comprehensible to the alert reader. Here are the main points . . . :

> “1. The standard explanation—that the weathering to the Sphinx is due to wind and sand—has been abandoned.

> “2. Mark Lehner finds that there has been not one, but three separate major repair campaigns carried out on the Sphinx. [Mark Lehner, *The Complete Pyramids* (London 1997) pp. 128-129] The dating of these campaigns poses perhaps insoluble problems. . .

> “3. Lehner notes that *until the past few decades, no substantial weathering has taken place on the Sphinx since the first repair campaign was carried out*. This is perhaps the most crucial point in the argument for a revised chronology and must be emphasized. [West’s emphasis]

\(^2\) Carl Sagan, *Broca’s Brain*, p. 126, p. 71, p. 90
“4. Lehner goes on to deduce that this leaves only ± 500 years for the Sphinx to have eroded from its original to its present condition—with channels worn two feet deep into its limestone walls.

“5. Tests by Dr Gauri prove that the weathering is due to water reacting with the natural salts in the limestone; in other words, that the damage to the Sphinx is due to water erosion.

“6. However, he insists that this is due to ground water leaching into the body of the Sphinx from below. . . . Neither he nor Lehner seem troubled by contradictions implicit in their conclusions.”

West then goes on to elaborate on these:

“But these contradictions are both manifold and glaring and, short of some kind of geological miracle manifesting itself at the Sphinx and nowhere else in the world, Dr Gauri’s reasoning will not suffice to explain the erosion on the Sphinx.

“The key to the fallacy is Lehner’s discovery that no substantial erosion has taken place since the earliest repair campaign was undertaken. At the same time Lehner claims that given the orthodox chronology only ± 500 years can be allowed for the erosion to have occurred.

“Where then was Dr Gauri’s underground water at this time [after the erosion had occurred]? And how did it leach up into the body of the Sphinx, eroding channels two feet deep into its sides in only ± 500 years, THEN CEASING SUCH DESTRUCTIVE ACTION FOR THE REMAINING ± 4000 YEARS? [capitalization added]

“It might be argued, perhaps, that the various repair campaigns effectively prevented further erosion by protecting the surface of the Sphinx. But this argument will not do. The walls of the hollow, carved out to free the body of the Sphinx, have never been repaired . . . , and these show an erosion pattern absolutely identical to that on the Sphinx itself. Since Dr Gauri’s explanation calls for erosion taking place at the

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22 West, op. cit., pp. 221-222
unthinkable rate of $\pm 4$ inches per century, the walls of the hollow, never having been repaired, should show more severe erosion than that of the body of the Sphinx itself. Yet they do not."\textsuperscript{23}

This claimed 4 inches per century over 4000 years requires 160 inches or 13 feet should have been eroded on the walls. Any explanations for erosion of the Sphinx must come to grips with these facts. They must explain why the Sphinx was eroded for a period of time, but also why the erosion stopped for four thousand years after the first repair. After all, the unprotected areas of the Sphinx were still exposed to water erosion for 4000 years beyond the time the repairs were made. They, too, should exhibit 13 feet of erosion in them, if Gauri’s thesis is correct.

Every argument that attempts to explain the erosion on the Sphinx fails unless it somehow also explains why the erosion completely halted after the first repairs on it were made and also stopped on the enclosure wall. To suggest that geological weathering processes worked for five centuries and then simply stopped operating for the next four thousand years—based on the established chronology—is simply incredible. Yet not a single theorist who opposed Schoch and West has ever answered or even addressed this dilemma when they offered their own hypothesis for erosion to the Sphinx.

If, however, rain into the sand in the Sphinx enclosure is responsible, it should have weathered the unprotected areas on the Sphinx and the enclosure walls far more deeply during the subsequent four thousand years as well. If floods from the Nile are responsible they should have weathered the enclosure walls far more deeply as well. These arguments by Schoch’s critics are not scientific; they defy logic and geological fact.

The only rational, geological way to weather both the Sphinx and its enclosure walls equally is to have a prolonged period of heavy rainfall in that region which weathered the Sphinx and its enclosure walls to the same degree and which then ceased after the first repairs. This point is so remarkable in its consistency and explanatory power that apparently not a single geologist who has proposed any other mechanism for the erosion has addressed this fact. They have simply ignored it, and as I see it, they ignore it because there is no escape from it. Among the other facts this one has not been addressed for nearly a decade.

\textsuperscript{23} ibid., p. 222
West’s further arguments are just as telling:

“The questions raised by Dr Gauri’s explanation are further compounded by the peculiarities of the Egyptian ground-water level. This, until the recent disturbing factor of the Aswan High Dam, has been tied to the annual flooding of the Nile and can be easily calculated and demonstrated. Over the millennia the Nile floods have gradually deposited successive layers of silt, raising the floor of the flood plain at roughly the rate of 10 feet per 1000 years. The underground water table has risen accordingly. This means that when the Sphinx was built, allegedly just short of 5000 years ago, the ground-water table level was some 50 feet below its present level (reckoning 10 feet per 1000 years).

“Following Dr Gauri’s explanation, this means that underground water 50 feet lower than at its present level seeped upward into the body of the Sphinx around the time it was carved, eroding away the surface stone at a rate of + 4 inches per century. And thereafter ceased eroding altogether as the water level continued to rise over the ensuing 4 1/2 millennia—for, as Lehner insists, all weathering to the Sphinx had virtually taken place by the time of the earliest repair campaign.”24 [West’s emphasis]

Schoch suggests: “Over 10,000 years . . . a little over 26 feet of soil accumulated burying ancient sites . . .”25

Therefore, when the Sphinx was constructed, according to conventional chronology, the Nile’s water level and groundwater level was about twenty to fifty feet lower than at present. It thus had to rise at least twenty to fifty feet higher to reach its present level, or the height of a three to five-story building. The question of course is: Where did all the additional rain come from to cause the Nile floods to rise twenty to fifty feet higher than today? And the other question is: If it rose thirty to fifty feet higher then, why did it stop doing so when the historians suggest that somewhat similar climatic conditions as today prevailed then in Egypt?

The fact of the matter is that we do have Nilometer records showing that the Nile did rise twenty-two feet higher than today. Velikovsky discussed this evidence:

24 *ibid.*, see also Joseph Davidovits, Margie Morris, *The Pyramids* (NY 1988), p. 22
“K.R. Lepsius observed that the Nilometer at Samneh . . . shows an average rise in the waters of the Nile at that place, where the river is channelled in rock twenty-two feet higher than the highest level of today. We obtain the remarkable result that about 4000 years ago [based on the established chronology] the Nile used to rise at that point on average twenty-two feet higher than it does at present.”

However, according to Duncan Steel, the Nile at present rises, on average, “only a five meter [15 ft] enhancement.”

This means that where the Nile River was broader it had to have risen to a far lower level. But let us accept that it did rise high enough to not only drown the Sphinx but also the blocks of the Valley Temple. Based on the established chronology, this occurred after the Sphinx, Valley Temple, and other monuments between them were built. Again we run up against West’s criticism. Why aren’t these other monuments between them weathered by the Nile floods to give them the same appearance as that of the Sphinx and Valley Temple? Further, since the Nile rose higher in ancient times than it does today, given it had a somewhat similar climate in that ancient period as today, it should be flooding thirty to fifty feet above the twenty-two foot level today. That is, its flood levels today should be fifty-two to seventy-two feet or so higher today. Thus not only should the monuments around and below the Valley Temple show this erosion, but all the monuments of Egypt below the level of the Valley Temple should exhibit this weathering pattern. This concept is so ridden with contradiction that it is drowning in contradiction.

There is yet a further dilemma with the idea that groundwater seeping upward into the Sphinx will produce this erosion. If the Nile rose and fell, as suggested by Gauri, it would linger longer at the lower levels of the Sphinx, having more time to erode the lower layers than those closer to the top of its body. Although the Sphinx possesses three different layers of limestone, each of slightly different hardness, the erosion should nevertheless be greater at the lower level of each of these layers where the water remained longer. That is, each layer of limestone, following Gauri’s hypothesis, should have greater erosion at its lower part than its upper. Instead just the opposite is the case. In all instances the upper part of each layer is eroded more greatly than the lower. Schoch, in discussing the Sphinx enclosure, specifically makes just this point.

27 Duncan Steel, *Marking Time: The Epic Quest to Invent the Perfect Calendar* (NY 2000), p. 38
“The fact remains that even taking . . . into account the harder layers at the top of the section [they] have been in general eroded back further than the softer layers lower in the section.”28

Elsewhere he states:

“Yet if inundation was the cause of the water induced erosion, the wear should be greater on the paws and lower body, when in fact it is more pronounced on the much higher back.”29

West’s further comments on these matters are equally damaging to those who propose Nile flooding as the culprit of erosion:

“. . . the agent responsible for weathering the Sphinx was also responsible for weathering the mighty core blocks of the Valley Temple. On this basis as well, Dr Gauri’s hypothesis must be rejected, since water seeping upward through capillary action cannot traverse the breaks in the individual courses of stone. In other words, even if there were no commanding reason to disqualify Dr Gauri’s theory when applied to the Sphinx, it would be disqualified on the basis of the erosion pattern on the Valley Temple—since the same weathering agent has to be responsible for both, and that agent cannot have been water leaching up from underground in the case of the Valley Temple. A somewhat similar argument may be drawn from the erosion pattern found in the so-called Mortuary Temple connected to the Sphinx.

“This temple was built on the [Giza] plateau more than 150 feet above the level of the Sphinx, but its erosion pattern is similar to that of the Sphinx (allowing for the rather drastic ravages resulting from its higher, more exposed position on the plateau). If water seeping up from underground were actually responsible for the erosion to the Sphinx, then that same water would have to have seeped still another 150 feet upward to weather away the Mortuary Temple. Also, it would have to have done so selectively, since it is only the Mortuary Temple amid the myriad mastabas and other structures built on the plateau in Old

Kingdom times that display that typical water erosion pattern. This is inconceivable. . .”

This problem of selectivity of erosion faces all other theories as well. Any process that posits these erosion patterns on the Sphinx, its enclosure wall, Valley Temple, and Mortuary Temple, must explain why only these monuments of that period were weathered but why the same process, miraculously, failed to erode any of the other numerous Old Kingdom monuments between these levels. Erosion is not selective in this sense. Monuments of rock in the same region subject to the same processes of weathering should also exhibit similar erosional markings on their surfaces. Based on this evidence the Sphinx, its enclosure wall, Valley Temple, and Mortuary Temple had to be built prior to all the monuments that surround them unless a miracle occurred and permitted these and only these monuments to be weathered.

In 1995 Gauri et al. presented an additional thesis to refute West and Schoch’s analysis. In it they attempted to explain how the Valley Temple and Mortuary Temple, which were not buried in sand, were eroded to display the same weathering pattern observed on the Sphinx. They suggested that since the limestone of which it was composed contains pores, these could be exploited to replicate the erosion pattern seen on the Sphinx. In the cool Giza mornings, dew would condense in these pores which would dissolve salts mixed in the limestone. As the salts recrystallized when the water evaporated away in the heat of the day the crystals would grow in size. Each time this occurred the salt crystals would grow larger, creating a tiny but significant pressure on the rock layer above, which would flake off exposing the rock beneath with its numerous pores to a new assault by the morning moisture. The flaking would tend to occur where fissures were present to admit water. According to Z. Hawass and M. Lehner, the limestone of which the Sphinx and Valley Temple had been built had experienced great pressures in Eocene times when the water table had fallen, causing the rock to buckle slightly, producing the innumerable tiny pores and fissures to be later exploited by morning moisture.

The morning dew thesis was an attractive explanation since it meant that the Valley Temple’s erosion could mimic that on the Sphinx. Did this new hypothesis save the dating of the Valley Temple, as Gauri et al. had intended? The problem with their process was apparent right from the start. The morning moisture

30 West, op.cit., pp. 222-223
must not only penetrate the openings in the limestone of the Valley Temple, it must also act in exactly the same manner on every other limestone building, temple, monument or any other structure on the Giza Plateau and, for that matter, on all the natural limestone structures in Egypt as well. All these various constructed buildings and natural formations should also exhibit severe erosional features quite similar to the Valley and Mortuary Temples. Although minimal erosion of this type is observed throughout Egypt’s limestone monuments, the severity of that on the Valley Temple and Mortuary Temple is so much more strongly pronounced that Gauri et al.’s hypothesis simply cannot survive. Schoch has also remarked on this uniquely selective process. “The limestones of the Giza Plateau are criss-crossed with fractures or joints, and these date back millions of years. However, the joints are not opened up as fissures on the Giza Plateau”\textsuperscript{32}

What Gauri et al.’s thesis requires is that the morning dew was highly selective, only entering the pores in the limestone of the Valley Temple and Mortuary Temple, but rarely entering the pores of other limestone buildings or monuments. There could not be such a selective form of weathering process that acts only where the geologists wish it to act but fails to do so in other places where the same conditions for it to act exist. Geological weathering processes given the same conditions have to act in the same way. A scientific process cannot be arbitrarily applied in the manner that Gauri et al. put forth. What they are suggesting is not scientific. Their attempt to salvage establishment Egyptian chronology is an ad hoc failure.

This was fully explained by David Coxill, a geologist with MSc and BSc (Honors) degrees in geology, and Fellow of the Geological Society of London, who analyzed Gauri et al.’s thesis and stated unambiguously:

“Firstly, condensation affects all monuments in the Giza complex, but very rarely do any show the same type of weathering features of the Sphinx, surrounding pit and stone blocks of the Valley Temple.

“Secondly, these weathering features require intense weathering to form their present profile, and condensation/evaporation is a relatively mild and insignificant form of mechanical weathering in this arid climate. Thirdly, fluctuations in the water table do not lead to fissures being wider at the top. Lal Gauri (1995) also suggest [sic] that

\textsuperscript{32} Schoch, “Sphinx Links,” op.cit., p. 10
the roundness of the laminars [layered limestone] is due to gradational differences in the hardness of the strata. This does not account for variations in the weathering profile.

“Therefore, by process of elimination, it appears that floodwaters and fluctuating ground water levels cannot explain these weathering features, but rainfall does.”

James and Thorpe erroneously have argued “Gauri’s [1995] paper has been overlooked by supporters of Schoch as if it had never been written.” Yet here we have a highly qualified geologist doing exactly what James and Thorpe say has never been done. Gauri’s paper was properly addressed and answered by a supporter of Schoch’s rainfall thesis who is a trained geologist knowledgeable about erosional processes.

In this respect Coxill’s analysis is in complete harmony with Schoch’s rainfall thesis. He informs us that dew in the pores or fissures of this rock is not a significant weathering mechanism, but rather a “mild and insignificant . . . mechanical weathering [process] in this arid climate.” That explains why the weathering on all the other limestone monuments is mild compared with that of the Valley and Mortuary Temples, because both were subject to a prolonged period of heavy rainfall and these other monuments were not. The reason none of the other limestone monuments exhibit as severe rainfall erosion is that they were built after the rainy era had ended and thus they were affected by a mild and insignificant form of mechanical weathering of that later arid climate. Had they been built around the same time they would be weathered to the same degree.

Furthermore, as with water erosion from below on the Sphinx which should do more damage at the bottom of each member, the dew on the Valley Temple should exhibit a fairly uniform type of weathering. It should not erode the top elements of the Valley Temple stone blocks more than those nearer the bottom. Rainfall does that quite effectively. But the Valley and Mortuary Temples also have greater erosion at the top and less at the bottom. The process Gauri et al. offer requires that the erosion open pores here and there fairly evenly and not selectively, more so at the top of the Valley and Mortuary Temples. Again, this fact that these temples’ erosional features are more pronounced at the top than at the bottom, just

33 David Coxill, “The Riddle of the Sphinx,” Inscription—Journal of Ancient Egypt (Spring 1998), pp. 16-17
34 James, Thorpe, op.cit., p. 225
the way rainfall weathers such rock, contradicts Gauri et al.’s hypothesis. And Gauri has also failed to address the criticism raised by West.

In 1994, geologist J.A. Harrell entered the dispute. James and Thorpe summarize Harrell’s work thus:

“When Schoch published his arguments . . . in the Egyptological journal KMT in 1992, a reply was soon forthcoming from Dr James Harrell, Professor of Geology at the University of Toledo. As the director of a six-year-long project studying the ancient quarries of Egypt, Harrell is no stranger to conditions there. Harrell argued that the weathering that Schoch insisted came from rainfall could have been produced by other mechanisms. During recent excavations of the Sphinx the sand that had accumulated in the enclosure ‘was completely soaked a few inches below the surface.’ As we know that the Sphinx was covered with sand throughout much of history . . . it is very likely that for much of its existence the Sphinx lay in a bog of wet sand. The moisture in the sand would have accelerated chemical weathering. Why, Harrell argued, has Schoch not considered such possibilities? He rejected Schoch’s case as ‘poorly supported’ and ‘outlandish.’

Schoch replied to Harrell’s paper, dismissing his arguments as ad hoc theorizing. The minutiae of their arguments are not important, as Harrell’s main point about chemical weathering had already been developed fully by Gauri at a 1992 meeting of the American Association for the Advancement of Science. His paper, published in 1995, makes short shrift of Schoch’s theory. Gauri and his team stress that the real cause of erosion of the Sphinx can still be observed today. Almost every day slivers of limestone the size of potato chips drop from the sides of the Sphinx. . . . Modern pollution, of course, has aggravated the problem. . . .” [They then go on to Gauri’s morning dew concept, which Coxill, above, has shown will not explain away the evidence.]

The potato chip-sized flakes are modern and are due to modern pollution. To suggest that large flakes had fallen off for thousands of years would

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36 James, Thorpe, op.cit., pp. 223-224
remove all the hieroglyphics on exposed limestone monuments. They complete their argument with the following claim:

“The point is that while no geologist has gone on record as supporting Schoch, two senior geologists, with years of experience in Egypt, have produced alternative explanations of the erosion on the Sphinx. As Egyptologist Mark Lehner has frequently tried to stress, this is not a case of ‘geologists against archaeologists. This is archaeologists and geologists against one geologist—Robert Schoch’.”37

This argument, based on majority opinion, is another one of the fallacies detailed by Fischer in his admonitions to historians, calling it

“The fallacy of the prevalent proof [which] makes mass opinion into a method of verification. This practice has been discovered by cultural anthropologists among such tribes as the Kuba, for whom history was whatever the majority declared to be true. . . .

“I witnessed one such occasion (circa 1962) as a student at Johns Hopkins University. A scholar who was baffled by a knotty problem of fact literally called for a show of hands to settle the question. An alienated minority of callow youths in the back of the room raised both hands and carried the day in defiance of logic, empiricism, and parliamentary procedure.

“If the fallacy of the prevalent proof appeared only in this vulgar form, there would be little to fear from it. But in more subtle shapes, the same sort of error is widespread. Few scholars have failed to bend, in some degree, before the collective conceits of their colleagues. Many have attempted to establish a doubtful question by a phrase such as ‘most historians agree . . .’ or ‘it is the consensus of scholarly opinion that . . .’ or ‘in the judgement of all serious students of this problem . . .’”38

37 ibid., p. 225
38 Fischer, op.cit., pp. 51-52
Whether the appeal is to the majority of historians or the majority of scientists, the expression of the prevalent proof James and Thorpe, and Lehner, put forth is appalling. But worse than that, they have presented a blatant falsehood regarding the “lone geologist—Robert Schoch.” First of all, at the February 1992 AAAS session devoted to the question “How old is the Sphinx?” Schoch debated with Mark Lehner and K.L. Gauri. But he was not alone nor unsupported in this debate. According to the Associated Press, debating on his side was Dr Thomas Dobecki, a geophysicist from Houston. This fact James and Thorpe failed to bring to the attention of their readers.

Furthermore, one year prior to the publication of James and Thorpe’s book, geologist David Coxill—whose credentials were noted above—wrote a scientific paper which clearly supported Schoch’s rain erosion thesis. That paper, titled “The Riddle of the Sphinx,” appeared in the Spring 1998 issue of Inscriptio—Journal of Ancient Egypt, pages 13-19. Coxill did present a thorough analysis of, and rebuttal to, Harrell’s critique. But James and Thorpe’s readers have not been informed of this. Here, then, is Coxill’s reply to Harrell:

“Harrell (1994) suggests that wet sands from Nile flood waters, and occasional rainfall, would have produced wet sands, leading to these weathering features. That is not acceptable since floodwaters would have produced a wave cut bench and notch which would certainly be seen today in the surrounding excavation pit. This is not there, and again this theory does not satisfactorily explain the presence of erosion features higher up the Sphinx’s body and pit face. In Karst topography, cavities and natural fissures open up not by surface run off [as Harrell suggests], but by percolating ground waters. This would not explain the smoothness of the rolling laminars at the top of the Sphinx’ body and the surrounding excavated pit.”

If, as Harrell suggested, either annual Nile floods or rainstorms flowed into the Sphinx enclosure, they would mostly rise to a certain level from which they would then flow back into the Nile. But the level of the impounded water in the enclosure would have tended to remain at some particular level and then move upward by capillary action to evaporate and escape as water vapor. When water tends to stay at a particular level in a limestone pit it cuts into the walls of the pit at that level, creating a notch or wave cut bench. This does not exist on the Sphinx or

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39 Coxill, op.cit., p. 17
its enclosure wall, and Coxill’s geological argument regarding this, so far as I know, has not been answered by Harrell or anyone else.

The next problem outlined by Coxill is the erosion higher up on the Sphinx’s body and pit face. Water has to somehow move up a few yards to reach this area to do its erosional work. The head of the Sphinx is about 20 meters (66 feet) above the base of the surface. Capillary action in sand and gravel can only raise water a little. Harrell relies on capillary action to carry the water up about two meters [6 feet 6 inches] into the overlying sand. In the analysis of soil mechanics capillary action is generally theorized to move water upward by soil suction and is governed by particle size and the interparticle spaces. In a text on soil mechanics, G.N. Smith states “For sands and gravels above ground water level suction effects are negligible.”

So Harrell’s expert analysis defies the well-tested geological understanding of soil mechanics. He has water rising to such levels in sand by capillary action that an authority says cannot be done.

Furthermore, Schoch points out that “according to Harrell’s theory the Twenty-sixth Dynasty tombs cut into the back wall of the Sphinx enclosure [which therefore should also have been inundated or rain should have wet the sand lying against the walls of the tombs] should show a similar weathering profile to be seen on the Sphinx and enclosure wall. They do not, however. Harrell’s wet-sand theory simply does not work.” He also shows that Colin Reader, a geologist to be discussed below,

“... has astutely pointed out that Twenty-sixth Dynasty tombs (circa 600 B.C.) cut into the rock along the back or western wall of the Sphinx enclosure itself, have been subjected to the processes Gauri describes [i.e. inundation by Nile floods] for over 2500 years, yet they still exhibit clear chisel marks and lack the type of weathering and erosional profiles seen on the Sphinx enclosure.

“The conclusions are inescapable. I do not deny that salt crystal growth is indeed damaging the Sphinx and other structures at present but this process does not explain the ancient degradation patterns seen

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41 Schoch, Voyages of the Pyramid Builders, op.cit., p. 290
on the Sphinx’s body and in the Sphinx enclosure but appearing virtually nowhere else on the Giza Plateau.”

In the most basic terms, inside the same Sphinx enclosure at the back end are clearly observed chisel marks from the 26th Dynasty. They had to be covered with sand since they were inside the Sphinx enclosure for about 2500 years. If Harrell is correct and rainfall wet the sand inside the Sphinx enclosure to weather the Sphinx and enclosure walls, the chisel markings from the 26th Dynasty inside that same enclosure should at the very least have been weathered away. If Nile floods wet the sand as Gauri et al claim, the same fate would have befallen these same 26th Dynasty markings. Both Gauri et al. and Harrell have proposed wet sand as a process to weather the Sphinx and its enclosure walls, but one of the enclosure walls somehow refused to allow the wet sand to do what it had done on all the other walls, and the Sphinx as well. Yet historians, archaeologists, and Harrell, Gauri et al. all suggest their theory has merit and they have embraced these wet-sand concepts. This brings to mind a statement from the Talmud, “Against stupidity, the Lord himself is helpless.”

Once more we encounter the same problem that existed with Gauri’s theory. After the rain water sank into and saturated the sand in the enclosure, it would tend to settle toward the bottom, and evaporation or seepage would tend to remove the water. Where there was more water over longer periods of time, there would be greater weathering of the rock. Harrell’s theory also requires that the lower sections of the rock members exhibit greater weathering than the upper sections of the Sphinx and its enclosure, but as we already know, just the opposite is the case. This is a contradiction to Harrell’s concept, not an agreement with it.

Even if the sand was fully saturated from the top to the bottom, Harrell’s theory again fails. In this case, the weathering would be fairly uniform in each member from top to bottom. This again is simply not the case.

In addition to this, Schoch points to another major contradiction in both Harrell and Gauri’s theories, namely the development of gulleys or runnels at the top of the Sphinx and its enclosure:

“There is another major problem with the work of Harrell and, for that matter, Gauri. Neither exfoliation for wet sand can create the

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42 ibid., pp. 289-290
coved rock and vertical fissures, or runnels, in the rock that are wider at the top than at the bottom and are a prominent feature of the western end of the Sphinx enclosure. Runnels come from running water."⁴³

Exfoliation or the spalling off of sheets of rock from a surface creates a dome or rounded feature. It does not form runnels.⁴⁴ Wet sand invading rock pores to remove soluble minerals in the rock also does not form runnels wider at the top than at the bottom. Since the water remained for a longer period of time at the bottom of the sand, if it could create runnels, they would be wider at the bottom of the Sphinx and its enclosure than at the top. This is precisely the opposite of what is seen on the Sphinx and its enclosure. Harrell’s conception is in complete contradiction to the clear-cut observation of runnels being wider at the top of the Sphinx and its enclosure. Of course, none of these contradictions have ever been answered by Gauri or Harrell, nor have they ever pointed to a single other rock formation that is known to have been weathered by either exfoliation or wet sand that left runnels wider at the top than lower in the formation. The reason, I believe, is that no such examples to support their theory exist. But even if they did—which this author doubts—does it explain the many other problems?

Then Harrell completely ignored the wall enclosure. Why did his wet sand weather the Sphinx for five hundred years but then fail to continue to do the same more vigorously to the unprotected parts of the Sphinx and the wall enclosure for the next four thousand years as well as to 26th Dynasty tombs at the back of the Sphinx enclosure for 2500 years? Is there the slightest attempt by Harrell to explain these contradictions to what he has presented? Unfortunately, no. It seems fairly clear that Harrell’s mechanism will not erode the upper portions of the Sphinx strongly by the upward migration of water through capillary action, nor does it explain why the walls of the enclosure and unprotected Sphinx areas are not more deeply eroded by an additional four thousand years of the same weathering.

What then is left of the arguments and evidence to support either Gauri or Harrell’s concepts? Neither Gauri nor Harrell have answered West’s arguments. Neither Gauri nor Harrell have answered Coxill’s arguments and geological evidence. However, things being what they are, two new proponents for the establishment’s chronology have come forth to criticize Schoch. Ian Lawton and Chris Ogilvie-Herald, in Chapter 7, “The Age of the Sphinx,” in their provocatively titled book Giza: The Truth (London 1999), also had a go at Schoch.

⁴³ Schoch, Voyages of the Pyramid Builders, op.cit., p. 290
⁴⁴ Encylopedia Britannica, Micropedia, vol. IV, (Chicago 1982), p. 4
According to Lawton and Ogilvie-Herald, the suggestion is made that flash floods will generate the type of erosion seen at the Menkaure Valley Temple on the Giza Plateau. Lehner points out that Menkaure’s valley temple “lies at the mouth of the main wadi”. Thus any flash floods will flow into the main wadi, or dried-up river bed, and generate water erosion. They suggest this caused the erosion to Menkaure’s valley temple and elsewhere. Their argument unfortunately has very little to do with geological reality. First of all, one needs an extremely heavy rain shower to create sufficient run-off to fill wadis with water. Egypt, of course, is situated in a desert where this will rarely occur, though it does happen.

Secondly, erosion caused by flash floods is totally different from that seen on the Valley and Mortuary Temples or the Sphinx. Floods in deserts do most of their erosion by abrasion. They erode only to a minute depth by chemical weathering. To elucidate, it must be understood that desert floods do not last very long and the waters subside into the sand and dry up in an extremely short time. They erode mostly by carrying in their waters particles that cut into the rock far more extensively on the one side of the rock monument that obstructs the flow than on the other. If there were numerous floods in these wadis, we would find that the cutting into the rock would occur almost exclusively on the side facing the onrushing waters. This kind of erosion is quite distinct from anything found at the Sphinx or the Valley or Mortuary Temples.

Let us assume for a moment that this was how the Sphinx was eroded. A large rain shower fell on the Giza Plateau and flowed across it into the Sphinx enclosure as one might observe at a rapids in a river. The enclosure would fill with water, but the run-off would continue to flow after it had filled. The direction of the flow would be down-slope toward the Nile River. In that rapidly flowing water would be untold millions of tiny particles of sand and these would abrade that side of the Sphinx which obstructs the rushing waters. But this type of distinct erosion is not observed on the Temples or Sphinx. Their theory does not fit all the facts discussed above.

Such storms and floods do indeed occur in Egypt and the waters do sink into the sand and rock covering monuments. But do these waters generate the type of erosion observed on the Sphinx? For example, one heavy rainstorm in Egypt occurred and was reported in KMT:

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45 Lehner, *op.cit.*, p. 137 and pp. 128-129
“The huge rainstorms this past October and November did considerable damage in the Valley of the Kings filling in [tomb] KV 32 . . . demolishing tourist walkways and flooding portions of various . . . royal tombs.

“The fall ’95 rains did other damage in the Luxor area especially at the Mortuary Temple of Seti at Quran where part of the wall around it . . . was knocked down by the raging waters. . . . Several private tombs suffered water-intrusion . . .”46

Thus, no matter where these ancient tombs exist, where these floodwaters flow into them the limestone rock into which the tombs were cut would become soaked just as the theory of wet-sand for the Sphinx suggests. Since this happened intermittently but repeatedly over the past 5000 years, the limestone tombs should be weathered just like the Sphinx. The level of erosion in that limestone should be of some depth and be greater at the top than at the bottom.

In this respect we have ample proof that the wet-sand thesis, presented by Schoch and West’s critics, is contradicted by the evidence in the Valley of the Kings. Egyptologist Kent R. Weeks in The Lost Tomb presents the following:

“In the first half of the Eighteenth Dynasty . . . preference was given to sites at the base of the sheer cliffs that defined the Valley of the Kings. Ideally, these tombs were situated immediately below a ‘waterfall,’ a cliff face over which water from occasional torrential rains poured, carrying tons of debris cascading into the tomb entrance, burying it under meters [yards] of rubble. The pharaohs of the last half of the Eighteenth Dynasty and the Nineteenth . . . were buried at the base of the more gently sloping hills that lay within the valley. These sites were also subject to flooding.”47

Weeks goes on to describe the excavation work that Giovanni Belzoni carried out in 1817 in the Valley of the Kings, and this same type of flooding:

“Crawling through a small channel cut by his workmen in the [tomb entrance] debris, Belzoni found that a rainstorm two days earlier

had sent water cascading into the tomb and had damaged parts of the first few corridors. The cause was his own excavation of the entrance, which removed the blocking that formerly had acted as a dam. Belzoni’s . . . reference to water in KV 17 [Kings’ Valley Tomb 17] is one of the earliest records of flood damage to a tomb in the Valley of the Kings. But it was by no means the last. Since his time, damage from rainfall, as well as from unthinking visitors to the tomb—and from Belzoni himself, whose squeezes and casts badly damaged the painted walls . . .”\(^{48}\)

Therefore, we have clear evidence that water flowed into the sand and rock debris in the corridors cut into the limestone rock in the walls of the cliffs of the Valley of the Kings. This occurred every so often and wet the sand in the corridors that filled in the entrances. Since that is the case, we should find the very same erosion on the corridor walls as that of the Sphinx. But this is not the case. Again, the process offered by the critics of Schoch and West is not consistently doing in one place, where the same conditions exist, what it is required to do on the Sphinx under the same conditions. Their concepts are simply too contrived and thus can have no standing.

Did these floods also do the same damage to the temples which may have been in the path of the raging waters? Weeks goes on to show:

“Supposedly, heavy rainstorms and resulting flash floods hit the West Bank [of the Nile] only every few centuries. But several years ago I had checked the official meteorological data available for the last sixty years in Luxor, and had searched out scattered references in journals and diaries from the past 150 years and found that such storms actually came much more frequently in three- or four-year clusters. Every two or three decades there are periods of heavy rain and flooding. . . .

“In November 1994, for example, a torrential downpour hit at the north end of the Theban Necropolis . . . doing serious damage to the temple of Seti I. [mentioned above] . . . it had two special things going for it . . . The temple is probably the cleanest, most frequently swept monument on the West Bank . . . in 1994 the temple lay directly in the

\(^{48}\) ibid., pp. 65-68
path of a torrent of floodwater, so it offers a depressing but dramatic lesson on the disastrous effects of rain on Egyptian monuments.

“Late on the afternoon of November 4, heavy rains fell suddenly on the hillsides surrounding the valley. Within minutes, many centimeters of water had fallen . . . the water that poured out of the valley watershed met heavier runoff from other walls further north . . . of the Seti temple, these several streams met and created a wall of water that residents . . . said was two or even three meters [6.5 or 10 feet] deep. Boulders the size of buses were tossed about like toys. Moving at great speed, this deluge raced down the paved road, crossed a narrow stretch of barren land and poured directly into the enclosure of the Seti temple. . . . [Soft materials] simply turned to mash; mud-brick walls had vanished; stone architectural elements, some weighing hundreds of kilograms, were washed away.”

But did the Seti I temple exhibit the same type of erosion that is found on the Sphinx, Valley and Mortuary Temples at Giza? Here we have the very same process that Lawton and Ogilvie-Herald proposed for the Mortuary Temple. If their hypothesis is correct, the weathering profile of the Seti Temple should be identical with those at Giza. Yet this is not the case. Again the geological process they are proposing like all others is selective, only producing the weathering profile seen at Giza but not seen at the Seti Temple. In fact, all the geological processes offered by Schoch and West’s critics exhibit this high selectivity. Their proposals are simply not good science because of this. If the processes they offer as criticism were uniformly operating and each was clearly demonstrated on the various monuments which showed the same pattern of erosion seen at Giza, they would have a case. However, since this pattern of erosion seen at Giza is not seen elsewhere, they have no case.

Zahi Hawass, the Egyptian archaeologist, who is the director of research at the Giza Plateau, has also raised an issue. As Schoch explains:

“Hawass claims that the Sphinx was carved from limestone of such poor quality that it needed repair almost immediately. The Sphinx has indeed been repaired repeatedly . . .

49 ibid., pp. 116-117
“The earliest repairs to the Great Sphinx utilized limestone blocks that conform to the style followed by Old Kingdom masons. Hawass maintains that the repairs were done during the Old Kingdom, most likely soon after Khafre had the Sphinx carved from the limestone bedrock . . .

“This solution, however, presents Hawass with a serious problem. How could the Sphinx have weathered so fast that it needed repair almost immediately after construction? To begin with, the Sphinx is carved from what we geologists call competent limestone, one that stands up well enough to weathering to perform effectively as a building material. Also tombs adjacent to the Sphinx and cut from the same limestone during the Old Kingdom did not require immediate repair as did the Sphinx. How can it be that the same material weathered so differently at the same site?”

Again, as in every other case, the weathering and erosion was selective, affecting only the Sphinx and its enclosure but not the same material at the same site. Lehner had also accused Schoch of incompetence. He claimed “I don’t think he’s done his geological work yet . . . One of the primary pillars of his case is that if you compare the Sphinx to Old Kingdom tombs, they don’t show the same rain weathering. Therefore the Sphinx must be older. But he’s comparing layers in the Sphinx to other layers.” Lehner, like Hawass, claimed the Old Kingdom limestone tombs near the Sphinx were of a different layer of rock and harder. He provided not a single piece of data to support this, only his ex cathedra statement that it was true. However, West and Boris Said on a BBC program explained that they had hired an independent expert to investigate this point. “Their export looked closely at a tomb only a hundred yards from the Sphinx, and known to date from the same time as the pyramids. He found that the tomb was made of the very same flaky limestone as the Sphinx, and contained exactly the same types of fossil. The tomb layer was the same layer that the Sphinx was carved from.”

Schoch and West’s critics have failed to see these clear-cut contradictions in fundamental understanding. Every time they invoke the morning dew process as the weathering agent of the stone blocks of the Valley Temple that was not buried in sand, they must ignore all the other limestone monuments that

50 Schoch, Voyages of the Pyramid Builders, op.cit., pp. 294-295
51 Colin Wilson, From Atlantis to the Sphinx (NY 1996), p. 50
52 ibid.
should be weathered to about the same degree. Every time they invoke torrential rains or floods to wet the sand in order to weather the Sphinx, they must ignore all the other buried limestone materials such as the corridors in tombs of the Valley of the Kings that were inundated by raging floods that wet the sand and that should also be weathered to a similar degree. They cannot scientifically cause these geological processes to only work selectively where and when they ask them to. Every time they invoke torrential floods to weather the Mortuary Temple at Giza, they must ignore the Seti I mortuary temple where the same torrential floods have failed to produce the same form of weathering profile. And again, this author repeats, they cannot scientifically cause these geological processes to work only selectively where they ask them to do so but fail to operate under the same conditions when they cannot find the same or a similar form of weathering given these identical conditions. They cannot have it both ways. They cannot rationally make their geological processes work only on the Sphinx and Valley Temple or Mortuary Temple but then have these very same processes fail to operate in the very same manner on limestone monuments or walls either free-standing or buried in sand. Geological processes always operate in the same manner. The geological processes offered by Gauri et al., Harrell, Lawton and Ogilvie-Herald are unscientific because they are not consistently doing to other limestone materials what these geologists insist they must do to the Sphinx, Valley Temple and Mortuary Temple.

Schoch has prepared a full response to Lawton and Ogilvie-Herald’s criticism which appears at his website.53

Interestingly, there is another geologist, Colin Reader, who has also concluded that both the theories of Gauri, et al., as well as Harrell, are incorrect. Reader holds an honors degree in Geological Engineering from London University. In this discipline, linking civil engineering with geology, he has much experience in the study of the historic development of sites based on field work and desk research. Reader has had a long-standing interest in ancient Egypt with a specific interest in the Giza Plateau which he has often visited and researched at length. Colin D. Reader, in an article in Archaeometry for February 2001, has also come to the conclusion that there are

“a number of features of weathering and erosion within the enclosure surrounding the Great Sphinx of Giza that suggest the action

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53 robertschoch@homestead.com; see also his book, Voyages of the Pyramid Builders, written with Robert Acquinas McNally, “Appendix,” pp. 278-298
of flowing water. That this erosion is not uniformly distributed is consistent not with erosion by rainfall *per se* but by rainfall run-off.”

In this case Reader says flood waters were responsible for the erosion. He is also of the opinion that the various other processes proposed by Gauri *et al.*, suggest “that these processes, although significant, are unable to account for all of the features of degradation that are present within the Sphinx enclosure.” Again the problem with the flooding thesis is that other structures like Seti I’s temple that have been subject to these massive floods over the past two to three millennia do not exhibit this form of erosion, nor do the corridors into the tombs in the Valley of the Kings exhibit the same erosion, though subject to innumerable floods.

Here are his conclusions based on his own detailed geological research from his original paper exhibited on Schoch’s website:

“Harrell attributed the degradation of the Sphinx and the adjacent exposures to the so-called ‘wet-sand’ hypothesis—in which the exposed limestone, buried in accumulations of wet sand, has been subject to chemical weathering. The processes Harrell described to promote the wetting of accumulated sand within the Sphinx enclosure, however, do not stand up to detailed scrutiny and, on this basis, the wet-sand hypothesis is regarded as largely untenable.

“The paper by Gauri *et al.* provides a summary of many of the features of degradation that are present along the body of the Sphinx and adjacent enclosures, attributing these features to the combined effects of subsurface groundwater movement and chemical weathering. I consider, however, that these processes, although significant, are unable to account for all the features of degradation that are present within the Sphinx enclosure.”

The interesting aspect of this highly technical geological paper is that he proposes that only 200 years of rainfall is responsible for the erosion on the Sphinx and Valley Temple. I suggest that about 600 to 800 years created this

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55 *ibid.*
56 robertschoch@homestead.com
weathering pattern. To save the establishment chronology, his thesis still demands rainfall and that the Sphinx, which historians claim was constructed after Khufu’s time, was actually built prior to that time. 200 years for this erosion is hardly believable. If 200 years of rainfall created the erosion on the Sphinx and Valley Temple, it had to do the same to the same extent to all the monuments built before them. Reader’s solution also suffers from selectivity.

The work of Schoch, West, Coxill, and Reader has addressed all the criticisms of Gauri et al., Harrell, Lehner, Lawton and Ogilvie-Herald. But interestingly, the critics of the rain-erosion hypothesis have failed to answer the points of evidence raised by the proponents of the rainfall hypothesis as outlined above. Let us see what they are.

1. They have failed to explain how the Nile River and ground-water level could have been 30 to 50 feet lower in the time of the Sphinx and rise via floods to inundate the Sphinx and its enclosure and higher up on the plateau.

2. They have failed to explain why, if the Nile River was capable of rising to such great levels at Giza, under similar climatic conditions as today, its floods presently fail to rise above the Sphinx today.

3. They have failed to explain why the enclosure wall around the Sphinx, though not protected by a layer of rock as are the damaged areas on the Sphinx, is eroded to the same degree as the Sphinx.

4. They have failed to explain why, after the first repair was made on the Sphinx, subsequent water damage stopped eroding the unprotected areas on its body over the past 4000 years, as well as the enclosure wall. They have failed to explain how it is possible that the 26th Dynasty tombs cut into the back wall, though subject to exfoliation or water damage for the next 2400 years exhibit no runnels or damage to any significant degree though they are in the same place as the Sphinx.

5. They have failed to explain why water seeping up from below in the Sphinx enclosure from Nile inundations or rainfall and evaporation from above, allowing the water to remain for a longer period of time nearer the bottom layers of the Sphinx, eroded the top layers more than the bottom areas, or how these processes would cut runnels wider at the top than at the bottom.
6. They have failed to explain why the process they invoke—wet-sand—has not generated the same weathering profile in the various corridors of the Valley of the Kings where the same process is operating as it has on the Sphinx and its enclosure walls or on Seti I’s Temple.

7. They have failed to explain how the inundation of the Nile into the Sphinx enclosure could remain there and yet not generate a wave cut notch on the Sphinx and its enclosure.

8. They have failed to explain why the morning dew only flaked away the limestone on the Valley Temple’s standing stones but failed to produce the same deep weathering profile in all the other limestone monuments, standing in the open and subject to the same morning dew, and why it did so more at the top, creating runnels.

9. They have failed to address Coxill’s criticism presented in *Inscription*—*Journal of Ancient Egypt*.

10. They have failed to answer the problem Reader presented that capillary action will not raise water above the level in the Sphinx enclosure to a height where it can effectively weather the Sphinx.

Schoch’s comments on the scientific issues are quite to the point:

“The arguments advanced to support the 2500 B.C. dating of the Sphinx and attribute it to Khafre are *ad hoc* ideas invented to protect an established chronology. It is as if the Egyptologists scratch their heads and say ‘But we know the date’s got to be 2500 B.C. Surely Schoch is wrong. He simply has to be.’ Unwittingly, they are caught in the trap of defending their own dogma rather than examining the evidence with the dispassion it deserves.

“. . . By and large Egyptologists don’t use this kind of scientific evidence; they rely instead on a mix of methods that includes historiography, archeology, philology, and literary analysis. Geological analysis is an alien form of thinking.”\(^57\)

\(^57\) Robert Schoch, *Voices of the Rocks* (NY 1999), p. 50
One measure of all good science is based on testing. If a geological process is proposed to generate a particular form of erosion it is required to operate as the theory demands. But the various processes presented by Schoch’s critics when tested in the same conditions on the same materials do not produce the same or quite similar results. In no uncertain terms their theories fail the very tests that exist to determine their validity. In simple terms their theories are bad science.

And that is the basic point of good scientific procedure. Testing a theory is fundamental to good science. The tests offered by other limestone monuments and natural formations in Egypt refute the geological theories offered by Schoch and West’s critics. When a theory is replicated it produces results that support the theory, but in all cases the replication of these various processes fails to exhibit the necessary support that science demands. In fact, the tests that were to replicate Gauri et al., Harrell, as well as Lawton and Ogilvie-Herald’s theories all contradict their theories. When tests via replication contradict a theory, that theory is invalidated. In essence, science has spoken rather definitively and demonstrated quite clearly that the theories proposed to deny the rainfall theory are falsified and, therefore, are unscientific. The methodology of science undeniably proves none of these theories are correct. The methodology of science undeniably proves all these theories are wrong.

On the other hand, Schoch’s rainfall theory is supported by this scientific testing, replication methodology. When he stated that the limestone of the Sphinx, its enclosure walls, the blocks of the Valley Temple and Mortuary Temple exhibit a classic example of rainfall erosion, he was claiming that this form of erosion can be observed on other limestone formations where rainfall is known to have replicated this process. That, too, is undeniably correct. His theory fits all the facts and evidence and is validated by all the facts and evidence. Not one of his critics has ever shown that rainfall does not erode rock in the manner Schoch presented. They have instead invented theories that are disproven by testing and replication. Until they present a theory that explains and answers all the problems raised here, they have nothing to support their established chronology of ancient Egypt. In essence, all that they have attempted to do with their *ad hoc* theories is uphold, in the face of the evidence, Manethon’s chronological framework. The erosion evidence does not in any way support—scientifically support—that framework.

It seems rather evident that the Sphinx and the Valley Temple blocks were constructed at a time in Egyptian history when rainfall was abundant. If the
First through Sixth Dynasties ever existed, they did not build all the limestone monuments attributed to them from between 3000 to 2000 B.C. because if they had, these monuments would exhibit the same weathering profile as shown on the Sphinx and Valley Temple. In terms of the shortened chronologies of Heinsohn, Rose, and Sweeney, this is only to be expected. The evidence suggests that high Egyptian civilization does not begin as far back as has been assumed. It further suggests that the hipsithermal rainy epoch was one of high rainfall as outlined in this author’s book, *The Extinction of the Mammoth*, and as delineated in that book, the rainfall lasted to around 1500 B.C. This would allow 600 to 800 years of erosion to deteriorate the Sphinx, its enclosure walls and the Valley Temple. Some time after this weathering damage stopped because the pluvial era had ended, Egypt began to build in earnest the great monumental civilization that we see today.

In this regard there is an almost insurmountable problem confronting Schoch and West, and all others who advocate ages for the construction of the Sphinx of from 5000 to 9500 B.C. After promulgating such distant ages they must still answer the question: What civilization was responsible for these water-eroded monuments? Although this author disagrees with the tone and approach of James and Thorpe’s criticism, their comments on this question do go directly to the heart of the matter:

“A huge gulf [exists] between . . . [the] Sphinx builders . . . and the Egyptians, whose civilization appeared around 3400 B.C. By the eleventh millennium there were settlers in the Nile Valley, hunters and gatherers who used stone tools, and by the late sixth millennium simple farming communities were flourishing. So there were Egyptians. . . . Yet before about 3400 B.C., shortly before the First Dynasty, there were no towns, temples, pyramids, obelisks, statues, inscriptions, or indeed any of the other things that we think of as characterizing Egyptian civilization. So if the Egyptians learned their arts and sciences from . . . [these earlier people], how did they remember them over all these thousands of years?

“John Anthony West . . . admitted that this is a serious difficulty, to say the least:

“‘The big problem with all this, from my point of view, is the transmission process: how exactly the knowledge does get handed on during the thousands and thousands of years between the construction
of the Sphinx and the flowering of dynastic Egypt. Theoretically you’re sort of stuck—aren’t you?—with this vast period in which the knowledge has to be transmitted. This is not easy to slough off.’

“Quite. All West could come up with is the weak suggestion that the ‘knowledge’ was transmitted orally, passed down through hundreds of generations. This might work well for religious or esoteric information. One could even imagine mathematical or astronomical knowledge being handed down this way. But how on Earth could knowledge—‘secret’ or otherwise—about highly practical matters such as working and moving large stones be preserved without people actually building things? And if they already possessed the skills to construct pyramids and other monuments in the eleventh millennium B.C., why did the Egyptians wait so long before they chose to benefit from such knowledge? Why indeed do the earliest pyramids of Egypt reveal trial-and-error experimentation showing that their builders were not working from an existing blueprint but clearly working things out as they went along? . . .

“In short . . . [West’s] explanation of the origin of ancient civilizations doesn’t do much explaining. All [t]his theory really does is set up a pattern of problems, composed of vast gaps between the imagined and the real beginnings of ancient societies.”

This is the general problem raised by the historians:

“According to the Associated Press, the heated debate . . . [about the age of the Sphinx, held in 1992] [that] spilled over into a confrontation in the hallway has not focused on the scientific merits of the meteorological findings, but as Mark Lehner expressed it, on whether it is permissible to ‘overthrow Egyptian history based on one phenomenon, like the weathering profile.’ The final argument by the debunkers was the absence of evidence that a civilization advanced enough to carve the Great Sphinx existed in Egypt between 7000 and 5000 B.C. ‘The people during that age were hunters and gatherers; they could not build cities,’ Dr Lehner said; and with that the debate ended.”

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58 James and Thorpe, op.cit., pp. 69-71
59 Sitchin, op.cit., p. 257
The question is not about building cities but erecting a large monument—the Sphinx. This is quite similar to the ancient people of England that built Stonehenge.

If there were people with tools such as copper to cut limestone who lived in vast farming communities in and around the Nile from 8000 to 5000 B.C., they would certainly have left stratigraphic evidence in the ground. Archaeologists suggest that we only begin to find evidence in the ground around Egypt of early civilization in and around 3000 B.C. But were all these great stone monuments historically placed between 3000 and 1500 B.C. erected then? If the Sphinx was built around 2000 B.C., we do not have quite as great a gap to fill in. Let us remember that the farming communities in England erected Stonehenge with enormous blocks of stone but did not erect earlier temples, palaces, cities, or other giant monuments of stone. They lived in a highly forested environment and employed wood to build their non-religious monuments, such as houses, etc. The question is: were the Egyptians building large monuments of wood prior to turning to stone when the climate changed and wood was not as available as before? According to Leonard Cottrell in discussing a Third Dynasty site near the Step Pyramid,

“so far as is known—[the Step Pyramid is] the oldest pyramid in Egypt and the first stone monument to be erected anywhere in the world.

“Much more interesting was the fact that, when archaeologists began to clear and examine the structures around the pyramid—the courtyard with its ‘dummy’ buildings, the colonnaded entrance, and the enclosing wall—it was realized that their design and construction was not characteristic of stone mason’s work. The stone blocks were needlessly small, hardly bigger than bricks. Some of the columns, instead of being free-standing, were ‘engaged,’ that is, attached to the wall behind. The stone-enclosing wall was built in a series of recessed panels with projecting bastions, highly pleasing to the eye, but it is doubtful if the effect was merely intended as decoration. It was far more likely that these structures reproduced in stone the mud-brick and timber buildings of the kings’ early palace...[which] had disappeared.

“Proof of this came when it was noticed that at the entrance to the colonnade through which the courtyard was approached stood a
reproduction, in stone, of a wooden door, complete with bolt. The ‘door’ was quite useless. It was solid, fixed to the ground. Like the buildings within the courtyard, which were partly of solid masonry and unusable as rooms for the living, it was a dummy. Next the archaeologists noticed that the roof which had been supported by the columns was not of flat stones, as would have been expected, but blocks of semicircular sections, like palmlogs. The flutings of the columns themselves, charming though they are in artistic effect, were probably reproductions of the bundles of reeds which the primitive pre-dynastic Egyptians used to support the roofs of their dwellings.

“Here, at Sakkara, for all to see, was a reproduction in stone of the mud-brick and timber buildings in which the Pharaohs had lived in the Archaic or Early Dynastic period, and doubtless they incorporated [wooden and mud-brick] features which had been in use in predynastic times.”

Related to this is that at the time of even the Third Dynasty, and earlier, Egypt was supposed to be a desert. J.E. Manclip White discusses the problem of wood for buildings in predynastic and early Egyptian history:

“The Egyptian woodworker must have acquired a marvelous dexterity in the use of wood at a time when the supply of timber was still for some reason plentiful. [Thereafter A]ll the traditional forms of wood or mud brick were faithfully reproduced in stone.

Therefore, the early Egyptians were building their monuments, buildings etc. out of timber and mud brick. Over time, much of the mud bricks was either washed away or leveled to build other structures over them. The wood was either reused in other buildings or burned to cook food. Hence, we do not need thousands of years to sustain information regarding stone cutting techniques when hundreds will do. Since wood was plentiful, the Egyptians built with the easiest materials available to them and only constructed their major religious monuments out of the most difficult to work material—limestone—which their copper tools could cut in the rarest instances. If the people of Stonehenge could build great monuments of stone, that does not require that they continue to do so when plentiful supplies of wood were nearby. The same applies to Egypt. It was only after the

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60 Leonard Cottrell, Digs and Diggers (Cleveland OH 1964), pp. 87-88
61 J.E. Manclip White, Ancient Egypt, Its Culture and History (NY 1970), pp. 68-69
climate had changed—which this author dates around 1500 B.C.—that the Egyptians would have had to turn to stone and mud-brick as their primary source materials of construction. Early peoples tended to employ primarily the most abundant materials available and those that are most easily manipulated. They do not build with the most difficult materials unless the former are no longer present in sufficient amounts to meet their needs. More advanced societies, of course, have no such restrictions upon them. They may build from a wide variety of building materials. But less advanced societies are largely controlled by the element of supply or availability and the cost or amount of work required to process the materials.

Schoch has presented other evidence and answers to the issues raised by his critics on his Internet website listed above.

This author has only dealt with the issues related to the ideas under discussion here. Let us now recall that Mark Lehner has asked: “Is the evidence of rainfall erosion on the Sphinx sufficient to overturn Egyptian chronology?” The answer, of course, is “no”, but if there are several other scientific forms of evidence that converge on the thesis that the established chronology is invalid, then that evidence should be sufficient to overturn the established chronology. Therefore, let us now turn to the foundation of astronomy, or Sothic dating, upon which Egyptologists have established their chronology. As we will see, Sothic dating is even more damaging to their chronology than the erosion of the Sphinx.
CHAPTER 3
ASTRONOMICAL SOTHIC DATING

“You can’t ask us to take sides against arithmetic. You can’t ask us to take sides against the obvious facts. . .”

Sir Winston Churchill in

“If Manethon’s list proves to be unreliable, and if conventional Sothic dating turns out to be a chimera, Egyptian chronology will need to be revised and reconstructed upon more secure foundations. And once that is done, the chronologies of all those neighboring peoples will also have to be revised and reconstructed. Move the Egyptians, and you must move their neighbors.”

Lynn E. Rose
_Sun, Moon, and Sothis_
*op.cit.*, p. XXIII

Once Manethon’s kings’ list had been accepted and modified according to the evidence from several areas of historical research, there still remained a basic problem. How could this series of kings be firmly established on scientific grounds so that the dates of the dynasties, pharaohs _etc._, accurately reflect the times of their rule? After all, many reigns that Manethon offered gave no indication of how long the various pharaohs ruled. What was needed was a way to establish an absolute date so that the chronology would have an anchor with which to fix the entire series of dynasties and their pharaohs. The solution to this problem lay in astronomy, as Ceram states:
“But the unique boon which Egypt offered the historians of ancient times was the Egyptian calendar.

“This calendar was priceless because it was relatively easy to understand and almost identical with the Julian calendar. . . . By means of this calendar the scholars arrived at their first fixed points in the ancient history of the Near East.”

In order to fix the history of Egypt, astronomy seemed to hold the key for the historians. They based their analysis on the rising of the star Sirius; one of the Egyptian names for Sirius was Sopdet, which was translated into Greek as Sothis. According to a Roman author, Censorinus:

“The Egyptians, in forming their great year, do not take the moon into consideration; the Greeks call it cynic, the Latins call it canicularis, because it begins at the rising of the Dog Star [Sirius, Sothis] on the first day of the month which is called by the Egyptians Thoth. . . . Also the span of four of their years is shorter than the span of four natural years by, approximately, one day; this reestablishes correspondences on the one thousand four hundred and sixty-first [Egyptian] year. This [great] year is also called by some the heliacal year, and by others, ‘the year of the God’.”

That means the Egyptians had a “great year” which was about 1461 Egyptian years long, based on the rising of Sirius just before dawn on the first day of the first month, Thoth, of the ancient Egyptian year. Thoth 1 Egyptian occurred on July 19 in the year +139 which marked the end of the “great year” of Censorinus. Velikovsky explains how this “great year” is computed:

“At first the star comes up only a little over the horizon before the rising sun blots out its light and that of other stars. Each succeeding night it rises a few minutes earlier and mounts higher in the sky before daybreak. The heliacal rising of a star is on the morning it is seen for the first time preceding the rising sun [Helios, ergo the term ‘heliacal rising’] . . .

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1 Ceram, *The Secret of the Hittites*, op. cit., pp. 143-144
“On a calendar that has only 365 days to the year and is short one day every four years [as was the ancient Egyptian calendar], the heliacal rising of any star, including Sirius, would occur one day later every four years.

“As explained by Censorinus, the ‘great year’ begins with the year when the heliacal (morning) rising of Sirius is on the first of the month Thot. After four years it would rise on the second of Thot [for four years, and in the eighth year on the third of Thoth and so on]. After 1461 years of 365 days, or 1460 years of 365 1/4 days, Sirius would again rise heliacally on the first of Thoth. This span of time comprises one Sothic period [or ‘great year’]. This means that the first of Thoth, or the [first day of the] New Year, and all the days of the year move through the four seasons during the Sothic period. Sirius rises heliacally each summer but on the first of Thoth only once (for four consecutive years) in 1460 years.”

This being the case, any mention of Sirius’ heliacal rising presented in the ancient documents of Egypt could be examined to see when a particular pharaoh of a particular dynasty might have been connected with such an event. And it was upon this kind of evidence that the Egyptologists claimed to have forged the link between Manethon’s list and an absolute date for their chronology. The Egyptologists’ foundation stone of chronology is astronomical data; this is the science upon which their history is built. Thus when they discovered documents that mentioned Sirius rising on a particular date in the reign of a pharaoh, they could calculate the time when Sirius did rise on that date, in terms of where that date fit within the “great year,” and that pharaoh would then be anchored to that date by astronomical science. The science and logic was impeccable and the historians believed that they had achieved their quest for a fixed date tying a pharaoh in Egypt to a precise year in the past.

Before proceeding, however, it must be understood that the “great year” or Sothic period is not exactly 1460 Julian years. As Rose points out, “According to [M.F.] Ingham’s calculations, the Sothic period ending in +139 had lasted 1452 years [not 1460 years]. Thus it would have begun in –1313, rather than in –1321.”

The length of this particular Sothic period would have been shorter by some 8 years. The Sothic period therefore could not have been derived by the simple calculation

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3 ibid., pp. 217-218
4 Rose, *Sun, Moon, and Sothis*, op. cit., p. 91
of 365 x 4 = 1460 years. The astronomical reason for this is the changing declination of Sirius.

If the Egyptians practiced Sothic dating in terms of the “great year” prior to about 200 B.C., all documents that mention the rising of this star NEVER mention the 1460 year cycle or the Sothic cycle of the “great year.” The Egyptians, if they actually employed Sothic dating as the historians claim, had to wait for Sirius to rise heliacally on the first day of Thoth, and then proclaim the onset of the next cycle. We have no documented evidence that they practiced this by calculation or otherwise in pharaonic times.

Let us then examine how historians established a fixed, absolute astronomical date for one of their pharaohs based on this cycle to support their chronological superstructure. Before doing so, it is important to recognize that some authorities in this field have raised serious doubts about the propriety of using this method in the deep past, and also about its accuracy. According to James:

“The key Sothic date for the Middle Kingdom is provided by two papyrus fragments found at el-Lahun, dated to year 7 of an unnamed pharaoh, but reasonably attributed to Senusret III [Sesostris III] on paleographic grounds [studying the particular form of writing used at a certain period or even of a particular scribe whose handwriting can be recognized the way modern handwriting is recognized today]. This document does not give the beginning of a Sothic cycle, but a calendar date for the rising of Sirius, which can be retrocalculated to 1872 BC if the sighting of Sirius was made in the Memphis-Lahun region. If, however, the sighting was made at the lower latitude of Elephantine, as Rolf Krauss has recently advocated, the date would be reduced to 1830 BC. For the early New Kingdom a Sothic date is provided by the Ebers papyrus for year 9 of Amenhotep I. If the observation were made at Thebes, where the document was found, the date produced would be 1517 BC; Krauss favours Elephantine, which would lower the date to 1506 BC.”

We have a document, one of the El-Lahun Papyri, dating Sesostris III of the 12th Dynasty to 1872 B.C., if the measurement of Sirius was made at Memphis, or 1830 B.C. if the measurement of Sirius was made at Elephantine. The 18th Dynasty is based on the Ebers Papyrus which, if the measurement of Sirius was

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5 James, et al., op. cit., pp. 226-227]
made at Thebes, dates Amenhotep I to 1517, but if taken at Elephantine, lowers his date to 1506 B.C. Where one takes the measurements of Sirius does influence the precise date of these particular pharaohs; for 12th Dynasty Sesostris III by 42 years and for 18th Dynasty Amenhotep I by 11 years. Nevertheless, over the last two decades Elephantine has been the site favored for making these measurements. James adds:

"Prima facie, the theory of Sothic dating may look watertight. Closer examination, however, reveals a web of interlocking assumptions, each of which requires intensive re-examination. Work is still needed to confirm the original astronomical and calendrical calculations (made at the turn of this century [mainly by the German historian, Eduard Meyer, in *Aegyptische Chronologie* or *Egyptian Chronology* in 1904]) with the aid of contemporary techniques. . . . Archie Roy, Professor of Astronomy at Glasgow University, has described the ‘meagre grounds’ on which ‘astronomy was brought in to fix the absolute dating of the pharaoh list of Egypt’, concluding that ‘the classical astronomical chronology rests on very weak ground and that there is a need for very much more careful examination of texts referring to calendars, festivals . . .’ etc."  

In essence, before one may truly accept these fixed Sothic dates, they must, according to Roy, be established by corroborating evidence such as festivals related to astronomical dates. These we will investigate below.

The dating of Sesostris III in the El-Lahun Papyri is outlined by Clayton:

"The occurrence of a heliacal rising of Sirius is recorded in the 7th year of the reign of Senusret III [Sesostris III] (1878-1841 BC) of the 12th Dynasty. The event is dated to the 16th day of the 4th month of the second season in the 7th year of the king (there were only three seasons, not four, in ancient Egypt: inundation [of the Nile], sowing and harvest . . .). By calculating from the ‘coincidences’ [of the Sothic cycles] . . . this rising can be fixed at 1872 BC."  

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6 *ibid.*, p. 227  
This date in terms of year, month and date is, however, not precise. There is a small period of 4 years to which this date may be assigned, as we will see. This date and the others related to the 18th Dynasty then became the firm anchor points upon which the historians organized their chronology. The dynasties that preceded the 12th Dynasty were given slots back as far as the evidence seemed to indicate. The dynasties between the 12th and 18th Dynasties were provided slots that seemed to fit the evidence, and those dynasties that followed the 18th Dynasty were slotted into the framework. Therefore, the Egyptologists felt that they possessed a scientific foundation onto which the history of Egypt fit reasonably, based on all the evidence. And it appeared to be impossible that after 150 or so years of digging, translating documents, and piecing together all the various materials at their disposal, there could be the slightest chance that the edifice of their achievement was in any way in error. Thus Gardiner, in *Egypt of the Pharaohs*, page 148, claimed that,

“To abandon 1786 B.C. as the year when Dyn. XII ended [based on the date for Sesostris III] would be to cast adrift from our only firm anchor, a course that would have serious consequences for the history, not of Egypt alone, but of the entire Middle East.”

But let us recall that in spite of all the weight placed on this dating system, Archie Roy felt that the grounds for basing the chronology of Egypt on Sothic dating were “meagre” and that the “classical chronology rests on very weak ground and that there is need for very much more careful examination of texts referring to calendars and festivals.” What was this other data that was needed to corroborate this 12th Dynasty placement?

There was additional astronomical information in the El-Lahun Papyri, related to 12th Dynasty pharaohs, listing lunar festivals. What Roy was arguing was that this data also had to fit with the Sothic date of Sesostris III. That is, if Sesostris III’s 7th year was truly and correctly placed by Sothic dating, in 1872 B.C., then the Moon festival dates of the 12th Dynasty pharaohs should also fit this date and therefore corroborate it.

William Edgerton in “Chronology of the Twelfth Dynasty”, *Journal of Near Eastern Studies*, I (1942), in attempting to fit Sesostris III as precisely as possible via Sothic dating, could only generate a fit at 1870 plus or minus 6 years.

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8 quoted in Rose, *op. cit.*, p. 209
Christine Hobson reiterates this, claiming that for this pharaoh there is a “deviation of + or – [plus or minus] 1 to 5 days, depending on where in Egypt the observation was made, allowing for the curvature of the earth between Alexandria and Aswan.”

Edgerton realized that the lunar festival data in the El-Lahun Papyri would insure a proper scientific-astronomical dating of the 12th Dynasty. With this accomplished, there could be little doubt or really no doubt that ancient Egyptian chronology was absolutely dated and absolutely correct. Because the exact year that corresponded to the 7th year, 4th month, second season, 16th day was not precisely known, Edgerton called upon his colleagues to see if they could make all the lunar data fit to establish the validity of the 12th Dynasty’s placement in conventional Egyptian chronology. Edgerton’s test or challenge to astronomically pinpoint Sesostris III and the rest of the 12th Dynasty via all the El-Lahun data was quoted by Rose on page 210 of *Sun, Moon, and Sothis*:

“Several Twelfth Dynasty papyri apparently refer to lunar months in connection with regnal years, months, and days. Future research may perhaps prove that some one of these texts belongs to a particular reign; and if the reign proves to be that of Sesostris III such evidence in combination with the Sothic date may enable us to equate the sixteenth day of the eighth month in his seventh year with a particular day in a particular Julian year B.C.”

This approach to testing the information in the El-Lahun Papyrus was taken up by some of the world’s leading authorities in archaeoastronomy and calendrics, namely Ludwig Borchardt, Richard Parker, Rolf Krauss, and Ulrich Luft. Although each attempted to make the lunar festival dates fit the reign of other 12th Dynasty pharaohs for which this information was given, in terms of year, month, and day of their reigns, each could not make an adequate fit in the time allotted to that pharaoh. Rose, in his paper “The Astronomical Evidence for Dating the End of the Middle Kingdom of Ancient Egypt to the Early Second Millennium: A Reassessment,” in the *Journal of Near Eastern Studies*, vol. LIII, no. 4 (October 1994), pages 237-261, and in his book, *Sun, Moon, and Sothis*, pages 236-254, showed that their attempts were inadequate:

“Borchardt, Parker, Krauss and Luft never saw anything even close to a good fit [for the lunar festival dates]. Their minds were firmly

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anchored in the early second millennium, where the pickings were quite slim. They had to settle for the ‘best’ of a very bad lot.”

As Claude Bernard explained,

“Men who have excessive faith in their theories or ideas are not only ill-prepared for making discoveries; they also make poor observations.”

Because Borchardt, Parker, Krauss, and Luft had excessive faith in the chronology formulated by Manethon and clothed by a century or more of evidence by Egyptologists, they were quite certain that the 12th Dynasty had to be situated in the early part of the second millennium B.C. But all their efforts could not sustain this; the astronomical corroboration was not there; the challenge had not been met.

Although lunar cycles repeat themselves and the yearly calendar repeats itself every 365 1/4 days, the two are not in resonance with one another. This makes it difficult to corroborate the Sothic date for Sesostris III around 1872 B.C., or any other timeframe. The orbit of the Earth around the Sun and that of the Moon around the Earth, and the year, month, and day of a 12th Dynasty king’s lunar festival, should all be in sync if the 12th Dynasty had been properly placed in time.

Rose, who lacked faith in the Manethonian chronology, took the path of moving Sesostris III’s reign and the entire 12th Dynasty out of the time slot given them in the conventional chronology; he made the breakthrough and found the corroboration that confirmed that breakthrough.

What Rose achieved is, in a sense, comparable to what Isaac Newton did for the Copernican-Aristarchian system. All the known data of planetary and lunar movement as well as the telescopic findings could not be integrated scientifically to fit together. What Newton did was to show how and why the data fit the Copernican-Aristarchian heliocentric universe and could not fit the older, Aristotelian, geocentric universe.

What Rose did was move the 12th Dynasty forward in time by about one Sothic period of 1477 years, because this would also fit a heliacal rising of Sirius,

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11 Rose, *op. cit.*, p. 254
as he explains in Chapter 19, and then he tested this placement against the Sothic data for Sesostris III and the lunar festival data for three pharaohs of the 12th Dynasty. The fit was astonishingly good. Out of 36 lunar festival data points for the documented material of the pharaohs in question, 34 were direct hits. Of the other two, one could be understood as being a miss because rainy or cloudy weather obscured the night sky and the onset of the month, when the weather did not permit the Egyptians to ascertain exactly where the Moon was on that day of the month. The second miss was amenable to how a scribe had interpreted the beginning or end of a month. Rose explains:

“Thus there are 34 hits out of 36. One of the two misses . . . is readily explainable in terms of bad seeing [bad weather]. The other one . . . is more complicated, but it, too, is amenable to some kind of understanding. I suspect that the original from which [this date] . . . was copied said II 3ht 19 and used Possibility II [the interpretation meaning] (‘down to [this date] and not including [it]’) and that the scribe of [this date] . . . read that source with [the interpretation of] Possibility I (‘down to [this date] and including [it]’) in mind; that gave him the appearance of a 31-day month [instead of a 30-day month], which he carefully “corrected” by emending II 3ht 19 to II 3ht 20 . . .”

The emendation made by Rose is an emendation that others in the field had employed. In any case, Rose had found 34 lunar festival dates that occurred at exactly the years, months and days of the 12th Dynasty pharaohs noted in the El-Lahun documents that corroborated the 7th year, 8th month, 16th day of Sesostris’ Sothic date precisely. The chance of this number of correct fits occurring was immensely small. 34 hits out of 36 yields an accuracy of 0.944. Rose shows:

“The scores for the four principal ‘solutions’ are as follows: Borchardt hits 7 of 14 [or 0.50]; Parker hits 8 of 14 [or 0.57]; Kraus hits 8 of 19 [or 0.421]; and Luft hits 21 of 36 [or 0.583]. The average of these four ‘batting averages’ is only 0.519. Good in baseball, no better than random in historical astronomy.”

Rose’s 0.944 score is not a random outcome from the astronomical facts while those of Borchardt, Parker, Krauss, and Luft are. The probability of finding a

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14 ibid., p. 707
Sothic date for Sesostris III anywhere in the established Egyptian chronology and having the Moon’s position related to this date occurring randomly around 50 percent of the time is to be expected. The probability of finding a Sothic date for Sesostris III, as Rose did, and having the Moon’s position related to this date fitting it 94 percent of the time is clearly not random. It is just the opposite: It is highly unlikely that astronomical data could fit these positionings of the orbits of the Earth and Moon as they relate to Sirius’ position in the sky so closely and directly.

Interestingly, this is supported by a highly unusual historical fact. When Rose completed his calculations he found to his surprise “that the Twelfth Dynasty was ended by the arrival of Alexander the Great in –331!”

When Rose moved the 12th Dynasty forward in time by 1477 years, the star Sirius rose just prior to the Sun in the 7th year, 8th month, and 16th day for the dating of Sesostris III, just as the Egyptologists anchored this pharaoh and the 12th Dynasty. When Rose moved the 12th Dynasty forward in time by 1477 years, the orbits of the Earth and Moon were in precisely the proper positions for the various lunar feast days in the precise years, months, and days for the various 12th Dynasty pharaohs documented, which the archaeoastronomers claimed was necessary to create a truly scientific foundation for the 12th Dynasty and Egyptian chronology. When Rose moved the 12th Dynasty forward by 1477 years, the dynasty now appears to end when Alexander conquers Egypt. What Rose has done is close a 1477-year chasm in Egyptian history!

On the basis of these empirical, astronomical and historical facts Manethon and the established chronology derived from Sothic dating cannot be valid. And this writer stresses that the data upon which this finding is based is scientific. None of the motions of the Moon, the Earth and Sirius are merely interpretations of the documents. The documents mesh with the astronomy and the astronomy meshes with the documents. This is not loose or messy data prone to great manipulations; no, it is clear-cut data which Borchardt, Parker, Krauss, and Luft accepted and employed when they undertook their failed calculations. Science—empirical science—in the form of astronomy proved that the 12th Dynasty existed in the first millennium B.C., not the second. The Sun, Moon, and Sothis (Sirius) solidly confirmed this fact.

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Astronomy is undoubtedly the bedrock upon which the chronology of the ancient world is based. As James et al. point out, the Babylonians, who very carefully kept records of the planets and stars, were so accurate in their observation that tablets from the 7th to the 5th centuries can be “precisely dated to a year, month and even a day by modern astronomers.”

James et al. go on to show:

“Final confirmation of the whole canonical system comes from another astronomical fixed point, this time provided by Assyrian records. A solar eclipse is clearly referred to in the Eponym List for the month of Simanu in a year which must, according to the links with Ptolemy’s Canon, have been 763 BC. The fundamental importance of this observation for ancient chronology was stressed by [P.] van der Meer [The Ancient Chronology of Western Asia and Egypt (Leiden 1947), p. 5]

“This eclipse of the sun has been astronomically fixed, on grounds that have never been questioned, as having taken place on June 15th, 763 BC., according to modern reckoning. This year is therefore used as a base reckoning for the Assyrian calendar. It is the sheet-anchor upon which depends not only the Assyrian chronology but also that of the whole of Western Asia’.”

In this regard the Sothic date, corroborated by nearly 36 lunar festival dates of the 12th Dynasty, as Rose amply proved, shows that it is the sheet-anchor upon which depends not only Egyptian chronology but also that of the whole of the ancient Near East. If one eclipse of the sun has fixed the date of Assyrian chronology and the whole of Western Asia, then the heliacal rising of the star Sirius, corroborated by nearly 36 lunar festival dates, does the same for Egyptian chronology.

Lehner has asked if the erosion on the Great Sphinx was enough to overthrow established Egyptian chronology. Surely the one great scientific anchor that he and his colleagues relied upon to solidify their chronology has crumbled away, making that chronology scientifically and therefore historically untenable.

17 ibid.
Given that the end of the 12th Dynasty occurred in 331 B.C., it is rather obvious that the entire stretch from the 13th through 31st Dynasties could not have followed it. These dynasties would have had to rule, based on their following one after the other, through Hellenistic times, Roman times, Medieval times, and practically into the early beginnings of the Renaissance. Having the 1st through 11th Dynasties precede the 12th Dynasty, one following the other, requires that the first Dynasty began its rule about 1490 B.C. Furthermore, since the 12th Dynasty reigned in the first millennium it would have reigned concurrently with several other dynasties that are dated in the conventional chronology to that period covered by the 12th Dynasty. Rose’s solution to the chronological construction created by his astronomical scientific proof strongly suggests that many dynasties ruled at the same time, as Eusebius and others have indicated.

Naturally, these empirical, astronomical facts which overturn some 150 years of Egyptological research cannot be tolerated by those who have devoted, and honestly and earnestly devoted, their entire professional lives to working, in their areas of specialty, within that chronological framework. They may raise several arguments such as: Rose’s findings cannot be right because pottery dating contradicts it, because stratigraphy contradicts it, etc. But all the modes of evidence were worked out to fit the Sothic dating method which was used in the first place to establish the Manethonian chronology. Once that chronology was fixed in the minds of the historians and archaeologists, whatever evidence came from all the various fields was interpreted to fit that chronology. The entire argument of Rose’s critics who employ these forms of evidence is reasoning in a circle. Since these modalities were invented to fit the chronology, it is only natural that they do, and to turn to them in order to criticize Rose is to really argue circularly, just in order to dismiss Rose’s scientific analysis. Nevertheless, in the chapter of this book on pottery dating and stratigraphy it will be shown that circular reasoning dominates Egyptological research. In fact, it will be fully demonstrated that these modes of evidence directly contradict the established chronology and support Rose’s, Heinsohn’s, Sweeney’s, and to a great extent also Velikovsky’s chronology.

Sothic dating was the chief anchor for establishing the foundation of ancient Egyptian chronology. The statements in the literature regarding this conclusion are quite numerous. For example, Clayton states:

"""Even with the chronological information, as outlined . . . it may come as a surprise to realize that it is extremely difficult to fix true or absolute dates in Egyptian chronology. Most of the information given
in the inscriptions [on monuments and other documents] mentioned is relative, in that it [i.e. the king lists] shows a sequence of kings relative to each other with sometimes a length of time between each reign, but to fix them in an absolute framework is a different matter altogether.

**ABSOLUTE DATES FROM ANCIENT EGYPT RELY ON ASTRONOMICAL DATING. THIS IS DONE BY REFERENCE TO THE CIVIL AND ASTRONOMICAL CALENDARS IN A COMPLICATED CALCULATION INVOLVING THE SOTHIC CYCLE OF 1460 YEARS BASED ON THE HELIACAL RISING OF SIRIUS OR SOTHIS, THE ‘DOG STAR’.**[^18]

Egyptologist Barbara Mertz also comments on the importance of this astronomical dating method:

> "What really matters for our [chronological] purposes is the Sothic cycle. From time to time the Egyptians saw fit to mention the rising of the Dog Star in connection with a date of their civil calendar. Now we know, from Roman sources, that a Sothic cycle . . . began in AD 139. By a simple process of arithmetic we can calculate . . . the previous cycle[s] . . . We have a mention of a Sothic rising, with date in the Twelfth Dynasty, and another in the Eighteenth. Hence we can establish these events in terms of our own time scheme [calendar] with as much accuracy as we can hope to obtain . . . and we find that the dates given by dead reckoning check out with the astronomical dates . . ."[^19] [emphasis added]

As we can see, this scientific dating method is the foundation stone upon which Egyptologists built their chronology. But since Rose’s work employing this methodology requires a major displacement of the 12th Dynasty, the dating of that dynasty must be abandoned and, as Gardiner warned, “To abandon 1786 B.C. as the year when Dyn. XII ended would be to cast adrift from our only firm anchor . . . [having] serious consequences for history, not only for Egypt alone, but for the entire Middle East.”[^20]

In fact, some of the earliest criticisms raised against Velikovsky’s historical reconstruction were based on astronomy, namely on solar and lunar eclipse

[^19]: Mertz, *op. cit.*, p. 38
[^20]: quoted in Rose, *Sun, Moon, and Sothis, op. cit.*, p. 209
data. Astronomy, the Queen of the Sciences, is the factual basis of historical chronology, and here is the crux of the matter. The Egyptologists have invoked Sothic and other forms of astronomical dating as the basis for their outline of history. When Sothic and astronomical dating are found to overthrow that outline, in no uncertain terms, they cannot then deny the potency of their own methodology. It is their methodology, their data, their corrections and applications of the data re Sothic-astronomical dating that Rose has relied on and utilized according to rules that they themselves established. His results, when fully analyzed, as he demonstrated, do not support their chronological edifice, but rather, leave it in a shambles. It is a concrete fact, an empirical fact, a scientific fact, and a historical fact that the chronology of ancient Egypt demands a stupendous overhaul.

Even denying the validity of Sothic dating changes nothing because the astronomical facts still exist and using these astronomical facts still requires that the 12th Dynasty be placed in the first millennium, not the second. The lunar festival dates by themselves place the 12th Dynasty in the first millennium B.C. Data is data, and the astronomical data places the 12th Dynasty squarely in the first millennium.

Thomas Kuhn, in *The Structure of Scientific Revolutions*, discussed the nature of a paradigm shift from one theory to another, citing Herbert Butterfield’s *The Origin of Modern Science*:

“One perceptive historian, viewing a classic case of a science’s reorientation by paradigm change, recently described it as ‘picking up the other end of the stick,’ a process that involved ‘handling the same bundle of data as before, but placing them in a new system of relations with one another by giving them a different framework’.”

This, in essence, is what Rose has done. He has created an immensely important anomaly in the heart of conventional chronology. Overcoming this anomaly requires that Rose’s work be shown to be in error based on the same terms, methodology, and data. To date only one critic of Rose has come forth to deny the validity of his hypothesis, namely Anthony J. Spalinger, but he did not present a single piece of factual evidence that proved Rose’s data was in error, or that any of Rose’s findings were in error. He could not point to a single error in the work that he criticized. Instead, perhaps realizing that he had nothing of real substance with which to invalidate Rose’s thesis, he chose the well-known low road of

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misrepresentation. As the astronomer E.J. Opik explained, the unseemly use of misrepresentation comes out of a dogmatic belief in one’s own or the profession’s own dogma:

“Dogma differs from hypothesis by the refusal of its adherents even to consider the aspects of its validity. Legitimate disagreement or controversy creates dogma when arguments are no longer listened to. Although usually belonging to the realm of theoretical models, where direct experiment (or observation) is not possible dogmatism may sometimes induce its followers to misquotation or misrepresentation of the most indisputable facts, even to statements made in print by their opponents.”

Spalinger states:

“This reviewer is not convinced that Rose can read his primary sources, the ancient Egyptian texts, in hieroglyphic and hieratic [the formal and informal styles of ancient Egyptian writing]. Strong doubt is raised by his reliance upon out-of-date and very inexact translations of the Canopus Stela. Caustic comments against Egyptologists who have mastered the language are uncalled for.”

Spalinger never tells us which out-of-date sources Rose relied upon, or what the inexact translations are that Rose cited. (The fact is that Rose quoted some sample passages from the Greek version of the Canopus Decree, as translated by E. Wallis Budge, one of Egyptology’s greatest historians. Spalinger seems to be of the opinion that the text being translated was Egyptian! By the way, Rose has a degree in Greek.) Spalinger fails to point out what the caustic comments are and why they are uncalled for. What he offers is a blizzard of wild accusations unsupported by anything resembling evidence. Therefore, let us examine Spalinger’s ability to read common, everyday English which is not out-of-date and which contains a very exact translation, to determine whether or not his own criticisms are convincing or even correct. Here, unfortunately, we will see that it is Spalinger himself who has not read his primary source, namely Rose’s book Sun, Moon, and Sothis, and that his misrepresentative comments against Rose are uncalled for. In this regard Spalinger writes:

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“But of far greater consequence is the author’s [Rose’s] Venus = Sothis equation. Here it is obvious that Rose has not covered the primary data . . . If one desires to reconstruct ancient Egyptian chronology then one must be as conversant in ancient Egyptian as in arithmetic.”

Spalinger specifically accuses Rose of equating Venus with Sothis instead of Sirius with Sothis. Velikovsky did equate Venus with Sothis, but not Rose, who states quite emphatically:

“. . . I disagree with Velikovsky’s suggestion that the Greek version of the Canopus Decree refers to Venus. As will be explained . . . both the ‘Sopdet’ [Sothis] of the Egyptian texts and the ‘Isis’ of the Greek text seem to refer here to the star Sirius, not to the planet Venus.”

Here it is made quite apparent by Rose that he does not equate Venus with Sothis; he equates Sothis with the star Sirius. To paraphrase Spalinger:

“But of far greater consequence is Spalinger’s misrepresentation of Rose’s Venus = Sothis equation. Here it is obvious that Spalinger has not properly reported the primary data from Rose’s book . . . If Spalinger desires to invalidate Rose’s material, then he must be as conversant with modern English as he is with ancient Egyptian . . .”

This is strikingly not the only time Spalinger raised this Venus equals Sothis misrepresentation. Again, he writes:

“One should not try to build a theory by stacking blocks without cement. Rose is convinced of two main ideas: first that Venus must be equated with ancient Egyptian Sothis (Spdw); and second, that the Middle Kingdom has to be placed considerably later in time . . . One wonders what the Achaemenides would have thought of this.”

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24 ibid., pp. 42-43
25 Rose, op. cit., p. XXXIV
26 Spalinger, “Review,” op. cit., p. 43
Again, let us turn to what Rose says about Sopdet being equated with Sirius by the Egyptians:

“The hieroglyphic and Demotic versions of the Canopus Decree speak of Sopdet, by which the Egyptians seem to have meant Sirius.” 27

This is the second time Rose directly claims that Sothis (Sopdet) means Sirius. Again, to paraphrase Spalinger:

“One should not try to build a critique by stacking blocks without cement. Spalinger is convinced of one main idea: namely that Rose equates Sothis with Venus; having presented Rose’s statement in clear English that Sothis (Sopdet) is Sirius one wonders what Webster—who compiled the English dictionary—would have thought of this.”

Again Spalinger comes back to the same theme that has now been refuted twice, only this time for Isis:

“Rose fails to disprove the well established identification of Isis with the star Sothis (Egyptian Spdw), nor does he prove his own equation of Sothis with Venus.” 28

Above, we cited Rose stating directly that “both the ‘Sopdet’ of the Egyptian texts and the ‘Isis’ of the Greek text seems to refer here to the star Sirius, not to the planet Venus.” This is in total contradiction to Spalinger’s claim. But Rose stated that Isis is Sirius. Rose never attempted to disprove this well-established equation because he accepts the fact that Isis equals Sirius and states this directly:

“IT SEEMS CLEAR TO ME THAT ISIS DEFINITELY MEANT SIRIUS IN THE CANOPUS DECREE.” 29 [capitalization added]

Elsewhere in his book he states:

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27 Rose, op. cit., p. 119
28 Spalinger, “Review,” op. cit., p. 43
29 Rose, op. cit., p. 119
“THUS THE ISIS OF THE GREEK TEXT WOULD HAVE TO HAVE BEEN SIRIUS, NOT VENUS”\(^{30}\) [capitalization added]]

Spalinger concludes:

“In fine Rose has not proved that Sothis = Venus or that the present chronology of modern Egypt is false. The burden of proof is upon Rose as the author of a new hypothesis. Perhaps it would be useful for him to study the Egyptian language for a mature understanding of the ancients’ words and accounts, all of which help modern scholars in constructing Egypt’s history. Too much math addled the outlook of Lewis Carroll’s Humpty-Dumpty, no matter how clever in Oxonian [Oxfordian] logic he may have been. I recommend to the author of this book more humility, more courtesy, and less arithmetic.”\(^{31}\)

Again, to paraphrase Spalinger who offers us such an account:

“In fine Rose has not attempted to prove that Sothis equals Venus. He has presented several unambiguous statements in his texts that Sothis equals Sirius and that Isis equals Sirius. Rose has also never attempted to prove, as Spalinger claims, that ‘the present chronology of modern Egypt is false.’ Rose’s work does not deal with ‘the present chronology of modern Egypt.’ His work deals with the chronology of ancient Egypt. If Spalinger wishes to suggest that Rose is dealing with the ‘chronology of modern Egypt,’ the burden of proof is upon Spalinger as the author of this criticism. Perhaps it would be useful for him to study the English language for a mature understanding of Rose’s words and accounts, all of which would help him to scholarly reconstruct Rose’s book rather than misrepresenting and maligning it.”

Spalinger has chided Rose for using “caustic comments against Egyptologists” such as Richard A. Parker. But Rose states:

“I have the greatest respect for the late Richard A. Parker, and I have always benefited from studying and restudying his writings . . .

\(^{30}\) ibid., p. 177
\(^{31}\) Spalinger, “Review,” op. cit., p. 44
Spalinger, however, seems to be of the opinion that you cannot respect a scholar except by swallowing his mistakes.”

In this regard, the reader is directed to the statement of Dr Thomas Young in reply to the *Edinburgh Review* which had chided him for failing to show the proper respect for Isaac Newton and his corpuscular theory of light while Young presented the wave theory:

“I have been accused of insinuating ‘that Sir Isaac Newton was but a sorry philosopher’ [as Spalinger does with regard to Rose’s respect for Parker]. But it is impossible that an impartial person should read my essay on the subject of light without being sensible that I have as high a respect for his unparalleled talents and accomplishments as the blindest of his followers. . . . But much as I venerate the name of Newton, I am not therefore obliged to believe that he was infallible. I see, not with exultation, but with regret, that he was liable to err, and that his authority has, perhaps, sometimes even retarded the progress of science.”

So, too, with Parker: he was not infallible and it is with regret, not with exultation, that Rose found he was liable to err, and that his authority has, perhaps, sometimes even retarded the progress of archaeoastronomy as it applies to ancient Near Eastern chronology.

Since Spalinger chose to employ Lewis Carroll and Humpty Dumpty to chastise Rose, he cannot demur if the author also cites Carroll when he deals with authorities of the great stature of Parker as opposed to that of Rose. Carroll wrote in *Sylvie and Bruno*:

“‘Do you mean to say,’ said Lady Muriel, ‘that these manikins of an inch high are to argue with me?’

“‘Surely, surely!’ said the Earl. ‘An argument doesn’t depend for its logical force on the size of the creature that utters it!’

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“She tossed her head indignantly. ‘I would not argue with any man less than six inches high!’ she cried. ‘I’d make him work!’

‘What at?’ said Arthur, listening to all this nonsense with an amused smile.

‘Embroidery!’ she readily replied. ‘What lovely embroidery they would do!’”

Fischer, in his chapter on “Fallacies of Substantive Distraction,” which shift the “attention from a reasoned argument to other things,” terms Spalinger’s type of argument that the authority of Parker is above and beyond reproach or criticism a fallacy. Mark Twain’s comment on this kind of exaltation of any man or group above that of their critics is found in Alex Ayres, The Wit and Wisdom of Mark Twain (NY 1989), page 15. “Any kind of royalty, however, modified, any kind of aristocracy, however pruned, is rightly an insult.” Fischer points to this as

“The fallacy of argument ad verecundiam [which] is an appeal to authority . . . This form of error is an egregious but effective rhetorical technique which puts an opponent in the awkward position of appearing to commit the sin of pride if he persists in his opposition.

“The most crude and ugly form of an argument ad verecundiam in historical writing is an appeal to professional status.”

Jeremy Bentham, the English philosopher, was acutely aware of the manner in which people like Spalinger support individuals in positions of authority by inventing absurd fallacies with which they covered up and suppressed any revision that conspicuously contradicts what they wished to be kept from public knowledge. In his The Book of Fallacies, he argued against this abusive argument:

“There are certain men . . . who, in discharge of their functions, arrogate to themselves a degree of probity which is to exclude all imputations and all enquiry. Their assertions are to be deemed equivalent to proof, their virtues are guarantees for the faithful discharge of their duties, and the most implicit confidence is to be reposed in them on all occasions. If you expose any . . . [error], propose

34 Fischer, op. cit., p. 285
35 ibid., p. 283
any reform, call for . . . enquiry . . ., they set up a cry of surprise, amounting almost to indignation, as if their integrity were questioned or their honour wounded. 36

This fallacy incorporates what Bentham further terms “Laudatory Personalities,” in the same way Spalinger indirectly lauds Parker. In Bentham’s words:

“The object of laudatory personalities is to effect the rejection of a measure on account of the alleged good character [and work] of those who oppose it, and the argument advanced is: ‘The measure [or correction] is rendered unnecessary by the virtues of those who are in power [or authority] — their opposition is a sufficient authority for the rejection of the measure. The measure proposed implies a distrust of the [institution’s] members . . .; but so great is their integrity, so complete their disinterestedness, so uniformly do they prefer the public . . . [and the truth] to their own, that such a measure is altogether unnecessary. Their disapproval [is sufficient] to warrant an opposition . . . here the high character [and authority] of the individuals in question is a sufficient guaranty against any ground for alarm or correction’.”37

However, their arguments are no guarantee of the truth and are proposed without a scintilla of evidence to support them:

“Such allegations . . . are never supported by specific disproof and specific disproof if offered could not be admitted . . .”38

That is just what we have from Spalinger. He lauds the work of Parker but offers nothing of his evidence to refute Rose. His argument in no manner at all attempts to show why Parker is correct and Rose is wrong. Unable to present evidence, he presents a vile attempt to humble and denigrate Rose whose work he simply cannot disprove. If he truly had proof that Rose’s analysis was in serious error, he would have presented it. Unable to do so, he offers an emotional and prejudicial attack that lacks merit, integrity, intelligence, or even wit.

37 ibid., p. 125
38 ibid., p. 128
This is the essence of Spalinger recommending that Rose exhibit “more humility, more courtesy.” To cite Samuel Johnson here is à propos:

“Nay, Sir, argument is argument. You cannot help paying regard to their arguments if they are good. If it were testimony, you might disregard it . . . Testimony is like an arrow shot from a long bow; the force of it depends on the strength of the hand that draws it. Argument is like an arrow shot from a cross bow, which has equal force though shot by a child.”

Rose has answered Spalinger’s critique and his conclusion does hit the target’s bull’s eye:

“Spalinger also wants me to more humble. That probably means silent. But I have so little to be humble or silent about. I have beaten these people at their own game, by their own rules, and with their own data. No wonder that Spalinger does not like what my numbers show! No wonder that he and many of his colleagues are so angry! No wonder that he is reduced to spitting out insults about people who rely on what he disparagingly calls ‘arithmetic’! I did learn one thing from Spalinger’s review: 2 + 2 = 4 can be both terrifying and infuriating when that is not the answer that somebody wanted . . .

“It is time for people like Spalinger to stop fooling around and to get to work. There is so much still to be accomplished. The first thing that they need to do is to face the fact that I have a fit for the El-Lahun papyri and they do not.”

Spalinger complains that “such rigid adherence [by Rose] to mathematical presuppositions leads the author down the primrose path of modern Egyptology.” But this is the method the Egyptologists used to give their chronology scientific backing. Fischer calls this

“The antinomian fallacy [which] is the erroneous idea that facts which count best, count least . . .

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39 Fischer, *op. cit.*, p. 283
41 *ibid.*, p. 27
“[Carl] Bridenbaugh allowed some latitude to counting, but not enough. ‘The finest historians,’ he wrote, ‘will not be those who succumb to the . . . methods of social [or physical] science, whatever their uses and values, which I hasten to acknowledge. Nor will the historian worship at the shrine of the Bitch-Goddess QUANTIFICATION. History offers radically different values and methods . . .’

“An intense hostility to quantification also appears among radical historians, who have [as Rose’s work shows] painfully discovered that counting does not easily coexist with their ideological preconceptions.”43 [capitalization added]

Spalinger would never have raised any of his objections if Rose’s numbers corroborated the chronology of which he is a proponent. In this sense he is like Barrington Moore who invoked historical approaches over numerical.44 But at the same time he claimed to “have no patience with the . . . mentality that rejects figures out of hand.”45

“But this statement is made in an appendix called ‘A Note on Statistics and Conservative Historiography,’ which is an obscurantist critique of three important quantitative studies [by others] that reach conclusions contradicting Moore’s ideological assumptions. Mr Moore comes close to arguing that statistics [or numerical analyses] which sustain his interpretation . . . are valid, but . . . [those] which contradict it are false.”46

Spalinger’s point essentially is a form of petitio principii, or begging the question. Spalinger understands that once Rose’s concept is established it establishes the framework of a new chronology. Therefore, in order to stop this from happening, he attacks the methodology so that Rose cannot establish the evidence for the readers of Spalinger’s critique. Once the readers understand the solid scientific and mathematical basis of Rose’s evidence, Rose’s thesis may become established and that would place Spalinger’s chronological preconceptions in doubt. Unable to prove Rose’s numbers are in error, he attempts to undermine them by

43 Fischer, op. cit., p. 94
44 ibid.
45 ibid., p. 95
46 ibid.
undermining Rose’s reliance on them. Since he cannot refute Rose’s numbers, he begs the question and devalues their significance.

In essence, Spalinger is really threatened by Rose’s numbers, and he responds to those numbers by attacking Rose and his numbers, but without refutation via counter-numbers or --evidence, which reflects his attempt to suppress them. The linguist Noam Chomsky explained this behavior quite vividly:

“In my own professional work I have touched on a variety of different fields. I’ve done work in mathematical linguistics, for example, without any professional credentials in mathematics; in this subject I am completely self-taught, and not very well taught. But I’ve been invited to universities to speak on mathematical linguistics at mathematics seminars and colloquia. No one has ever asked me whether I have the appropriate credentials to speak on these subjects; the mathematicians couldn’t care less. What they want to know is what I have to say. No one has ever objected to my right [or legitimacy] to speak, asking whether I have a doctor’s degree in mathematics [or in terms of Rose, competence in reading hieroglyphics], or whether I have taken advanced courses in this subject. That would never enter their minds. They want to know whether I am right or wrong, whether the subject is interesting or not, whether better approaches are possible—the discussion dealt with the subject, not with [my background and competence], not with my right to discuss it.

“But on the other hand, in discussion or debate concerning social issues or American foreign policy, Vietnam or the Middle East, for example, the issue is constantly raised, often with considerable venom. I’ve repeatedly been challenged on grounds of credentials, or asked, what special training do you have that entitles you to speak on these matters? The assumption is that people like me, who are outsiders from a professional viewpoint, are not entitled to speak on such things.”

To paraphrase the rest of what Chomsky says: Compare Spalinger’s response to Rose’s understanding of hieroglyphics and his reliance on mathematics—it’s quite striking. In academia people are supposed to be concerned with what you say not with your academic background. But because Rose speaks [writes] about historical reality based on Egyptian documents, using mathematics to

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47 Noam Chomsky, Language and Responsibility (NY 1979), pp. 6-7
challenge the accepted chronology, Spalinger raises the issue of Rose’s knowledge of hieroglyphics and his use of mathematics to support his case, to undermine the substance of Rose’s thesis. Generally speaking, it seems fair and reasonable to say that the greater the intellectual stakes involved in a field, the greater is a concern for credentials and the less there is at stake, the less is that concern and the greater the concern for content.

But notice how Spalinger faults Rose for his lack of knowledge of ancient hieroglyphic writing while at the same time faulting him for his apparent excellent knowledge of mathematics as it applies to ancient calendars. Notice that in neither case does Spalinger present hieroglyphic evidence as disproof nor does he explain why the mathematics is not to be relied on. The venom underlying Spalinger’s attack tells us how much is at stake. And his capacity to misrepresent Rose’s position that Sothis is Sirius is a measure of this enraged state of mind.

My own conclusion regarding Spalinger’s thoroughly uncompromising position which rambles all over the map is to suggest it resembles a critique of everything and nothing at the same time. Like Mark Twain’s weather forecaster who predicted

“Probable nor’east to sou’west winds, varying to the southard and westard and eastard and points between; high and low barometer, sweeping round from place to place, probable areas of rain, snow, hail, and drought, succeeded or preceded by earthquakes with thunder and lightning.”

Spalinger’s critique is simply pointless, like Twain’s weather report. It is not that his arguments are vapid, not only because they fail to address the data laid out by Rose, but because they are trivial, shallow and banal (to paraphrase Fischer). It is a desperate attempt on his part to stop his colleagues from reading Rose’s book. Clark Whelton once told me a bad review of a book is better than no review at all; a review which misrepresents another’s book repeatedly, as Spalinger has Rose’s book, creates a lynching atmosphere where the subject under criticism, Rose, cannot have his full day in court, that is, have his evidence presented to the archaeoastronomical community unfettered by the taint of misrepresentation. His work is prejudged by Spalinger’s false statements that now surround that work and few within the field itself will wish to sully their hands or thoughts with a brutally condemned piece of research. False rumor can be as damaging and corrosive of the

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truth in academia as it is in ordinary life. Once condemned in the academic press by Spalinger (an insider), Rose (an outsider) and his book can be suppressed by the rest of the profession.

However, true research and true education demands precisely the kind of consideration that Spalinger is determined to suppress. As A. Bloom of the University of Chicago points out in *The Closing of the American Mind*:

“Freedom of mind requires not only, or not even especially, the absence of legal constraints but the presence of alternative thoughts [and alternative concepts]. The most successful tyranny is not one that uses force to assure uniformity [of thought] but the one that removes the awareness of other possibilities, that makes it seem inconceivable that other ways are viable, that removes the sense that there is an outside. . .”49

As Mark Twain wrote: “The history of our race, and each individual’s experience, are sown thick with evidence that a truth is not hard to kill and that a lie well told is immortal.”50

But some may read Rose’s work open-mindedly and understand that his remarkable insight has opened a scientific door to the past that may finally enable historians to write a more valid chronology of the ancient world. They may enlist others to examine and perhaps even agree with Rose’s conclusion that ancient chronology is in serious need of a major revision. Such possibilities must be harrowing to people like Spalinger. But let us now turn to the various other areas upon which ancient Egyptian chronology was constructed and examine what the evidence from these phenomena indicates.

**Note:** Professor Gunnar Heinsohn does not subscribe to the concept of astronomical dating. That he does not, changes nothing with respect to the evidence Rose has produced. Belief can carry no weight as evidence. To overturn Rose’s thesis requires that it be discredited on its own terms, which are scientific and astronomical in nature. Alluding to evidence that is not scientific in nature fails to

50 Mark Twain, “Advice to Youth” (1882) in *The Wit and Wisdom of Mark Twain, op.cit.*, p. 139
come to terms with the facts of science and the assumption of history. Science is the one legitimate determinant of historical truth.

Before proceeding, however, let us analyze this question in terms of testing and replication, which is the heart of the scientific method. There is a large set of data points in the El-Lahun Papyri, which gives specific dates for Sothis-Sirius and the Moon and the Earth’s orbits for the 12th Dynasty. Roy and Edgerton formulated a scientific test with which the data points had to agree with each other to corroborate the placement of the 12th Dynasty. They both claimed that before this placement would be acceptable scientifically, as solidly based astronomy, and arithmetic, they had to be congruent for both Sothis and the lunar dated festivals. Those archaeoastronomers, such as Parker, who took up the challenge, failed to make the data fit the established chronology. When testing the data for the second millennium B.C., they could not reconcile the orbits of the Earth around the Sun, and the Moon around the Earth, with the heliacal rising or Sirius. Rose, on the other hand, found that all the astronomical data points meshed and were congruent by testing these 1477 years closer to the present. In so doing, he reconciled the orbits of the Earth and Moon with the heliacal rising of Sirius. Anyone who examines Rose’s work with an open, dispassionate, and unbiased mind will find that he has solved the problem in terms of astronomy, mathematics, and historical data.

That is the essence and basis of good scientific procedure. The theory of Manethon’s chronology, when tested, could not be replicated by the science of astronomy and mathematics for the placement of the 12th Dynasty. It failed that test! The theory of the shortened chronology, when tested, in fact reconciled the astronomy with mathematics for the placement of the 12th Dynasty in the first millennium B.C. It passed the test! The methodology of science failed to uphold Manethon’s chronological framework. The methodology of science upholds a greatly shortened chronological framework. As Julian Reade said:

“[Chronological a]nomalies can usually be dismissed by one expedient or another. The result is distorted history.”

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ADDENDA

In the October 2002 issue of the *Journal of Near Eastern Studies*, Ronald A. Wells of the University of California at Berkeley wrote a caustic review and critique of Rose’s book, *Sun, Moon, and Sothis*. In his criticism he wrote:

“The book exhibits a verisimilitude of scholarship with a fairly extensive relevant bibliography given at the end, *but almost none of it discussed, analyzed or critiqued in either footnotes or chapter endnotes.*”\(^{52}\) [emphasis added]

By offering the words “but almost none of it discussed, analyzed or critiqued in either footnotes or chapter endnotes,” Wells suggests that some of the bibliography is discussed, analyzed or critiqued in either footnotes or endnotes. This is patently false. Rose discussed, analyzed and critiqued various aspects of the bibliography directly in the text of the book. In fact there are no footnotes or endnotes in his book. For example, in discussing the Julian Calendar, Rose writes:

“Since the Romans counted *backwards* from March 1, it is not entirely clear whether the ‘second’ [date of] Calends VI preceded or followed the regular Calends VI in time. The evidence is conflicting; perhaps the Romans were not always clear about it themselves. For further discussion of this problem, see Samuel, *Greek and Roman Chronology*, page 156.

“Leap years were supposed to occur every four years. But the Romans were accustomed to counting inclusively, and for over thirty years they made every *third* year intercalary. Matters were not fully straightened out until more than fifty years after the introduction of the Julian Reform. See Samuel, *Greek and Roman Chronology*, page 156-157, and Snyder, “When was the Alexandrian Calendar Established?” *American Journal of Philology* LXIV (1943), pages 385-398.\(^{53}\)

Anyone who reads Rose’s book can clearly see that the points that would be in the footnotes as well as the endnotes are discussed, analyzed and critiqued directly in the text. The fact of the matter is that it is Wells who has

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\(^{53}\) Rose, *Sun, Moon, and Sothis*, *op.cit.*, p. 5
presented a verisimilitude of scholarship that has failed to honestly discuss, analyze or critique the book he claims he is reviewing. Wells then writes:

“Curiously enough, Rose does not bother to list the month names in the Egyptian and Macedonian calendars, which would have been helpful to students, even though he draws many comparisons between them.”

Is any of this true? Like Wells’ false discussion of footnotes and endnotes this, too, is false. Rose writes on page 11 of *Sun, Moon, and Sothis* about the Egyptian calendar:

“Traditionally, the Egyptian year was divided into three seasons. The first season . . . contained the months Thoth, Phaophi, Athyr, and Choiak; the second season . . . contained the months Tybi, Mechir, Phamenoth, and Pharmuthi; and the third season . . . contained the months Pachons, Payni, Epeiphi, and Mesore.”

As one can plainly see, Rose did indeed list the month names in the Egyptian calendar for Wells to see. What then of the Macedonian months, has he failed to list them? Of course not: on page 14 of the same book, Rose writes: “. . . the Macedonian calendar contained twelve lunar months: Dystros, Xandikos, Ar Thamesios, Daisios, Panemos, Loios, Gorpiaios, Hyperberetaios, Dios, Apellaios, Audynaios, and Peritaios. Various of these months (usually Peritaios, but sometimes others) were repeated as intercalary months.” Thus, one can see the flagrant disregard Wells shows toward evidence that Rose clearly presented which he claims was not presented. Curiously enough, Wells seems not to have bothered to read these pages, or worse, he read them and disregarded the fact that Rose presented lists of the Egyptian and Macedonian months in his book to be helpful to students to understand the many comparisons Rose draws between them. Again, Wells has falsified and misrepresented the evidence.

About half of Wells’ review deals with the works of Bradley E. Schaefer and L.E. Doggett. These researchers, especially Schaefer, claim naked-eye observations of astronomical objects can misdate historical events, i.e. chronology. Wells claims that “ancient scribal recording of various [observers] have been blindly accepted.” This is based on several problems with naked-eye observations, viz.,

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54 Wells, *loc. cit.*
55 Wells, *Review, op. cit.*, p. 313
“the limiting stellar magnitude at the zenith . . . in a fully dark sky,” “the visual extinction \((k)\), or stellar magnitude lost by a beam of light vertically through the atmosphere” which cannot be known for ancient Egypt.\(^{56}\) There is also \textit{arcus visionis} or the level at which the sun must be set to see a star at the horizon just prior to the sun rising to blot it out in its light which varies, as Rose pointed out in his book, page 116, because “horizon levels, observational post locations (including altitudes), atmospheric conditions (including pollution levels), and even such matters as competence, diligence and eyesight [of the observer]!”

Because of all these problems presented by Schaefer and Doggett, Wells states “It is quite likely, on the basis of this hard statistical evidence, that a significant fraction of ancient Egyptian papyri carrying lunar dates contains errors of one or more dates . . . obliterating any internal consistencies that might otherwise be used to validate uses of the documents for chronological purposes.”\(^{57}\)

All this leaves the distinct impression that Schaefer and Doggett, especially Schaefer, have invalidated ancient celestial observations and deriving chronological dates from them. What Wells has carefully omitted is the work of Robert D. Purrington, who has dealt with Schaefer’s evidence as long ago as 1988. Purrington writes:

\[. . . \text{THE RELEVANCE OF SCHAEFER’S WORK IN ARCHAEOASTRONOMY IS LIMITED.}^{58}\] [capitalization added]

Why is this so? On the same page Purrington explains:

\[. . . \text{it is not always true that a more elaborate treatment or more accuracy is needed, and an approximate approach may sometimes be adequate. This is especially true in archaeoastronomy, since the whole spectrum of atmospheric/climatic phenomena which degrade observing conditions cannot be reconstructed for historical observations. Nor in some cases, can the horizon topography be retrieved. In general, the uncertainties introduced by a lack of knowledge of these variables will often render his [Schaefer’s] precision redundant.”}\]


\(^{57}\) Wells, Review, \textit{op.cit.}, p. 314

If this was not enough, in May of 2000 Schaefer himself fully admits that for all his exacting work, his dates for the heliacal rising of Sirius “are substantially the same as those adopted by most Egyptologists” and further remarks “the answer remains much the same.”

Let us assume for the moment that Wells’ argument is correct. Does it then invalidate Rose’s 34 of 36 lunar dates? Wells will claim it does. But let us analyze what Wells states:

“Schaefer and Doggett . . . found 75% of observers were able to detect the thin crescent Moon with binoculars, but this number dropped to only 50% with the unaided eye. This . . . does not bolster confidence that ancient Egyptian astronomers were any better at performing their observational duties. It is quite likely, on the basis of this hard statistical evidence, that a significant fraction of ancient Egyptian papyri carrying lunar dates contain errors of one or more days.”

Rose analyzed 22 lunar dates that occurred either on, or are geared to, the New Crescent. Of these he was able to match or hit 21 of 22. The other 14 lunar dates are either on, or geared to, first invisibility or lunar day one of the lunar month. There Rose was able to match or hit 13 of 14. Wells claims that 50 percent of the dates of lunar observations would be off, and therefore all one might expect is about a 50 percent number of matches or hits of lunar dates in any group of observations. But on the basis of 50 percent errors occurring in any group of lunar observations, this would never allow a score of over 90 percent which Rose obtained! What Wells requires is that 50 percent of Rose’s hits (about 16 or 17 lunar dates) just happen to be correct on the basis of bad observation carried out at some other time than that found by Rose. Statistically, this is immensely improbable. The probability that out of 36 lunar dates taken at another time than the time Rose worked with there should be 16 or 17 dates that by coincidence just happen to fit perfectly with the actual lunar dates of the time Rose analyzed, beggars description. Above and beyond that to find that the other 50 percent of those 36 lunar dates (again 16 or 17) should be taken at another time than the time Rose worked with, and should also just by coincidence happen to fit perfectly with the actual lunar dates of the time Rose worked with, is asking for miracles.

60 Wells, Review, op. cit., p. 314
This Wells never considered when he raised this issue. The fact of the matter is that the lunar dates Rose obtained appeared to follow the New Crescent Moon by fixed intervals of days: the dates were 0, 2, 3, 8, 16, or 19 days. Does Wells believe anyone will accept that lunar dates scattered over a fairly long period of time would by coincidence all fall directly on Egyptian lunar festival days after the New Crescent Moon by 0, 2, 3, 8, 16, or 19 days when these are based on faulty observations taken at a different time and yet fit perfectly into the time period Rose employed? On the basis of Schaefer and Doggett’s analysis, this would be another miracle of probability. What Wells’ argument requires is not one, not two, but three miracles of probability acting in concert as proof that Rose’s analysis is in error. With these three miracles of probability he claims that he has scientifically discredited Rose’s research. This author hopes that the reader understands the jejune poverty of Wells’ argument and evidence.

The argument that Wells raised by invoking the analysis of Schaefer and Doggett is what Fischer properly terms:

“The fallacy of misplaced precision [which] is an empirical statement which is made precise beyond the practical limits of accuracy. One fanatical quantifier in the sixteenth century was curious to know the weight of a stone cannon ball 10 3/4 inches in diameter. There was, of course, considerable variation in the size and weight of stone cannon balls, even those prepared for a specific gun. And there was undoubtedly some small change in the weight of any single shot, as it was rubbed and chipped by careless gunners, etc. But our inquirer figured the weight of a 10 3/4 inch ball as ‘61 lb, 1 oz, 2 drams, 1 scruple and 15685644/1414944 grains,’ thus attempting to ascertain the weight of a various and variable object within a millionth part of the weight of a grain of barley. The modern scholar who reported this episode commented that it showed ‘a kind of theoretical meticulousness which is medieval in flavor.’

Fischer adds “an empirical statement must not be more precise than its evidence warrants.” Schaefer’s work in this respect is made precise beyond the practical limits of accuracy. This he fully admitted when he stated his super-punctilious calculations of the heliacal rising of Sirius “are substantially the same as those adopted by most Egyptologists.” How then could he know that his lunar data

61 Fischer, op.cit., pp. 61-62
62 ibid., p. 63
were not the same as those adopted by most Egyptologists as well? Remember Purrington claimed “the relevance of Schaefer’s work in archaeoastronomy is limited” and “his precision redundant.” Note, he was not there when these observations were made and therefore could not know the conditions in terms of clarity of the atmosphere, nor the place from which these observations were made, nor the accuracy and experience of the observer who made and recorded the observations. He assumes (and it is only his assumption) that these readings must have been in error. If that was truly the case, there would be no place in ancient Egyptian history where these 36 lunar dates would fit. But over 90 percent of them fit into the chronological framework that Rose presented. If they were truly so hopelessly inaccurate by days, then even Rose’s thesis should fail to make them fit at such a high percentage rate. Wells’ entire argument does not explain away this solid fit of the data. In fact, he fails to discuss this unparalleled level of correlation of Sirius and lunar data and for good reason. The evidence is stunning in both senses of the word and he clearly does not wish his colleagues to look closely at it. Should they do so objectively, they might accept Rose’s thesis. This is unacceptable to Wells, so he, like Spalinger, does a hatchet job on Rose.

Therefore, there can be no argument with Rose’s use of Sirius’ heliacal rising date for determining this date. The only question is: do the lunar data corroborate this date? The problem for Wells, Schaefer and the rest of the archaeoastronomers is that of 36 lunar dates, nearly all fit and corroborate Rose’s analysis. These lunar dates are not only for one month after another, but are for months often widely separated across the 12th Dynasty. The only chronology that fits all this data is Rose’s. Schaefer’s work is valueless in this respect. To accept Wells and Schaefer’s arguments requires that one throw out retrocalculations of ancient astronomy or accept three probability miracles.

The probability that 34 (or even 33 or 32) lunar dates should fit Rose’s analysis by coincidence is simply too great a number to be caused by accident and scribal error or misreadings of the Moon’s position back at that time. The scientific retrocalculations of the heliacal rising of Sirius and these lunar positions nearly all fit with what the ancient documents say about them! Even if Rose had 33 fits out of the 36, that would be 91.5 percent accuracy; for 32 fits out of the 36, that would be 88.8 percent or nearly 89 percent.

Wells, like Spalinger, does not show that Rose’s mathematics or data have a single mistake, though these may exist. Unable, like Spalinger, to disprove the mathematics, Wells cites Lord Brougham that “Calculation is nothing but
cookery.” But archaeoastronomy and retrocalculations clearly demand calculation. Even Schaefer used mathematics to derive his thesis and to show that Sirius’ heliacal risings are “substantially the same as those adopted by most Egyptologists.” Unfortunately, Wells’ review, with its erroneous information, will be read by historians who will most probably not see Rose’s response from which much of this response was taken and which will appear in a 2nd edition of his book, because even a negative review cannot be critiqued. *JNES* has treated Rose decently but sadly this episode makes this author aware of how easy it is for insiders to discredit an outsider in much the same way Spalinger did.

In the final analysis, Wells was left to argue circularly that “synchronisms [of Egyptian history dovetailing] with neighboring countries cannot be ignored.”63 These will be dealt with, in part, elsewhere below. But in order to see how mathematics *is abused*, let us examine radiocarbon dating where numbers are repeatedly cooked to death by the historians.

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63 Wells, Review, *op. cit.*, p. 315
CHAPTER 4
“SCIENTIFIC” RADIOCARBON DATING

“I mistrust laboratory methods because what happens in a laboratory is contrived and dictated. The evidence is manufactured; the cases are what reporters call frame-ups. If the evidence is unexpected or unaccountable it is re-manufactured until it proves what the laboratory controller wants it to prove.”

George Bernard Shaw
quoted in
Robert Youngson,
Scientific Blunders
(NY 1998) p. 173

“. . . material is often submitted for [radiocarbon] dating in the spirit of adding scientific precision to the archaeologists’ pre-existing beliefs.”

R. Hedges,
quoted in
David M. Rohl,
Pharaohs and Kings
(NY 1995) p. 384

Radiocarbon dating is considered by many researchers as a scientific process that can also deliver absolute dates for organizing the chronology of the ancient world. What is rarely presented to the public is that there are a significant number of researchers who are adamant in rejecting the validity of this method of dating. As Gwen Schultz remarked, dating via radiocarbon has “a host of geologic believers . . . [and] a host of geologic doubters.”¹ She was citing William T. Pecora and Meyer Rubin of the U.S. Geologic Survey who admit that with the various dating

¹ Gwen Schultz, The Ice Age Lost (NY 1974), p. 29
schemes “The possibility that all methods used today are wrong must be acknowledged.”\(^2\) David Wilson presents the following shocking admission:

“Some archaeologists refused to accept radiocarbon dates. The attitude probably in the early days of the new [radiocarbon] technique was summed up by Professor Jo Brew, Director of the Peabody Museum at Harvard, ‘If a C14 date supports our theories, we put it in the main text. If it does not entirely contradict them, we put it in the footnote. And if it is completely ‘out of date,’ we just drop it.’”\(^3\)

In spite of these statements in the literature, one of the more outspoken proponents of conventional chronology, Egyptologist William E. Stiebing Jr, advocates radiocarbon as a valid technique for finding absolute dates that support the established chronology. This evidence, in fact, is one of the foundations upon which he also dates the 12th Dynasty:

“Carbon-14 dates . . . place the Twelfth Dynasty later than the Sixth Dynasty . . . In sum radiocarbon dates support the conventional chronology rather than the radical revisions . . .”\(^4\)

Stiebing suggests here that the 12th Dynasty is dated properly to the early second millennium B.C. Here we have what would appear to be a clash between the astronomical evidence presented by Rose and the radiocarbon evidence put forth by Stiebing. If two scientific methods contradict one another about the placement of the 12th Dynasty, of course, this calls these techniques into question. But one of the problems we pointed out above is that radiocarbon dates that are ‘completely out of date’ have been dropped if they fail to support the accepted chronological paradigm. Nevertheless, sometimes certain discrepant dates do see the light of day. Notwithstanding Stiebing’s statement regarding the manner in which radiocarbon properly places the 12th Dynasty into the chronology he advocates, the David H. Koch Radiocarbon Project team reports in *Archaeology* that there is

“fair agreement between our radiocarbon dates and [conventionally accepted] historical dates for the Middle Kingdom. . . .

\(^2\) *ibid.*  
\(^3\) David Wilson, *The New Archaeology* (NY 1974), p. 97  
[But when they] sampled the Middle Kingdom of Dynasty 12 pharaohs Amenemhet I, Senwosret II and Amenemhet III [they found that] two samples from mud brick and mud layers on the ruined core of the pyramid of Amenemhet I produced dates more than 800 years younger than the end of his reign in 1962 B.C.”

Amenemhet I was the first pharaoh of the 12th Dynasty. If we place him where the radiocarbon date fixes him over 800 years or more closer to the present then his reign may have actually begun around 1150 B.C., rather than 1991 B.C. This does not fit Rose’s placement of the 12th Dynasty in the first millennium B.C., but radiocarbon dates have deviations. The dates are given with plus or minus years around the date that is published. If the deviation for this pharaoh is a standard deviation of 50 years, for example, then it has a 67 percent possibility of falling within a range of 1200 to 1100 B.C. However, to be almost absolutely sure three standard deviations give a 99.8 percent possibility of being correct, placing the beginning of Amenemhet I’s reign perhaps between 1300 and 1000 B.C. All this shows is that one cannot trust radiocarbon dates.

The question is: would the researchers who used these most modern techniques, and ran several tests on this 12th Dynasty material, accept their own findings? This date was, of course, unpalatable and an *ad hoc* (invented) scenario was offered to reject this contradiction to their chronology. They assumed that the materials that made the mudbrick—clay with straw—and perhaps other organic materials such as bits of wood had come from a later time. They suggested that the pyramid, after being built, required repairs and that over 800 years after its construction the repairs were made with materials that naturally gave the structure a radiocarbon date far closer to the present. There was no empirical proof that their *ad hoc* scenario had actually occurred, but because the facts, which they themselves had found, failed to agree with the chronology they accepted, they invented a story to explain away inconvenient evidence.

Other dates that were not as contradictory and were not regarded as too damaging were more acceptable and thus published may have been seen as roughly upholding their time line. Senwosret II, one of the later pharaohs of the 12th Dynasty, was found to have radiocarbon dates that “ranged from 103 years older to 78 years younger.” These dates were probably accepted with little question and not disposed of by *ad hoc* scenarios because they supported the paradigm. Thus we

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5 “Dating the Pyramids,” *Archaeology*, vol. 52, no. 5 (Sept./Oct. 1999) p. 31
6 *ibid.*
observe how the conventional chronology derives its support from radiocarbon dates. When the data agrees with their chronology it is never questioned; if it clearly upsets the established chronology, it may be argued that it is either contaminated or a scenario is created to make the anomaly go away. This is unfortunately endemic with too much of the way in which this methodology is handled.

James _et al._ point to radiocarbon results from the tomb of Horemheb, the last pharaoh of the El-Amarna period, dated to the last quarter of the 14th century B.C.:

“While the result of 1410-1000 BC from chopped straw in one wall fit the accepted dates, the date of 1255-920 BC from straw in another wall had to be explained as later ‘embellishment and replastering’ during the 19th Dynasty . . .”

It is relatively easy to invent “just so stories” to undermine data that is not reconcilable with expectations. As with the pyramid of Amenemhet I, this scenario is simply pulled from thin air.

Fischer, as a critic of historians’ fallacious reasoning, maintains in his chapter on “Fallacies of Factual Verification,” “that erroneous understandings of the verification process have caused historians to adopt dysfunctional methods and procedures, which are utterly destructive of empirical scholarship.” In his chapter “Fallacies of Generalization” he holds that this kind of manipulation of evidence by historians only accepting data that supports their theses and removing data derived by the same method that contradicts their theses, is

“The fallacy of statistical special pleading [which] occurs whenever an investigator applies a double standard of inference or interpretation to his evidence—one standard to evidence which sustains his generalization and another to evidence which contradicts it.”

As an example of this Fischer points to historical writings of David Donald, _The Politics of Reconstruction_ (Baton Rouge 1965)

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7 James _et al._, _op.cit._, p. 387
8 Fischer, _op.cit._, p. 41
9 _ibid._, p. 110
“Donald tried to explain why Republican congressmen in the Thirty-ninth Congress (1866-1867) voted as ‘radicals’ or ‘moderates’ on legislation before them. He hypothesized . . . that Republicans who held safe seats—wide electoral margins—tended to be radical, while Republicans who were elected by narrow and precarious pluralities tended to be moderate. Unfortunately for Donald’s hypothesis, the results of his research showed that margins of electoral victory for both radicals and moderates were almost identical—59.3 percent to 59.2. But Donald proceeded to ‘interpret’ his evidence until it conformed to his hypothesis. He argued that in some states, where there was an allegedly powerful Republican organization, a majority of 52 percent was security itself for a radical, but that in another state, a majority of 58 percent was too small to serve the same purpose. One qualification was heaped upon another with industry . . . and ingenuity . . .”

Fischer then turns to David Rothman who took Donald to task for manipulating the data to support his thesis in spite of the fact that it did not:

“To maintain his proposition . . . Donald is forced to resort to a ‘cake and eat’ approach. A Radical’s election by a large vote is prima facie evidence of a safe seat. But when a moderate takes office with large margins Donald introduces the realm of psychology, that is, his guess as to how secure the congressman felt. . . . There is no shortage of ingenious explanations here, but there seems to be little sense in accumulating data only to shuffle and disregard it [if it goes against one’s thesis].”

Fischer, however, suggests “It would . . . be a great mistake to assume that statistical special pleading is a sign of fraud whenever it appears. It is extraordinarily easy for a historian to distort his evidence in this way without ever intending consciously to do so.” But to suggest that one should accept Stiebing’s assertion that the 12th Dynasty is properly dated by the radiocarbon method and is properly placed in the chronology he advocates, when there exist clear radiocarbon dates that dispute this, is applying a “dysfunctional procedure” that is “utterly destructive of empirical scholarship” to the methodology.

10 ibid., pp. 111-112
11 ibid., p. 112
12 ibid., p. 113
In this respect Rose’s section on “Radiocarbon Dating” asks about scattered radiocarbon dates from a specimen: “who is to decide such questions? People who reject all [radiocarbon] findings inconsistent with the conventional chronology?” Unfortunately, in this characterization Rose is correct. This is now the explicit way the Oxford radiocarbon laboratory has been operating over the past decade or so. The policy they have instituted is to dispose of dates that contradict the expectations of their chronology. They call these “rogue dates”.

“On occasion, a laboratory as ‘producer’ will take a slightly different view of [radiocarbon] problems from the archaeologist as ‘consumer’. Our approach in the laboratory has been to vet [re-evaluate] the output of dates to try to pin down ‘unacceptable’ or ‘rogue’ dates as early as possible and to redate material where questions remain.”

This is corroborated by T.A. Thompson et al.,

“Relative [radiocarbon] ages are always subject to interpretation, and radiocarbon dates are often ignored or dismissed as a ‘bad date’ if they do not fit an a priori hypothesis.”

With respect to radiocarbon dating of the Bronze Age in the Near East Professor Ingrid Olsson, a senior radiocarbon dating scientist at the Gothenburg conference on ancient chronology, admitted:

“Honestly I would say that I feel most of the dates from the actual Bronze Age are dubious. The manner in which they have been made . . . forces me to be critical.”

As we well understand, the 12th Dynasty dates to Bronze Age times.

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13 Rose, Sun, Moon, and Sothis, op.cit., p. 280
16 James et al., op.cit., p. XIX
Speaking of radiocarbon dating, R. Stuckenrath Jr writes that “unhappily, this most useful tool has been subject to considerable abuse.”\(^{17}\)

The abuse is simply that the dates that don’t fit the archaeologists’ expectations are disregarded. As Stuckenrath explains:

“Imagine, if you will, a hearth buried several feet below the surface. Associated with this hearth are artifacts which for one reason or another the archaeologist estimates to date to 2000 B.C. He submits a sample of that hearth charcoal to a Carbon-14 laboratory, and eventually receives a date of 1000 ± 100 B.C. [He complains:] It’s a lie and Carbon-14 is a fraud.”\(^{18}\)

This is extremely similar to the way the 12th Dynasty date of 800 years closer to the present was handled. The dates are in reality selected by the archaeologist to fit the chronology he believes to be correct. However, J. Gordon Ogden III proposes “that there is a greater responsibility upon the submitter [the archaeologist or historian] of a sample to explain why a [radiocarbon] determination is rejected than why it is acceptable.”\(^{19}\) As part of his conclusion Ogden admits “I realize that most of what I have said is highly discouraging. In fact, many of you may resolve never to trust a radiocarbon date again.”\(^{20}\) That is, the laboratory operator (Ogden) believes in radiocarbon dating so he may work within this field, but his work assumes that his understanding of the field is sound, not that he can prove that it is. It is basically accepting, without clear evidence, that radiocarbon dates are accurate within certain limits.

Not only is this a pernicious kind of practice condemned by the historians and by Fischer, as noted above, but it is also condemned by one of the world’s greatest scientists, Richard Feynman, who warned:

“If science is to progress what we need is the ability to experiment, honesty in reporting results—the results must be reported without somebody saying what they would like the results to have

\(^{18}\) ibid., p. 305
\(^{20}\) ibid.
been—and finally . . . the intelligence to interpret the results.”21
[emphasis added]

For those who would suggest that this problem is not endemic to the
field of radiocarbon dating, but rather a thing of the past, here is what a
geochronologist, isotope geochemist and stratigrapher, Thomas W. Stratford Jr, who
heads a dating laboratory, had to say in February 2000:

“Human bias influences radiocarbon chronologies far more than
is acknowledged. While human subjectivity should not be a factor, it
may be one of the most intransigent problems in radiocarbon dating
. . .”22

When we have authorities in the field of radiocarbon dating admitting
to procedures that are deplorable, not only from a scientific but also from a historical
standpoint, then what we have is not good science or good history, but rogue science
and rogue history.

To uphold their belief in the established chronology, historians are
required to maintain a double standard for evidence. A date that conforms with and
corroborates their historical paradigm is undoubtedly, in their minds, proof of that
fact. However, when a radiocarbon date contradicts and undermines that paradigm
by great margins, the researchers introduce the realm of psychological invention,
that is, a guess as to the validity of that date, which in their mind must be a rogue
date. They can imagine and introduce these ingenious explanations against every
item that fails to conform with their thinking. But of what value is a dating
methodology that rejects a priori data when it disagrees with the historians’
expectations?

As Fischer states, this shows “a double standard of inference and
interpretation to . . . evidence. He [the historian] is often forced to argue that his
historical subjects [the items dated by radiocarbon] really meant the opposite of what
they said.”23

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2000), p. 45
23 Fischer, op.cit., p. 112
James R. Newman, the historian of science, has also spoken out against this type of manipulation of evidence:

“. . . removal . . . [of] the observer [researcher] . . . [is] indeed an essential part of the scientific procedure, a necessary guard against bias and irrelevant intrusions. After all, if we are looking for honest weight, we must keep our hands off the scale.”

But the historians and archaeologists as well as some radiocarbon dating laboratory researchers cannot keep their hands off the scale. There is not a single historian, archaeologist, philosopher of science or critic of historical methodology that would give credence to a process described as scientific which is so far removed from objectivity. From the very researchers in the field we have statements that say if dates are “rogue” they “will be redated” so that they are not rogue—meaning the dates will fit the chronological expectations. Or another researcher saying that radiocarbon dates are always “subject to interpretation and are ignored or dismissed as a bad date if they do not fit the a priori hypothesis”—again the dates are only acceptable if they fit the chronological expectations. Or another researcher saying that if the dates contradict the thesis, “it’s a lie and radiocarbon is a fraud,” unless the date fits the chronological expectations. And finally another researcher saying “human bias influences radiocarbon chronologies” and “may be one of the most intransigent problems in radiocarbon dating.” Is it any wonder that many historians and others may “resolve never to trust a radiocarbon date again”? As Stuckenrath sarcastically remarks of radiocarbon dating:

“Your 5000-year date [of a sample] is just as valid as anyone else’s, and just as invalid. . . . So relax. After all, this whole blessed thing is nothing but 13th century alchemy, and it all depends on which funny paper you read.”

Nevertheless, how does one account for the fact that there are so very many radiocarbon dates from the ancient Near East that do go back to 3000-4000 B.C.? Surely, one would assume, if ancient history is not as recent as Heinsohn, Rose, and Sweeney suggest it is, then the vast majority of these dates would tend to fall a thousand or more years closer to the present. The simple fact that this is certainly not the case must indicate that ancient Egyptian and Mesopotamian history is as old as the conventional chronology suggests. The researchers cannot nor could

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25 Stuckenrath, *op.cit.* p. 188
not all have been disreputable and these discrepant dates should exist somewhere in the literature. That there are so many dates that do corroborate the established chronology cannot be ignored. These are reasonable arguments and do indeed require an answer. And that is what this author will attend to now.

There are certain questions that need to be addressed in order to respond to these seemingly valid propositions. First, is the material being dated by the radiocarbon technique really as old as the strata in which it is found or may it not be younger but give a far older age date, due to the nature of the material itself or from contamination? Second, are there climatological and geological processes that may cause the specimens being carbon-dated to give the far older dates that the published dates suggest? Third, are there problems inherent in the sources from which they are derived that also indicate that the dates may be far older than they appear? With respect to the first question, contamination, Ogden admits “the greatest problem in sampling is contamination.”

In 1984 various radiocarbon dates were taken from mortar from the Old Kingdom pyramids. The researchers “removed from consideration [ten radiocarbon dated which were younger than the historical chronology] and ten others that were single dates . . . (agreement between multiple dates is needed for reliable dating).” As the reader will note, ten dates that were multiply tested were omitted. These evidently failed the chronological fit too greatly and were dismissed. The others that received tentative recognition were averaged to be “374 years older.”

We are often told that radiocarbon dates are unreliable unless a large number of them are taken and are shown to be internally consistent with each other. Nevertheless, two widely spaced dates had large numbers of samples associated with them at Jarmo. “One showed the settlement to have been founded around 6750 B.C., the other around 4750 B.C., and each had a cluster of other carbon-14 dates around it.” This difference of two thousand years between both series of dated samples indicates how dangerous it is to accept a C-14 date from the ancient Near East as valid even when it is supported by there being numerous samples as corroboration. In 1995 a new assault was launched wherein 300 samples were taken of which 163

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26 Ogden, op.cit., p. 171
27 “Dating the Pyramids,” op.cit., p. 30
28 ibid.
29 Mary Settegast, Plato Prehistorian — 10,000 to 5,000 B.C. — Myth, Religion, Archaeology (Hudson NY 1990), p. 293
gave dates. These tended to be 100 to 200 years older. "The 1995 dates were scattered widely even for a single monument."³⁰

How in reality do we know if these dates are really 100 to 200 or even 450 years older than they should be, and not 1000 or 2000 years older than they should be? What few if any readers will have noticed is that the researchers have taken it as a given that these Old Kingdom monuments are dated correctly and properly by the established chronology. This is not only taking the established chronology as the benchmark of the correctness of the paradigm but using it as the ruler with which to gauge the correctness of the radiocarbon dates.

This is specifically stated and acknowledged by Mertz:

"The reason why the radiocarbon method has not been widely used in Egyptian dynastic periods is simply that the chronology was already fixed before the technique was discovered, and fixed so accurately that the margin of error in the carbon-14 process is greater than the area of uncertainty remaining in the known dates."³¹

Woolley explains how this came about prior to radiocarbon dating:

"What the field worker wants to establish is a sequence so that he may arrange all his material in correct order to illustrate the progress of the culture, and with that done his immediate task is finished. But if different countries have to be compared, a mere sequence is not enough; for such comparison the time relation is all-important, and therefore you want not only to define your periods but also to date them in terms of years. You cannot do that on the basis of pottery types or what-not; exact dates must depend on literary evidence, and therefore the establishment of a fixed chronology is the task not of the field worker [or scientist] as such but of the historian."³²

The historians having already arranged the chronology cannot accept any scientific date or scientific evidence, for that matter, that discredits their chronological achievement.

³⁰"Dating the Pyramids," op.cit., p. 31
³¹Mertz, op.cit., pp. 33-34
As pointed out earlier, this is circular reasoning. How do we know when a sample is truly older or younger than any measured date, unless we have already concluded that we have the correct date prior to testing? This is viciously circular reasoning because the researchers discarded dates that they (based on their own assumptions) said were too young or closer to the present. As we can see, even with radiocarbon dating, circular reasoning permeates the thinking of the researchers. But our question is, can the mortar contain organic materials such as wood, charcoal, and seeds or reeds, that would generate a much older date? Joseph Davidowits, a specialist of the geochemistry of stone, reports on the 1984 dating research:

“The test results announced by the research team are startling. The team claimed that their tests indicate that the Great Pyramid is up to 450 years older than Egyptology had established for the archaeological record. Most remarkably the team reported that mortar at the top of the Great Pyramid was older than that on the bottom [requiring that the top of the pyramid was built before the bottom] and that the Great Pyramid dated older than the Step Pyramid of Zoser [Djoser], which Egyptologists have established as the first ever built.

“All Egyptologists are in firm agreement that the Great Pyramid was built about 100 years after Zoser’s pyramid. Those questioned about the recent carbon-dating project deny the possibility of the accuracy of the tests.”33

Their radiocarbon findings were based on the most rigorous methods and applications available. Mark Lehner commented that:

“The dates run from 3809 BC to 2869 BC. So generally the dates are significantly earlier than the Egyptological data for Khufu[‘s pyramid] . . . In short, the radiocarbon dates, depending on which sample you note, suggest that the Egyptological chronology is anything from 200 to 1200 years off. You can look at this almost like a bell curve, and when you cut it down the middle you can summarize the results by saying our dates are 400 to 450 years too early for the Old Kingdom Pyramids, especially those of the Fourth Dynasty. . . . Now this is really radical . . . I mean it’s a big stink.”34

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34 *Venture Inward*, May/June 1986, p. 13
However, there was no big stink because the results were totally ignored by the professional Egyptologists. Worse yet, Rose published the results of actual radiocarbon tests carried out by the University of Pennsylvania, which were privately sponsored. Here is the range of dates for 20th and 21st Dynasty materials; from oldest to youngest, they are: “2343 B.C., 1905 B.C., 1021 B.C., 960 B.C., A.D. 860, and A.D. 1010.”\textsuperscript{35} The scatter of dates is about 3,353 years. The 20th and 21st Dynasties date, according to the established chronology, from 1185 B.C. to 945 B.C. These dates also suggest something “stinks.” But as with so much else in radiocarbon research, they are merely dismissed without a hint of the fact that this evidence is either being ignored or explained away by \textit{ad hoc} notions of contamination.

Again the radiocarbon method was not accurate according to the Egyptologists, because they believed they knew their chronology was the only correct one. But back to contamination:

“The problem [according to Davidowits] is one of contamination, not through careless sampling, [or mass spectrometer measurement] but because of chemical makeup. One of the ingredients used to make the mortar is natron (sodium carbonate), which contains [$^{12}$C] carbon. The actual date of the geological formation of the natron in the samples is uncertain, \textit{and very small quantities dramatically affect the age evaluated by carbon-14 dating}. Carbon dating could only produce such illogical results as the mortar at the top of the Great Pyramid dating older than the bottom. The illogical dates obtained for some of their samples suggest that established chronology is off from 200 to 1200 years. Although the charcoal and reeds found in the mortar were subjected to acid leaching to remove carbon contamination prior to the dating process, \textit{there is no pretreatment that can eliminate contamination due to a concentrated alkaline solution of sodium or potassium carbonate.}”\textsuperscript{36} [emphasis added]

E. Mott Davis explains the dilemma related to contamination:

“”The immediate lesson to be drawn . . . has already been pointed out most cogently by [E.A.] Olson . . . a study which should be published and force-fed to every archeologist who submits samples for

\textsuperscript{35} Rose, \textit{Sun, Moon, and Sothis}, \textit{op.cit.}, p. 280
\textsuperscript{36} Davidowits, Morris, \textit{op.cit.}, p. 232
radiocarbon dating. The lesson is that the concept of ‘contamination’ provides a ready explanation for the archaeologist who is confronted with an inexplicably young date—an explanation which unfortunately, as the archaeologist uses it, is likely to be meaningless. Only too often when an archaeologist invokes ‘contamination,’ he is really saying that the date in question baffles him.”

How can one positively know that a radiocarbon specimen being tested is truly uncontaminated? R.S. Bradley points out that this is “frequently impossible”:

“It is self-evident that a contaminated [radiocarbon] sample will give an erroneous date, but it is frequently impossible to ascertain if a sample has indeed been contaminated.”

If it is frequently impossible to determine whether or not a specimen is contaminated, then one must assume that it is or isn’t without any solid criteria to make such a determination. This leaves the question up in the air or, in truth, up to the bias of the submitter.

Thus the ten radiocarbon dates that indicated that the pyramids may have been built closer to the present could be dismissed as containing contaminated Carbon 14. But Davidowits shows that the natron could be contaminated with Carbon 12 which would give it a much older date than it had in reality. Contamination can cut both ways and raises the problem of which contamination is creating the discrepant or anomalous dates. That is, one must be absolutely sure that the material being dated is absolutely pure, meaning lacking in contaminants. As explained by R.E. Taylor:

“Careful documentation of the archaeological, historical, and/or geological context of sample material is of primary importance in a critical evaluation and utilization of 14C data. The most careful analytical work will not overcome the problem of context and association. The most exacting attention to detail in laboratory procedures cannot ensure an accurate temporal assignment for archaeological or historical events in the absence of an unambiguous

38 R.S. Bradley, Quaternary Paleoclimatology (London 1985), p. 472
and direct relationship between sample and event or phenomenon for which temporal placement is sought . . . The great variety of depositional conditions and techniques of recovery of sample materials from archaeological sites makes it essentially impossible to provide a rigid framework that would apply in every situation.\(^{39}\)

Hence, dating of mortar from the pyramids can indicate contamination making it older or younger than it is in reality. But what that temporal reality is, is based on circular reasoning which begins with the proposition that conventional chronology is true. If one begins with such a presupposition, why test the materials if they only become valid when they validate that presupposition?

The second question has to do with the climatological and geological factors as they impinge on the materials to be radiocarbon dated. In this respect Fredrick Johnson presented the most fundamental problem facing this dating method:

“[A radiocarbon date] does not date a site or a grave or a level. The date is that of the sample and it is the task of the archaeologist to discover the true relationship between the sample and the area or place it came from.”\(^{40}\)

In this regard the provenance of trees for wood takes first place. If a piece of wood is found at a site in the ancient Near East one would have to know when the tree was cut down, how old it is, how long after it was harvested it was incorporated into the building site or elsewhere, and how many times it was removed from one building and reused in another. During a phone conversation this author had with Lewis Greenberg, I was informed that Velikovsky had raised the issue of how long a tree had been harvested before being shipped to the site for construction. This may range from ten years or twenty up to perhaps fifty years or even longer.

According to Cyril Aldred,

“Only in one thing was Egypt notably deficient and that was good construction timber which had to be imported from . . . Lebanon


The native trees, mostly acacia, sycamore and tamarisk, were too knotty, contorted or unresilient to provide good quality timber, though they were used for simple domestic furniture, boxes and coffins. . . . The palm tree, however, served a multitude of purposes: its trunk for ceiling rafters and staircase supports, . . .

That is to say, most of the useful timber employed in Egypt had to be imported from Lebanon, not just cedar. According to Glenn E. Maroke, Egypt imported its wood from “Phoenician coastal towns nearly all of which (with the notable exception of Tyre) had large reserves of marketable wood. Extensive tracts of cedar, pine, oak and juniper were located all along the Lebanon range. . . .”

The same applies to Mesopotamia. Hawkes explains:

“Much of the wood came down river from the mountain valleys to the north. This was the source of oak, walnut and fir. All of these were in use in the Ur workshops, as was also ebony. The famous cedar wood of the Lebanon . . . was used for furniture, but the greater part of it must have gone into the architectural woodwork of temples and palaces.”

Cutting, hauling, sizing the logs, drying them and then shipping them from the Lebanon may have taken decades. Then hauling the wood to construction sites also took time. Under no reasonable interpretation of this process is it possible to suggest the outermost tree ring at a building site reflects the year that the site was constructed, that is, the year tree was harvested.

According to M.B. Rowton, the mountainous regions of Western Asia from which much of the timber for Egypt and Mesopotamia came, were extremely difficult to transport logs from:

“The mountainous country lacked roads, it lacked security, and it lacked population; moreover, efficient tools [to cut and transport large logs onto wagons] were very expensive in the Bronze Age. Even when the trees could be felled there remained the problem of getting the timber out.” [and citing Kraus in the Archiv für

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42 Glenn E. Maroke, Peoples of the Past: Phoenicians (Berkeley CA 2000), p. 19
"even in recent times in Turkey in the more inaccessible regions it was not uncommon for large trees to be cut for fuel, and only the branches removed. Due to transport difficulties the trunk would be left where it fell and logs by the thousand could be seen rotting away in the mountains."

Consider hauling a few hundred to a few thousand pound log over mountainous terrain for perhaps fifty to a hundred or more miles before being able to load it on a wagon. This could not be done in a few days. At best it would take a few to several months, at worst a year or more. Then the wagon had to transport the logs to the coast, or a river, if one existed, anywhere from a few miles at best to a hundred or more miles at worst, to load it onto a ship. This would take days to weeks to months. The log had to be loaded and shipped over water to a distant site and unloaded, put back on wagons and transported to a building site. The ship may not have arrived for months to a year before the log could be loaded and loading would have taken perhaps a month or more before the ship left port. At the site the timber had to be unloaded, sorted, and sawn to various dimensions, which would take days to weeks or a few months before it was installed into a building. At best we are looking at a year between harvesting and installation at a site; at worst, five to ten or even twenty or more years before the wood was actually utilized at the site.

This is not a small discrepancy. However, there is a much more serious problem associated with types of trees that grow in arid climates. Charles Keith Maisels outlines the problem inherent in using such trees to date ancient Middle Eastern civilizations:

"Xerophyte trees [which grow in arid regions] stop taking in C14 during extreme stress (i.e. drought) but either survived in dormancy for a long time or eventually died but remained standing in the desert. They were subsequently harvested by local peoples and used as construction elements . . or as fuel . . .

"As such xerophytes . . . are a major source of radiocarbon dates right across the Near East, the consequences for possible radiocarbon anomalies are obvious. What the radiocarbon indications from such

stressed/overstressed xerophytes would in fact be dating is the normal period before the onset of drought.”45

Therefore, since “such xerophytes . . . are a major source of radiocarbon dates right across the Near East,” the dates of such specimens used for construction or as fuel would be considerably older than the building from which they were taken by the archaeologist. The tree might have experienced perhaps two, three, or more droughts and survived, which would add perhaps several centuries or more to the age of its oldest, innermost wood and less to that of its outer, younger rings. But Maisels has also told us that these trees may have died in the desert and stood, or quite probably were buried beneath sand, for centuries before being uncovered and subsequently harvested. This process would add an enormous age to the building site from which samples were later removed and radiocarbon dated. The question is, how long can trees last after dying in a desert, covered by sand?

Elizabeth Wayland Barber, in her book The Mummies of Ürümchi, describes the wooden graves of a nomadic Aryan people marked by large wooden posts in the Tarim Basin of western China, a bleak and arid region. The grave sites exhibit thousands of log posts over large cemetery sites, and the buried wood is still well preserved. She asks:

“. . . how could the ancient dwellers of Loulan, nearly four thousand years ago, have found so many good-sized trees to fell if this had been total desert in their day? Did they really drag both their dead and hundreds of logs over vast distances just to bury them in this salty sand?”46

The general explanation (not proven) for this paradox is that a river flowing near the cemetery was able to provide water along its banks to allow a forest to thrive. But whatever the explanation, we have huge amounts of wood preserved for thousands of years. Thus, it is quite clear that dead trees buried in sand can survive for millennia. As Evan Haddingham reports on the wooden artefacts and logs buried in the sand of these cemeteries:

“Just a couple of feet below the sand the archaeologists came across rush matting and wooden logs covering a burial chamber, lined with mud bricks. [Victor] Marr [Professor of Chinese at the University

46 Elizabeth Wayland Barber, The Mummies of Ürümchi (NY 1999), p. 83
Charles Ginenthal, *Pillars of the Past*

of Pennsylvania] was surprised by the appearance of the logs: they look as if they had just been chopped down.”

Even forests may be buried in sand. In fact there are still small regions in the Tarim Basin on which dead forests stand like ghosts of the past. We are told that such an event was actually recorded at Findhorn in Scotland:

“The Culbin and Maviston sandhills near the mouth of the Findhorn, on the southern shore of the Moray Firth, furnish a classic example of destruction of cultivated land . . . by advancing sand. Prior to 1694 the sandhills had already reached the fringe of the . . . estate. In that year a great storm started a phase of accelerated encroachment which finally led to the complete obliteration [burial under sand] of houses, farms and orchards, even to the burial of fir plantations.”

Can trees and wooden objects survive in the sands of the Sahara as in the Tarim basin? Robert Claiborne points out that in ancient Egypt of the Old Kingdom, “Tombs from that period dug into the desert soil have been found to contain well-preserved wooden objects . . .”

Now while dead forests still survive in the isolated Tarim Basin, this region was not directly near a civilization that needed its wood. In Egypt and the surrounding Sahara, wherever buried forests were discovered as the sand dunes moved to expose them, these sources of lumber would have been harvested, shipped, and utilized for construction, utensils, and fuel.

As this author pointed out in *The Extinction of the Mammoth*, trees thrived in the Sahara and somehow clung to life in a few less arid areas. But once desert conditions developed, nearly all the rest of the trees there had to die and whole regions, as in the Tarim Basin, would thereafter be buried and uncovered by sand dune migration. Dunes develop best where they encounter obstruction and a forest of trees would be an ideal obstruction to form dunes and bury these forests. Cuvier, in the *Discours Préliminaire*, discussed the fact that sand dunes do indeed bury forests:

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“Wherever human industry has not known how to stabilize them . . . [sand] dunes advance on the land as irresistibly as rivers advance on the sea . . . in many places they advance with frightening rapidity. Forest, building, cultivated field: they invade all.”

This has also occurred in the Sahara. According to Barbara Bell:

“On the fringelands of deserts the drifting of sand is a particularly sensitive indicator of changes in aridity and in Aeolian [wind] activity. In Middle Egypt, Butzer . . . found evidence that a chain of dunes from the western [Sahara] desert invaded the valley and covered the alluvium with several meters [yards] of sand over a stretch of about 175 km [108 miles] in length and 0.5 to 3.5 km [1640 feet to 2 miles] in width . . . The dunes cannot be dated precisely.”

Budge notes that “woods obtained from trees in the deserts [around Egypt] . . . were used [for building materials, artefacts and fuel].”

A. Cecilia Western states quite specifically: “In dry and desert countries wood can remain undecayed for thousands of years . . ., if the soil is completely dry, and numerous wooden objects, coffins, furniture, figurines, combs and so on, dating [via radiocarbon] from 3000 BC onwards have been found in Egypt.” If wooden artefacts and lumber can survive in the sands of Egypt it is only reasonable to expect that trees in the Sahara Desert will be preserved.

Robert Silverberg tells us: “The stumps of ancient sycamore and acacia trees still rise from the sand in what now is lifeless desert west of the Nile.” Surely, then, in ancient times there must have been entire groves and forests of dead trees buried by sand dunes in the Sahara which the Egyptians exploited when the dunes had moved on and left the caches of valuable wood visible.

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50 quoted in Rudwick, op.cit., p. 238
52 E.A. Wallis Budge, The Dwellers of the Nile, reprint (NY 1977), p. 122
Thus the provenance and age of the wood used by the ancient Egyptians faces three problems related to the age of the building sites from which they were taken to be dated. We do not know how long the log lay in the ground after being cut down, or how long it took to haul it to a site to be cut into boards. We do not know how long it sat in the lumber yard before being shipped to the construction site where it was ultimately sawn and placed in a building. We do not know whether the xerophyte tree from which it came had been through one, two, three or more droughts, making the age of the inner core wood or at middle log depth several centuries older than it should date had there been no droughts. Finally we have no idea whatsoever how long the tree had been lying well preserved in sand, perhaps for centuries before it was found, cut, shipped, and employed in a building site. Each of these phenomena had to generate radiocarbon dates that are much older than the sites from which they were taken and dated. But there are yet further problems related to wood used for radiocarbon dating.

In a desert, wood is a rather scarce commodity and would not be wasted needlessly. Every usable piece of wood would not only be used carefully but when once it was no longer needed at a site it would be removed and cut to fit in at another site. It would be reused as often as possible so long as it was in good condition, and in a dry climate would last for several centuries. A beam from a building which was razed could have been reused after a century or more as a door post in a new building, and then reused a century or more later as a window post, and then after a century or more as a utensil or other small object. Michael A. Hoffman shows that this is what occurred in Egypt:

“When we date a large wooden beam from an ancient tomb, for example, we are dating the time at which the tree was cut down but not necessarily the time at which the beam was carved or installed in the tomb. Many years may have passed before the wood was used, and it might have been reutilized many times before arriving at its final resting place.”

Richard H. Wilkinson further claims, “Temples were often taken over for new use in the course of time; this might consist of the reuse of... stone or other materials [such as wood] from the existing temple in a new structure.” Jacquetta Hawkes points out an example in Mesopotamia of the reuse of wood:

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“In spite of all these imports, fine woods were too valuable to be wasted. The accounts of a carpenter’s workshop at Ur . . . tell us that three old tabletops and four chests were broken up and their wood used to make one table, two beds and a small box.”57

Heather Pringle shows just how far the Egyptians were willing to go to preserve wood:

“The Egyptian pharaoh Sesostris I conceived a magnificent plan for a sailing expedition to obtain the precious golden-yellow resin, frankincense . . . The trees that yielded the tiny resinous teardrops did not grow anywhere in Sesostris’ realm. Instead, the finest quality came from a region the Egyptians called Punt . . .

“Sesostris . . . wanted to cut out some of the middlemen. He ordered his vizier to construct a fleet of wooden boats sturdy enough to travel the often stormy Red Sea. At the town of Koptos along the Nile, the vizier put a small army of Egyptian laborers to work, and after months of sawing and cutting and planing they finished the new fleet all but for the stone anchors. Then the vizier instructed his men to take the ships apart, piece by piece. When the new fleet had been reduced to tons of cedar planks, he ordered his men to prepare for a long journey to the Red Sea coast. They packed the ships in pieces across nearly seventy miles of austere desert and over mountain passes. On the coast, the vizier ordered them to reassemble the boats and find local craftsmen to carve quarter-ton blocks of local stone into anchors. This they did. They set sail for Arabia.

“When the fleet returned, it carried frankincense and many other treasures. The vizier’s representative, Ameni, ordered the men to dismantle the ships. The vessels couldn’t be left behind for thieves [because the wood was very valuable]. The stone anchors were too heavy, however, to haul back across the desert. So Ameni made a shrine of the anchors above the harbor on the Red Sea and had the walls inscribed with the account of the journey, which archaeologists found in the 1970’s. Then Ameni and his men carried their ships back in pieces across the sweltering Eastern Desert.”58

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57 Hawkes, op. cit. (NY 1973] p. 116
This wood, if cedar, came first from the Lebanon and took quite a while to reach Egypt. Then it was carried to the place on the Nile to build the ships. These were dismantled, carried across the desert, reassembled, sailed for who knows how long, were loaded over time, sailed back to Egypt, were unloaded, disassembled, and carried back to the Nile. The wood was obviously then used for various other purposes, and probably reused several times thereafter. Wood was a valuable commodity so its use and reuse and further reuse is only to be expected.

Thus, it becomes rather obvious that any wooden object dated by radiocarbon may have been used and reused many times prior to its final use and with all the other problems cited above could easily be a millennium or more older than the time it was first harvested. Can an archaeologist truly know with any kind of specificity how many times a particular wooden specimen was reused before being placed in the site where he found it? Of course not! He could no more tell us the provenance of this wooden article than he can any of the other problems outlined above. Taylor claims “that the cause of the majority of seriously anomalous 14C values is a misassociation or misidentification of sample context or provenance.”

This brings us to the old wood problem or more accurately what part of the beam or other wood specimen will be used to give a radiocarbon date. The outside ring of a log will be the date on which the tree was chopped down if it was alive. However, once the log is sent to a lumber mill or lumber yard to be sized and cut into a board or beam, the chances are quite good that the outer, rounded parts of the log will be sawn or hacked away to create a useful board. Now if that board is subsequently reused for some other purpose after its original placement in a building, the carpenter may very well cut the board to new dimensions and plane its sides to remove even more outer rings so that its outermost ring is really an inner one of far greater age.

For example, Stuckenrath discusses a juniper beam or log that “contained as many as 600 growth rings in a 40 centimeter [16 inch] diameter.” If that board or log was cut into several boards, the outside ring of most of the boards would have to be considerably older than the building in which it was incorporated. Knowing from the circular shape of the annual rings of the board how far the outside ring is from the core, and counting or estimating how many annual rings there are between the core and outer ring fails to tell us if that outside ring still seen was the last ring of the living tree. It may very well be properly located halfway from the

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59 Taylor, *op.cit.*, p. 108
60 Stuckenrath, *op.cit.*, p. 308
core and not an outer ring, and in a juniper board may be 300 years older than the true outer ring of the tree from which it came. Stuckenrath states “The date of the outside ring will be the felling of the tree and presumably the date of [its use in] construction.” But one cannot know that one has the outermost ring of a tree unless one has the log or a cross-section of it. Once the log has been trimmed to make boards, the outermost rings are cut off and one cannot know, when radiocarbon dating the outer rings of the board, if they represent old wood or young wood. This applies to all timber that has been squared from logs. The view that the outermost ring of a board is the ring that reflects the time when the tree was cut down is an assumption that cannot be proven without having the cross-section of the entire log in one piece. Putting together several pieces of wood to establish an outer ring with a one-piece cross section is still only an assumption that one has the outer ring, not proof.

Furthermore, the larger the board in terms of thickness, the more outer rings must be cut away to form such a squared piece of timber, and the older the inner rings are that are left at the outer margins of that plank. On the other hand, if the plank is thin, one cannot in any way know how many annual rings existed between its outer margin and that of the outermost ring of the log from which it came. The 600 rings in 16 inches of a juniper board could cause a variation of from one to 500 years in the radiocarbon dating of a beam.

Many of the radiocarbon dates are taken from timbers which have been burnt. Clearly, a fire burns away the outer layers of a board and with it the outer tree rings. Therefore, any radiocarbon date from a burnt piece of wood cannot truly reflect the age of the tree when it was harvested. What we have instead are dates of the inner, older tree rings. Since all the problems noted above must on one count or on several counts affect the radiocarbon date of wood, the burning of timber only exacerbates the problem further.

Let us recall that during the 1980 radiocarbon survey researchers found the dates were too old. How did they explain these discrepancies? According to Schoch:

“The researchers involved in this latest study concluded that the Old Kingdom must have utilized massive quantities of wood, so they took whatever they could find, including wood that was already

\[\text{ibid.}\]
hundreds of years old. This, they suggested, gave the anomalously old dates.”

Again we encounter the fallacy of the double standard of inference. A sample of wood or charcoal found in mortar that gives a radiocarbon date that supports the established chronology proves the wood or charcoal from which it was derived came from a tree harvested that year. But when a piece of wood or charcoal found in mortar or elsewhere gives a date that is clearly too old, then the ancient Egyptians used wood that was hundreds of years old, or if the date was extremely old, then the wood had been lying around for several hundred to a thousand years or more before being used. There were indeed dates that were found that were extremely old compared to what was accepted. Charcoal samples from Khufu’s pyramid gave a date of 3809 B.C. (± 160 years). Others also gave much older dates than expected, as Schoch points out on page 15 of *Voyages of the Pyramid Builders*.

But the archaeologists and historians never consider for a moment that dates on wood or charcoal that support the established chronology might have been derived from wood that had been lying about for several hundred to over a thousand years because it had been reused again and again. As Mark Twain so wisely understood: “Figures often beguile me, particularly when I have the arranging of them myself.”

Thus the question of the provenance of wood for this dating method is subject to so many assumptions regarding its true age that it cannot give valid dates. But all the problems of provenance converge on the fact that the wood to be dated must always give dates that are older than the site from which it is derived.

1. Xerophyte trees must always be older or considerably older than the site at which they were used as building materials.

2. Due to the time elapsed between the harvesting of a tree and the time the wood of that tree was employed in the building where it is later found, the date of the wood must always be older or considerably older than that building.

3. Wood taken from dead trees found preserved and standing in the desert must always be older or considerably older than the site in which it was used.

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62 Schoch, *Voyages of the Pyramid Builders, op.cit.*, p. 16
63 in Ayres, *op.cit.*, p. 221
4. Wood that is reused again and again for new construction purposes must always be older or considerably older than the building or site where it was last reused.

5. Wooden logs that are squared to form beams or are cut into many boards or beams will always have the outer rings at their margins removed and therefore must always be older or considerably older than the outermost annual ring that was cut away to square the log to form a beam.

6. Wooden beams that were burnt have the outer tree rings removed and therefore give radiocarbon dates that must be older or considerably older than the date of the site where they are found.

These six characteristics related to radiocarbon dating wood show not only the fundamentally flawed aspects of this dating method but are not necessarily only individual problems. Rather they are compounded problems. A radiocarbon date on a piece of wood may be fraught with several of these problems of provenance. The wood may be from a xerophyte tree that survived a few droughts. The tree may have then died and stood in sand for centuries before being found. It may have taken many years before it ultimately reached the site where it was used for construction purposes. Thereafter, it could have been reused a few times over several centuries as a construction board at new building sites. There the board may have been planed down or cut so as to remove hundreds of outside tree rings. Finally at the last site of its use there may have been a fire which burned away a hundred or so outer tree rings. Any combination of these problems is likely to occur with a wood specimen from an ancient site that is then radiocarbon dated.

Radiocarbon dating of wooden objects in the ancient Near East is in reality what Fischer terms:

“The fallacy of the pseudo proof [which] is committed in a verification statement which seems at first sight to be a precise and specific representation of reality which proves, on close inspection, to be literally meaningless.

“Consider the work of . . . Carl Bridenbaugh of Brown University . . . Many of his conclusions are sustained by one fact at most, and some by a pseudo fact.
“In a book entitled *Cities in Revolt* . . ., Bridenbaugh argued that Bostonians were heavily taxed in the period 1743-1760. His evidence consists in an exclamatory assertion that “at the close of this period the levy on the “Estates Real and Personal” of Bostonians amounted to 13s 6d in the pound, or 67 percent!” But this statement, in itself, tells the reader nothing. Were those thirteen shillings and six-pence extracted from a pound of property at market value or from assessed valuation of estates? Bridenbaugh doesn’t tell us. Let us assume the latter, which was probably the case. If so, what were the assessment rates in proportion to real value—100 percent? 50 percent? 5 percent? If rates were high, then Bostonians were very heavily taxed . . . But if they were low, then the Boston tax rate might have been absurdly small. Bridenbaugh’s ‘fact’ helps not at all to clarify the confusion. As it is presented to the reader, it has no more evidential value than the exclamation point which ends his sentence.

“This may serve as . . . one sort of pseudo fact, in which a relative quantity is stated in absolute terms, without clarification of its reference.”

By the same token, radiocarbon dates of wood, even given plus and minus tolerances, represent a form of pseudo dating of the ancient Near East. A date on a wooden object tells the reader nothing. Was the wood from a xerophyte tree? If so, how many droughts did it weather? Where was the tree harvested and when? This neither the historian nor the archaeologist can answer with anything resembling precision, nor how long it took to transport it to the site where it was later used. Was the wood from buried trees? Who knows? Nor can one say for how long the tree was buried prior to being harvested. Was the wood reused several or a few times before its final placement at a site? Again, no answer is possible. How many outer rings were removed on the beam in order to make its size suitable for construction? Who can say? If the beam or wooden artefact is burned, how many outer rings crumbled away as ash? Not a hint. The fact that a radiocarbon date on wood is repeated on it or even on several other pieces of wood with it answers none of these questions. As the date is presented to the reader, it can have no evidential value. It is a pseudo fact, in which a very relative quantity is stated in absolute terms—even with plus and minus tolerances, but without clarification of all these provenance

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64 Fischer, *op.cit.*, pp. 43-44
problems its real value may be absurdly unrelated to reality. The entire field of radiocarbon dating is contaminated with these problems of provenance.

When every form of evidence speaks so directly and so convergently to the fact that wood used in radiocarbon dating can only generate older or considerably older dates, it becomes quite clear that they must make the age of these civilizations older or considerably older than they are in reality. The basis of radiocarbon dating as a scientific method for upholding established chronology is messy evidence. It exhibits nothing resembling the clarity of astronomical dating that Rose has employed. The entire process is based on circular reasoning in organizing the evidence. Materials that are considered too old or too young are said to be ‘contaminated” or are explained away by ad hoc theories that cannot be proven by those who have invented these scenarios. But worst of all, it is fully admitted that historians and archaeologists will not accept highly divergent dates that would upset their chronological expectations, showing a process of culling data that has been going on since the inception of the radiocarbon method. With such a plethora of problems and manipulations related to this dating process one can surely understand that radiocarbon was doomed from the start to corroborate the established chronology. The method does not reflect good scientific or good historical practice. It reflects something that has little to do with real science or objective history. And its problems strongly converge to create radiocarbon dates that are unquestionably several hundred to well over a thousand years older than they should be in reality. Is it any wonder that radiocarbon dating creates the impression that the ancient Egyptian and Mesopotamian civilizations are over a thousand years or more older than they may actually be? Radiocarbon dating must, by its very nature, create this condition, giving the false impression that ancient history is much older than it is in fact. This is an inevitable outcome of the methodology.

Let us briefly list the numerous ways radiocarbon dating is manipulated. If a date is too old, it is claimed that ancient wood was used to generate the date. If the date is too young, it is claimed that materials of a time closer to the present were placed in the site. If the date is extremely old or young, it is claimed that there was contamination by old carbon or that water removed some old carbon. But if the radiocarbon date upholds the expectations of the historians and archaeologists, then there was no use at the site of ancient wood, there was no refurbishing of the site at a time closer to the present, there was no contamination by old carbon, and water never seeped into the sand—as it was claimed it did at the Sphinx—to remove old carbon.
What we encounter repeatedly are whole series of fudge factors that are employed to manipulate the data. R.S. Westfall’s discussion of Isaac Newton applies to generations of historians and archaeologists who have created a mountain of fudge, as Michael Ruse elucidates:

“Isaac Newton, . . . the greatest of the great, discoverer of the laws of gravitation, author of the *Principia*, mathematical genius. Or so you might think. However, for a start it appears that he was one of the shiftiest data manipulators in the history of physics, the kind of man who sends shudders down the spines of honest researchers and who has congressional critics in spasms of investigative frenzy. He trimmed, cooked and forged the data until his science was as stylized as a painting by Picasso. And he [like the historians and archaeologists who employ radiocarbon dating] was . . . brazen and arrogant about it all: [As Westfall states:] ‘Not the least part of the *Principia*’s persuasiveness was its deliberate pretense to a degree of precision quite beyond its legitimate claim. If the *Principia* established the quantitative pattern of modern science, it equally suggested a less sublime truth—that no one can manipulate the fudge factor quite so effectively as a master mathematician himself.”65

Consider that since the 1950’s radiocarbon dating has been employed by experts—masters—who have used so much fudge that they have smeared the chronology of the ancient Near East over in a layer of brown fudge so that it now seems well-nigh impossible to remove the muck that has passed for accurate and enlightened science. Further consider that, as one reads this, truck loads of fudge are being delivered to other sites to bury them ever and ever deeper below the surface of reality. Though the chronology table is set and smells of sweet scientific truth, it is a diabetic and cholesterol concoction that is sickening and killing the body of chronology and scientific integrity.

Let us examine radiocarbon dating from the perspective of the shortened chronology. We thus find that most of the ancient Egyptian dynasties ruled, say, from about 1200 B.C. to 331 B.C., some 869 years or so. What would the radiocarbon dates show if this were the case and the Egyptologists had made most of the dynasties older? Based on this understanding, the 1st Dynasty through the 5th Dynasty had been pushed back in time more than all the others; therefore the radiocarbon dates for them should show this by being much older or very different. Their dates in the short chronology would be closer to the present by several hundreds of years to a thousand years or even more. Then the dynasties pushed back

in time, but not so far, should exhibit discrepant, older dates but not as old, say, as the first five. And those dynasties set closest to the time to which they belong on the basis of the short chronology would exhibit an even smaller discrepancy. Of course, there would be fluctuations here and there that would arise because of sampling or other laboratory errors.

On the basis of this analysis, the radiocarbon dates for the oldest to the youngest dynasties would show a trend such that the radiocarbon dates for the oldest would appear to be much younger than those that followed it. And that is exactly what has been found. As Paul Dunbavin writes:

“Back to about 1000 BC, radiocarbon dates closely support historical dating. But beyond this point the results appear to be ‘too young’. The margin of error then increases steadily. An artefact which could be dated to about 2500 BC in ‘calendar’ years would yield a radiocarbon date of only about 2000 BC. A discrepancy of some 500 years! At 3000 BC, the margin increases to about 700 years.”

What we find is that uncalibrated radiocarbon dates are always closer to the present. But what appears to be most significant is that the older the dynasty, the more there is a general trend for the historical dates to be older on average than the radiocarbon dates. This, of course, does make perfect sense in terms of the short chronologies. If the dynasties ruled over about 900 years and often co-reigned together, then the farther back in time one pushes their date of reign, the greater must be its divergence from radiocarbon dates, which is precisely what is found.

Above and beyond all that, based on the provenance of wood used for gathering these dates, the dates of all these dynasties must be pushed much closer to the present than even these radiocarbon dates. The Egyptologists were extremely shocked by these findings and rejected the evidence of radiocarbon as having no value for the chronology of Egypt. As Robin M. Derricourt wrote in 1983:

“As the number of . . . dated samples increased, numerical discrepancies have been observed, with radiocarbon dates for Egyptian historic finds considerably lower than the calendar dates established from textual evidence. This discrepancy has led many Egyptologists

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either to cast doubt on the C-14 method or to reject its applicability to Egypt.”

On top of all this, radiocarbon dates are often presented with variations of plus or minus 50 to 100 or even 200 years. If we take the minus side for our dates, then the dates of Egyptian history are even lower than those which take into account the provenance of wood.

The stunning conclusion that derives from this discussion is that ancient civilization is much younger than we have been led to believe. Because the radiocarbon dating of wood from ancient sites must, on the basis of several aspects of its provenance, generate older dates, it becomes an inescapable fact that the great age of ancient civilization is false. Therefore, Heinsohn’s, Rose’s, Sweeney’s, and Velikovsky’s down-dating of Near Eastern history cannot be falsified by this method. The evidence directly supports their shortened chronology and does so quite unambiguously.

This is simply required by the preceding facts. Since nearly every radiocarbon date from wood must always be older or considerably older than its construction site, this demands that the construction site is younger or considerably younger than the radiocarbon date. None of the processes related to the provenance of wood that have been discussed converge to indicate that a historical site is older than the radiocarbon date. Instead, the processes related to provenance converge consistently and unmistakably on the fact that the historical site from which wood for dating was derived is always younger or considerably younger than the radiocarbon date produced. In no way does radiocarbon dating support the established long chronology for Egypt or Mesopotamia. Quite clearly radiocarbon dating supports the revisionists’ shortened chronology. What was offered as support for conventional chronology via radiocarbon dating turns out to be a contradiction to that chronology. What was offered as refutation for the shortened revised chronology via radiocarbon dating turns out to be supportive of it. And all of this comes directly from the established methodology of radiocarbon dating. It is the very process the historians have turned to that lends support to Heinsohn’s, Rose’s, Sweeney’s, as well as Velikovsky’s shortened revisions. This aspect of their methodology never occurred to those who had turned to it as a scientific vindication of conventional chronology. The methodology gives scientific vindication for a shorter or considerably shorter chronology of the ancient world.

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Two final points before proceeding. Mummies are also radiocarbon dated. But in this case it is known that they were placed in baths of either natron or cedar or other wood to preserve the body. Natron, we understand from Davidowits, cannot be trusted because of the very nature of the material. Natron was part of the cement used in Egypt. As for wood,

“The Egyptians used cedar saw dust in mummification. Cedar resin, which makes the wood decay-resistant, was used in embalming and to coat coffins and papyrus to preserve them for the afterlife of the entombed.”68

How old can cedar saw dust be? Of course it can be as old as any part of the tree from which it was taken. According to a report in The New York Times by Deni Seibert, for Sunday, July 12, 1970, “A Lesson From the Cedars of the Lord,” the “oldest groves [of cedars in Lebanon] . . . are estimated to be up to 2000 years old.” According to H.C.D. de Witt, “the greatest known Lebanon cedar has a height of 80 feet, a diameter of 13 1/2 feet, and a probable age of 3000 years.”69 Of course, a body lying for 70 days in such a bath of cedar resin will become saturated with it. Then after being radiocarbon dated it would quite probably give a far older age than it originally was. While some have questioned this process of using cedar saw dust and have suggested other types of wood,70 whatever trees were employed to make embalming oil, if it was used in baths it certainly generated radiocarbon dates far older than the body that was mummified.

Lastly Sass informs us: “Egyptian worshipers [sic] purified themselves by chewing natron (the first tooth paste) or rinsing in a solution containing it.”71 Whatever natron somehow got into the bodies of the Egyptians almost certainly could have affected their radiocarbon age.

A last point regarding cedar trees: cedars grown in temperate latitudes grow to maturity on much shorter time scales because of abundant rain.

Finally, we come to the question of short-lived materials such as reeds, seeds, grasses, etc. Surely, these can give us fairly accurate radiocarbon estimates, or can they? According to F.A. Hassan and S.W. Robinson, the tomb of Tjanefer,  

68 William R. Chaney, “Tree of the Month Cedrus libani,” Arbor Age (Jan 1993), p. 26
70 B. Mertz, op.cit., p. 86
with an established, historically estimated date of 1234 to 1135 B.C., was radiocarbon dated based solely on short-lived material such as reeds, grass, and wood. The deviations in age for this material were extremely small and therefore one might conclude that they properly fixed the age of the tomb. However, the radiocarbon date was found to be as low as 888 B.C. But what is most unusual, according to James, *et al.*, these “materials . . . have a well-known tendency to produce results that are too old.” In essence, these short-lived materials that tend to give older dates suggest that the tomb of Tjanefer should be moved even closer to the present than 888 B.C.

With respect to contradictions to the established chronology from short-lived materials, let us examine how the British Museum dealt with anomalously young dates on reeds and dom-palm nut kernels:

“A notorious case of selective publishing concerns two tests performed by the British Museum on reeds . . . and dom-palm nut kernels . . . from the tomb of Tutankhamun. The results, c. 846 bc and c. 899 bc (standard deviations not available), were never formally published. Their existence came to light only when they were ‘leaked’ by Bruce Mainwaring, co-ordinator of a radiocarbon project conducted by the University of Pennsylvania in conjunction with the British Museum.”

These dates supported Velikovsky’s dating and were thus suppressed. But since such short-lived materials tend to give older ages, they may indicate that King Tut’s tomb may be dated even closer to the present, but certainly not as old as has been assumed. The germane point is that one of the most important institutions in historical research was willing to suppress radiocarbon data that upset historical chronology. To date no establishment historian or archaeologist has come forward to condemn this pernicious practice or any of the others. This fact, in and of itself, suggests that inquisitorial practices lie at the heart of the radiocarbon dating method. As Pindar said thousands of years ago,

“Every noble deed dieth, if suppressed in silence.”

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73 James, *et al.*, *op.cit.*, p. 386
74 ibid., p. 387
75 Pindar, “Eulogy on Alexander, son of Amyntas”
Again, the radiocarbon evidence indicates that short-lived materials require that ancient history is shorter in length than historians and archaeologists believe they have established, and again this gives support to Heinsohn’s, Rose’s, Sweeney’s, as well as Velikovsky’s lowered chronologies. Though it does not support their lower chronologies in detail, that is not the issue. Radiocarbon can only give estimates, and rough estimates at best. But, nevertheless, all the aspects of this methodology indicate uniformly that the established chronology has to be lowered, and lowered considerably.

Professor R.J.C. Atkinson rather succinctly sums up the concept of radiocarbon dating:

“If one looks at... [the] new [radiocarbon] method... simply as [an] alternative approach... or, if you like, alternative tool... , then it seems to me that one can get a great deal out of... [it] as long as one always bears in mind... [it is a] method and not an end... and there is a danger of the method being used wrongly in the sense that it is applied to data which... [is] not suitable for it. At worst... you use... [this] method... simply because this appears to be a more scientific way of approaching archaeology. It isn’t, of course. You can tell lies with numbers just as easily as you can with words.”

The entire process of radiocarbon dating can be summed up by the words of Lord Brougham:

“You have only to take in what you please and leave out what you please; to select your own conditions of time and place; to multiply and divide at discretion, and you can pay the National Debt in half an hour. Calculation is nothing but cookery.”

The process allows historians to take in the samples and dates that please and leave out the samples and dates that displease; to select their own interpretations of the time a tree was harvested, and use that year to date a place; to multiply their dates by repetition and divide what they think is acceptable from unacceptable at discretion; and they can arrange the chronology of the ancient Near

76 David Wilson, *op.cit.*, p. 277
East in half an hour. Calculation can be useful and valid but the way radiocarbon dates are processed is nothing but cookery.

Repeatedly we have found that radiocarbon dating has been, and is being, misapplied because it is constantly employing materials that give older dates than they should from which historians and archaeologists derive their chronological expectations. By simply understanding that this increased age of ancient Near Eastern history is built into their system, those of us who are proponents of the highly shortened chronology have nothing to fear from radiocarbon dating. Rather, all these facts tend to support the highly shortened chronology and tend to contradict the established chronology. In this instance, it is not remiss here to suggest that not only does astronomical Sothic dating undoubtedly support the lower chronology, but in tandem with radiocarbon we arrive at further support for Heinsohn’s, Rose’s, Sweeney’s, as well as Velikovsky’s lowered chronologies. Both scientific phenomena cast extreme doubt on the history that has been so long taken as fact. Again, as with scientific Sothic astronomical dating, scientific radiocarbon dating makes greater sense as factual evidence in support of the thesis advanced in this book.
CHAPTER 5
POTTERY DATING, FAIENCE, AND TIN

Pottery sherds and sometimes entire vases are found buried at sites all over the Near East. Because broken pottery bits were no longer useful, they were cast aside and generally left where they were last used, or nearby. Since pottery by and large is subject to accidental breakage, it doesn’t often last very long and thus may be used to date various sites by its style—its form or shape, the types of glazes that were used, the quality of the workmanship, the thickness of its clay walls, the design on it, and perhaps other attributes. This material has provided historians and archaeologists with what they conceive to be a major tool or foundation for retrieving the past and forming a valid chronology, that is, a chronology that goes back to at least 3000 B.C. The entire methodology was largely developed by Flinders Petrie about a century ago. David Wilson has outlined how pottery dating operates:

“Pottery, the commonest single type of debris left behind by human beings which can survive the erosion of time and weather and bacteria, has always provided the strongest links and clues which have enabled archaeologists . . . [and] prehistorians [and historians] to build up an ordered time-sequence of past events and places. Pottery, furthermore, is made by individuals. . . . There are therefore styles and traditions in the shapes of pots, in the decoration on them, in the materials of which they are made which provide identifying strands running through centuries of time and which the archaeologist can use to pick his way among the political and cultural movements of peoples and traditions. It is possible to follow even the process of the development of a particular line of pottery as it becomes steadily better in material and techniques, steadily more beautiful or more elaborate in style. The relative dating can be carried even further—for a level in a ‘tell’ [a hill formed from many strata where buildings were erected on the debris of earlier, destroyed buildings so that finally it becomes a hill above the surrounding countryside] may contain pottery of a certain type which is plainly a more developed, and therefore later, version of pottery found elsewhere.
“There are limits, however, to a system of relative dating of this sort, where the pottery and artefacts in different layers at many sites are cross-related to give a ‘time structure’ over any large area. First, the whole system is only as strong as the weakest link in the chain of argument. Any large system of evidence and argument will eventually have a number of weak links, any one of which could lead to errors affecting all the other arguments and conclusions. . . .

“[Even with its problems t]he use of ‘stylistic’ evidence, especially based on pottery finds, had great success in sorting out the problems of Middle Eastern chronology.”¹ [emphasis added]

In the previous chapter we dealt with the fallacy of statistical special pleading wherein the investigator applies a double standard of inference or interpretation to his evidence—one standard to evidence which sustains his thesis and another to evidence which contradicts it. With regard to pottery dating, this fallacy is also an important tool in the armory of the archaeologist and historian in dealing with materials that are found in a stratum dated by their chronology to one period of time but which presents the researcher with an anachronism because the material is dated to another, sometimes very distant period. In this respect, this author will employ a material somewhat like pottery, namely scarabs, to describe the manner by which competent archaeologists and historians apply a double standard to evidence which contradicts their chronological expectations. This material is taken directly from Velikovsky’s *Ramses II and His Time*, pages 237 ff, sometimes without attribution. Here all the reasons employed to mitigate and remove inconvenient evidence by the investigator are clearly observed.

Scarabs are models of beetles made of ceramics, glass, stone, and metal, and may have the names of kings or ordinary people on them. Sometimes these were used as seals. They were commonplace objects in ancient Egypt that might be used to denote an important event or act to offer good wishes. In this latter use, they may be thought of as amulets. If a pharaoh’s name was on a scarab, it was assumed to be dated to around the time of his reign. According to P.E. Newberry, “Their [scarab-shaped seals] value as corroborative evidence [for chronological dating and] to other historical data must not be overlooked.”²

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¹ D. Wilson, *op.cit.*, pp. 33-35
The problem is that innumerable times these objects have been uncovered at digs where they were many hundreds of years younger than the age of the site based on the conventional chronology. It has been presented that some scarabs were produced by present-day forgers to sell to the unwary. These forgers would not bury their fakes at sites for archaeologists to find later. There could be no profit for them in so doing. Hence, if scarabs are dug up at sites, say an undisturbed ancient cemetery, they ought to be considered as dating to the great past. But if forgers exist today, weren’t they also plying their trade in ancient times? This raises a problem. One can argue that a scarab found, like a piece of pottery, in the wrong chronological sequence to the date expected, based on the conventional chronology, is a forgery, but when it fulfills the chronological expectations, it is not a forgery. The double standard for evidence is thus clearly observed when such an argument is raised.

On the other hand, if a scarab is thought to be genuine but again contradicts the conventional chronology, it is argued that these are heirlooms carried down from generation to generation for centuries. But if the scarab fulfills the expectations of the archaeologists and historians, it is not regarded as an heirloom. Here also a double standard is applied to make the evidence say what the investigator wishes it to say.

When a very large number of scarabs, all belonging to a particular period, is found at a dig but gives an anachronistic date from another era hundreds of years removed, it is then considered to have been carried wholesale from an old site or grave to the newer one. Yet when a great number of such scarabs uphold the chronology of a site, these were not transferred. The double standard for evidence is invoked once again.

R.A.S. Macalister, digging in Palestine at Gezer, ran into just such problems. According to Albright:

“[He] tried to arrange his chronology so as to cover a hiatus of several centuries (circa 9th–6th centuries) in the history of the city and consequently reduced most of his dates between 1200 and 300 B.C. by several centuries. This erroneous telescoping of chronology was carried much farther by the Germans, misled by similar gaps at Jericho and by premature historical interpretation of their finds; in their case the error amounted at one point to about eight hundred years.” (W.F. Albright, From the Stone Age to Christianity, p. 26)
“As a matter of fact,” [Albright wrote elsewhere] “Macalister’s shift to lower dating for this . . . pottery is easy to explain. At Gezer there is an almost complete lacuna after the tenth century.” (W.F. Albright, The Excavation of Tell Beit Mirsim (New Haven 1932), vol. 1, p. 76)

According to Velikovsky, “The real cause of these changes [introduced by Albright] is in the conflicting evidences of Palestinian archaeology which relies on Egyptian chronology.” Thus, because there was a complete break at the Gezer site after the tenth century, according to Albright, the scarabs that gave these much older dates could be disregarded. But what was one to do when archaeologists working in Palestine found scarabs with Egyptian signs and names of pharaohs all over the place that indicated these sites were also dated anachronistically several centuries out of their conventional place in the chronology, and these sites did not have complete breaks?

Bliss and Macalister at Tel es-Safi and elsewhere in Palestine uncovered just this. They uncovered thirty scarabs with the names of Thutmose III, Amenhotep III, and other pharaohs in various settlements. To deal with the fact that these scarabs contradicted the chronology established for Palestine, they said:

“Evidently some of them, if not all, are mere Palestinian imitations of imported specimens, and are therefore of no value in fixing the date of associated objects. It is an elementary archaeological canon that under the most favourable circumstances scarabs alone can give a major limit of date only; when the element of copying, perhaps long subsequent to the engraving of the original exemplar, is introduced, their chronological importance practically disappears.”

However, if the scarabs were to fit snugly into their chronological expectations, we would be told, “Evidently none of them are mere Palestinian imitations of imported specimens, and are therefore of great value in fixing the date of associated objects. When this element is taken into consideration the introduced scarabs are of great chronological importance.” The entire device employed to dismiss this evidence is “easy to explain”: when the scarabs conflict with the chronology, simply invoke a double standard of inference and dismiss them.

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3 Immanuel Velikovsky, Ramses II and His Time (NY 1978), pp. 239-240
Scarabs were presents of pharaohs and used as official seals in Egypt and countries under Egyptian rule. In Palestine their impression appeared on the handles of jars or on stones used as weights. Now the Palestinians could have used other forms to impress their jars or weights but they used these Egyptian forms, apparently because this was the way taxes and weights for oil and wine were controlled, therefore they could not be imitations as Bliss and Macalister suggested. Yet this consideration was never raised by them. In one case the scarabs are imitations, but with taxable and important measured weight they are evidently not. But when the scarabs are used on jars and weights, they are not even considered.

How then do the archaeologists deal with indisputable Egyptian scarabs in Palestine? Sellin and Watzinger, who excavated Jericho, admit: “It is beyond doubt that all scarabs found are of genuine Egyptian workmanship of their time, not one a foreign or late imitation.” However, when these authentic Egyptian scarabs once again were found to be much older than the site, a rationalization had to be thought up to get rid of these bugs. Sellin and Watzinger crushed them, because:

“It has already been frequently established in the Palestinian excavations that the old scarabs were worn centuries later as unintelligible amulets, and therefore, when we find them, we obtain but a terminus a quo [a limit]. Furthermore, handles stamped with scarabs exactly like those from Jericho were never found in the same level of the excavations as the handmade Canaanitic ceramics.”

Thus if genuine scarabs were used in Palestine long after their value had been forgotten but were not found in Canaanite levels at Jericho which was contemporary with the pharaohs, then the problem is, who made these scarabs? The scarabs had to be made in Egypt and brought to Jericho in Canaan during the time of these pharaohs. From that time forward these scarabs had to be worn for centuries by the Palestinians. But then why are they at the limit of their age? Surely they could have been buried with people immediately after they were introduced and then through hundreds of years thereafter. Therefore, the scarabs had to have been produced even before the pharaohs named on them lived. And further, why didn’t the Israelites use any amulets and scarabs of their own time, but only old scarabs? Also, why would the Israelites only use old scarabs for impressions on their jars and weights? There is not a penny’s worth of logic in the explanation that suggests only old scarabs were used. It would be similar to finding a graveyard in which everyone

6 *ibid.*
was buried in the clothing of a period five or six centuries farther back in time and then claim they all wanted to dress in ancient costumes. The argument for only old scarabs is as threadbare as the idea that people would only be buried in ancient costumes. Is the heirloom theory tenable in terms of this understanding? No, emphatically no!

Then there is Beth-Shemesh, modern Ain-Shems, dated from 1000 to 600 B.C. Here also as this site is connected to the trunk of Egyptian chronology, it was found to date up to 400 years older. In this case, the very same argument was raised, namely that a scarab found in the tomb had been worn and passed along for 400 years. Velikovsky goes on to show:

“This ‘limestone scarab with its ten lines of writing’ is no different from such scarabs in the Aegean tombs and in Enkomi on Cyprus, where they are regarded as the chief evidence of the age of the levels and of Mycenaean culture in general. ‘It dates from 1400 B.C., and was a treasured antiquity when it was deposited for its magical value’.”

Therefore, when one particular type of scarab was found in Palestine, it had been worn for 400 years by generations and then around 1000 B.C. left in a tomb. When the same type of scarab was found at Enkomi on Cyprus, it was not worn for 400 years by generations, but was buried soon after—historically speaking—it was produced. Again, the enchanting spell of the double standard of inference comes into play.

In each and every case, rather than using one standard of inference, the investigators turn to psychology to create one scenario after another to do away with contradictions. Let us examine one last example of this psychological game of hide-and-seek.

At Megiddo, overlooking the Jezreel Valley, Schumacher’s digs in the early 20th century found material that appeared to belong to a wide range of different periods. Watzinger concluded: “It becomes clear that in the process of digging too freely the deeper strata were invaded [from above by objects] and finds from these

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8 *ibid.*, p. 66
9 Velikovsky, *op.cit.*, pp. 242-243
more ancient layers were marked as belonging to the same layer. . .”\textsuperscript{10} That is, the dig was done poorly and materials from different levels evidently became mixed together, giving an erroneous date.

However, when an American campaign was carried out at Megiddo and did a careful excavation, the same results were found. To remove these anachronisms they claimed an unknown people came there and settled. “A new people with a strong artistic feeling for its religion was invading the country at the end of the Middle Bronze Period. From the evidence of scarabs we must conclude that it was closely related to the earlier Hyksos. . .”\textsuperscript{11} Velikovsky’s response bears repeating:

“But the Hyksos are known to have been devoid of ‘artistic feeling’ for their religion or anything else; they did not manifest any artistic activity in Egypt. Then who could have been the invaders who carried a new culture into Palestine in the early days of the Hyksos Empire . . . ?”\textsuperscript{12}

There is much more, but the reader now has an understanding not only of the way scarabs are employed by archaeologists and historians, never to test their chronology, but just as with radiocarbon dating to force everything to fit it. So when it comes to pottery dating, the same armory of double standards of inference comes into play. A whole host of pretexts exists by which the conventional chronology is maintained. It is a fixed game: heads, the established chronology wins, tails, the established chronology wins. But this is only achieved by turning logic and ethics on their heads.

How then can one argue with that chronology based on pottery if the game is rigged? As we were earlier informed, pottery chronology is only as strong as its weakest link. And that is the Achilles heal of this methodology because at the very beginning of pottery dating, the very first anchor point upon which the entire chain was constructed, stands a monumental and fatal error that destroys that entire chain as it presently holds that chronology together. Let us examine it.

\textsuperscript{10} C. Watzinger, \textit{Tell el-Mutesellim} (Leipzig 1929), vol. 2, p. V
\textsuperscript{11} H.G. Gray, \textit{Material Remains of the Megiddo Cult} (Chicago 1935), p. 35
\textsuperscript{12} Velikovsky, \textit{op.cit.}, p. 244
In terms of Egyptian chronology, Barbara Mertz claims that “pottery has been one of the most useful tools of the archaeologist.”\(^\text{13}\) Colin Renfrew maintains that pottery dating “constitutes the bread-and-butter of archaeology.”\(^\text{14}\) Stiebing has also given pottery dating and cross referencing of it a prime place in analyzing ancient chronology. In an unqualified statement of endorsement at the Hampton Institute in Hampton, Virginia, in the late 1980s he held that pottery dating was about the strongest evidence for the conventional chronology that he could point to as proof of its validity. As an example of how the process operates, in his book on the history of archaeology Stiebing described Petrie, “the Father of Pots,” doing his work on the earliest history and prehistory of Egypt:

“Petrie . . . introduced typological [pottery] sequence dating into Near Eastern archaeology. At Naqada and at Abydos he discovered graves that contained no written materials, forcing him to find another way to date them . . . After defining several such . . . [pottery] assemblages, Petrie constructed typological series for some of the objects within the groups. The most obvious series involved ledge-handled jars with well-made wavy ledge-handles to enable the bearer to obtain a better grip. In the assemblage from another group of burials the jars were less rounded and the handles smaller and less functional. In examples from still other groups the handles had disappeared completely to be replaced by a painted wavy line and the jars had become cylindrical. Since the handleless jars in this series also occurred in datable deposits from the First Dynasty while the ledge-handled jars were not found in assemblages from historical times, the direction of the sequence [from highly-rounded jars in prehistoric time with ledge handles to much more cylindrical jars that could be carried easily without ledge handles] could be firmly established.”\(^\text{15}\)

Notwithstanding this rendition of Petrie’s work, Stiebing has omitted some basic information regarding the digs at Naqada as it relates to the dating of pottery. What he originally found at Naqada is outlined briefly by Colin Wilson and Rand Flem-Ath:

“In the 1890s, when the great Flinders Petrie excavated a village called Naqada, on the Nile, and found pottery and vases of such

\(^{13}\) Mertz, \textit{op.cit.}, p. 4
\(^{14}\) Colin Renfrew, “Foreword,” \textit{James et al., op.cit.}, p. XX
sophistication that he assumed they must date from the Eleventh Dynasty, around 2000 BC, he even coined the term ‘The New Race’ to describe this unknown people, whose artifacts seemed oddly unlike those of the Egyptians. But when he found more of their typical pottery dating from 1000 years earlier, he decided to drop Naqada from his chronology rather than face the embarrassment of explaining how ‘primitives’ of an earlier [Egyptian] civilization could produce work of such excellence.”16

The germane point is that the pottery that Petrie found in the earliest stages of development of ancient Egypt was so sophisticated that he felt it was produced long after Egypt had flourished for a millennium or more. The pottery was so advanced that this seemed a logical conclusion. Placing this refined pottery much farther back in time made the sequence described by Stiebing fit the chronology but leaves open the possibility that the end period of the sequence may not be related to the prior period of development.

There is a certain amount of judgment in these determinations, as Wilson explains:

“There are limits, however, to a system of relative dating of this sort, where the pottery and artefacts in different layers at many sites are cross related to give a ‘time structure’ over any large area. . . . For instance it is likely, but not necessarily true, that there must be a direct human connection between two distinct places where the same sort of pottery is found. It is conceivable, however, that two quite unconnected human groups might make the same sort of pottery if both groups had the same needs and the same raw materials. Or it is possible that the two similar types of pottery might represent similar answers to a single set of problems with a large time-gap between them. . . . In modern engineering developments we can see remarkably similar answers developed by different groups of men faced with the same set of problems (the shape of supersonic aeroplanes is a good example).

“When archaeologists, like detectives reconstructing the past from clues left in the present, conclude that one type of pottery is later in date than another because one sample seems to be ‘finer’ or ‘more artistic’ or ‘more sophisticated’ or ‘better glazed’, then the possibility

of errors becomes greater. Such judgements are necessarily subjective—what seems ‘finer’ to us may well have appeared the opposite to men of earlier cultures. [Or there many have been various qualities of pottery, ‘fine’ for the well-to-do and ‘simple’ for the ordinary purchaser.] And even if we accept the judgement of the excavator or art-historian about the relative qualities of the two samples, we know that cultures, or civilizations, or artistic traditions, can degenerate as well as advance. Hence it is quite possible that some samples of poor-quality pottery in a certain stylistic tradition might be later in date than better-quality examples because the culture, or the materials available, are degenerating rather than improving. . . .

“It is not only that such methods can lead to mistakes and wrong deductions—anybody can be forgiven for this—but that the scientific archaeologist feels that the profession has been led, in some cases, into blind alleys and wrong attitudes.”17

Therefore, there is a subjective quality to pottery dating that may affect the chronology. The question of course is: Did Petrie apply a subjective interpretation to his early work in founding this methodology that persists in the literature to this day? Stiebing has informed us that Petrie established the early dating of Egypt via pottery dating at Naqada and Abydos, where the 1st Dynasty of the Old Kingdom left relics in the ground. Stiebing, through Petrie’s work, maintains that the pottery at Abydos is from the third millennium B.C. But was it? Could it possibly be from the second or even first millennium B.C.?

Here one may ask, How dependable are the sources from which Petrie was able to derive his dating of the 1st Dynasty via its pottery? How independent was his interpretation of this evidence from the conventional chronology to which he subscribed? In a certain sense Velikovsky dealt with a similar form of dating in Peoples of the Sea. He described colored, glazed tiles excavated by the Swiss Egyptologist Edouard Naville at Tell el-Yahudia, twenty miles north-west of Cairo. These were dated to the time of Ramses III, who is conventionally thought to have ruled from 1198 to 1166 B.C. Nevertheless, on the reverse side of these tiles were found Greek letters of the alphabet, which only appear before 750 B.C., or some four to five centuries later.18 In terms of those Greek letters it was quite obvious that

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17 Colin Wilson, op.cit., pp. 34-35
18 Immanuel Velikovsky, Peoples of the Sea, op.cit., pp. 6-12
Ramses III did not reign 1198-1166 B.C., in the latter part of the second millennium B.C., but around the middle of the first.

Prior to doing his work at Abydos in Egypt, Petrie had “found . . . Aegean pottery that substantiated dates of ancient Greek civilizations, including the Mycenaean.”19 Jacquetta Hawkes further shows that Petrie,

“excavating in Egypt at Gurob near Heracleopolis in 1889 . . . found pottery of the Mycenaean age of Greece together with Egyptian objects dating to the late XVIIth Dynasty—about the fourteenth century B.C. Then the next year at Kahun in the Fayum [district] pottery came to light which with a sure instinct Petrie also attributed to the Aegean, although its like was not to be known until the discovery of the Minoan civilization a decade later.”20

Petrie thus had an excellent understanding, not only of Egyptian pottery in terms of shape and design, etc., he also had an excellent understanding of Mycenaean and Aegean pottery, based on these parameters. He could observe such pottery and recognize it at once. Here then we come to how he ultimately dated or rather misdated the Old Kingdom from pottery he unearthed at Abydos. John Dayton, in his monumental work Minerals, Metals, Glazing & Man charges that “It has been sad to find that Petrie in 1901 was responsible for the original error in dating Abydos . . .”21 Dayton cites Petrie’s report as it specifically identified the 1st Dynasty, via pottery, to the third to fourth millennium B.C., stating quite unequivocally:

“From the objects alone it is clear which are the earliest and which are the latest. . . .

“Last year many pieces of pottery closely resembling the Aegean ware came to light in the tomb of Mersekha and a few in that of Den [at Abydos]. THE BODY OF THE WARE IS IDENTICAL WITH THAT OF THE LATER AEGEAN OR MYCNAEAN POTTERY; THE PATTERNS ARE COMMON ON MYCNAEAN POTTERY; AND INDEED NO PATTERNED EGYPTIAN POTTERY IS KNOWN UNTIL THE XVIIIITH DYNASTY. . . [which conventionally

flourished between 1570 and 1293 B.C.] . . . This pottery of a fabric [texture] and of forms entirely foreign to Egypt, and of European character is now absolutely dated to the second king of the 1st Dynasty about 4700 [now about 3050] B.C.”

Dayton explained that the designs and shapes of pottery were foreign to Egypt until around 1750 years closer to the present when this pottery began to be imported from Aegean, Syrian and Mycenaean sources. The “later Aegean and Mycenaean pottery” was actually identical with late Helladic pottery that dated from about 800 to 700 B.C., or over 2000 years closer to the present. Rather than date the beginning of Egyptian dynastic rule on the basis of the clearly discernible shapes and designs of the pottery which were foreign to Egypt and were dated from between 1750 to over 2000 years later in time to between about 1500 to 800 B.C., Petrie simply denied the facts he witnessed with his own eyes and his educated understanding and dated them thousands of years farther back in time. Furthermore, he did not attempt to hide these facts that were obvious to him. He simply denied the factual basis upon which pottery dating is supposed to rest and allowed the chronology of Manetho to determine what the evidence said instead of allowing the pottery dating methodology to determine what the chronology actually was. That is, the pottery dating method was erroneous from the very beginning of Egyptian history.

Interestingly, Petrie in 1901 was so thoroughly convinced that he had properly organized Egyptian chronology on a sound foundation that in his address to the Egypt Exploration Fund, based mainly on his own pottery research, he claimed:

“It is now twenty-one years since I began work in Egypt. . . . In those days the pyramid of Khufu [Cheops] was our boundary of history. . . . The situation is now completely different. The monumental history has been carried back to the very beginning of the written record, which has been entirely confirmed. . . . The archaeology is better known than that of the most familiar countries: not a vase nor a bead, not an ornament or a carving, but what falls into place with known examples, and can be clearly dated.”

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22 Ibid., pp. 407-408
23 Quoted in Peter James et al., Op. Cit., p. 20
On page 400 of his book, Dayton reproduces the pottery forms Petrie found at Abydos and adds that the “important thing to note is that Petrie quite clearly and correctly describes the pottery as Aegean.” He further adds there that some of these are “clearly . . . Syrian jugs and combed ware, with Aegean sherds of Cypriot [from Cyprus] ‘white painted’ ware and Late Helladic I pottery.” Some of the pottery quite clearly exhibits geometric designs which only came into being in the Aegean in the mid-first millennium B.C.

Based directly on the fundamental methodology of pottery dating, the 1st Dynasty could only have had these forms of pottery in the first millennium B.C. and should have been placed there with the Late Helladic Period of 800-700 B.C. Having ignored the straightforward evidence, Petrie and his numerous students and followers compounded the error. What followed was incredible. The misinterpretation and misplacement of the 1st Dynasty vis-à-vis the pottery at Abydos not only became incorporated as fundamental evidence for the dating of the Old Kingdom, it also became a straightjacket which would then permit no other interpretation: everything was forced to fit the conventional chronology.

If pottery or sherds with designs on them or of a shape like those Petrie found at Abydos were discovered at a site that was supposed to be about as old as the 1st Dynasty, then that was proof that the site was quite old. If, on the other hand, the very same forms of pottery or sherds, also like those found at Abydos, were discovered at a site that was dated to the first millennium B.C., then that was proof that the site was quite young. The pottery dating reinforced the established chronology, and the established chronology reinforced the pottery dating. It was, therefore, one giant piece of circular reasoning—or broken piece of circular reasoning. Dayton describes how this process of circular reinforcement of Petrie’s error became entrenched:

“Because of these wrongly dated ‘Aegean’ pots in Egypt Kantor (1965) dated a similar painted jug at Jericho, tomb [number] A127, to the Early Bronze Age. The tomb also contained two examples of twin cups joined together . . . This style of joined pot is common at Troy and in the Aegean. A spouted jar was also found in this tomb, and the painted jug was used by Kantor as a dating link for Palestine, which carries the 1st Dynasty error onwards.

24 John Dayton, op.cit., p. 400
“At the great Middle Bronze Age II site of Hazor a ‘Syrian’ bottle was found in a typical Hyksos context, and its occurrence in such a context is significant. White-on-dark painted sherds were also found on this site together with a sherd of early Mycenaean Shaft Grave style. We thus have two types of ‘Syrian’ bottle, one of Middle Bronze II date and the other type . . . of Early Bronze Age II, both with a burnished red slip!

“As Amiran said, ‘the chronology of this period—an entire millennium—is pegged to a few reliable dates, and these are only relative and not absolute’.”

Petrie “was the first scientific excavator of Egyptian sites and personally trained many students [for over thirty-nine years, while he was Professor of Egyptology at University College, London] who became masters in the field. Through them his influence is still felt by students today.” How could these students, who rose to great prominence in the field of Egyptology, interpret the pottery dates of the rest of Egypt and the Near East other than as they were instructed by their master? For example, Stiebing criticized Heinsohn because “his chronology totally disregards the relative [pottery-dated] chronology which has been firmly established by archaeological excavations in Syria-Palestine.” But as Dayton just pointed out, these sites were firmly misdated by following Petrie’s dating error.

Petrie’s error is based on the belief system he brought to the excavation at Abydos, so what he clearly recognized as first millennium B.C. pottery he believed had to be third millennium or older. This however became the traditional understanding of pottery dating passed on to his pupils and they passed this understanding (or more properly misunderstanding) to their own pupils who practice historical research at present. It is well illustrated by Ruben Able thus:

“The story of Dürer’s rhinoceros . . . illustrates the overwhelming weight of tradition in determining representation even when an actual model is before the artist’s eyes. Dürer (who died in 1528) had never seen that famous exotic beast . . . Nevertheless he made a woodcut of one relying on second-hand evidence and his imagination. For centuries thereafter, natural history books used his half-invented
creature as a model. When James Bruce visited Africa in 1790 and saw a rhinoceros, he called attention to how ‘wonderfully ill executed’ Dürer’s woodcut was. Yet Bruce’s own illustration, drawn from life, was so strongly influenced by his idea of what a rhinoceros ought to look like (i.e. like Dürer’s woodcut) that no zoologist can identify what Bruce actually saw! (H.E. Gombrich tells this story in his *Art and Illusion* to show the persistent influence of convention.)”

The same applies to Petrie and his pupils. Petrie, unlike Dürer, had seen Late Helladic pottery. Nevertheless he made a dating of it relying on his conviction that his chronological belief of ancient Egypt required a third millennium date. For about a century historians used his dating scheme to develop the rest of the established chronology. However, when Dayton pointed out the enormity of Petrie’s error, the Egyptologists could still not identify what Dayton showed them correctly, unlike James Bruce. Yet no Egyptologist will identify the pottery at Abydos as properly belonging in the first millennium. (This illustrates the persistent influence of convention over evidence.)

Wilson has told us that pottery dating is only as strong as its weakest or missing links. Petrie’s erroneous analysis of the pottery at Abydos was the foundation of that chain of reasoning for the established chronology. It did not fit Manethon’s analysis but was forced to do so. This is, in a major sense, what Fischer discussed, calling it:

“The fallacy of declarative questions [which] consists in confusing an interrogative statement. It violates a fundamental rule of empirical question-framing, which requires that a question must have an open end, which will allow a free and honest choice with minimal bias and maximal flexibility. If a historian goes to his sources with a simple affirmative proposition that ‘X was the case,’ [as Petrie went to Abydos holding the simple affirmative proposition that all material found there dated to early fourth to third millennium Egyptian times], then he is predisposed to prove [or state] it. He will probably be able to find ‘evidence’ sufficient to illustrate his expectations, if not actually to sustain them. If, on the other hand, he asks ‘Was X or Y the case?’ [or does the pottery at Abydos date to either the third millennium or the first?], then he has an empirical advantage, at least in some small degree. And if he asks ‘Was X or not-X, Y or not-Y, Z or not-Z . . . the

28 Ruben Able, *Man is the Measure* (NY 1976), p. 37
case?’ and if he designs \( X \), and \( Y \), and \( Z \) in such a way that his own preferences are neutralized, and if he leaves the way open to refinements in the form of \( X_1 \), \( Y_1 \), \( Z_1 \), and if he allows for still other unexpected possibilities, then the probability of empirical accuracy is still further enhanced.

“A historian, like any other researcher, has a vested interest in answering his own questions. His job is at stake, and his reputation, and most important, his self-respect. If he substitutes a declarative [sentence, say, that whatever is found at Abydos dates from early third millennium Egypt] for an interrogative statement [that what is found at Abydos is to be dated in terms of the shape and style of the pottery and to allow that evidence to act as criteria for dating Abydos], then the result is literally a foregone conclusion. The best will in the world won’t suffice to keep him honest.

“In historical writing, declarative questions tend to be mimetic [imitative] questions, with a frequency which calls to mind an epigram (variously attributed to Max Beerbohm and Herbert Asquith), that whether or not history repeats itself, historians repeat each other. If historical research were as empirical as it can be, then we might hope to see very large heuristic [guiding to discovery] hypotheses put to very small controlled tests. Now and then a study appears which suggests that this dream is more than merely a mad utopian fancy. But many historians follow a different method. In common practice, a general interpretation is fashioned by an essayist [or researcher] not as a heuristic hypothesis but as an affirmative proposition [as did Petrie at Abydos]. In the next twenty years or so, a legion of gradgrinds manufacture monographs which reify the essay [or research], with a few inconsequential changes.”

In addition to all this, 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 millennium B.C., but would certainly be true for the first. In this respect W. Stevenson Smith discusses the fact that the Old Kingdom 5th Dynasty employed the hieroglyph “Haunebut” which refers to the Greeks of the Aegean. He states “that it is unlikely that the Aegean or its inhabitants were meant

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29 Fischer, *op. cit.*, pp. 24-25
by the term ‘Haunebut’ in the Pyramid Texts or in the inscriptions of the time of Cheops and Sahure.”

This evidence was pointed out to me by Emmet Sweeney in an unbound book, *The Pyramid Age*, 2nd ed. (2002), p. 11. In essence the Old Kingdom Egyptians had pottery that was Greek, as admitted by Petrie, but then denied by him. They also had inscriptions with the hieroglyph for the Greeks but this was denied as well. What the Egyptologists have done is invent excuses to deny the very facts of the methodology that they employed to establish their chronological framework when these facts contradicted it. Pottery found at Abydos that they recognized as Greek from the Late Helladic they said could not be Greek. Pyramid texts that used the hieroglyph “Haunebut”, which they knew referred to the Greeks, they said could not refer to the Greeks. Rather than let the historical evidence speak for itself, the Egyptologists *a priori* determined that their chronological structure meant Aegean pottery was not Aegean pottery and “Haunebut” which means Greeks or Greece of the Aegean referred to some other completely unknown and unidentified people. The historical evidence clearly referred to the first millennium B.C. but the historians claimed it had to fit the third. No interrogative questions or analyses were presented to test these questions, only declarative statements were evoked to reinforce their chronological expectations.

In addition to using the name “Haunebut” or Greeks in the Old Kingdom, we also find the following item in *SIS Chronology and Catastrophism Workshop*, Number 2, June 2003, p. 8:

“THE GREAT PYRAMID (Channel 4, 28.4.03 [April 28, 2003]). Peter Fairlie-Clarke writes:

“The local [Egyptologist] expert, Dr Zahi Hawass, said that graffiti written by one of the Great Pyramid builders showed that the workforce, divided into units of 200, were called by the Greek word for tribe. It did not seem to occur to him that there was anything odd about this use of a Greek word 4,500 years ago; that being the construction date mentioned earlier in the programme.”

Thus we have Old Kingdom pottery of a style dated to the first millennium as well as the Old Kingdom people writing of the Haunebut/Greeks and

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also using a Greek word for 200-man work-teams. The contradiction to the established chronology is thus rather direct.

So far we have not dealt with the scientific aspects of pottery but rather the historical, interpretive aspects. Yes, it may be possible, but it is highly improbable that the early 1st Dynasty Egyptians would have developed shapes and styles of pottery that never existed in their country until over 1000 years had passed, and these forms were produced outside Egypt and were then imported. But there is scientific evidence that denies even this highly improbable possibility. That evidence comes from faience and tin.

Old Kingdom Egypt and Sumerian civilization in Mesopotamia are dated to the third millennium B.C. as they are related by pottery dating. The problem that Dayton points to is that although the so-called Sumerians have tin bronzes, there was no known source of tin to produce these bronze objects. In Egypt the Bronze Age can only begin after 1100 B.C., but the sources of the tin required to make tin bronze or tin faience have also not been discovered. While Mesopotamia’s Bronze Age is over 1000 years out of chronological connection with its source of tin, Egypt is 400 to 500 years out of chronological connection with its source of tin. Furthermore, the Old Kingdom Egyptians were producing forms of faience before the metal ores necessary for their production were being exploited in the Near East. In essence, Dayton shows that the Old Kingdom faiences are displaced by about 2000 years and that the Middle Kingdom was manufacturing tin faience and tin bronze hundreds of years before they ever had tin!

There are two methods (and perhaps more) by which pottery may be glazed. One is to bake the clay pot and then paint it with a glazing material, the other to add glazing materials to the clay so that when the pottery is fired in the kiln the glaze in the clay gives the surface a sheen. The glazes in the ancient world often came from a mixture of certain ores with clay that when fired were self-glazing. Dayton points out “In fact it is during the Second Intermediate Period [1786-1567 B.C.] that faience production really begins in Egypt. . . .”

The problem related to faience pottery found at Abydos and other Old Kingdom sites is that certain metals were part of the mixture of these relics. Production of tin faience and tin bronze in the Middle Kingdom unquestionably requires that these ancient Egyptians had to have a source of tin somewhere near enough to be mined or imported, and that these tin deposits were exploited during

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31 Dayton, *op. cit.*, p. 21
the second millennium B.C. Dayton spends a great deal of time at the beginning of his 1978 book to show that there were no tin deposits available in the Near East to supply Egypt or Mesopotamia with this metal. Over twenty years later, the problem of tin faiences as well as bronze (which of course contains tin) in Mesopotamian and Egyptian Middle Kingdom sites persists. Lloyd Weeks, in Antiquity for March 1999, discusses the “tin problem” as it still exists:

“Geological research has yet to locate substantial tin deposits in Anatolia [Turkey].

“Alleged tin deposits in other areas of Western Asia have largely failed to withstand the scrutiny of modern geological research . . . no tin deposits exist in Syria or the Levant. . . .

“Thus the majority of regions of Western Asia have no geologically-verified tin deposits, and those that are known show no archaeological evidence of Bronze Age exploitation and are located in areas where the local metallurgy does not incorporate production of bronze alloys.

“This discrepancy is the crux of the ‘tin problem’ as formulated by archaeologists.”

He goes on to add to his list that “geological research has proven that tin deposits do not exist in Dilmun [thought to be located in Southern Arabia] or in the Indus Valley.” Therefore, it is quite evident that tin faiences and bronzes (in particular with more than two percent of tin) could not have been manufactured prior to the Second Intermediate Period in Egypt or after 1567 B.C. Nevertheless, Dayton points out that

“. . . it soon became apparent that . . . glazes could not be considered out of their ‘alleged’ chronological context. Anachronisms rapidly became apparent. The appearance of trace elements of the metals used to colour the glazes [of pottery], others to form or modify them, immediately raised problems of geology. From geology and the location of ore deposits it was but a short step to trade routes.

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33 ibid., p. 51
“It was rapidly found that the technical and geological evidence [related to these metals] was in direct contradiction to archaeological theories, some of which had been accepted blindly and uncritically for over a hundred years. As long as there was an authority to quote, then that was a good enough foundation upon which to build a whole ‘card-house’. . . . Myths as to the use of tin . . . in early glazes had no analytical fact to support them. . . .

“Inevitably the occurrence of anachronistic objects, hundreds of years out of context, raised serious doubts as to the secure provenance of the objects themselves. What is more, chronology reared its head. The more absurd the technological appearance . . . often a thousand years earlier in one part of the Near East than another a hundred or so miles away, the more fanatically the art historians defended them. Traditions die hard, and the good old Victorian attitude of keeping the scientist in his place as a glorified lab assistant still prevails [as with radiocarbon dating]. . . .

“Convenient explanations existed for the tin bronzes: they came from the Caucasus, a conveniently remote area little known, or they came from tin deposits in Turkey or behind Byblos now worked out without [leaving] a trace—a geological impossibility. Both these myths were traced back to their origins, the Caucasus theory to a letter written to Palmerston in 1837 and quoted as gospel ever since, which shows the power of the printed word!”34 [Historians repeat each other!]

Herodotus admits that the tin came to the Aegean region from far outside the Near Eastern and Greek world:

“. . . nor do I know anything of the existence of islands called the Tin Islands, whence we get our tin. In the first place, the name, Eridanus [a river which supposedly flows into the northern sea of Europe] is obviously not foreign, but Greek, and was [probably] invented by some poet or other; and, secondly, in spite of my efforts to do so, I have never found anyone who could give me first-hand information of the existence of a sea beyond Europe to the north and

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34 Dayton, *op.cit.*, p. 5
west. Yet it cannot be disputed that tin and amber do come to us from what one might call the ends of the earth.”

It seems apparent that Herodotus is discussing the tin mines of Cornwall, England. And surely it is inconceivable that in 1500 B.C. Egypt was trading with England, or even Spain, or with Bohemia, to obtain tin. With all these sources seemingly closed, historians began to invent one source after another based on little other than hearsay created by themselves and repeated over and over to each other and to the unwary student of the subject.

Petrie states that the tin in the Near East came from “surface sources” which “have been exhausted.” Jacquetta Hawkes invents the following story about the origin of tin:

“The second source of uncertainty is past and present ignorance about tin itself. There are no ancient accounts to tell us anything about where it was obtained or how it was prepared. . . . Casserite or tinstone occurs in quartz veins in volcanic rocks, where it may be associated with gold. Both ores were often washed out and redeposited by streams. IT SEEMS that men seeking the bright specks of gold in the streams came across heavy dark nuggets of tin and recognized them as metallic. . . .”

Dayton cites other historians who suggest various other sources of tin for export to Egypt after the Second Intermediate Period, showing that these sources are fictions. Hawkes’ streams have to get their nuggets of tin from somewhere, but where that somewhere is, is left to one’s imagination. The historical fallacy involved in assuming as a fact a source of tin for the Egyptian Middle Kingdom is based on nothing. It is what Fischer warned historians about, calling it

“The fallacy of the negative proof [which] is an attempt to sustain a factual proposition [such as that tin sources were available to the third millennium Sumerians and Middle Kingdom Egypt] by negative evidence. It occurs whenever a historian declares that ‘there is no evidence that X [tin] is the case,’ and then proceeds to affirm or assume that not-X [there was tin] is the case. He may have spent all the years

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36 Petrie, op.cit., p. 101
37 Hawkes, op.cit., p. 108
of his youth in the Antiquarian Society, seeking the holy $X$ and never finding it. He may have examined every relevant scrap of evidence in every remote repository, without reward. He and every other reasoning being on this planet may know in their bones that not-$X$ [there was tin available to Middle Kingdom Egypt] is the case. But a simple statement that ‘there is no evidence of $X$ ’ [tin] means precisely what it says—no evidence. The only correct empirical procedure is to find affirmative evidence of not-$X$ [there was tin available]—which is often difficult, but never . . . impossible [if tin was then really available].”

As Fischer further states:

“Not knowing that something exists is simply not knowing. One thinks of Alice and the White Knight [sic]:

“‘I see nobody on the road,’ said Alice.

“‘I only wish I had such eyes,’ the King remarked in a fretful tone. ‘To be able to see Nobody! And at that distance, too!’”

What appears obvious to even the disinterested observer is that the Old Kingdom pottery at Abydos could never have been manufactured from around 3000 to 2200 B.C. All the various forms of evidence deny this possibility. In terms of provenance, the pottery Petrie found at Abydos was not made by the Egyptians, neither during the Old Kingdom nor later. It was foreign, coming from Mycenaean, Aegean, and Syrian sources. The Egyptians of the Old Kingdom wrote about the Greeks of the first millennium B.C. and used the Greek word for “tribe” before the Greeks ever existed. But, more destructive to the established chronology, it did not date to the times of the Old Kingdom in terms of its styles and designs. These styles and designs were only produced during the mid to late second millennium and not the third. Based on the very methodology pottery dating employs, the foundation of styles and designs was misapplied to establish and conform to the chronology that Petrie believed was absolutely correct. Therefore, he may have felt that what he did by dating the pottery at Abydos to the third millennium was valid. Based on the

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38 Fischer, *op.cit.* p. 47
39 *ibid.*, p. 48
revised chronology employing no Dark Age of Greece, this pottery would be dated to the first millennium B.C.

Nevertheless, it was upon this error that pottery dating was force-fitted to Petrie’s and every other Egyptologist’s and archaeologist’s chronological belief system. As Dayton expresses it:

“. . . it became clear that Petrie, one of the pioneers of Egyptian archaeology and on whose work in the 1890s so much modern chronology rested, had had a fatal idée fixe of a high chronology and had, on scanty evidence . . . dated all the objects in the deposits of Abydos and Hierakonpolis to the 1st Dynasty or earlier, thus causing much of the archaeology of the Near East that has been built up on the evidence of the important objects in these deposits to be founded on a profound error.

“In a similar way archaeology in the Near East has built up a card-house of inter-related ‘facts’ by the stylistic comparisons of artefacts from one area to the next, each system building upon the flimsy evidence of the past—often, indeed, on a single find of doubtful date and stratigraphy . . . Unfortunately, the myths in this vast corpus are almost self-perpetuating while providing rich pastures for studies in minutiae. Few have had the courage or the time, or for that matter the overall knowledge, with the ‘quasi-science’ split into so many specialized compartments and tiny geographical areas, to question the very bases. The tombstones [destroyed careers] of those rash souls who have questioned the fundamentals lie scattered along the dusty by-ways of history, forgotten and unlamented. . . .

“When the foundations of ‘the art’ were being laid, and even down to recent times, great hordes of native workmen, often five hundred strong, were employed to shovel and pick, and clear rooms and temples. Admirable attempts to record and plan were made by the early excavators, usually short of time and in dreadful conditions, and always short of staff and money. It is unfortunate that odd intrusive finds discovered under such conditions have been given, in many cases, a fatal and dangerous prominence in the general edifice, and then by
being cross-referenced to other sites and regions, perpetuating and extending the error [that Petrie made].”

But Stiebing and other historians and archaeologists will claim that pottery dating is so well confirmed and so well established that one cannot greatly revise the history of Egypt. Yet they never mention the fact that the design and style of Old Kingdom pottery does not belong to Egyptian ware or that it dates directly to the Late Helladic Period of 800 to 600 B.C., i.e. to the first millennium B.C. and not to the third.

In a letter of reply to Stiebing’s criticism of Velikovsky’s shortened chronology, John Bimson and Peter James pointed out that pottery dating in Palestine does not support the established chronology:

“As Stiebing states, finds of Assyrian objects in Palestinian strata are used to date the Iron Age, phase II . . . Yet . . . it is clear that the [pottery] dates assigned to the Iron Age phases are much too high.

“. . . ‘Assyrian Palace Ware’ from Samaria and Tell el-Far’ah North is currently dated to c. 720 B.C., it now appears that such a date is roughly a century too early. J.S. Holladay notes that similar pottery from two groups at Nimrud [Assyria] ‘is to be dated very closely to 612 B.C. [a hundred years or so after 720 B.C.] and later, instead of to the late eighth and early seventh centuries B.C.’ (J.S. Holladay et al. (eds), *Magnalia Dei* (1976), p. 282, note 59) Such forms are ‘actually post-Assyrian in date [i.e. not datable to Assyrian times] . . . [Furthermore i]mported pottery of this type was also found at Tarsus in Cilicia in the Early Iron Age levels . . . conventionally dated to c. 1100-850 B.C. (H. Goldman, *Excavations at Gözlü Kale, Tarsus*, Vol. III, “The Iron Age” (Princeton, 1963), pp. 20, 91-92)”

That is, Stiebing’s pottery date for the Assyrian ware used for dating Palestine has been shown to be in error. The pottery belongs closer to the present and not to Assyrian times at all, while at Tarsus the pottery dated prior to Assyrian times only occurs in Assyria 250 to 500 years later. Bimson and James go on to produce several other examples from pottery that are thoroughly out of sync with

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40 Dayton, *op.cit.*, p. 6
what Stiebing has presented about dating Palestine by Assyrian pottery. Stiebing has failed to publish any response to these negations of his work.

Also destructive to their chronology is the evidence of faiences found at Old Kingdom sites. It is abundantly obvious that these early ancient Egyptians did not have the metals available to manufacture faiences. This form of analysis is the specialty of Dayton, and he was the one to expose these facts. Again, he is given space to speak of these scientific; empirical facts:

“Fortunately for the archaeologist, the Near East is not rich in certain raw materials. Tin is one of the rare metals absent from the region. . . . The appearance of . . . [tin] in the Near East [could be employed and] fitted into . . . [another] fairly logical pattern of [pottery dating based on] technological progress which could [also] be followed with the development of wheel-made pottery, painting techniques on pottery, and the use of the kiln and reducing techniques.

“It has therefore seemed just as valid to approach the archaeology and history of the Near East and the Aegean from the basis of the hard facts of analysis and geology, coupled with technology, as from the very subjective methodology of the art historian, the first appearance of glazing and glass being as important as that of copper, bronze and iron.

“From the evidence provided by analysis and technology it soon became apparent that the dating of many sites and periods was seriously wrong . . . the dates have tended to become lower and lower . . . after the fashion of athletic records.”

Tin is used, according to Dayton, after the time of the Second Intermediate Period, 1790-1567 B.C. in the conventional chronology. Therefore, at the very least, the faiences at Abydos have to be dated not to around 2800-2900 B.C., but a thousand or more years closer to the present. Lucas mentions that the “earliest reference . . . to tin that can be found . . . [is] in the Harris Papyrus, an Egyptian document of the Twentieth Dynasty (1200 B.C. to 1090 B.C.)”. This is based on the accepted established chronology. Lucas also shows that the first tin object in

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42 Dayton, op.cit., p. 7
Egypt “probably dates from about 600-700 B.C.” This makes excellent sense in terms of the shortened chronology.

In Mesopotamia as well, tin becomes available after 1100 B.C. The style of the pottery even requires that the date of Abydos is about two thousand years closer to the present and the fact that the Old Kingdom knew the Greeks of the first millennium B.C. points to the very same state of affairs. It must be pointed out that this is all based on the very chronology with which establishment historians have erected their historical edifice. Since that edifice is so deeply steeped in error, one cannot properly assign the time of the Second Intermediate Period to any time, and revisionists do not have to adjust their revised historical chronologies so as to comply with that chronology which is so ridden with error. Based on this data this author will maintain that the pottery Petrie found at Abydos is from the first millennium B.C. Rose’s work on Sothic dating lends itself to this analysis. Dayton holds to the established chronology somewhat more than the present revisionists this author supports.

The vision that pottery dating can be employed without the immense corrections indicated by Dayton’s work disqualifies it (in its present form) as a methodology for arguing against the revisionist work of Heinsohn, Rose, and Sweeney, and to some extent Velikovsky. The Egyptologists have merely arranged the pottery forms in an order to fit their myopic vision. Their failure to accept the implications of Dayton’s brilliant scientific, empirical, technological findings reflects quite directly what Lord Joseph Lister (who introduced antiseptic surgery) himself found regarding evidence and eyesight:

“I remember at an early period of my own life showing to a man of high reputation as a teacher some matters which I happened to have observed. And I was very much struck and grieved to find that, while all the facts lay equally clear before him, only those which squared with his previous theories seemed to affect his organs of vision.”

So it is with pottery dating.

Lehner has asked if erosion on the Sphinx is sufficient to overturn conventional chronology. But now there is scientific evidence from astronomy, namely Sothic dating, and pottery dating based on style and shape, as well as tin

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44 ibid.
45 quoted in Youngson, op. cit., p. 163
bronze and faience, that contradicts this chronological edifice in no uncertain terms and I hasten to point out to Lehner, Stiebing, and their colleagues who uphold that chronology, that the factual evidence that overthrows that chronology is not only scientifically empirical but is also based on the very methodologies they themselves have erected to construct their history.

Furthermore, related to tin, Hawkes admits “During the Middle Kingdom at a time when true bronze had come into full use in Mesopotamia, it was still exceptional in the Nile valley.” That is, some true bronze objects existed prior to the Third Intermediate Period in Egypt. Without trade (known not to have existed), how did the Egyptians produce true tin bronze objects before the introduction of tin?

As for the Old Kingdom, James D. Muhly points out that “Very few analyzed Old Kingdom artefacts are of tin-bronze but there are several, and perhaps further analyses would increase the number.” In essence, we may have true tin bronzes in the Old and Middle Kingdoms before tin was available to the Egyptians.

Thus we can say that the Old Kingdom also belongs to the Bronze Age as do the so-called Sumerians of Mesopotamia. The Old Kingdom having tin bronze is, on the basis of the availability of this metal, at least over 1500 years or more too old. On the basis of Old Kingdom pottery design we have the same problem. On the basis of the Old Kingdom knowing the Greeks and using a Greek term we have the same problem. All these forms of evidence place the Old Kingdom Egyptians in the first millennium B.C. and not the third. The most important form, of course, is the scientific metallurgical evidence.

Rose’s analysis is derived directly from the astronomical historical work they themselves employ. Dayton’s analysis is also derived directly from the technological, geological, as well as stylistic analyses they have also organized. Taken together with erosion on the Sphinx and Valley Temple and the fact that radiocarbon dating of materials will always give older dates for the various materials discussed above, we have a growing number of scientific facts that demand a drastic lowering of Egyptian chronology.

46 J. Hawkes, op.cit., p. 366
We will return to pottery dating in the chapter on stratigraphy in Mesopotamia to expose further major errors in the methodology.

The assumption that tin was available in Egypt from some unknown source when geological research proves that tin was imported to the region after 1100 B.C., poses the problem to prove it was available at the time the established chronology claims it was; this requires proof of its source and that tin mining was carried out at that time. Assuming it was imported at that time is assuming what has to be proven. That has never been done. Saying the people who left these artefacts were able to obtain tin somewhere, somehow, is a leap of faith not based on fact.

With respect to the 12th Dynasty, dated from 1991 to 1782 B.C., not only does Rose’s astronomical evidence place that dynasty in the first millennium B.C., but so too does the question of bronze artefacts. According to William C. Hayes, razors dated to the 12th Dynasty have “blades . . . of hammered bronze . . .” On the same page, Hayes informs us that these tin-bronze razor blades contain “A ten per cent copper-tin alloy.” He reports on that page a “second pair of razors [are] entirely of bronze.” He points out at a 12th Dynasty site “A knife from el Lisht, . . . the property of a scribe . . . Both handle and blade are of bronze riveted together, and the former has a hooked shape characteristic of knives which in the New Kingdom we know were owned by scribes.” He speaks of a “handle of a bronze mirror of the late Twelfth Dynasty.” And of a “folding stool from a tomb of the twelfth Dynasty at Mir, “. . . [with] bronze rivets which hold the legs together.”

There is no source of bronze that is known to exist in or around Egypt at that time to produce tin bronzes. Tin does not become available for several hundred years, or after 1100 B.C. Thus, not only does Rose’s astronomical data require that the 12th Dynasty must be moved into the first millennium B.C., so too does the evidence of tin bronzes suggest the same. When we come to a discussion of iron in ancient Egypt, we will find that this metal also requires that the 12th Dynasty be placed in the first millennium B.C.

And yet, once again, the scientific method of testability rears its head. Tin faïences and tin bronzes are found at sites attributed to the Old and Middle

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49 *ibid.*, p. 296
50 *ibid.*, p. 241
51 *ibid.*, p. 258
Kingdoms prior to the time historians claim that tin was available. One cannot manufacture tin faïences and tin bronzes without tin. The geological fact that tin was not available in that part of the world, aligned with the technological evidence that these Egyptians were making tin faïences and bronzes, defies the established chronology. Since these products were made with tin, they could only have been made when tin became available.

The contradiction of science, in the form of geology and technology, to the established chronology is direct and considerable. Manethon’s chronology has been falsified yet again by scientific evidence. If that chronology was valid it would have passed the test and proved scientifically and technologically where tin came from during the Old and Middle Kingdoms. It failed that test and that failure does not uphold Manethon’s chronological framework. Rather the test upholds the much shortened chronology of the revisionists. We will return to this problem in other contexts below to show that it relates not only to Egyptian chronology, but to Mesopotamian and Hittite Anatolian chronology as well.
Stratigraphy deals with the materials of mankind found in the soil. In Egypt, unlike Mesopotamia, cities were not built one on top of the other. Various pharaohs, according to Manethon, ruled for a period of time at a particular capital and then the kingship might be taken over by another pharaoh and those who followed him (or her, in rare instances), moved to another capital city. If Manethon and those proponents of the long chronology are basically correct, then pottery and other artefacts uncovered at a particular city should in large measure constitute the material of the time that city thrived. Most of these materials have been unearthed at grave sites and thus represent a time capsule of that era. What one would expect to find in these cemeteries and other sites are the relics that reflect several generations from the past which are relatively close together in time, covering perhaps a few hundred years. At another site, supposedly founded closer to the present, we would expect to find few, if any, relics of the older period, and certainly Old Kingdom sites should not contain relics of the Middle or New Kingdoms. Cemeteries and other sites that date to 2500 B.C. should not contain relics that date to 700 B.C. This, in a general sense, is what the stratigraphy of Egypt would exhibit if the dynasties ruled one after the other over about 3000 years.

If, however, the dynasties ruled contemporaneously in different districts over a far shorter period of time, then the relics found at the various sites in Egypt should show a very different spread in time. That is, sites would have pottery and other artefacts that are from several different dynasties—in terms of the established chronology—that are from supposedly very different times. If Manethon’s serial chronology is correct, then Old Kingdom sites would contain Old Kingdom artefacts, Middle Kingdom sites would contain mainly Middle Kingdom artefacts with very few from the Old Kingdom, and New Kingdom sites would contain mainly New Kingdom artefacts with very few from the Middle Kingdom and fewer yet, if any at all, from the Old Kingdom.

But if many dynasties ruled at the same time, the sites would show this: Old, Middle, and New Kingdom sites would have a considerable mixture of artefacts from all three kingdoms, not from every dynasty, but from particular Old, Middle,
and New Kingdom dynasties that co-reigned around the same time. What does the archaeological evidence indicate regarding these associations of relics in Egypt?

Petrie in his 1904 book, *Methods and Aims in Archaeology*, embodied the methodology of sequence dating in terms of developing a chronological sequence as outlined above:

“Let us suppose some old country mansion, where it has been the habit to close permanently any room in which the owner has died and leave everything in it undisturbed. If we went through such a series of rooms we could not doubt their order of date, if we looked at their contents. The William IV room could not be put to the middle of George III’s reign; the George II room could not be supposed to go between those of James II and Anne. Each room full of furniture would have some links of style with that of the generation before, and of the generation after it, and no doubt could exist as to the sequence of the whole series. What is true of a room full of furniture is equally true of a grave full of pottery.”¹

This sequence dating is also one of the foundations by which the Egyptologists established their chronology. The question, of course, is: was this sequence dating of the 31 dynasties built up on clear-cut, unimpeachable grounds or, like Petrie’s error at Abydos, were the Egyptologists, including Petrie, blinded by their allegiance to Manethon’s chronology and did they therefore fail to observe that the pottery and other artefacts, in the various cemeteries they excavated in Egypt, presented a totally different picture of the chronological sequence, one that contradicted Manethon but indicated that various dynasties ruled around the same time?

Although Dayton has presented us with clear-cut evidence of the misapplication of pottery dating, his chapters on Egyptian archaeological stratigraphy are a *tour de force* that supports and corroborates Rose’s, Heinsohn’s, Sweeney’s, and Velikovsky’s, highly shortened chronology. In his chapter “Faience in the Third Millennium” he writes:

“In the Badarian [pre-dynastic] cemeteries of Egypt (c. 4200 B.C.) beads of blue-glazed steatite were found together with beads of real turquoise. . . .

“Most writers state quite uncritically that glazed faience appears in Egypt in the Pre-Dynastic Period—i.e., before 3000 B.C. . . .

“However, it is clear that Beck was not happy about the attribution of some of the faience beads and queries the Pre-dynastic beads from cemetery 3700 where blue beads ‘from another grave of the same number have a different technique’ [of manufacture]. . . . Other beads at Qau and Badari range [in age] from the IVth, XIth, XVIIIth and even XXVIth Dynasties.”

At these Old Kingdom sites the archaeologists found beads from the 6th Dynasty, dating from 2345-2181 B.C., from the 12th Dynasty, supposedly dating from 1991-1782 B.C., from the 18th Dynasty, dating from 1570-1293 B.C., and from the 26th Dynasty, dating from 664-525 B.C. Beads, like pottery, can be identified by their size, shape, style, etc., as organized by the archaeologists. Yet here we have, in supposedly pre-dynastic to around early dynastic times, sites with artefacts that came from four different dynasties from the Old, Middle, and New Kingdoms.

Speaking of other materials found at the main Old Kingdom deposit of Hierakonpolis, Dayton points out further anomalous artefacts:

“Mrs. Adams was able to study Green’s diaries in Cambridge, wherein Green privately admits that an XVIIIth Dynasty scarab was found with part of the archaic objects of the Main Deposit. Green believed he had good reason for his private view* (*Dayton’s footnote: ’It is not always politic if one needs to eat to quarrel with the Establishment [of Egyptology] in English academic circles.’) that the [supposedly Old Kingdom] Main Deposit was [really] made in the Middle Kingdom. On technical grounds . . . the writer [Dayton] would agree with this view, but (as will be explained below) favours the New Kingdom date of Thothmes III.

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2 Dayton, *op. cit.*, p. 195
“When one considers that found in this Main Deposit were the Palette of Narmer [dating to around 3150-3050 B.C.], the famous copper statues of Pepi I [third king of the 6th Dynasty, dating to around 2345-2181 B.C.], a faience plaque of Thothmes III [Thutmosis III, fourth king of the 18th Dynasty, dating from around 1506-1450 B.C.], a vase with the cartouche [the king’s name in an oblong circle] of Necho of the XXVIth Dynasty [dating from around 664-525 B.C.], a late XVIIIth Dynasty inscription and the seated statue of Inyotef, probably of the XIth dynasty [from around 2134-2069 B.C.], one can only agree with Professor Smith that ‘unless these doubts can be resolved, no object from the Main Deposit can be safely attributed to an early date’.”

Buried in the same deposit at Hierakonpolis, which modern archaeologists, Egyptologists, and general historians date specifically to the Old Kingdom, are artefacts that undeniably come from dynasties supposedly as far apart in time as 3150 to 525 B.C. Archaeologists have concluded that relics of the past buried in the same place at the same level in the strata are contemporaneous or closely so, unless it can be specifically proven that at a later time constructions or excavations were carried out that would account for these anomalous artefacts being found together. We have no evidence that the site discussed was refurbished. What we have quite clearly is solid evidence that several dynasties, namely the 1st, 6th, probably the 11th, 18th, and 26th, reigned around the same time, and left easily datable artefacts behind that were placed in the main deposit at Hierakonpolis. These relics would not have been found at one site in the same deposit and level unless these dynasties reigned relatively closely to each other in time or were contemporaneous with one another.

If Rose’s, Heinsohn’s, Sweeney’s, and to a degree Velikovsky’s great shortening of Egyptian chronology is correct, then these finds at Hierakonpolis are excellent corroboration and confirmation of their theories.

Nevertheless, one might argue that Hierakonpolis is itself an anomaly and that the other sites in Egypt do not show this type of anachronistic range of artefacts, but this is simply not the case. Dayton presents the same case at several other sites. In Chapter 16 he presents similar evidence related to “Egyptian Ist Dynasty Pottery.” In Chapter 17, he does the same for “The Egyptian Middle

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3 ibid., p. 196
In each and every site that he discussed are found artefacts from several dynasties from the Old, Middle, and New Kingdoms. The archaeological contradictions to the established chronology could not be more destructive to that chronology and more supportive and corroborative of Rose’s, Heinsohn’s, Sweeney’s, and in part to Velikovsky’s highly shortened chronologies. This evidence taken together with Sphinx erosion, Sothic dating, radiocarbon dating, and pottery dating once again points in the same direction—to a highly shortened chronology. On the basis of archaeology, the long chronology of Egypt and the rest of the Near East cannot be sustained.

One final note before moving on. How did the Egyptological establishment deal with Dayton and his revolutionary scientific-empirical evidence? P. John Crowe describes that response.

“In 1978, a major publication . . . by John Dayton must be mentioned, entitled Minerals, Metals, Glazing and Man. . . . this was reviewed enthusiastically by James in SISR 3:4, who said ‘. . . Dayton develops his work, which started out as a simple study of glazing technology . . . into a massive and devastating attack on traditional chronology . . . In challenging the accepted dates and synchronisms of the . . . Bronze Ages, his work is potentially more disruptive than [Velikovsky’s] Ages in Chaos itself.’ In 1971, when a student at the London Institute of Archaeology, Dayton wrote a paper in World Archaeology on ‘The Problem of Tin in the Ancient World.’ This demonstrated that metallurgy and related arts spread along trade routes from Europe into the Near East, not from East to West, as is the popular view. The Bronze Age, contrary to current dogma, could not have started in Mesopotamia, where the required metals were absent. By exposing so much establishment dogma about the sources of metal ores, metallurgy, and their use in bronze and iron artefacts as false and misleading, he caused great annoyance to the English establishment. Their response, since they were powerless to avenge themselves on
Dayton, was to take steps that led to the closure of the university section that had fostered him.”

In no uncertain terms, the establishment has decided that there cannot be any revision of the chronology that they have erected. The behavior of the English historians in taking steps to shut down the section of the university that allowed Dayton to expose the scientific evidence, so destructive to their dogma, reflects the kind of Big-Stick policy of Theodore Roosevelt who would brook no foreign opposition to his imperialist policy. The historical establishment also will not brook opposition to their own chronological dogma and their behavior toward Dayton’s university was a stern warning that what they have established vis-à-vis Egyptian chronology is not to be challenged. This prohibition is almost religious in nature and does not reflect an open and honest scholarly approach to historical truth. It is rather a closed, dishonorable, and unscholarly attempt to squash dissent. Their behavior reflects the religious warning found in the Book of Revelation 22:18-19:

“I testify unto every man that heareth the words of the prophecy of this book, If any man shall add unto these things, God shall add unto him the plagues that are written in this book: And if any man shall take away from the words of the book of this prophecy, God shall take away his part out of the book of life, and out of the holy city, and from the things which are written in this book.”

Even Michael Polanyi, the great proponent of the establishment’s dogma and of academics and scientists as honorable, open and honest researchers, would have been revolted by this attempt to stifle dissent.

“Any attempt to gain complete control of thought by explicit rules [or actions] is self-contradictory, systematically misleading, and culturally destructive.”

Make no mistake about it, as will be shown below, Stalinist tactics will be employed against anyone in an establishment institution who attempts to give voice to evidence that refutes its rigid chronology, if the establishment has the power to punish that person or institution. Those who maintain that stratigraphy in Egypt contradicts the highly shortened chronology and supports the chronology of the

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5 Fischer, op. cit., p. XVI
establishment have failed to understand what that stratigraphy quite clearly indicates.

The question this evidence presents is: What is the statistical probability that at dozens of sites examined by Dayton, there should generally be found relics from dynasties of the Old, Middle, and New Kingdoms? This is exceedingly improbable based on the established chronology.

Dayton, using the methodology established by Petrie, found throughout Egypt, that wherever excavations were carried out, artefacts of pottery and other items did not correspond with Manethon’s chronology. It would be similar to finding in the mansion, earlier described by Petrie, in the William IV room materials from the middle of George III’s reign and George II’s room as well as that of James II and Anne. By wearing the blinders of Manethon, historians saw only what they wanted to see. This myopic analysis is what Fischer calls the

“Fallacies of statistical sampling [which] occur in generalizations which rest upon an insufficient body of data—upon a ‘sample’ which misrepresents the composition of the object in question. Sound methods of sampling have been satisfactorily discussed at length by statisticians with a degree of clarity and accuracy which no historian can hope to attain. But when it comes to unsound sampling, historians qualify as experts, by reason of their long experience in the ways of error. Rarely have so many generalized so much from so little as have impressionistic historians in the past generation.

“One example is an assertion, sometimes made by historians of the framing of the United States Constitution, that there was a significant age difference between Federalists and Antifederalists. A recent statement of this thesis is by Stanley Elkins and Eric McKitrick, in The Founding Fathers: Young Men of the Revolution (Washington 1962). Elkins and McKitrick attempted to develop a generational interpretation of the fight over the Constitution as an alternative to Charles Beard’s battered economic thesis [that those for the Constitution were of wealth and those against were of humbler means], as Beard himself said of another scholar, sans fear and sans research. Their argument rests upon a so-called sample of nine Federalists [that were on average younger than] . . . nine Antifederalists . . . [which Fischer presented, then went on to state:]
“The authors conclude that ‘the age difference between these two groups is especially striking. The Federalists were on the average ten to twelve years younger than the Anti-Federalists.’ The arithmetic is slovenly—the average (mean) difference is in fact 9.8 years. But more serious is the validity of the sample which is absurdly small and extremely biased.

“It is easy to invent a different list of equal length [nine Federalists and nine Anti-Federalists] which would support an opposite conclusion . . . [which is just what Fischer presents]

“These Antifederalists are, on average 13.9 years younger [not older as Elkins-McKitrick claim they should be] than the Federalists. . . . we [therefore] conclude ‘The age difference between these groups is especially striking. . .’”

So, too, with the various excavations of Egyptian cemeteries. The generalization that the various dynasties one followed the other for about 3000 years rests on an insufficient body of data—upon a ‘sample’ which misrepresents the composition of the age of the objects in question. Sound methods of sampling were not presented by the historians with the degree of clarity and accuracy necessary to support the Manethonian chronology. When it comes to unsound sampling, the historians who qualify as experts on these methods presented error rather than reasoned evidence.

Their assertion regarding the artefacts found in the various grave sites in ancient Egypt suggests there was a significant difference between the stylistic ages of the pottery relics etc. from each cemetery. Their argument rests upon their sample showing that each site contained relics that were generally much older or much younger than those of other sites and therefore of older or younger periods. But as Dayton carefully showed, their relic ages at each site were chosen in a manner that was slovenly. More serious is the validity of their sample at each of these burial sites, which omitted from serious consideration artefacts that contradicted their sample, which is absurdly biased. Only by consideration of a different list which includes all the objects of all ages and styles can one gather a balanced and logical understanding of the stratigraphy and chronology of Egypt. This in essence is what Dayton presented and which has been ignored for decades.

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6 Fischer, *op. cit.*, pp. 104-106
Dayton employed their methodology of sequence dating, using a valid sample from burial sites throughout Egypt and elsewhere, and correctly derived a chronological sequence of artefacts based on style and age that defied the established chronological paradigm. In so doing he clearly proved that their data based on an improper sample had misrepresented the evidence in question. He proved that when it comes to unsound sampling, historians of Egypt qualify as experts, by reason of their long experience in the ways of error such as Petrie’s error at Abydos. As Fischer concludes:

“Impressionistic sampling errors are sometimes so gross that nobody is taken in by them [especially when they are exposed as, say, by Dayton] . . . A Russian [Communist] historian, B.I. Bukharov, has used opinions expressed in the [Communist and biased newspaper] The Daily Worker as representative of American opinion. . . . The difficulties entailed in [poor] statistical sampling have sometimes tended to make statistics generally suspect in the eyes of some historians, more than a few of whom are prepared to accept Disraeli’s famous statement that there are lies, damned lies, and statistics. But statistics themselves do not lie—only statisticians, and if they lie, they are apt to lie to themselves. . .”

When a full sample of artefacts from dozens of grave sites throughout Egypt exhibit elements that in style, age, etc. are from the Old, Middle, and New Kingdoms, that is simply too great a coincidence to occur by chance or by any other ad hoc explanation to dispose of these contradictions to the established chronology. The statistical probability that this great sampler should fit the shortened chronology of Heinsohn, Rose, Sweeney, and to some extent Velikovsky by chance is simply immensely small. Like all the evidence above that also fits their short chronology the statistical validity of their chronology being false now becomes extraordinarily small.

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7 ibid., p. 108
CHAPTER 7
IRON, DIORITE, AND OTHER HARD ROCK

Heinsohn cites Herodotus, the Greek historian, about the pyramids being built, and since hardened iron is needed to cut and engrave hard stone such as granite or diorite, the Egyptians of the Old Kingdom could not have built the Giza pyramids and others during the Copper Age. The only critic to deal with this scientific-technological aspect of Heinsohn’s work, Dwardu Cardona, has discussed this matter in *A Return to the Two Sargons and Their Successors*. That being the case, Cardona’s work will be cited in extenso. He writes:

“How the Egyptians were able to work these hard stones remains controversial. Copper tools are not hard enough to work granite and other hard stones, but as some have suggested, the Egyptians might have ‘mastered a process now lost of giving copper a very high temper, but this surmise has not yet been proved.’ . . . I.E.S. Edwards supplies other ways in which hard stone could have been worked by the Egyptians of the Old Kingdom, . . . as so, also, had Flinders Petrie before him, . . . but to be quite honest and fair to Heinsohn, none of these methods has been verified.

After discussing this problem of cutting and incising hard rock, Cardona asks:

“So how were the granite, and other hard-stone items in the pyramids dressed? That, I am afraid, remains something of a moot question.”

This issue, to which Edwards suggested the answer was “giving copper a very high temper” as a “surmise [that] has not yet been proven,” or Edwards and

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1 *AEON*, vol. V, no. 4 (July 1999), unpaginated. This author has paginated the material on iron as pages 45-48
2 *ibid.*, p. 45
3 *ibid.*, p. 47
Petrie hinting at “other ways in which hard stone could have been worked,” “none” of which has “been verified”, is not a “moot question”. These suggestions have nothing to support them and are therefore proof of nothing. As pointed out above by Fischer, this is the fallacy of the negative proof. Edwards, Petrie and every other historian theorist who posits a possible method to cut and incise hard rock with copper is simply wrong because they do not answer the problem with evidence, as Fischer has told us. “Not knowing that something exists [such as hard copper or hard iron to cut and incise hard rock in the Old Kingdom] is simply not knowing,” “that there is no evidence of $X$ [hard copper or hard iron] means precisely what it says—no evidence.”

But let us examine what the ad hoc, not validated, forms of rhetoric say. Do any of the various theories, bandied about by establishment historians, in any way come to grips with the problem? Of course not, as we will see. The early theories put forth by Petrie, as we will discover, are based on inventions that have been shown to be false. Exactly the same thing can also be stated of the inventions presented by more modern historians. They seem incapable of accepting “that there is no evidence means precisely what it says—no evidence.”

Cardona cites Peter Tompkins’ Secrets of the Great Pyramid as his source for Petrie’s explanation of how the hard stones were worked. Since this relates to the problem at hand, from Heinsohn’s view, let us examine Petrie’s arguments and reasons as mentioned by Petrie himself:

“The granite and hard stones were also sawn and cut with tubular drills. The saws were blades of copper, which carried [in the copper] the hard cutting points. The material was sand for working the softer stones, and emery for harder rocks. As far back as prehistoric times blocks of emery were used for grinding beads, and even a plummet [weight on a string] and a vase were cut out of emery rock (now in University College). There can be no doubt, therefore, of emery being known and used.

“The difficult question is whether the cutting material was used as loose powder, or was set in metal tools as separate teeth. An actual example was found at the pre-historic Greek palace at Tiryns. The hard limestone there had been sawn, and I found a broken bit of the saw left in a cut. The copper blade had rusted away to green carbonate; and with it were some little blocks of emery about a sixteenth of an inch
long, rectangular, and quite capable of being set, but far too large to act as a loose powder with a plain blade. On the Egyptian examples there are long grooves in the faces of the cuts of both saws and drills; and grooves may be made by working a loose powder. But, further, the groove certainly seems to run spirally around a core, which would show that it was cut by a single point; and where quartz and softer feldspar are cut through the groove floor runs on one level, and as the feldspar is worn down by general rubbing, the quartz is actually cut through to a greater depth than the softer feldspar. This shows that a fixed cutting point ploughed the groove, and not a loose powder. Also, the hieroglyphs on diorite bowls are ploughed out with a single cut of a fixed point, only one hundred and fiftieth of an inch wide, so it is certain that fixed cutting points were used for hand-graving. There is no doubt that sawing and grinding with loose powder was the general method, but the use of fixed stones seems clearly shown by the instances above."

Petrie adds that “large hieroglyphs on hard stone were cut by copper blades fed with emery, and sawn along the outline by hand.” Elsewhere, he states: “The material of these cutting points is yet undetermined, but only five substances are possible: beryl, topaz, chrysoberyl, corundum or sapphire and diamond.” W.B. Emery discusses a further difficulty:

"Another puzzle left us by the ancient craftsmen lies in the teeth of the [copper] saws and eye-holes of needles; in the case of the former, the missing portions of metal have been punched out and in the latter the hole has been punched through not drilled. The question is of what material was the punch. Presumably it must have been harder than the metal [copper] on which it was used."

Petrie has made it quite clear that the saw markings are indicative of blades that cut, not with powder, but with hard teeth. He also offered that diorite, an extremely hard rock, was engraved by an extraordinarily thin point, 150th of an inch thick. His list of minerals to accomplish these tasks includes “beryl, topaz,
chrysoberyl, corundum or sapphire and diamond.” But did the Old Kingdom craftsmen possess these necessary materials as teeth for saws or points for punches or graving tools? Barbara Mertz examined this question and explains:

“Saw marks have been found on the granite sarcophagus from the Great Pyramid and on basalt [another hard stone] pavement blocks from the temple of that pyramid, and drills were certainly used for hard stone statues and vases. It is true that cutting basalt with a plain copper saw would be somewhat difficult. Nowadays we sometimes cut hard stones by using points of even harder stones which are set into drill or saw. The use of diamond points in industry is well known. Diamonds rank 10 on a measuring device known as the Mohs scale; it can cut just about anything, including quartzite, the hardest stone the Egyptians quarried (7 on the Mohs scale). But sad to say, the Egyptians did not have diamonds. Neither did they have topaz (8) or rubies and sapphires (9) or even beryl (8) before the Greek period. We must conclude, then, that the Egyptians did not use hard stone points, or teeth to cut their granite and quartz.

“There is another method of cutting hard stone: with an abrasive powder. Diamond dust is used to cut diamonds. Having no diamonds, the Egyptians had no diamond dust, either, nor, so far as we can tell, did they use pumice or emery powder.”

“The gem forms of corundum [are] ruby [and] sapphire.” Thus it is quite clear that the Egyptians lacked corundum for teeth in saws or as an abrasive.

Along these same lines, A. Lucas writes:

“Although beryl occurs in Egypt, there is no evidence that it was known before the Greek epoch. It is highly improbable that it was ever obtained in the large quantities that would have been required had it been [used] for cutting stones. The other stones enumerated are not found in Egypt, and there is neither evidence not probability that they were used in Egypt for any purpose, or even that they were known, if at all, until a very late period.”

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8 Barbara Mertz, Red Land, Black Land, revised ed. (NY 1978) p. 217
9 A. Lucas, Ancient Egyptian Materials and Industries, 2nd ed. (London 1934), p. 67
Nevertheless, saw markings are found in numerous objects from the Old Kingdom:

“Thus saw markings exist . . . on the red granite sarcophagus of Cheops and Chephren respectively, on the red granite sarcophagus of Hordedef (Fourth Dynasty) found . . . at Giza; on the lid of the grey granite sarcophagus of Meresankh; on the back of one of the triads of Mycerinus. The marks of tubular drills are found on . . . the well-known diorite statue of Chephren . . . Tubular drills were also used for making sockets in the granite of the pyramid temple of Mycerinus.”

Undoubtedly, the Egyptians were cutting granite with saws with fixed teeth during Old Kingdom times. But they lacked beryl, topaz, corundum (in the form of rubies and sapphires), as well as chrysoberyl and diamond prior to the first millennium B.C. Therefore, the only time at which these various hard rocks could be sawn by fixed teethed tools was the first millennium. No one has ever sawn a block of granite with saws of fixed teeth of copper; it simply cannot be done. The fact that granite was sawn by tools with fixed teeth points to the only period when steel, beryl, topaz, corundum (in the form of rubies and sapphires), chrysoberyl and diamonds were known in Egypt, namely in the first millennium B.C. Further, how did the ancient Egyptians cut these gemstones without a harder, sharp material to give these gems faceted and sharp edges? Rawlinson frankly admits that such “gems as the diamond, the ruby, the emerald, the sapphire, and the chrysoberyl, defied [the early Egyptians’] skill.” The idea that the Old Kingdom Egyptians were using these gemstones to saw, cut, engrave, and chisel hard stone is supported by nothing and makes not a bit of sense.

The problem is not one of negative evidence, but rather of positive evidence. Clearly granite was sawn and diorite was engraved with hard points in the Old Kingdom. These could not have been sawn or engraved with copper or any of the hard minerals posited by Petrie during this ancient period. But quite clearly these materials were available to the Egyptians in Greek times as well as iron tools hardened with these minerals which could undoubtedly cut and engrave granite and diorite. That is, the very evidence suggested by Petrie as proof that the Egyptians could have employed these minerals to work hard stone does not fail, but rather it strongly suggests that these materials were used in Greek times—when they were obviously available—to saw and engrave hard rock. This clearly indicates that the

pyramids were built in the first millennium, when these minerals were sufficiently abundant to do this work, and not in the third millennium, when they were not at all available.

Hardened iron, also available in Egypt during the Greek period, will cut and engrave hard stone. This is not a small point. Before a method for cutting granite or engraving diorite or any other hard stone can be even considered, it must be proven to have actually worked at that early time and shown to have been used just as historians suggest. Handwaving is proof of nothing.

But there are other abrasives that have been invoked by historians to also solve their dilemma, without proof that these were employed as they claim. I.E.S. Edwards puts forth the concept, as do several other historians, that copper saws with quartz sand and gypsum as abrasives can cut, shape, and engrave limestone, a relatively soft stone. But then he and they suggest the same method had been applied to hard stones as well. Lehner writes: “It is most likely that a copper drill or saw was employed in conjunction with an abrasive slurry of water, gypsum and quartz sand. The copper blade simply acted as a guide while the quartz sand did the actual cutting.”

The problem with this method is that copper is actually softer than quartz sand and also softer than granite, schist, basalt, and diorite. Rather than cutting into these stones the quartz sand will destroy the copper blade. While this method will work with soft limestone, it is not possible to use it with these harder stones. This was shown to be simply false in the early part of the 20th century by H. Garland, a metallurgist, who actually tested this methodology with a copper saw and an abrasive harder than the rock to be cut. The method clearly failed to work.

Emery is both a mineral and a rock with a hardness of about (8) on the Mohs scale. It can be made into a powder of small bits and pasted on thin strips of wood to form emery boards to file and shape finger and toe nails. It may also contain corundum with a hardness of (9), and spinel, an extremely hard mineral. Thus, emery meets all the requirements of an abrasive similar to quartz sand with a hardness of (7). If one were to use wet emery powder as an abrasive on granite in

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13 Mark Lehner, *op.cit.*, p. 210
16 ibid., p. 163
the manner offered by Lehner, Edwards, Petrie, and several of their colleagues, the granite should be cut. Here is what Garland’s experiment showed:

“A consideration of the [abrasive cutting] process would seem to give support to the idea that a copper-emery [or other abrasive material] process might have been used by the first Egyptians, but the author [Garland] has proved by experiment the impossibility of cutting granite or diorite by any similar means to this. By the use of emery powder anointed with oil or turpentine, no measurable progress could be made in the stone, whilst the edge of the copper blade wore away and was rendered useless, the bottom and sides of the groove being coated with particles of copper. For some of these experiments a start was made by sawing a small groove with a steel saw, whilst for others an attempt, devoid of satisfactory results, was made to start a way for the copper blade by scratching with flint points, as it was thought that the latter might have been a method employed by the ancients, and it was quite impossible to start a passage way [a groove] with the copper tool itself.”[emphasis added]

The test proved that the copper-abrasive method will not cut granite or diorite using emery powder or, as Garland stated, “by any similar means.” Thus, sand-quartz will also destroy the copper blade and will fail to cut the hard stone. Cardona stated “none of these methods have been verified.” The fact of the matter is that these methods have been tested and falsified. The question of how these hard rocks were cut, shaped, and engraved, is not a “moot” point as Cardona offers. These methods have been disproved in straightforward tests by a competent scientist. But no one, so far as this author knows, has presented these facts, when this disproven concept is being put forth as a possible method by which to shape hard rock.

But please note that Garland was readily able to cut granite and diorite for “some of these experiments . . . with a steel saw.” That is, Garland unquestionably proved that hardened iron (which we call steel) will definitely cut these hard stones. Therefore, it has been fully proven that the Egyptians—when they did have steel or hardened iron in the first millennium B.C.—would have been able to work these rocks. One of the most powerful concepts of science is replication: producing the very same results using the same materials. When the copper-abrasive experiment was replicated, the results proved this method will not work. When the

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17 H. Garland, C.O. Bannister, Ancient Egyptian Metallurgy (London 1927) p. 95
hardened iron-steel experiment was replicated, the results proved this method will work. The plain scientific facts speak for themselves.

For example, it has also been put forth that by hammering hard stone with an even harder one—though this is a long, difficult, and tedious process—one can produce finished results. The hard hammer strikes the granite and crushes the surface layer which is then rubbed smooth with a hard stone to clear away any debris or tiny bumps left behind. But Garland has shown that if one attempts to employ this method, say, on a granite sarcophagus, one will not be able to cut straight vertical corners inside the coffin; yet the insides of granite coffins exhibit straight vertical corners where the walls meet.\(^\text{18}\) “Any rubbing process would surely have robbed the corners [of any stone] of all sharpness.”\(^\text{19}\)

Denys Stocks further points out:

“The use of stone mauls for pounding calcite, granite, basalt, quartzite and graywacke from the interiors of sarcophagi is impracticable: the force of the blows would soon have cracked the shaped stone blocks.”\(^\text{20}\)

Let us also recall that Petrie spoke of finely engraved hieroglyphics on the sides of diorite vases. Thus, we are further asked to believe that chisels and other pointed tools of copper somehow were able to accomplish this work. Garland emphatically stated: “It is obvious to the metallurgist that sculpting of granite and similar materials could not have been done with copper chisels. . .”\(^\text{21}\) On the other hand, Paul Jordan argues: “Hammered copper could produce very hard tools which, when used with quartz sand abrasive, were capable of cutting up stone blocks, like those of the pyramids.”\(^\text{22}\)

If he is referring to granite, this again is simply not possible with copper chisels. Garland explains, however, “The hammering of copper increases its hardness but it also renders the metal more brittle. . .”\(^\text{23}\) The reason is that the

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\(^\text{18}\) ibid., p. 92  
\(^\text{19}\) ibid., p. 96  
\(^\text{21}\) Garland, Bannister, \textit{op.cit.}, p. 91  
\(^\text{22}\) Paul Jordan, \textit{Riddle of the Sphinx} (NY 1998), p. 62  
\(^\text{23}\) Garland, Bannister, \textit{op.cit.}, p. 97
hammered copper point or chisel will break “and the harder [copper] metal can only be of use if it exist as a skin supported by unaltered metal. In a fine cutting edge this combination cannot be achieved.”

Christopher Dunn, a master craftsman and engineer thoroughly familiar with the capacity of metals to do work, points out:

“I.E.S. Edwards, British Egyptologist and the world’s foremost expert on pyramids, said ‘Quarrymen of the Pyramid age would have accused Greek historian Strabo of understatement as they hacked at the stubborn granite of Aswan. Their axes and chisels were made of copper hardened by hammering.’

“Having worked with copper on numerous occasions, and having hardened it in the manner suggested above, I was struck that this statement was entirely ridiculous. You can certainly work-harden copper by striking it repeatedly or even by bending it. However, after a specific hardness has been reached, the copper will begin to split and break apart. This is why, when copper is worked to any great extent, it has to be periodically annealed, or softened, in order to keep it in one piece. Even after being hardened in this manner, the copper is not capable of cutting granite. The hardest copper alloy in existence today is beryllium copper. There is no evidence to suggest that the ancient Egyptians possessed this alloy, but even if they did this alloy is not hard enough to cut granite.”

So even if the ancient Egyptians could create hardened copper, it would not have cut granite. Simply put, a thin, sharpened, hammered copper point or chisel will be so brittle that under the stress of hammering it will crack or break. To overcome this difficulty requires that beneath the hammered surface of the copper tool there is an unhammered core of copper to act against the force of the hammer which would stop the outer edge of the tool from breaking. But how does one do this to a very thin point or a sharp-edged chisel? It is quite evident that when one hammers a tool to a thickness of a few 10ths of an inch or a few millimeters, the force will harden the entire tool so that it has no inner support.

24 *ibid.*
Therefore, how thin were these points and chisels of copper? Petrie has described engraved diorite, cut to “one hundred and fiftieth of an inch wide.” Lehner tells us that to dress soft limestone with chisels it is assumed that the blades were about 8 millimeters or one-third of an inch wide, “because wider blades of soft copper will not work.” Thus to incise fairly soft limestone requires very thin blades, but to engrave harder stones would also demand thin blades or points. The method of hardening copper by hammering again fails to correspond to the facts. Lucas frankly states in this regard:

“In connexion [sic] with the working of hard stone, too much stress is usually laid upon the use of chisels [and pointed tools], and those who think they still must have been used point out that copper and bronze chisels, no matter to what extent they have been hardened by hammering, will not cut such hard stones of diorite, granite and schist, and that they cannot be used with an abrasive powder. This is fully admitted.”

The fact of the matter is that the ancient Egyptians did not know of any method for hardening copper other than cold hammering, as Garland explains:

“Professor Gowland and others have stated their opinion that the earliest metal workers had no other means of hardening copper and bronze than the simple one we know today, viz., cold hammering. From a close microscopical examination of a number of early tools dating from the First Dynasty onwards . . . I am able to confirm this view.”

Cardona’s own source, Tompkins, pointed out:

“Petrie estimated that in order to cut through the hard granite a pressure of two tons would have had to be placed on the drill. How this could be done [and yet not break the drill when boring into the granite] was a mystery to Petrie who concluded: ‘Truth to tell, modern drill cores cannot hold a candle to the Egyptians . . .’

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26 Lehner, *loc.cit.*
Those historians, and especially Egyptologists, who suggest that various hard stones utilized by the Egyptians in the Old Kingdom were carved with copper tools and the abrasive available at that time, namely quartz sand, haven’t one scintilla of real evidence to support their contentions. Worse yet, diorite, one of the hardest of the various stones being worked, was used profusely in these archaic times:

“It is a striking fact that diorite—one of the most difficult stones to carve—was worked almost exclusively during the Fourth Dynasty, when according to present accepted theories copper was the only base metal in use. We may quote the statue of Cephren [sic] in the Cairo Museum as an example of which Sir Gaston Maspero says—’It is most surprising that the Egyptian artists were able to model with so much delicacy and skill such a hard and difficult material as diorite.”

Garland himself was wedded to the long chronology of Manethon and believed that the Old Kingdom Egyptians possessed hardened iron. His comments on the perfection of the statues of granite and diorite bear stating:

“Many of the statues are perfect examples of the sculptor’s art: the hardest stones are all carved and shaped with unfailing accuracy, faultless symmetry and definition: sharp corners with perfect angles and knife-like lines, grooves and serrations and plumb-straight lines . . . deep and shallow depressions, or rigidly plane surfaces. To observe all these, together with the exquisite tooling of the hieroglyphs, is to be convinced that there is one and only one way of obtaining such results and that is by the use of a chisel.”

All the early historians and those of the present fully comprehended that steel (hardened iron) is the only tool with which to carve and engrave these hard stones. However, because they were unable to free themselves from the shackles of Manethon, they have been trying vainly to make copper and even bronze do jobs that they cannot do; Sir Gardiner Wilkinson long ago realized this but could not disentangle the problem because he also supported the long chronology:

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30 Garland, op. cit., p. 187
31 ibid., p. 194
“... the hieroglyphs on obelisks and other granitic monuments and sculptures with a minuteness and finish which is surprising, even if they had used steel as highly tempered as our own.

“Some are cut to the depth of more than two inches, the edges and all the most minute parts of the intaglio presenting the same sharpness and accuracy; and I have seen the figure of a king in high relief, reposing on the lid of a granite coffin, which was raised to the height of nine inches above the level of the surface. What can be said, if we deny to men who executed such works as these the aid of steel, and confine them to bronze [or copper] implements? Then, indeed, we exalt their skill in metallurgy far beyond our own, and indirectly confess that they had devised a method of sculpting stone of which we are ignorant. In vain should we attempt to render copper, by the addition of certain alloys, sufficiently hard to sculpture granite, basalt, and stones of similar quality. No one who has tried to perforate or cut a block of Egyptian granite will scruple to acknowledge that our best steel tools are turned [blunted] in a very short time, and require to be re-tempered: and the labour experienced by the French engineers, who removed the obelisk of Luxor from Thebes, in cutting a space less than two feet deep, along the face of its partially decomposed pedestal, suffices to show that, even with our excellent modern implements, we find considerable what to Egyptians would have been one of the least arduous tasks.”

Wilkinson quotes Sir R. Westmacott that chisels “of strong tempered iron, about three-quarters of an inch in diameter, ... [can] resist the heat [of being pounded into granite before becoming blunted] sometimes half an hour, seldom longer ... Tools of less diameter ... of steel ... will not resist 300 strokes ...”

Yet we are expected to believe that copper chisels with edges much thinner than these engraved diorite with exquisite hieroglyphs.

Since this author has relied so heavily on the writing of Garland, whose work was done in the early 1900s, a few comments are in order because some critics may use the historical errors in his book to discredit the solidly done metallurgy. Garland unfortunately died only five days after returning to England from the Near

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33 *ibid.*, p. 157
East and his book was completed by C.O. Bannister, who was also a metallurgist but whose knowledge of Egyptology was limited. Thus, as pointed out by S.R.K. Glanville, in spite of the historical “howlers” the metallurgy and technology is of “first rate importance.” He directly claims that Garland’s work “has the better of it” regarding the facts related to “cutting stone by the ancient Egyptians.”

All the *ad hoc* explanations offered by the Egyptologists to use copper to cut, shape, and engrave hard stone have very little to do with reality. As recently as 1999, Denys A. Stocks attempted to explain how to carve stone sarcophagi, especially granite. Stocks is a member of the Department of Art History and Archaeology at the University of Manchester, England. His work is based on estimates from his own efforts to perform the various tasks associated with manufacturing a sarcophagus. Yet a careful reading will disclose that his statements that copper and quartz-sand abrasive will accomplish these tasks are filled with provisos such as “probably represents,” “experiment indicated” (not that it *proved*), “experiments suggest” (not that they *prove*), “stone-cutting saws were probably . . .” (not stone-cutting saws *actually did* . . .), “holes were probably drilled” (not holes *were* drilled), “the true number of holes in the central mass [of stone to hollow it out] can never be known,” “polishing was probably done with” (not polishing *was* done with).

Stocks assumed that all this work took a very long time, and since the Egyptians had lots of time on their hands, there is no problem. The problem he failed to address is the stone-working anomaly mentioned by Petrie regarding tool marks that left a spiral groove on a core removed from a hole drilled into granite. Petrie stated “On the granite core No. 7, the spiral of the cut sinks .1 inch in the circumference of 6 inches, or 1 in 60, a rate of ploughing out of quartz and feldspar which is astonishing.” This is an extremely rapid rate; 100 turns of the drill allow it to go one inch into the rock. Petrie was astonished by this evidence and attempted to explain it at three places in a chapter of his book. This is outlined by Christopher Dunn in whose view the Egyptians were using power tools to accomplish this. This writer suggests that a large diamond on a drill point, not employing power tools, was at work. As Wilkinson points out,

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35 Denys A. Stocks, *op.cit.*, pp. 918-922
37 *ibid.*, pp. 75, 76, 78
38 Christopher Dunn, *op.cit.*, pp. 83-84
“although we do not know the precise method adopted by the Egyptians for cutting . . . hard stones, we reasonably conclude they were acquainted with the diamond and adopted it.”

The rate of cutting these rocks was not at all slow as Stocks suggests. Stocks’ case for cutting granite with copper and quartz-sand abrasive is again merely probabilities and possibilities (actually, rhetoric), never fully carried out by actually saw-cutting, shaping, and engraving a granite coffin. It is mostly hypothesis based on educated guesses, not on fully maintained proofs. As was pointed out above, replication is one of the most basic aspects of empirical science.

The most fundamental point overlooked by Stocks and all proponents of the copper saw and quartz sand theory is the fact that Petrie himself explained that the granite sarcophagus he observed had been produced by a “fixed cutting point [that] ploughed the groove and not a loose powder. Also the hieroglyphs on diorite bowls are ploughed out with a single fixed point . . .”. For Stocks to replicate the way the Egyptians actually cut diorite requires that he use only fixed points to achieve the same results as did the Egyptians. His experiment has nothing to do with the fixed pointed saws, drills, and chisels that the Egyptians employed without abrasives. His experiment has nothing to do with the fact that only fixed points were utilized to produce granite sarcophagi. That is the immense difficulty with Stocks’ work. To replicate the ancient Egyptian method of cutting granite demands that only fixed points or copper saws, drills, and copper chisels be employed. Employing sand abrasive to aid in the process is completely beside the point in this regard. It does not deal with reality, as Petrie and others have carefully pointed out.

To truly prove that the ancient Egyptians of the Old Kingdom did all that is claimed for them vis-à-vis the hard stones requires that the hardest of these—diorite—be fully carved into a statue, say, such as that of Chephren, with hieroglyphs upon it as it is now found at the Cairo Museum. Once that statue was replicated with copper tools and even with quartz-sand, there would be no argument possible to suggest that the ancient Egyptians actually did to diorite what historians claim they did. As Dunn, an expert in the field of tool machinery, suggests:

“Chronology of the historical development of metals, identifying copper as the metal of the ancient Egyptians for cutting granite is like

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saying that aluminum could be cut using a chisel fashioned out of butter.”

Another expert in the field of cutting granite, Roger Hopkins, wrote:

“I am a stone mason by trade and in 1991 the PBS [Public Broadcasting System] series NOVA invited me to go to Egypt to experiment with building a pyramid; I quickly got bored with working the soft limestone and started to ponder the granite work. Here in Massachusetts my specialty is working in granite (see my web page http://tiac.net/users/rhopkins).

“When I was asked by the Egyptologists how the ancients could have produced this work with mere copper tools, I told them they were crazy.”

The fact of the matter is that they cannot replicate the experiment. They have replicated several experiments to show how obelisks are stood upright, how great heavy stones could be moved, etc., but no one has ever been able to carve a large diorite statue and engrave hieroglyphics onto it with fixed points of copper or copper chisels. What has been put forth is a mountain of words, suggestions, hypotheses, all unproven and improvable. They are improvable because they will not work; “there is no evidence” means precisely what it says, “no evidence” that this can be accomplished. The critical tests have been carried out. Their entire procedure is dysfunctional. The question revolves around the concept that Fischer discusses thus:

“The fallacy of the lonely fact is the logical extension of a small sample [copper tools can cut soft limestone], which deserves to receive special condemnation. It may be defined as a statistical generalization from a single case. [Since copper tools can cut limestone, they can cut diorite.] There is a story, perhaps apocryphal, of a scientist who published an astonishing and improbable generalization about the behavior of rats. An incredulous colleague came to his laboratory and politely asked to see the records of the experiments on which the generalization was based. ‘Here they are,’ said the scientist, dragging

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40 Dunn, op.cit., p. 85
41 ibid., p. 91
a notebook from a pile of papers on his desk. And pointing to a cage in the corner, he added, ‘there’s the rat.’

“There are many astonishing and improbable generalizations in historical scholarship in which the critical reader will smell a singular rat. As long as the majority of historians continue to conduct their ‘research’ impressionistically and to cast their findings in a single narrative, the fallacy of the lonely fact is likely to flourish. A special stylistic device has developed around it. Whenever the reader sees a mighty generalization [such as copper tools and quartz-sand abrasive was used to cut diorite in ancient Egypt], followed by a minute example, [such as copper tools and quartz sand abrasive will cut soft limestone] and the tell-tale phrase ‘for instance’ or ‘for example,’ [it can also cut granite or diorite] he should be on guard against this error.”

Cardona, however, has offered his own hypothesis to explain how these various hard stones can be cut:

“But even if we were to allow Heinsohn the use of iron tools . . . that, in itself, is not enough reason to redate the pyramids to the iron age. After all, meteoric iron has been known and used long before that.”

Cardona’s hypothesis is that if iron is necessary to cut hard rock, since the ancient Egyptians had iron in the form of meteorites, then they used it to work these materials. As support for this thesis he cites the name for “iron” from the Sumerians as an-bar, which means “fire from the heavens.” The Hittites called the metal “ku-an, which also translates as ‘fire from heaven’.” In Egypt, “iron” was “bia-en-pet or baat-en-pet, which translates as ‘metal of heaven’.”

But did the Old Kingdom Egyptians know that the meteorites they found, which had fallen out of the sky, had iron? The impression given by various sources is that the Egyptians knew of iron as a metal distinct from copper, which is the metal they commonly used. G.A. Wainwright, one of Cardona’s sources, claims

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42 Fischer, *op.cit.*, p. 109
43 Cardona, *op.cit.*, p. 47
44 *ibid.*
that iron as a separate metal was known to the Egyptians. However, R.J. Forbes in 1954 refuted Wainwright’s identification:

“Wainwright never brought forward one text in which bi3 must have meant iron, the old and recognized translation fits just as well . . . there is no reason to support that their meteoric iron [was] just a form of black copper and that they recognized its celestial origin much later. . . . Only when they had attained the full Iron Age [in the first millennium B.C.] and iron was smelted and worked in Egypt itself copper and iron were distinguished as separate metals.”

The 1982 Britannica succinctly states this in respect of meteorites: “The Egyptians called it black copper from heaven . . .” Though this is a small point, it is rather important to understand that the Egyptians did not know of the existence of iron as a different metal in early times but thought of this metal which they found as merely “black copper.”

Would they have expected “black copper” to do the work that burnished reddish copper could do? In spite of Cardona’s statement that the “ancients were well acquainted with iron meteorites,” “iron” was not known as a different metal than copper in early times. The proper interpretation is that the Egyptians were familiar with “black copper meteorites,” not “iron” meteorites. To assume that both iron and copper were known in early Egypt is simply false. It is also false to assume that the Old Kingdom dynasties built the pyramids with copper tools in the third millennium. If they did not know of iron then they did not build the pyramids which contain blocks of granite. We are not tied to the established chronology by any means. Whatever dynasties built the pyramids had carburized iron—steel—and hard minerals for teeth and points and knew of these substances.

However, Cardona has failed to inform his readers of an extremely important piece of evidence from his own source, Wainwright, that makes his entire argument highly questionable. The question “How many meteorites are really useful as metal tools?” was also discussed by Wainwright:

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“But all meteorites are not solid iron. Very many are a mixture of iron and stone and so [are] useless to man as a source of iron.”

Meteorites fall into three classes: siderites are made up of iron with nickel, stony aerolites are made up of stone, but siderolites are made up of both iron and stone. Those made up of iron-nickel are the class needed to make iron tools, while those made of stone and iron are, as Wainwright stated, “useless to man.” Tylecote explains:

“The brittle constituents present in some [meteorites] reduce the amount of deformation that may be applied with [their] cracking. While layered pieces of meteorites crack and splinter on hammer[ing] . . . some of the small pieces so formed can be hot hammered into arrowheads. This difficulty is due to the coarse crystalline structures of meteorites, particularly those exceeding about 6% nickel.”

As one can see, meteorites are generally a poor metal for tools. Yet we should not preclude, for the moment, that tools of these iron-nickel meteorites could have been, and were, made by the ancients. Cardona goes on to point out that there was a taboo against iron in Israel, Rome, Greece, and also among the Egyptians. But how could the Egyptians of the early times have a taboo against a metal that they didn’t know even existed? The time that the Egyptians seem to have learned about iron was the first millennium B.C.; therefore, if there was a taboo it came in existence only then, and the Old Kingdom, if it had this taboo, had to have existed in the first millennium B.C., not the third.

But there are statements in the Book of the Dead that one of the most important rites after mummification was that of “the opening of the mouth”; to resuscitate the mummy for after-life required iron. For example, a papyrus showing the “opening of the mouth” ceremony being performed on the mummy of Hunefer, dating in conventional chronology to 1350 B.C., states:

“I have opened thy mouth with the instrument of Anubis with the iron tool. Horus hath opened the mouth of Osiris with the iron which

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came forth from Set with the iron tool . . . with which he opened the mouth of the gods.”\textsuperscript{49}

There are other citations related to iron as well.\textsuperscript{50} If the Egyptians had a taboo against iron, why were they using it in this sacred rite?

Howard Carter, who opened Tutankhamen’s tomb, succinctly answered this question:

“Based upon the discovery of accidental pieces of iron it has been claimed . . . that iron being considered an impure metal by the ancient Egyptians was never used for religious purposes. . . . if iron was considered impure by the Egyptians, why were [wrought] iron emblems such as an Urs pillow and an Eye-of-Horus, as well as an iron dagger . . . placed on the hallowed remains of the Pharaoh, Tut-ankh-Amen? As a matter of fact, from his reign onwards we find amulets made of this material for the dead.”\textsuperscript{51}

This evidence certainly does preclude the taboo among the ancient Egyptians.

Ultimately, Cardona’s meteoric iron was used or the Egyptians developed the ability to smelt iron from ore or to make tools such as saws and chisels to cut diorite, granite, schist, etc. Before examining the questions related to meteorite iron let us turn our attention to when and how the early Egyptians began to smelt this metal (if, in fact, they did). To this question is also related, for our purposes, the hardness of smelted iron. Thomas Arthur Rickard, in his two-volume work, \textit{Man and Metals}, discusses the question of how and when smelted iron was produced. The idea has been advanced that somehow an accidental

“. . . smelting operation yielded little lumps of iron that were fabricated into small objects. It is assumed that long before the art of smelting iron was discovered and developed, there were accidental and sporadic productions of the metal, and that these explain such finds of iron as those of the Great Pyramid, of the Abydos grave . . . [etc.]

\textsuperscript{49} E.A. Wallis Budge, \textit{The Egyptian Book of the Dead}, Dover ed. (NY 1967), p. 267
\textsuperscript{50} ibid., pp. clx-clxi
[It is possible that these accidents occurred.] “This is not disputed. It is accepted as a likely contingency; but what is questioned is that such fortuitous smelting of iron could happen frequently in many places [in Egypt] and go unregarded or unappreciated. The production of iron of usable quantity was too great an event to be ignored; it would call for attempts to repeat the act, and presumably would lead eventually to the discovery, after many trials, of a method of reduction; in other words, it is extremely improbable that several accidental reductions of iron from its ores, even at intervals apart, would not provoke purposeful repetitions of the performance, and the consequent discovery of a rudimentary smelting process. We cannot suppose that more than a millennium would elapse before such accidental smeltings would become rationalized.

“The discrepancies of chronology are enormous—no less than three millenniums. A knowledge of any method of producing iron from ore argues the desire and ability to make use of this serviceable metal; it is most improbable that each of the few known iron relics of remote antiquity was merely the result of an accidental smelting operation, for the quality of the iron found in the Pyramid and the tomb of Tut-ankh-Amen appears to be excellent. We are justified, on the contrary, in presuming that if and when the Egyptians, or any other people, discovered how to make a pound of metallic iron out of ore [as found, according to G.A. Wainwright, “The Coming of Iron,” Antiquity, vol. X (1939) p. 9-10, in a tomb from the 12th Dynasty which supposedly ruled 1991-1782 B.C. or almost 1000 years before iron was smelted in Egypt] they would make more of it, because many deposits of iron ore, in large quantity, were available in Egypt, as elsewhere. The discovery of a process for smelting the metal would be of tremendous importance to the Egyptians, or to any other people, in their commerce and in their warfare with their neighbors.”

To suggest that the Egyptians smelted iron to make tools around 2500 B.C. would require that the Iron Age began around that time in defiance of the claims of the established historians. But the fact that smelted iron implements are found in Old Kingdom times suggests that the Old Kingdom existed at a time when the Egyptians could do so and that again points unambiguously to the first millennium.

Nevertheless, let us suppose for the moment that the Old Kingdom Egyptians could smelt iron, does this solve the problem of cutting hard stone? Even when iron is smelted, it is still soft, forming what is known as wrought iron. Wrought iron is not very useful for tools that are exposed to high stresses. Sass informs us that “bronze . . . is stronger than wrought iron.”  Lucas has earlier told us that “bronze chisels, no matter to what extent they have been hardened, will not cut such hard stones of diorite, granite, and schist, and that they cannot be used with abrasive powder.” Wrought iron being softer than bronze clearly cannot be called upon to cut and engrave the hard rocks that bronze cannot, either.

Woolley concedes that iron derived, say, from gold ore “was valuable because it was rare, but because it was only soft [wrought] iron . . . [it] would obviously be far less serviceable than bronze for tools or weapons . . .” According to Wheeler and Madden, “Uncarburized iron is not an acceptable substitute for bronze because it is not as strong . . .”

The Britannica bluntly states:

“During most of its history, iron was not recovered in a molten state but reduced from a spongy aggregate of iron and slag formed at a temperature well below the melting point of pure iron (1,535° C, or 2,795°F). This plastic metallic sponge was consolidated by hammering to squeeze out slag and weld the iron particles into a compact and ductile mass; thus it is called wrought iron, essentially pure iron with remnants of unexpelled slag coating the iron particles. Wrought iron contains so little carbon that it does not harden usefully when cooled rapidly (quenched [in water or oil]). [Only w]hen iron containing 0.4 to 1.25 percent carbon is heated to 950°C, or 1,740°F, and then plunged into water or oil, is it hardened.”

In essence, unless iron contains a small but significant amount of carbon and is heated and then quenched in water or oil, it is wrought iron, and unsuitable for making tools to work hard stone. The germane point to remember is

56 Encyclopedia Britannica, Macropedia, vol. 8, loc. cit.
that iron has to possess an amount of carbon to enable it to become steel. Soft wrought iron is again, like copper and bronze, incapable of shaping diorite, granite, schist, etc. Lucas adds this stunning remark:

“Early wrought iron on account of the way it was made would contain little or no carbon (less than 0.2 percent) and such iron is not hardened but softened if heated and suddenly cooled.”

Wrought iron made by smelting is clearly not a possible candidate for operations on hard stone. Cardona, nevertheless, suggests that meteoric iron would have done the job that neither copper, bronze, nor wrought iron could. Does meteoric iron in its purer form contain sufficient carbon for it to be harder than these other metals? That is the fundamental question Cardona has failed to address. Again, the Britannica tersely reports:

“Meteoric iron is practically carbonless and hence, cannot be hardened in the manner of steel; . . .

“Much rarer than copper, meteoric iron was often used for jewelry. . . . Small meteorites were the most convenient sources, but larger bodies were hacked at with copper and stone tools to yield tool-size pieces for knives, spear points, arrow points, axheads, and other implements.”

Copper tools can hack away pieces of iron meteorites only if the copper is almost as hard as the iron. Emery explains: “Experiments have shown that copper with an initial hardness of 87 can be increased to 135 (Brinell scale).” If copper can be hardened to 135 (Brinell scale of hardness), how hard can meteoric iron be? According to Tylecote, meteoric iron with 8.6 percent content of nickel has a hardness of 250 (Brinell scale). If we compare the Brinell scale of hardness with that of the Mohs scale, we arrive at the following: a value of 250 Brinell is much softer than quartz, Mohs scale (7), Brinell scale 700; it is softer than orthoclase [feldspar], Mohs scale (6), Brinell scale 400; it is softer than apatite, Mohs scale (5), Brinell scale about 325. That means meteoric iron has a hardness of below (5) on the Mohs scale. Stocks, however, informs us that copper and bronze chisels can cut

59 Emery, op.cit., p. 226
60 Tylecote, op.cit., p. 11
“all stones of hardness Mohs 3 . . . However, stones of Mohs 4, and above, cannot efficiently be cut by such metal tools; test tools’ cutting edges were blunted, or torn away, to such an extent that constant sharpening, even for cutting calcite [Mohs scale (3)], caused unacceptable losses of metal from the tools.”  

One does not cut granite with a hardness of (7) Mohs scale with meteoric iron with a hardness below (5) Mohs scale. Each step on the Mohs scale corresponds to an approximate doubling of the scratch indentation of the mineral in the step below. With a hardness of about (4.5) Mohs scale, meteoric iron is about ten times softer than granite. Meteoric iron, like all the other soft metals, will never cut a stone that is ten times harder than itself. There is absolutely nothing to support Cardona’s contention that meteoric iron was used to work these hard materials.

The further question that Cardona has failed to address is how and when hardened iron or steel was produced in the ancient world.

The process is known as carburizing or heating iron in a bed of charcoal or an atmosphere of carbon so that the carbon will migrate (during the heating process) into the metal. The Britannica defines the process:

“Carburization is the modern form of an old cementation process. The part, made from a copper or alloy steel low in carbon, is heated to about 900°C (1,650°F) in a bed of carbonaceous material [like charcoal with a great deal of carbon] or a carburizing [carbon] atmosphere.”

To harden wrought iron and meteoric iron required carburization so that the tools made from these metals would be hard and useful. This process, according to the chronology that Cardona seems to uphold—the established chronology—began around about 1200 B.C. Again the Britannica states:

“By about 1200 B.C. when iron had become important in the Near East man had learned how to create a steel surface, or case, on wrought iron, a case that could be hardened by heating and quenching [placing the hot iron in water or oil]. This case had been produced by the prolonged heating of wrought iron packed in a deep bed of glowing charcoal; the principle is that the surface of red-hot but carbon-less iron

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61 Stocks, op.cit., p. 918
readily absorbs up to 1 percent carbon from the carbon monoxide generated in the enveloping charcoal fire.”

The carbon infiltrates the iron and disjoins the lattice iron crystals, which causes the metal to hold together more strongly and become harder. For an understandable explanation of just how this carburizing process works, see Stephen L. Sass’ *The Substance of Civilization* (NY 1988), chapter 5. This makes it clear that the Egyptians of the third millennium B.C. could not have employed the carburizing process to harden iron because this process only developed in the latter part of the second millennium B.C. according to their chronology. Their metal tools, whether of copper, bronze, wrought or meteoric iron, were simply too soft to shape and engrave hard stone. These metals were never up to the job. There is simply no escape from these facts.

Robert Drews shows rather conclusively that the Iron Age came long after the third millennium B.C. in the Near East. Most interestingly he tells us it came after the Bronze Age catastrophe:

“Although in conventional terminology the ‘Iron Age’ commenced with the Catastrophe, it is quite clear that iron did not come into regular use until well over a century after the Catastrophe ended. Although there were premonitions of this discrepancy all along it was demonstrated conclusively in the 1960’s. 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 percent bronze and 20 percent iron, and for the tenth century 46 percent bronze and 54 percent iron). It now appears the technical progress that culminated in the production of carburized iron extended from the late thirteenth to the ninth century B.C., . . .”

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63 Encyclopedia Britannica, Macropedia, vol. 8, op.cit., p. 612  
64 Robert Drews, *The End of the Bronze Age* (Princeton NJ 1993) pp. 75-76
This, again, is based on the established chronology which may be in error. With respect to the Iron Age in Egypt, Garland states: “Egypt was the last country in the Near East to enter the iron age . . .” Garland’s source, Wainwright, also reports: “Actually Egypt was the last country to enter the Iron Age.” A. Lucas and J.R. Harris “refused to accept the age of the iron [pieces found in the Great Pyramid at Giza] as contemporary with it.”

Woolley confirms this and adds:

“The evidence of archaeology, of folk-lore and religion is conclusive on the point of iron being a late metal. The fact that iron-working not only implies a knowledge of the techniques used in dealing with other metals but also requires special techniques of its own (and special tools as well) confirms its [late] appearance. . .”

They further add this caveat: “. . . The Iron Age begins with the production of hardened iron—‘steel’—the only form in which the metal would be superior to bronze and so would naturally replace it. The point of departure is not the first use of iron as such, but the metallurgical discovery which made iron really useful.” On the basis of this historical definition, the Iron Age begins with the development of steel, and neither wrought nor meteoric iron produces such an age since they are too soft to be really useful as tools to cut hard stone. The process that makes iron really useful only develops (according to the established chronology) around 1400 to 1200 B.C., long after (according to the established chronology) the hard stones of the pyramids and diorite statues were shaped and engraved with hieroglyphs.

None of the crucial questions regarding the problems related to iron were ever addressed by Cardona, although the answers to these are easily found in an encyclopedia. According to the established chronology which Cardona upholds, the Old Kingdom Egyptians did not have iron that was carburized and therefore could not shape and engrave hard stone. None of these important problems was ever discussed by Cardona in his criticism of Heinsohn with respect to iron, it was as if they did not exist. Was he unaware of all this? Unfortunately, Cardona’s own

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66 Wainwright, *op. cit.*, p. 23
69 *ibid.*, p. 560
sources say in very clear and direct terms that these problems exist, and these he withheld from his readers and failed to address. In his original paper, “The Two Sargons and Their Successors,” in AEON vol. I, no. 5, page 13, he cites Jacquetta Hawkes’ book The First Great Civilizations (NY 1973) page 69. Nevertheless, Hawkes’ statement on page 113 of that book presents us with the following:

“Iron oxides reduce at about the same heat as those of copper, but owing to the much higher melting point, the experimenters [in ancient times] were not rewarded with a flow of metal. The small lumps of metal were lost in the spongy mass of the ‘bloom’. It is hardly surprising that millennia went by before men discovered the many processes needed to produce an iron that was superior to bronze—the heavy work of extracting the metal by much heating and hammering was not enough, for this wrought iron remained soft. It was more fit for ornaments than for tools or weapons.

“It was not until the second half of the second millennium B.C. that the essential process of carburizing, or steeling, by repeated heating in the presence of charcoal, and hammering was fully mastered. It seems to have been accomplished by those famed metallurgists, the Chalybes, then subject to the Hittites. For some two centuries (1400-1200 B.C.) the Hittites held a near monopoly. . . . Although it had been known and sparingly used centuries earlier, it can be said that there was no true Iron Age there before the ninth century [B.C.]”

In essence Cardona was told directly that in order for iron to be useful it had to be carburized. Cardona may suggest that this does not apply to his work since he was discussing meteorite iron. But here also his own source C.G. Starr whose book A History of the Ancient World (NY 1999), which he cites as reference 77 on page 40 of his criticism “A Return to the Two Sargons. . .” emphatically made this very point on page 129:

“While iron ores can be melted and refined at a lower temperature than that required for copper, the process takes a longer time, and truly useful weapons and tools of iron involve a complicated technique of repeated heating, quenching, and hammering. Iron objects have turned up even in fifth-millennium levels (SOMETIMES OF METEORIC SOURCE); Hittite smiths in Asia Minor went further in working metal; but the adequate methods of hardening iron products
became commonly known only after 1000 [B.C.].”  [capitalization added]

Both Hawkes and Starr have made it quite clear to Cardona that iron products to be truly useful as tools involve “carburizing,” “a complicated technique” that did not exist in the Egyptian Old Kingdom.  Starr includes among these iron products those of “meteoric sources.”  And after specifying this, he explains that there were no “adequate methods of hardening” any of these forms of iron until the first millennium B.C.

Why did Cardona fail to deal with these facts after he had read them?  His failure to do so makes his work, as that of the other historians who have failed to deal with these negations of their chronology, highly suspect if not thoroughly disingenuous.

Velikovsky understood the implications regarding iron as they related to chronology and made use of this fundamental understanding.  In *Ramses II and His Time*, pages 234-235, he discussed the 19th Dynasty, conventionally dated from about 1320 to 1200 B.C., well before iron came into regular use in Egypt.

“A letter in the Boghazköy archives probably written by Hattusilis (Nebuchadnezzar) to Ramses II, reads:

“‘What concerns the pure iron, about which thou hast written to me, there is no pure iron in Kiswadna in my storehouse which is closed.  The time was unfavorable to make iron.  But I ordered in writing to prepare iron.’

“Thus Hattusilis and Ramses II lived in a fully developed Iron Age. . . .

“. . . Actually, the Nineteenth Dynasty ruled in the seventh-sixth centuries [B.C.]”

Velikovsky understood that the fully developed abundance of hard iron only came to Egypt in the first millennium, not the early second or the third.  The fact that hard stones require hardened iron—steel—and abrasives to cut and engrave them requires that any of the Egyptian dynasties that employed and worked these materials could only have done so in the first millennium B.C.  Therefore, if the Old
Kingdom dynasties reigned and constructed monuments of granite, diorite, schist, basalt, etc., as did the Middle and New Kingdom ones, then these had to exist in the first millennium also.

The problem of why so few iron objects were recovered from certain regions may have to do with the amount of iron available, which would raise the cost. For some period of time, iron was extremely expensive, but its price fell as it became more plentiful. Though I do not agree with the chronology of the quotation presented next, it explains why iron objects for some time were so valuable that they were stolen by grave robbers if they were left in tombs, or were carefully retrieved from work sites. Only after iron was plentiful, and therefore cheap, did grave robbers fail to remove it unless it might be of some use to them, or workers and supervisors at sites failed to carefully retrieve all their iron tools.

“Innovation in the metallurgy of iron had a drastic effect on its price. In the nineteenth century B.C.E., forty ounces of silver [2 \( \frac{1}{2} \) pounds] bought one ounce of iron, a ratio of forty to one. By the seventh century B.C.E., technology had advanced so far that one ounce of silver now purchased 2,000 ounces [125 pounds] of iron, a ratio of 1 to 2,000. In other words, over a period of one thousand two hundred years, the price of iron plummeted by a factor of 80,000 (assuming silver kept a constant value).”

Cardona has argued, citing Erman, that since bronze tools were found fairly plentifully in ancient sites, “the scarcity of retrieved iron tools and weapons” can have nothing to do with the corrosive nature of iron.” What Cardona has failed to consider is the abundance of these materials and the cost of them. Since copper and bronze became abundant and cheap fairly early in civilization (even in the shortened chronology), they were not that valuable to grave robbers and workers. According to James D. Muhly, in Mesopotamia iron was 35 to 40 times more valuable than silver and 400 times more expensive than tin in its early introduction. Iron, in terms of the short chronology, became so abundant over some hundreds of years that it became a less valuable item and was then treated as such. Corrosion has little to do with the question.

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70 Sass, *op.cit.*, p. 93
71 Cardona, “A Return to the Two Sargons, *op.cit.*, p. 48
72 James D. Muhly, “The Bronze Age Setting,” *op.cit.*, pp. 35 and 36
Let us recall from our discussion of rain damage to the Sphinx, Valley and Mortuary Temples that one of the pyramids of Giza still has facing stones of limestone around its apex. These exhibit no evidence of rain erosion like that which is found on other monuments. This proves that the water-damaged structures were built prior to that pyramid as well as to all the others of limestone. We now have a second scientific, technological form of evidence that demands that all the hard stone monuments of Egypt, no matter to which dynasty they have been assigned, were also built in the first millennium. To criticize these empirical, scientific proofs by invoking historical interpretations as counter-evidence is an exercise that defies the scientific facts. One cannot deny scientific fact—Heinsohn’s first millennium steel and abrasives that are known to have existed, which will cut and engrave diorite—as compared with assumptions: the historians’ blithe inventions or Cardona’s out of the sky soft meteorites which cannot produce the required results with these hard stone materials. This is a very hard fact to bite into, chew on, and have to swallow. It is steel in the belly of that established chronology.

Since it appears rather clear that all the dynasties that worked with diorite could not have done so in either the third or second millennium, but only in the first millennium B.C., they had to reign in the first millennium. But there are several other dynasties that, according to established chronology, also reigned at that period of time, namely the end of the 21st Dynasty, the 22nd Dynasty, the 23rd Dynasty, the 24th Dynasty right through to the 31st Dynasty. This requires that these older dynasties had to reign, or better co-reign, at the same time. We have direct statements by the Assyrians that when Assurbanipal invaded Egypt, it was ruled by no fewer than 20 kings in 666 B.C.

At this point we face a fundamental issue, namely that if the early dynasties were cutting hard rock and had steel to do this work, then they had to have known about steel and written about it. But there is no evidence for this in the First through Sixth Dynasties and certain later ones. Yet this, in itself, cannot salvage the established chronology. As was pointed out above, there is no clear-cut evidence for the existence of these dynasties. The dynasties that created these monuments and sculptures are those that had steel, or wrote or knew of this metal. To argue that the scientific (in this case technological) facts can be disregarded in order to support the conventional chronology, is contrary to scientific fact. Historical analyses of documents cannot overthrow scientific evidence, ever! Either the written evidence has been lost, or the dynasties to which historians attribute these hard stone constructions did not exist or are misplaced in the chronology.
To support this conclusion, let us examine an iron object found in the Great Pyramid. Cardona states:

“... an ... object discovered in the southern shaft of the King’s Chamber of the Great Pyramid in 1837 was, in 1881, studied by Petrie who vouched for its genuineness. Having then lain for over 100 years in the British Museum, it was analysed again in 1989 ‘and found to be made of thin laminates of wrought iron, inexpertly welded together by hammering at moderate temperature.”

Cardona earlier prefaces these remarks with “... the actual truth of the matter is that iron has been known and used at least since the early third millennium,” (Cardona’s emphasis) This assumes that the establishment chronology is correct and that the Egyptians knew the process of smelting iron in the early third millennium. Yet the established chronology presents, as the actual truth of the matter, that iron was smelted in Egypt in the first millennium B.C., not the third, and the object of wrought iron found in the Great Pyramid was actually smelted in the first millennium; as Cardona stated “iron tools [indicative of an Iron Age] did not come into general use until the 7th/6th century B.C.E.”

The reader is asked to recall that Rose placed the 12th Dynasty in the first millennium. That being the case astronomically, the 12th Dynasty should also have known of, and been able to smelt, iron. Wainwright in this respect states:

“We now come to the Nubian spearhead [of iron] ... [dated via the established chronology to 1800 B.C.] and here the difficulties cluster thick so that whatever view is taken it remains an enigma for the present. Its discovery is recorded in full detail by competent archaeologists, and it was found in the farthest recess of the tomb by the head of the innermost burial. The skeleton [with the spearhead] was protected from intrusion by several untouched burials lying between it and the entrance. There is no sign of intrusion of a later date, and the discoverers record the skeleton as ‘untouched’ ... Apart from the spearhead the objects, as well as those from the neighbouring graves, are all late 12th Dynasty type and call for no sort of comment. Hence, it seems difficult to doubt the antiquity of the iron spearhead. Yet this

73 Cardona, A Return to the Two Sargons, op.cit., pp. 46-47
74 ibid., pp. 45-46
75 ibid., p. 45
object is unparalleled in a number of features. Petrie says of it, ‘no such form of iron is known nor any iron weapons till long after this’ and it is ‘one of the strangest specimens’ . . . nearly a foot long.”

Various explanations are offered to explain it, such as it is like others of recent date though smaller in size, etc. However, the fact that there is not the slightest evidence that this tomb was opened after burial and there is no sign of intrusion, because the outer skeletons blocking access to this innermost one were not disturbed, proves the iron spearhead was placed there during the 12th Dynasty. Wainwright adds:

“On top of all this now comes a new difficulty. At my request the museum authorities have kindly had the metal analysed and report that it contains no nickel. This means that the iron is not meteoric in origin, but smelted by man from ore.”

One can see that both astronomical dating and smelted iron from ore places the 12th Dynasty in the first millennium B.C., not the early second. Van der Merwe points out that “Egyptian axes and adzes of copper, for example, remained unsocketed for nearly 2000 years after socketed haftings had become accepted elsewhere in the Near East” But Wainwright shows the spearhead “is not tanged . . . but socketed.” This again fits the shortened chronology. After 2000 years, or in the first millennium B.C., did the Egyptians use sockets which again places the 12th Dynasty in the first millennium B.C. If we examine the minor art of jewellery production by the artisans of the 12th Dynasty, we find they were the most advanced which also points to that dynasty being of the first millennium. “If we turn to the minor arts it is clear that the jewellers of the Twelfth Dynasty had reached a level of technical skill never exceeded at any other period in Egyptian history.” Did the advanced craft of jewellery making become totally lost for over a thousand years? This is hardly conceivable.

The point of greatest significance is that the iron slab found in the Great Pyramid WAS ALSO SMELTED! It was smelted from iron ore by fusion in a

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76 Wainwright, op.cit., pp. 9-10
77 ibid., pp. 10-11
78 Van der Merwe, “The Advent of Iron in Africa,” The Coming of the Age of Iron, T.A. Wertime, J.D. Muhly eds., op.cit., p. 466
79 Wainwright, “The Coming of Iron,” op.cit., p. 10
80 W. Stevenson Smith, The Art and Architecture of Ancient Egypt (Baltimore 1965) p. 111
furnace. To make this wrought iron slab by hammering several pieces of smelted iron into a single unit requires the knowledge of smelting. If this was actually known in the third millennium B.C. in Egypt, then the Egyptians would have had all the wrought iron they desired at this early time. The actual truth of this matter is that knowledge of smelting iron only came to Egypt in the first millennium B.C., and then, and only then, did smelted iron products come into general use.

The two metallurgists who analyzed that iron plate, Dr. M.P. Jones, senior tutor in the Mineral Resources Engineering Department at Imperial College, London, and Dr. Sayed El Gayer, who obtained his doctorate in extraction metallurgy at Aston University in Birmingham, “determined that the plate was not of meteoric origin and that it must have been smelted at between 1,000 and 1,100 degrees centigrade.”\footnote{Dunn, \textit{op.cit.}, p. 265} This means that several lumps of iron ore were smelted and then were hammered and heated to form a single plate. Since each of the several lumps of ore had been smelted, the smelting process must have been understood by those who fabricated this plate.

Of course, Egyptologists fully understand the implications this technological evidence has for their chronology, so they suggest that the slab was introduced during the time when Howard Vyse blasted into the Great Pyramid. However, it is quite clear that this smelted wrought iron element was incorporated into the pyramid when it was being built. Vyse, in his letter to the British Museum, sent along with the iron plate the following certificates:

“This is to certify that the piece of iron found by me near the (outside) mouth of the air passage, in the southern side of the Great Pyramid at Giza, on Friday, May 26th, was taken out by me from an inner joint, after having removed by blasting the outer two tiers of the stones of the present surface of the Pyramid; and that no joint or opening of any sort was connected with the above-mentioned joint, by which the iron could have been placed in it after the original building of the Pyramid. I also showed the exact spot to Mr Perring on Saturday, June 24th—J.R. Hill.

“To the above certificate of Mr Hill, I can add that since I saw the spot at the commencement of blasting, there have been two tiers of stone removed, and that, if the piece of iron was found in the joint, pointed out to me by Mr Hill, and which was covered by a larger stone
partly remaining, it is impossible it could have been placed there since the building of the Pyramid—J.S. Perring, C.E.

“We hereby certify, that we examined the place whence the iron in question was taken by Mr Hill, and we are of the opinion, that the iron must have been left in the joint during the building of the Pyramid, and that it was not inserted afterwards—Ed. S. Andrews—James Mash, C.E.”  

Thus, the iron plate was made when the pyramid itself was made, and since it was smelted when the smelting process was fully known and understood, both the pyramid and the wrought-iron slab had to have been made in the first millennium. Whatever dynasty built the pyramid and whatever dynasties were able to cut, shape, and incise hard rock, did so—just as Heinsohn indicates—in the first millennium B.C.

Velikovsky pointed out that “several pieces of a[n iron] pickax from the Sixth Dynasty were unearthed at Abusir, and a heap of broken [iron] tools from the same period at Dahshur, . . .” But of these, “one or two iron objects of the Sixth Dynasty are declared to contain no nickel and therefore to be not of meteoric origin.”  

Again, as with the iron plate found in the Great Pyramid at Giza, there were smelted and thus point to the first millennium B.C. for placement of the Old Kingdom.

Velikovsky further cites Robert A. Macalister’s *The Excavation of Gezer* (1902-09), vol. II, page 269:

“A curious exception to the total absence of iron in the earlier Semitic periods must however be mentioned. At the very bottom of the sloping part of the Water-passage were found two wedge-shaped lumps of iron, apparently parts of axe-blades or hoes. How these had got down to their resting-place, which was sealed up some four or five hundred years before the use of iron became general, is not easily explained.”

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82 ibid., pp. 263-264  
83 Velikovsky, Ramses II and His Time, op.cit., p. 226  
84 ibid., p. 227  
85 ibid., p. 228
But as we will see in our discussion of Mesopotamia, there is considerable evidence that iron was smelted and known supposedly prior to the Iron Age. The problem of these iron materials is “easily explained” by shortening the chronology of the Near East greatly so that the civilizations that possessed smelted iron ruled during the first millennium B.C. This evidence then fits well with the rest of the evidence discussed earlier.

Upon completion of this chapter, this author learned of a recent book, *Sticks, Stones, & Shadows: Building the Egyptian Pyramids*, by sculptor and researcher of the pyramids, Martin Isler. In it, Isler presents the most recent evidence that the hard stone found in the pyramids at Giza could actually be cut with copper and also bronze with quartz sandstone. According to James P. Allen, Curator of Egyptian Art at the Metropolitan Museum of Art in New York City, “Isler’s book is a major new look at the mystery of how the pyramids were built, and it is perhaps the most well-argued and convincing approach yet to the problem.”

What does this most recent evidence indicate? Does it support the thesis that granite and other hard stones were cut and shaped and incised with copper, or copper with quartz and abrasive, or does it support the thesis of this chapter that hard stone was cut with iron and abrasives or iron with gem stones, available only in and around the first millennium? Isler writes:

“Yet while copper or bronze saws can be used to cut limestone, they cannot cut hard stones such as granite. Still examples of such have been found on the granite sarcophagus of Cheops. . . . Saw cuts have also been found on the basalt triads of King Mycerinus, where parallel, well-defined scorings on the back of the statues indicate a penetration [through to stone] of 0.125 inches (3 mm) with each stroke of the saw blade. Being too soft to cut hard stone, the metal [copper] blades must have had corundum jewels, or even bits of diamond inserted into their edges. Amazingly even before historic Egypt, the hardest stone could be sawn and drilled with the linear stroke of a blade or the rotary motion of a drill.”

Having realized that copper will not saw granite without an abrasive, as the saw marks clearly indicate, Isler, like Petrie about 100 years ago, presents the

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86 Martin Isler, *Sticks, Stones, & Shadows: Building the Egyptian Pyramids* (Norman OK 2001), back cover
87 *ibid.*, p. 129
concept that “corundum jewels, or even bits of diamond [were] inserted into their edges”. However, it was pointed out above that these hard materials for teeth in saws were not available until the first millennium B.C. What Isler’s argument does is support the first millennium as the era when these sarcophagi were sawn without an abrasive. Let us look at his second argument:

“Copper or bronze blades may simply have been used as carriers by which an abrasive such as sand, emery or diamond dust was rubbed against the stone. Working along these lines Deney’s [sic] Stocks experimentally has cut soft stone with flint chisels and hard stone with dry granulates of quartz sand carried by copper tools. Roger Hopkins, an American stone mason [on a NOVA television documentary in 1994 titled “Obelisk”] has demonstrated the same ability with a two-handed bronze saw having a blade of 0.250 of an inch (6 mm) thick with a flat cutting edge, the handles being wooden rods placed crosswise in the holes at opposite ends. Finding that the quartz sand cut faster with the addition of water, Hopkins tilted the granite block slightly, put the slurry upgrade and let it flow in to recharge the kerf [the channel made by the saw] whenever the blade was lifted. Driven by a tube of bronze, the same slurry was used to drill holes in granite or to polish its surface after shaping with dolorite hammers.” 88

Isler further claims that this was the method by which copper and bronze drills were “invariably used to hollow out granite coffers.” 89 But what he failed to discuss was the work of Garland regarding copper and quartz sand which showed that copper saws cannot cut granite or diorite and this must therefore apply to copper drills with quartz sand abrasive. He failed to discuss all the many qualifying phrases that Stocks used which indicated his experiment was not proven, only conjectured to have proven what he presented. He failed to deal with the fact that neither emery nor diamond dust was known to the Egyptians until the first millennium B.C. and thus could not be employed as an abrasive prior to that time. He failed to explain how bronze saws could have been made in Egypt prior to the Second Intermediate Period, according to the established chronology, or after 1100 B.C. according to a shortened chronology, without a source for tin. He failed to explain how sharp corners and edges were cut into these sarcophagi with abrasives which, as shown above, will not produce these forms. To cut sharp corners and

88 ibid.
89 ibid.
edges in these coffers still demands steel, a first millennium B.C. development in Egypt, according to the established chronology.

The approach to this problem is what Fischer terms:

“The fallacy of the irrelevant proof [which] consists in asking one question answering another. Suppose, for example, that a historian asks, ‘Was Senator X a thief?’ And suppose, moreover, that Senator X was a very great thief. But our hypothetical historian, who is an admirer of Senator X, proceeds to prove that Senator X had often declared that honesty was the best policy. He demonstrates that Senator X was acquitted of theft by a jury (without mentioning that all the jurymen became postmasters immediately after the trial). He publishes an affidavit in which Senator X’s mother solemnly swore that her son could never be a thief. He establishes that Senator Y was a bigger thief than Senator X, and that the Senate itself was a den of thieves. He shows that Senator X only stole from the government, and was kind to children, and faithful to his wife, and loyal to his party. He proves that Senator X used some of the money to pay for an operation which was desperately needed by a crippled orphan in Cincinnati, Ohio. He argues, in a learned Keynesian disquisition, that Senator X was a big spender in a state where money was scarce, and that his spending, compounded by many multipliers and linkage effects, brought prosperity to thousands and factories, jobs, schools, and churches. All of these statements in fact may be true, and yet Senator X remains a thief.”

By the same token Petrie asked the question: How could the ancient Egyptians have saw-cut granite and other hard stone leaving clearly visible saw marks, admitting they did so without an abrasive, or have drilled holes in these stones with drill bits that left clearly visible drill marks also, without an abrasive? But the historians who are admirers of the established chronology have proceeded to state that copper saws with abrasives can saw cut granite and diorite. They have also stated that copper drills or others can drill holes with abrasives. Although they may indicate all of this as statements of fact, yet their saws do not leave clear-cut saw marks and their drills do not leave clear-cut drill marks. Their historical disquisition proves absolutely nothing related to the question. They have answered one question by answering another, and have done so for over a century.

90 Fischer, op.cit., pp. 45-46
One major question which never seems to have occurred to the Egyptologists is that since they had saws with fixed teeth capable of cutting straight planes in granite, diorite, etc., why then did they need to use saws with quartz sand abrasive to do the very same job? Further, since they also had drills that bored holes into granite that required no abrasive, why would they then have to use drills with quartz sand to do the very same job? Finally, since they evidently were able to incise very fine and deep hieroglyphs into diorite with extremely slender chisels without quartz sand abrasive, why then do the very same work with quartz sand? This ability to saw-cut, drill, and incise hard stone without quartz sand as abrasive contradicts all the experiments conducted by researchers who employ quartz sand to work these hard stones. Their entire approach is a contradiction of the fact that the Egyptians were capable of performing all these operations with tools that left markings that proved no abrasive was used.

All these ad hoc explanations and experiments with quartz sand abrasive are beside the point and beg the question. This would be similar to finding hard wood with saw marks, drill marks, and finely incised letters and then to argue or produce experiments to show that soft wood saws, drills, and chisels with quartz sand abrasive caused these markings. An abrasive does not leave clear saw marks, drill marks, or finely grooved incisions; it leaves smooth planes because it removes only a tiny fraction of material with each stroke. As with wood so too with hard stone. It is utterly illogical to assume that the Egyptians, while having the proper tools to saw, drill, shape, and incise hard rock without an abrasive, would then use an abrasive to do the very same work.

Furthermore, as the saw marks as well as the drill marks indicate, the method of sawing and drilling these hard stones was fairly rapid. But then the historians and researchers suggest that the ancient Egyptians turned to a method of doing these operations, employing quartz sand which, as they say repeatedly, had to take much more time and require a much greater amount of physical exertion. Would any people in their right minds turn to techniques of sawing and drilling hard stone that were long, tedious, and required great exertion, when they already knew how to do the same jobs more rapidly with less toil? Once the ancient Egyptians could do this work the easy way, why in heaven’s name would they decide to do these jobs the hard way? This contradiction is so absurd that it beggars description. Yet the historians will have to cling to their abrasive methodology to maintain their chronological structure. It is a castle in the air, built on evasion of the observed facts—which are the saw marks and drill marks—that require first millennium steel and hard gem stones.
These indisputable facts speak for themselves and directly prove that the only time the Egyptians used saws, drills, and chisels to do this work without quartz sand abrasive was in the first millennium B.C. when these tools of steel or gemstones were available. But this simple understanding has failed to penetrate the minds of all those who claim that quartz sand abrasive was required in the first place when the observed saw and drill mark evidence presented by Petrie and others is in total contradiction to that notion.

For about 100 years now the historians, knowing of the observational facts, have been, in a real sense, ignoring them as if they simply did not exist, and turning to methods that have nothing to do with the facts. This author suspects that they will continue to make these same abrasive claims forever, but will fail to perform all these operations with copper tools without quartz sand. Only in this way can they maintain the illusion of their chronological construction. The hard facts regarding hard stone do not support that illusion.

The capacity of physical anthropologists and historians to maintain for about a century that soft metals can cut, shape, and engrave hard stone indicates that psychological and sociological factors can lead one to believe things that do not exist. This is not a new observation among historians of science. It can be observed in the work of a French scientist, René-Prosper Blondlot who achieved great renown and received much interest and attention from members of the scientific community. His discovery was that of N-rays which could not have existed except in Blondlot’s mind and in the minds of other scientists who were willing to suspend their ordinary skeptical beliefs and accept a fantasy as a reality.

In the historical and anthropological community, with respect to soft metal working hard stone, we discover the same psychological and sociological factors to believe something that does not exist. Throughout the literature are statements that clearly explain to these professionals that what they are presenting in their books and journals as well as in their classrooms as a method of working hard stone with soft metal cannot be upheld. These professionals’ will to believe has evidently created for them a permanent suspension of their normal scientific as well as historical disbelief necessary for good science or good history.

When scientific evidence precludes the possibility of soft metal working hard stone, and the historians refuse to accept this most obvious fact, papering it over with meaningless words that cannot for a moment explain away this
fact, then one may well understand the depth of irrational belief and commitment these professionals have to uphold their chronological structure.

**Note:** Denys A. Stocks in *Antiquity*, vol. 75 (2001), pp. 89-94, claims that he has cut and drilled Egyptian rose granite with copper saws, drills, and sand abrasive at about one inch per ten hours. This is directly the opposite result Garland achieved. Something is very much amiss. If we accept Stocks’ work as valid, does it solve the problem? No. Isler has reported that saw cuts in basalt, a very hard rock, “indicate a penetration [through the stone] of 0.125 inches (3 mm) with each stroke of the saw blade.” With 8 saw-strokes, the blade penetrated one inch! This took at most only a few minutes while Stocks’ saw took several thousands of strokes to cut one inch in about 10 hours. Petrie showed that the drill that penetrated into granite required “100 turns of the drill [to] allow it to go one inch into the rock.” With 100 turns of the drill it penetrated one inch which took only a few minutes while Stocks’ drill required several thousands of turns over several hours to accomplish the same task. Even if Stocks is correct and Garland is wrong, Stocks has not replicated with copper saws, drills, and sand abrasive the work produced by the ancient Egyptians. And neither he nor anyone else has done this work with diorite which is about as hard as steel.
CONCLUSION TO EGYPTIAN CHRONOLOGY

The scientific, technological facts are corroborated by historical statements or documents, and the historical statements or documents are corroborated by the scientific, technological facts. Since the Old and the Middle Kingdom dynasties, if they ever existed, could never have worked diorite with the copper and bronze they possessed prior to the first millennium, but it is proven such hard diorite was worked, this again requires that all the various dynasties had to co-reign in different districts of Egypt and that Egyptian history is much shorter than assumed by followers of Manethon. It simply means that the chronology of Egypt is too long and deeply flawed. That chronology cannot then be invoked as a measuring standard by which historians can arrange the history and chronology of the rest of the Near East to conform with that of Egypt. Until Egyptian chronology is corrected to conform to the scientific, technological facts, it can tell us nothing of the history and chronology of the ancient world.

Egyptologist Mark Lehner once stated, “I was excited by the process of reconstructing the past from empirical evidence.” But what does the empirical scientific and technological evidence say with respect to the chronology of Egypt? Clearly, it does not support the long chronology that Lehner and his colleagues have staunchly advocated. In fact, the empirical evidence contradicts the established long chronology of Egypt and agrees quite strongly with a chronology that is far shorter. The trunk of the conventional chronology of Egypt onto which all the surrounding civilizations have been grafted cannot now be upheld because their measuring rod—Egypt—has been shown to be invalid. In terms of the empirical scientific and technological evidence the established chronology fails because of the facts outlined above and now summarized.

In order to maintain that the Sphinx, the blocks of the Valley Temple and the Mortuary Temple were constructed during Old Kingdom times, the establishment supporters of the conventional chronology have invented ad hoc geological processes that selectively erode these limestone monuments but fail to do so as dramatically to other limestone monuments of supposedly the same age under similar conditions. This is an empirical geological contradiction to that chronology. In order to uphold the Sothic date for the placement of Sesostris III of the 12th Dynasty to the early part of the second millennium B.C., the astronomical lunar data of the 12th Dynasty should correlate with and corroborate that Sothic date. They do

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not! The astronomy correlates with Sothis-Sirius and the lunar data for the placement of Sesostris III and the 12th Dynasty in the first millennium. This is a direct astronomical contradiction to that chronology. In order to uphold radiocarbon dating as a support for the established chronology, the Egyptologists have been engaged not in employing this methodology as a means of testing that chronology; rather, their chronology has been utilized as the gauge by which they determine whether or not the radiocarbon dates they have found are valid. This is circular reasoning, not empirical testing. As for the provenance of wood as it relates to that chronology, several aspects related to provenance all fail to support that chronology either in general or in detail. Radiocarbon dating evidence cannot be construed in any way as empirical support of that established historical framework, and in a certain sense, the provenance of wood indicates that the chronology of ancient Egypt is shorter or considerably shorter than assumed.

In order to uphold the established chronology on the basis of pottery dating, the Egyptologists and archaeologists must split their minds so as to hold contradictory concepts as related to Petrie’s pottery finds at Abydos. Petrie thoroughly understood and recognized that the pottery at Abydos was foreign to Egypt, and said so. He knew it was from the regions of Mycenae, the Aegean, and Syria, and he said so. He understood these pottery forms were from the Late Helladic. But in complete defiance of the rules that were to be established by him and those who followed him, he claimed what he knew was first millennium pottery was third millennium pottery. This, too, is a fundamental contradiction to the established chronology. But then science in the form of geology and technology related to tin bronzes came into the picture. The finding of tin bronzes in Egypt prior to the end of the Second Intermediate period, when it is taken that tin was brought to Egypt, cannot sustain the established chronology. The geological and technological evidence regarding tin and the making of bronze is another direct contradiction to the established chronology. And then the Old Kingdom writings speak of the Greeks who only existed about 2000 years closer to the present. This, too, is a contradiction, not support for the long chronology.

In order to uphold the stratigraphical placement of the Old, Middle, and New Kingdoms in the conventional chronology, Egyptologists must either ignore facts, or invent ad hoc hypotheses again and again to explain away artefacts, especially pottery, from all three kingdoms found together at Old, Middle, and New Kingdom sites. This is not correlation and corroboration by the stratigraphical record for the established chronology. Clearly it is in contradiction to that chronology.
In order to uphold the fact that hard rock such as diorite, granite, etc., could be shaped and incised by either copper, bronze, or meteoric iron prior to the development of carburized iron—steel—or tin bronze, the Egyptologists must once again ignore the evidence presented by Garland or invent ad hoc hypotheses to suggest that these soft metals can do what no one has done, namely carve a diorite statue and incise it with hieroglyphs with soft copper. This they have only done strictly with their imaginations and with words, never by a public demonstration. This failure to do so is an empirical failure and is again on empirical contradiction to that established chronology.

In order to maintain the established chronology, numerous ad hoc hypotheses have been invented by the proponents of that chronology to shore it up. These have never been proven, although they are repeatedly invoked. Beyond all this the advocates of the long chronology of Manethon have repeatedly violated the logical procedures of what constitutes proper historical research as outlined by Fischer. These numerous lapses and violations of logic are historically and fundamentally inconsistent with the rational development of historical research. As with the slew of ad hoc hypotheses presented to maintain the established chronology, these repeated violations of procedural logic cannot be the basis for building and upholding the long chronology. Both the scientific and technological facts as well as valid logic deny any support or credence for the chronology presented by the historians while circular reasoning to support that chronology abounds.

On the other hand, whatever shape the revisionist short chronology ultimately takes, it is upheld by the empirical scientific and technological evidence, nor, as I believe, does it require ad hoc hypothesis upon ad hoc hypothesis, nor does it violate historical procedural logic or require circular reasoning. One does not require selective geological processes to generate rainfall erosion on the Sphinx, Valley Temple, and Mortuary Temple. These were eroded by rainfall while the others in Egypt were not because the former were constructed during an older, pluvial period and the others were constructed after that period ended. This is geological corroboration and correlation for the shortened chronology, not contradiction to it. The 12th Dynasty, in terms of empirical astronomy, does correlate with and is corroborated by 12th Dynasty lunar data, which places the 12th Dynasty in the first millennium B.C., not the third. This is a mathematically rigorous form of empirical corroboration and correlation upholding the shortened chronology, not contradiction to it. The radiocarbon dating of wood in terms of provenance along with other materials will always generate older dates or considerably older dates for sites from which these materials were taken. In no way can this methodology be invoked to suggest the short chronology is invalid. Pottery
dating and tin bronze as related to Old and Middle Kingdom sites, strongly corroborates the shortened chronology. Late Helladic pottery found at Abydos places the Old Kingdom, if it existed, squarely in the first millennium. Tin bronzes can only be manufactured, on the basis of the established chronology, after the Second Intermediate Period, chronologically placing all those Old and Middle Kingdom dynasties which had tin products after the middle of the second millennium B.C. This is corroboration and correlation for the short chronology, not contradiction. Stratigraphy in Egypt, where Old, Middle, and New Kingdom artefacts are found mixed together at the Old, Middle, and New Kingdom sites, requires no \textit{ad hoc} explanations. They are found mixed together because these kingdoms ruled concurrently in different regions over a much shorter period of time than that allowed by the Egyptologists. This, too, is correlation and corroboration for the short chronology; it is not contradiction to it. First millennium B.C. steel and hard minerals were cutting diorite and other hard rock in Egypt for all those dynasties that worked these hard materials. The finding of smelted wrought iron in the Great Pyramid at Giza and in a 12th Dynasty tomb also demands that they existed in the first millennium B.C., not the third. This is corroboration of and correlation with the short chronology, not contradiction to it.

The empirical scientific and technological evidence, as it relates to Egyptian chronology, fully fits the short chronology and on all levels refutes and contradicts the established chronology. This author is as excited by the process of reconstructing the past from empirical evidence as is Dr Lehner. But, as a matter of fact, the empirical evidence does not support, corroborate, or correlate with the chronology that Lehner and his colleagues uphold. The empirical evidence thoroughly refutes and contradicts their chronology. It strongly supports the short chronology without recourse to \textit{ad hoc} hypotheses.

The Egyptologists have failed to face these fundamental forms of evidence which speak so soberly and directly against their chronological edifice. In spite of this overwhelming empirical support for the short chronology it seems clear that these historians are incapable of accepting that these facts prove that the chronology which they embrace can in no way be upheld. The empirical scientific and technological facts on every level contradict their history. What they have been left with to answer those who presented these facts are emotional outbursts and misrepresentations like those of James and Thorpe respecting Schoch’s evidence on the Sphinx, and Spalinger’s deplorable misrepresentation of Rose’s astronomical evidence, as well as the wholesale failure of the historians to face up to the evidence of one of their own researchers—Dayton—regarding tin, as evidence for bronze as well as stratigraphy. In all, Dayton raised over 20 technical aspects of minerals that
contradict the established chronology. This author has raised only a few of these contradictions. With so many scientific and technological forms of evidence contradicting the Egyptian trunk of historical chronology it cannot be the measuring rod for the chronology of the rest of the ancient Near East. In fact, it cannot stand on its own feet. The very foundations upon which this chronological framework was erected, namely, astronomy, pottery dating, stratigraphy, bronze production, iron production, etc., and to a great extent radiocarbon dating, prove that the empirical scientific and technological facts are in opposition to it and that it cannot stand.
CHAPTER 8
MESOPOTAMIA AND GHOST EMPIRES

As in Egypt with its king lists, Mesopotamian history was in part derived from similar sets of lists. They, too, indicated that civilization in the Fertile Crescent was exceedingly long, especially that presented by a priest-scholar named Berosus.

“Berosus, a Chaldean priest and a contemporary of Manetho, tried to prove to the Greeks under the Seleucid rulers [who as foreigners dominated his homeland] the [great] antiquity of Assyro-Babylonian history and therefore he extended that history into tens of thousands of years.”

Also as with Egypt, there are a number of these lists which have come to light. Therefore, the early foundation of Mesopotamian chronology was built on the same kinds of evidence as that of ancient Egypt. That is, the foundation of that chronology was constructed on historical documentation rather than empirical scientific facts. The fundamental error, this author submits, lies in the way the problem of chronology was attacked. As we will see again, many of the very same findings of empirical science that deny the validity of Egyptian chronology deny the validity of Mesopotamian chronology as well. How problematic are these King Lists? Ceram describes them thus:

“What we call ‘king lists’ are those tables which give the names of kings and the length of their reigns. The oldest list of this type found in Asia Minor is an eight-inch square block of stone enumerating the ‘proto-kings,’ the mythical rulers who reigned between the Creation and the Flood [and who each reigned for about 28,000 to 64,800 years]. This table extends as far as historic times—just before the beginning of the second millennium B.C.

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1 I. Velikovsky, *Peoples of the Sea, op.cit.*, pp. 207-208
“This list, which scholars call the ‘Old Babylonian King List, WB 444,’ is complemented by two others known as ‘A’ and ‘B.’ With the aid of these, of the ‘Assyrian King List’ found at Khorsabad in the winter of 1932-3, and of several fragmentary lists of later dynasties, chronology can be brought down to the first millennium B.C.—at which point there is sufficient information from other [documentary] sources to make exact dating fairly easy.”

Are these lists accurate and reliable, and can one rely upon them to build a chronology of Mesopotamia? Ceram points out that these lists are no more reliable than those of Manethon:

“Archaeologists and historians would be delighted if they could at least depend on the order of succession [of the king lists]. But that they cannot do. The Babylonian listers casually omitted kings whom they considered unimportant. Or else they copied wrongly. Or else they took dynasties which reigned simultaneously and listed them one under [meaning after] another instead of side by side [meaning contemporaneously].

“A document from the time of King Sargon I (about 2350 B.C.), for example, maintained that no less than 350 kings had ruled over the land of Assur before the said king—an utterly implausible statement which nevertheless gave rise to enormous confusion. Scholars finally discovered where the mistake lay: the copyist had simply added together several different, concurrent king lists.

“Exploring the mazes of chronology, scholars are sometimes able to find help in the ‘date and eponym lists.’ From the time of Sargon I to the time of Hammurabi (almost 700 years) Babylonians were in the habit of giving names to the years and writing down catalogues of these names. This sounds simple enough—but the ancients made things hard for themselves and for us.

“True dates, which cite a particular year, or a definite month or day within that year, are very rarely found. The Babylonian scribes would name the year for an important event which had taken place the

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previous year—to our minds an illogical procedure, but one which was long practiced in Egypt also, during the Old Empire [Kingdom]. . . .

Scholars soon recognized that these lists were tricky. The custom was for the beginning of a new reign to be counted officially from the time of the New Year Festival; the period from the actual change of government until the end of the year was included within the old [dead] king’s reign. But there was no way of telling whether the customary method of reckoning [to the end of the year] had been followed. Moreover, the names of years were often changed—in obedience to the whims of kings, or in deliberate falsification of history, or for whatever reason. Finally, the scribes had a habit of abbreviating the names—often distorting them beyond recognition. Or else they cited [them] from memory and made mistakes. . . .

“There were still further complications. The ‘eponym lists,’ in which the year was named after a high official, general, or king, rather than after an event, were equally perplexing. . . . But as with the date lists there were irrational deviations from the norm.”

Ceram claims: “All this should make it clear that the establishment of a chronological framework for ancient [Mesopotamian] history was no easy matter.” As with the king lists of Manethon for Egypt, the king lists for Mesopotamia, considering all the fixing up that the historians employed to make clear sense of the chronology, hardly qualify as anything other than an exercise in guesswork. Though this is educated guesswork, it is still guesswork.

Nevertheless, Professor Stiebing has invoked these king lists to criticize Velikovsky’s historical revision of Bronze Age chronology in Mesopotamia, which he presented in the Biblical Archaeology Review (Dec. 1976), p. 43; this was reprinted in KRONOS, vol. VII, no. 3 (1982), pp. 72-74. Stiebing states:

“According to Velikovsky’s theories, both Asshur-uballit and Burnaburiash must be identified with the ninth century Assyrian king Shalmaneser III (c. 858-824 B.C.)”

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3 ibid., pp. 138-139
4 ibid., p. 139
5 Bimson and James, op.cit., p. 73
But this “identification” is false, as Stiebing himself admits: “He [Velikovsky] claims that this king assumed the name Burnaburiash after his conquest of Babylon.”

First Stiebing claims Velikovsky identified Shalmaneser III with both Asshur-uballit of Assyria and Burnaburiash of Babylon, but then he offers that Shalmaneser only used the name of Burnaburiash in Babylon. Nowhere in his book *Ages in Chaos* does Velikovsky even discuss Asshur-uballit, nor in his book *Peoples of the Sea*. Only in *Ramses II and His Time* does Velikovsky discuss Asshur-uballit twice: “There he [Mursilis] met and fought Assuruballit, king of Assyria” and “. . . Assuruballit, a younger brother of Assurbanipal . . . proclaimed himself king of Assyria. According to the Chronicles, ‘Assur-uballit in Harran took his seat on the throne as king of Assyria. . . .’”

And two kings “united their armies and toward Harran, against Assuruballit . . . they marched.” Egypt gave assistance to Assyria as its ally, “Assuruballit in Harran”.

Nowhere does Velikovsky state, as Stiebing claimed, “that Assur-uballit . . . must be identified with the ninth century Assyrian King Shalmaneser III . . .” Stiebing has simply fabricated this identification and assigned it to Velikovsky. What then of Stiebing’s criticisms based on the king lists? John Bimson and Peter James analyzed this material and report:

“Stiebing then claims that ‘Assyrian king lists giving the names of Assyrian kings as well as their counterparts in Babylon disprove Velikovsky’s reconstruction.’ In fact there is only one such list, the so-called *Synchronistic Chronicle* or *King List*. (A.L. Oppenheim translated it in *Ancient Near Eastern Texts Relating to the Old Testament*, J.B. Pritchard, ed. (Princeton 1969), pp. 272-274)

“Stiebing [writes]: ‘These lists [sic] indicate that the Kassite kings Kadashman-Enlil (I) and Burnaburiash (II) did rule Babylon at about the same time that Assur-uballit (I) was king of Assyria.’ [Bimson and James reply:] The *List* does not [even] mention these three rulers—in fact, it does not cover the period of Assuruballit I (14th century B.C.) at all.
“[Stiebing writes:] ‘Furthermore, the Assyrian king lists make it clear that the period when these kings ruled was long before the time of Shalmaneser III . . .’ [Bimson and James reply:] While ordinary Assyrian king lists do place an Assuruballit, the First (and not necessarily the el-Amarna correspondent) some five centuries before the time of Shalmaneser III, neither the Synchronistic Chronicle nor any other list includes Kadashman-Enlil (I) or Burnaburiash (II). How then is it ‘clear that the period when these kings ruled was long before the time of Shalmaneser III’? [Cf. Peter J. James, “Some Notes on the ‘Assuruballit Problem’”, SISR IV:1, Autumn 1979, pp. 18-22.]

“[Stiebing writes:] Shalmaneser III’s ‘counterpart in Babylon is given as Murduk[sic]-zakir-shumi.’ [Bimson and James reply:] Shalmaneser’s counterpart is actually given as Nabu-zakir-shumi in the List, though this is presumably a mistake for Marduk-zakir-shumi, the ruler of Babylonia known from Shalmaneser III’s annals.”

Stiebing did reply in a letter to support the validity of his criticisms, but refused to allow it to be published. Nevertheless, James citing from it showed that Stiebing’s errors only multiplied.12 Those interested in the evidence should obtain this material to understand the jejune poverty of Stiebing’s handling of the king list materials. As James states:

“In conclusion, the seven mis-statements of fact I picked on still remain seven mis-statements of fact, and your [Stiebing’s] answer has only multiplied the number. When you accuse other people [who are revisionists of ancient history] of ‘ignorance’ it would behoove you to check your own ‘facts’, and at least look at the documents you are citing. Absurdities such as the suggestion that Assuruballit I reigned before the period covered by the Synchronistic Chronicle, or your total misunderstanding of what Chronicle P has to say, would shame anyone who claims to have a passing acquaintance with the material in question. . . .

“In the meantime, careless sweeping generalisations about king-lists and chronicles get us nowhere. The debate is about the evidence, not what you think you ‘know’ to have happened in the 14th century.”

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11 Bimson, James, op.cit., pp. 74-75
12 ibid., pp. 78-85
The point that must be emphasized is that a historian of standing in the community of historical scholarship was prepared to misrepresent basic evidence in the king lists to destroy the shortened chronology of Velikovsky. Stiebing mangled this material in order to make it conform to the established chronology and therefore it may also have been mangled by historians from its discovery to conform with their chronological expectations.

Nevertheless, a historical construction of the four to five major civilizations was built by the historians that organized the chronology into the following order for Mesopotamia:

1. The First Chaldeans were the earliest
2. The Assyrians followed and conquered these earlier people
3. The Second Chaldeans regained their power when the Assyrians were overthrown
4. The Medes for a time came to power subjugating the Second Chaldean Empire
5. The Persians were the last to arise and rule in Mesopotamia.

This was the historical framework and the chronology of ancient Mesopotamia up until excavations carried out from the middle of the 1800s unearthed what can only be termed six completely new civilizations:

1. Sumerians
2. Akkadians
3. Neo-Sumerians
4. Mitanni
5. Old Babylonians and Neo-Assyrians
These new civilizations therefore had to be placed inside the earlier conventional framework. However, Professor Gunnar Heinsohn and later also Emmet J. Sweeney, in a series of books and papers, based on the stratigraphical record, have challenged the existence of these new Mesopotamian empires. Stratigraphy, they claim, indicates quite clearly that the first five new empires noted above have no basis for their existence. They are, according to Heinsohn, duplications of the original five empires. His chronology equates these as follows:

1. The Sumerians are equated with the First Chaldeans
2. The Akkadians are equated with the Assyrians
3. The Old Babylonians are equated with the Second Chaldeans
4. The Mitanni are equated with the Medes
5. The Old-Babylonians and Neo-Assyrians are equated with the Persian conquest of Babylonia and Assyria during Persian times
6. Sweeney has equated the Lydians with the Hittites but accepts most of Heinsohn’s “alter ego” equations.

Historians would claim that the documented records of Mesopotamian history fail to support Heinsohn and Sweeney’s interpretation of the chronology. But their criticism fails to take into account that the documented record is not only fragmentary but distorted. These records were kept in two or more languages evidently by different scribes. Events important to one scribe writing in one language might have been trivial to another writing in another language. The same also applies to kings, as Ceram stated: “The Babylonian listers casually omitted kings whom they considered unimportant,” etc. From the start this has been one of the obstacles to evaluating Heinsohn’s thesis: critics simply cite some kings from one known dynasty that is listed and claim that they are not included in that of the “alter ego” dynasty; ergo Heinsohn is disproven. But that is based on the assumption that the Mesopotamian record is fairly complete and not largely incomplete, as in fact it is. What we have once again is circular reasoning: Heinsohn and Sweeney must be in error because the documented record is inconsistent and does not corroborate their thesis. Their critics argue circularly from a highly distorted and fragmented record that has been rearranged by establishment historians to conform to their chronology.
As James et al. show:

“There is ample evidence to show that the ‘dates’ given by the ancient Mesopotamians for their own history were often wide of the mark. An inscription of the 6th-century Babylonian King Nabonidus states that the famous King Hammurabi reigned 700 years before Burnaburiash, while modern reckoning separates them by some 300 years. Nabonidus also claimed that 3200 years elapsed between his own time and that of King Naram-Sin of the Akkad Dynasty. This would place Naram-Sin c. 3750 BC, a full 1500 years older than any modern estimate!”

But what of the stratigraphy?

Importantly, the stratigraphy in Mesopotamia is quite different from that of Egypt. It is based not only on the relics found at a site, but on the particular layer or stratum in which it occurs. The various empires of Mesopotamia which spread out over that region from their homelands left indelible evidence of their existence by a host of identifiable objects. These include foundations of buildings of stone, brick, etc., pottery, jewelry, tools, graves with objects in them, knives, weapons, pins, the bones of animals they ate or domesticated, garbage dumps, middens (human wastes), leather and cloth that may survive in highly acid soil, and many other products. It is this material by which the archaeologists organize the chronology by its placement in a particular layer in the various strata of an excavated settlement.

In Mesopotamia this stratigraphic record is the alpha and omega of Heinsohn’s approach to chronology.

Because water and arable land or land near enough to a secure water source—usually a river—is necessary to carry on agriculture, one prerequisite for Mesopotamian society, composed of a large population, to flourish was to build only in certain areas near that water. The river not only provided drinking water for man and his domesticated animals, but allowed irrigation to support large agricultural production. The clay and mud found in and around the river provided the wherewithal with which Mesopotamian artisans could fabricate sun-dried and burnt

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13 James, et al., op. cit., p. 293
brick to build towns, cities, and farm houses, as well as the pottery necessary to these fairly advanced cultures.

The winter rainstorms that periodically drenched these abodes over time caused damage to these brick cities which were probably repaired to maintain them; but over time rainfall was so damaging that the buildings fell into such disrepair that they were torn down and new ones were erected on their rubble. This building and tearing down process created the numerous hills throughout Mesopotamia known as “tells” in which city upon city had been constructed. Furthermore, if a city was conquered by a foreign power, the people who settled in these towns and cities would bring their own distinctive forms of pottery, etc., with them; they, too, would live in that same settlement and build and tear down buildings, leaving their distinctive relics in the layers above those of the people they had vanquished. Thus Mesopotamia offered a splendid series of strata upon which archaeologists and historians could interpret the history and erect their chronology.

Basically their analysis is quite like that of geology. Here the process of super-position comes into play. The oldest settlement at these tells is always that at the lowest/deepest level in the city. The various peoples who came one after the other are always located by the same principle. If four different empires settled in the same region and occupied the same city, the one at the bottom of the tell would be the oldest, the strata above that would contain those that came second; those above that came third, and at the top those that came last.

What one would expect to find somewhere in Mesopotamia, based on the established chronology, is a major city, conquered by all the various empires, exhibiting in the layers of the ground evidence that the eight to ten of them existed one above the other. This has never been found. (This statement is entirely based on Heinsohn’s criticism of the established chronology.)

But there is another problem that Heinsohn points to in the stratigraphical record that defies probability. If indeed there were truly eight or more empires that reigned seriatim (one after the other) in various regions of Mesopotamia, then there should exist, in the ground, stratigraphical evidence for each and every one of them. Such archaeological evidence would corroborate and confirm the historical chronology. That is, the archaeological record should exhibit strata for Sumerians as well as Chaldeans. It should exhibit strata for Old-Babylonians and Neo-Assyrians as well as Persians. It should exhibit strata for Mitannians as well as Medes, etc. This would fully justify the fact of their existence.
in terms of the established chronology. Surely civilizations that inhabited and reigned over broad swathes of Mesopotamia for centuries could not conceivably have done so and then failed to leave artefacts in the strata that would prove they really existed? Interpretation may allow for such identifications, but to be sure we would expect to find at one site Sumerians and Chaldeans, Neo-Sumerians and Chaldeans, Old Babylonians, Neo-Assyrians as well as Persians, and Mitanni as well as Medes.

Let us recall that Peter James demanded evidence from Schoch and West of the pre-Egyptian civilization that built the Sphinx and Valley Temple to support their thesis. The very same form of evidence applies to all the empires that ruled Mesopotamia. If they all indeed existed, then they had to have left plenty of archaeological evidence as proof of their existence.

But if Heinsohn and Sweeney are correct that these recently discovered civilizations are the “alter egos” of the originally well-known five empires and that only five or so ever reigned and existed, then the archaeological evidence should contain evidence of only these five or so civilizations. If one takes all of the archaeological evidence found in the ground for the Chaldeans and claims it is that of the Sumerians, then there will be nothing or almost nothing left in the ground to prove that the Chaldeans ever existed. This is especially so since the historians claim both ruled over and inhabited the same region in southern Mesopotamia. What should be found in that area is archaeological strata here and there for both Chaldeans and Sumerians in the same tell. This cannot be doubted.

The very same condition also applies to the Old Babylonians (sometimes called the First Babylonians) as well as the Neo-Assyrians and the Persians. If three of these civilizations existed in the same region, then archaeological evidence should unambiguously exist for all three cultures. But if one takes all of the archaeological evidence found in the ground from the Persians and claims that it is that of the Old Babylonians and Neo-Assyrians, then there will be nothing or almost nothing left in the ground to prove that the Persians existed. What should be found quite clearly in the archaeological record is evidence for the existence of Persians, Old Babylonians, and Neo-Assyrians. This, too, cannot be doubted.

The same applies to the Mitanni and the Medes, etc.; civilized people do not occupy large regions and then fail to leave behind all the various artefacts in the ground to tell us that they existed. To suggest that such a state of affairs could
exist for several different societies defies reason. The beauty of Mesopotamian strata is that they are found in tells in a highly arid region. Pottery, we were told, is the “bread and butter” of archaeology and history; much of it also survives. To propose that the Chaldeans, Persians, Medes, etc. could have lived in Mesopotamia, each for several centuries in cities manufacturing all the products of civilized society but then failing to leave little if anything in the ground to testify to the fact of their existence, is so highly improbable that one cannot sustain the conventional chronology unless all these various empires are found by their archaeological remains in great numbers.

Therefore we have a methodology by which to ascertain which of the chronologies is correct. If the long chronology upheld by the establishment historians and archaeologists is correct, then archaeological evidence (and plenty of it) will be found in Mesopotamia for all eight to ten civilizations. There will be lots of archaeological evidence for Chaldeans as well as Sumerians, for Old-Babylonians and Neo-Assyrians as well as Persians, for Mitanni as well as Medes and Neo-Assyrians, etc. If, on the other hand, Heinsohn and Sweeney’s concept is correct, that only five or so empires existed, and not eight or ten, then five or so of them will have been found and there will be nothing or almost nothing in the archaeological record for the others. The astonishing fact is that evidence in support of Heinsohn and Sweeney’s thesis is undeniable. This writer will only deal with the strata for the Chaldeans and Sumerians, the Old-Babylonians, Neo-Assyrians and the Persians, and the Mitanni and the Medes.

Before proceeding it is important to point out the true significance of the difference between the established chronology and that of Heinsohn and Sweeney. With Egypt, as noted above, the scientific and technological data indicate that Egyptian history does not go back to around 3000 B.C., because the evidence places several dynasties of the Old and Middle Kingdoms in the first millennium B.C. The same applies to Mesopotamia. If, as the historical-archaeological establishment claims, there are some eight to ten empires that ruled seriatim, each for some centuries, then Mesopotamian history would be exceedingly long and its chronology would begin about 3500 B.C., making it even older than Egypt is assumed to be. If Heinsohn and Sweeney’s claims are correct, and instead of eight or ten civilizations there were only four or five that each ruled for some centuries, then Mesopotamian history would be exceedingly short and its chronology would begin about 1200 to 1500 B.C. This in large measure is what the following chapters of this book will demonstrate.
Both the Sumerians and Chaldeans, according to established chronology, dwelt in southern Mesopotamia. But only the Chaldeans were known to subsequent civilizations while the Sumerians, who may not have existed, were unknown. Will Durant, in his first volume of human history, informs us:

“The unearthing of this forgotten culture is one of the romances of archaeology. To those whom . . . we call ‘the ancients’—that is, the Romans, the Greeks, and the Jews—Sumeria was unknown. Herodotus apparently never heard of it, as something more ancient to him than he to us. Berosus, a Babylonian historian writing about 250 B.C., knew of [a land] called Sumeria only through the veil of legend. He described a race of monsters led by one Oannes, coming out of the Persian Gulf, and introducing the arts of agriculture, metal working and writing; . . . Not till two thousand years after Berosus was Sumeria rediscovered.”

Berosus had attempted to make the history of Mesopotamian civilization stretch as far back into the past as possible. Yet when he examined the evidence of his ancient homeland, he was unaware that such a civilization had existed. It is extremely difficult to accept such a highly improbable state of affairs in view of the great influence these so-called Sumerians are supposed to have had on all the empires that followed them. Regarding this influence, A. Leo Oppenheim explains:

“The immense problem of the extent, the validity, and the effect of the Sumerian legacy on Mesopotamian civilization cannot and will not be dealt with here. The Sumerians left their imprint in varying degrees on all things Mesopotamian. Their traces are palpable; they range from the most obvious, such as the preservation of Sumerian texts in certain cultic practices and the use of Sumerian as the vehicle of special literary expression, to the mass of individual Sumerian loan words that refer to all levels of literature, to all aspects of Mesopotamian civilization. There is a Sumerian influence, real or seemingly real, in the social sphere, as in the concept of kingship, and in the phenomenon of urbanization, and the arts, in the motif repertory of the mythological and in the monumental architecture and in the glyphic.”

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14 Will Durant, Our Oriental Heritage (NY 1954), p. 118
15 A. Leo Oppenheim, Ancient Mesopotamia (Chicago 1977) p. 4
With this profound influence of a literate people on the succeeding literate nations, the fact that neither they, nor their history, nor their kings, nor their cities were known is simply incredible. It would be comparable to forgetting that the Romans had existed by the literate people who followed them. To paraphrase Oppenheim:

“The immense problem of the extent, the validity, and the effect of the Roman legacy on European civilization cannot be dealt with fully here. The Romans left their imprint in varying degrees on all things European. Their traces are palpable; they range from the most obvious, such as the preservation of Roman texts in certain practices and the use of Latin as the vehicle of literary expression, to the mass of individual Latin loan words in European languages that refer to all levels of literature, to all aspects of European civilization. There is a Roman influence, real or seemingly real, in the social sphere, in the arts, in the motif repertory of the mythological, and in the monumental architecture and in the scripts.”

What historians are asking us to accept with respect to the Sumerians would be similar to their later literate successors not knowing of the Romans. Yes, many of the illiterate of Europe were largely unaware of these people, but not the literate, like the Byzantines, who survived for about a thousand years longer than the Romans. This is the enormity of the problem historians are offering us regarding the Sumerians. Jacquetta Hawkes also refers to Sumerian influence on later Mesopotamian culture:

“The scribes and scholars of the Sumerian edubbas [who wrote of religious traditions] and the schools that followed them were marvelous conservationists, keeping a dead language and its literature current through two millennia; this long-drawn effort represents an exceptional reverence for ancient traditions which is manifest also in most aspects of Mesopotamian culture.”

The Roman Catholic Church was also organized on the same lines as Roman government. It kept alive a dead language for some 2000 years, yet these literate churchmen never forgot that Roman civilization existed. They knew of their kings and emperors such as Julius Caesar, etc. Thus, what historians apply to

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16 Hawkes, *op. cit.*, p. 245
Sumerian civilization they do not apply regarding Roman civilization though it follows similar lines.

Nevertheless, we must still examine the evidence from a stratigraphical standpoint. The so-called Sumerians lived in large cities, towns and villages in Southern Mesopotamia. So, too, did the well-known Chaldeans. According to H.W.F. Saggs, the Chaldeans

“... often took Babylonian names, and at least some of the more prominent of them settled in the old cities within their tribal areas... before the end of the ninth century they had begun to build their own fortified towns. By 700 BC, when Sennacherib undertook a major campaign against some of the Chaldean tribes, he listed eighty-eight fortified cities of theirs, many of them new creations, although a few were ancient Babylonian cities...”\(^{17}\)

Georges Roux also discusses these “fortified cities and well-defined territory” [of the Chaldeans].\(^{18}\)

The Greek geographer Strabo well knew of the Chaldeans and the territory they occupied:

“There is also a tribe of the Chaldeans, and a territory inhabited by them, in the neighbourhood of the Arabians and of the Persian Sea, as it is called.”\(^{19}\)

“The country of the Babylonians is surrounded on the east by the Susians and Elymaeans and Paraetaceniens, and on the south by the Persian Gulf and the Chaldeans as far as the Mesenian Arabians...”\(^{20}\)

“But as for the parts on the far side of the Euphrates, those near its outlets are occupied by Babylonians and the tribe of the Chaldeans.”\(^{21}\)


\(^{19}\) *The Geography of Strabo*, transl. H.L. Jones (Loeb Classical Library), 16:18

\(^{20}\) ibid., 1:8

\(^{21}\) ibid., 3:1
Thus, we have the specific area in which the Chaldeans and Second Chaldeans lived. We have historical documentation that they dwelt in 88 fortified cities and other small towns for many centuries. As with their supposed predecessors, they lived in cities of sun dried brick, they used pottery, jewelry, artefacts of all kinds to carry on their lives, wrote laws and business contracts in cuneiform writing on clay slabs, produced garbage, needed weapons, carved reliefs in stone, erected temples and statues of their kings and gods, etc. With all this activity, there should be plenty of these materials left in the strata to tell of their existence. Nevertheless, D.O. Edzard makes the statement:

“Of the Chaldeans (Kaldu . . .) . . . not even a brick, not even a potsherd could be discovered.”

Where the tells that contained 88 Chaldean cities are, is quite unknown, yet we have numerous Sumerian cities. The implication surely supports Heinsohn and Sweeney's thesis that all the strata given to the Sumerians and Neo-Sumerians are really the strata of the Chaldeans and Second Chaldeans.

Since the historians assume that the Sumerians are among the earliest people to settle in Mesopotamian, while the Chaldeans came much later in time, geological destruction should be more devastating on the remains of the older Sumerian civilization than on that of the Chaldeans that built their cities long after. Older cities are subject to greater seepage of rain into them than younger ones. As one goes deeper back in time the elements of fire, flood, rain erosion, earthquake, and man digging into the ground to lay new foundations for new houses, palaces, etc., would have created greater damage and decay than that to be found on those built closer to the present.

“The golden palaces of the ancient East are today traced from ruins, bricks, washed down and so blended into the soil that it is hard to see where the wall ends and earth starts.”

Then we come to the devastation of the land by new people settling in the older regions who often uproot the remains of the people they replace:

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23 *The Oxford Companion to Archaeology*, Brian Fagan, ed. (NY 1996) p. 43
“The landscape is a palimpsest on to which each generation inscribes its own impressions and removes some of the marks of earlier generations. Constructions of one age are often overlain, modified, or erased by the work of another.”\textsuperscript{24}

Generation upon generation ripped into and modified the older cities more than the younger ones. But in spite of all these destructive processes, the older Sumerian cities survived while the later Chaldean cities were destroyed to the last brick, to the last shard of pottery. On the subject of Sumerian pottery, Kramer writes:

“One of these craftsmen in clay was the potter who learned very early to construct and fire his kiln, as well as to burnish and decorate his indispensable ware with colorful designs; by 3500 B.C., he had begun to use the potter’s wheel and was turning out his product on a mass-production basis.”\textsuperscript{25}

Thus there were great numbers of pottery and broken pottery pieces left in the ground by the Sumerians. As we were told earlier, pottery is among the best-preserved materials left in the ground. But living in the same region for centuries at a time closer to the present, the Chaldeans, who must certainly also have produced enormous amounts of pottery, never even left a potsherd behind. This is far too improbable to be accepted.

With respect to language, the ancient peoples of Mesopotamia kept their records in clay, using cuneiform writing. The thousands of tablets that they used can survive in the ground, somewhat like pottery.

James \textit{et al.} explain:

“By its very nature cuneiform was never written on perishable materials such as parchment or papyrus. Clay is the only suitable medium for the script, written by impressing a series of wedge shapes into wet clay with a stylus. Baked tablets [like pottery] can last almost indefinitely.”\textsuperscript{26}

\textsuperscript{24} M. Astron, T. Rowley, \textit{Landscape Archaeology: An Introduction to Landscape Techniques on Post-Roman Landscapes} (London 1974) p. 14
\textsuperscript{25} Samuel Noah Kramer, \textit{Cradle of Civilization} (NY 1967) p. 146
\textsuperscript{26} James \textit{et al.}, \textit{op.cit.}, p. 281
Can one actually accept as historical fact that people simply stopped writing cuneiform for centuries and then abruptly some other people did so?

This is what Kramer has to say on the subject of Sumerian tablets:

“In spite of the obstacles, the excavating [at Sumer] continued, and the expedition achieved magnificent, and in some respects unparalleled results, at least in the inscriptional field. The Nippur expedition succeeded in excavating some thirty thousand tablets and fragments in the course of its four campaigns, the larger number of which are inscribed in the Sumerian language and range over more than two millenniums, from the second half of the third to the last centuries of the first millennium B.C.”

Without doubt, the Chaldeans must have written thousands upon thousands of clay tablets, just as did the Sumerians. But as Edzard explains: “Apart from a few personal names and some place names—not yet analyzed linguistically—there is no known material on the language of the Kaldu.” This, too, is so highly improbable that it again strongly suggests that the Sumerians are, as Heinsohn claims, the Chaldeans.

All the cities, pottery, tablets, and the whole panoply of artefacts found in southern Mesopotamia attributed to the Sumerians are in reality, according to Heinsohn, the cities, pottery, tablets, and artefacts of the Chaldeans. Are we to believe that almost every shred of Chaldean evidence in the ground was destroyed but immense amounts of the material of the Sumerians inhabiting the same region, and under the same conditions, but at a much earlier time, survived? Perhaps the Chaldean kings were all modest and self-effacing and wished to leave nothing of their glory to posterity, and therefore destroyed all evidence of their existence. Or perhaps the scribes’ motto was “silence is golden” and therefore they wrote almost nothing on clay tablets. Or perhaps pottery and bricks were taboo objects and were never produced. The only way for the Chaldeans to have failed to leave any legacy in the form of material in the ground in Mesopotamia is if they never existed! But since we know they existed from innumerable statements by other civilizations that thrived in the Near East, then they must have left cities, pottery, bricks, inscribed cuneiform tablets, and all the rest in the ground. The people that did not exist, the

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28 Edzard, *op.cit.*, p. 291
Sumerians, have been invented by the historians and have been invested with, and been given, all the archaeological strata that in fact belong to the Chaldeans. As Abram Joseph Ryan wrote in the foreword of his *A Land without Ruins*:

“A land without ruins is a land without memories—a land without memories is a land without history.”

It is the Sumerians that are the people without any memories in history. They are without memories and without history because they did not exist. The Chaldeans were a people with memories in history because they are, and were, known to have existed. The ruins in the ground attributed to the Sumerians apparently belong to the Chaldeans. They could never have existed without leaving numerous cities in tells containing pottery, shards of pottery, by the tens of thousands, and tablets with cuneiform script and all the rest.

What then of the strata of the Medes and Mitanni? What do they tell us with regard to Velikovsky, Heinsohn, and Sweeney's hypothesis? It must be stated that in terms of priority, Velikovsky was the first of the revisionists to equate the Medes with the Mitanni: “We assume that the Mitanni was the original name of the Medes. . .”29

Of the Medes we learn:

“In many respects, the internal organization of the Median Empire probably resembled that of Assyria, but little is actually known. Few identifiable ‘Median’ objects have been found . . . since no Median written documents of any kind have ever been uncovered, their spiritual and economic life is also a matter of conjecture.”30

Here we have, as with the Sumerians and the Chaldeans, two civilizations occupying roughly the same territory at different times, yet there is plenty of evidence and documents for the earlier Mitanni, but almost nothing for the later Medes. Michael Roaf discusses all that is attributed to the Medes:

29 Velikovsky, *Ramses II and His Time, op.cit.*, p. 179
“A fortress at Tell Gubba in the Hamrin region and a building with columned halls at Tille Hüyük on the Euphrates in southern Turkey have been attributed to the Medes, . . .”\textsuperscript{31}

With regard to their sites, Oppenheim admits,

“the Medes—left us but scanty archeological evidence.”\textsuperscript{32}

As Charles Burney explains, “In all three [sites] are buildings which can plausibly be associated with a local clan chief. . . . The ordinary people . . . [were] tent dwellers.”\textsuperscript{33} Thus, like the Chaldeans in the southern region of Mesopotamia, who did not leave cities and are therefore assumed to have lived in tents, it is assumed that the numerous missing Median cities are represented by buildings for chieftains but that the people were tent dwellers.

Yet the so-called Mitanni, unknown to history, have left archaeological evidence in the ground while the well-known Medes have left almost nothing in the strata to tell us of their existence. Heinsohn and Sweeney explain this highly improbable stratigraphical evidence by stating that the Mitanni are the Medes, and all the archaeological relics attributed to the Mitanni belong to the Medes.

Sweeney, using Velikovsky’s work, presents the evidence (in part) about the similarities between the Medes and the Mitanni:

“From the personal names of the Mitanni kings we know that they were of a race who spoke a language closely related to Persian [W.F. Albright, \textit{The Amarna Letters from Palestine} in \textit{CAH} vol. 2, pt. 1 (3rd ed.), pp. 109-110], virtually identical in fact to the Indo-Iranian language of the Medes. The text of a treaty between Mitanni and the Hittite land shows that Mitra, Varuna, and Indra, deities of Indo-Iranian origin, comprised the Mitanni pantheon. Indo-Iranian technical terms appear with great frequency in the Mitanni vocabulary. [e.g. W.F. Albright, \textit{From the Stone Age to Christianity}, p. 153] True, another racial and linguistic group, designated Hurrian, is evinced in Mitannian documents and personal names. The exact relationship between the Hurrian and Iranian elements is unclear, though it would appear that the

\textsuperscript{32} Oppenheim, \textit{op.cit.}, p. 69
\textsuperscript{33} Charles Burney, \textit{The Ancient Near East} (Cornell NY 1977), p. 201
Iranian group was dominant, for all the Mitanni kings clearly had Iranian names. Hurrian is non-Indo-European, and is closely related to the language of Urartu, the region of eastern Anatolia immediately south of the Caucasus. From the Iranian royal names, and the Indo-Iranian gods worshipped by the Mitanni, it seems certain that the Mitanni kingdom was comprised of an Indo-Iranian aristocracy who had subdued, and ruled over, a largely non-Indo-Iranian population [of the Hurrians/Urartians]. Confirming this is the fact that the Mitanni warrior-class bore an Indo-Aryan name, Mariyanna, a term recognised as related to the Sanskrit marya, a nobleman.

“One thing is clear: the original Mitanni kingdom occupied almost exactly the same position as historical Media. Even the name Mitanni, or more correctly Mita, is indistinguishable from that of the Medes, the Madai; and Herodotus . . . names Matiene as a province of the Persian Empire located very close to Media.

“The capital of Mitanni is generally given as Washukanni, or Washuganni; though, since cuneiform vowels are conjectural, the name could equally be reconstructed as Awshakanna, or Ebshakanna. Furthermore, since in many languages the sounds ‘sh’ and ‘t’ are frequently confused . . . the name could even be reconstructed as Ebtakanna. The capital of the Medes, rendered Ecbatana by the Hellenic authors, is apparently little more than a hypocoristion [diminutive] of this word.

But the parallels between the Mitanni and the Medes go far beyond mere similarities in homelands and names [for gods, technical terms, and class position of warriors, and the capital city]. For the Median-speaking Mitanni were conquerors of Assyria, as were the Medes themselves, supposedly eight centuries later.”^34

Is it reasonable to accept that a nation—the Mitanni—that ruled eight hundred years before the Medes, in virtually the same region, should worship the same gods, have the same technical terms, have kings with the same names, and a capital that could clearly be similarly named, all by coincidence? This could be argued against by those in support of the established chronology. However, what takes precedence over this is again scientific and technological facts that corroborate

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^34 Sweeney, *op. cit.*, pp. 8-9
a much shorter chronology; these cannot be dismissed, and they corroborate the Mitanni as being the Medes.

Finally, we come to the Persian strata and those of their conquered empires, the Babylonians and Assyrians. The historians claim that the strata of the conquered Babylonians are really those of the Old or First Babylonians of a much earlier time, and that the strata of the conquered Assyrians are really those of the Neo-Assyrians. Again, if the established chronology is correct, then not only would archaeological evidence of Old or First Babylonians and Neo-Assyrians exist, but also that of the Persians. According to Sweeney,

“The absence of the Chaldeans, in the very place they should by all accounts have occupied, was bad enough. But it soon emerged that an even greater disappointment was in store. For the Persians, who were known to have occupied Mesopotamia for two centuries, appeared to have left virtually no evidence of their stay in the region at all. [A.L. Oppenheim, “The Babylonian Evidence of the Achaemenian [Persian] Rule in Mesopotamia,” in The Cambridge History of Iran, vol. 1 (Cambridge 1985), p. 530 [states] “The encounter between the Achaemenian [Persian] Empire and Babylonia [Mesopotamia] seems to have left surprisingly little impact on the latter.” Also, A. Kuhrt, “Babylonia from Cyrus to Xerxes” in CAH, vol. 4 (2nd ed.), p. 135 [writes] “It should be clear from the foregoing that the evidence for Persian rule of Babylonia from 539 to 465 presents major problems and that a reconstruction of the political history of the area is an almost impossible task.”] This was an incredible state of affairs. How could it be that the Persians, whose two centuries of rule in the region marked one of the most vibrant periods of Mesopotamian history, did not appear in the archaeological record?

“Asking themselves this question, scholars desperately sought for the Achaemenid material in site after site. But to no avail. The expected Persian material was to be found in neither Lower nor Upper Mesopotamia. The stratigraphies of the latter two areas generally contained significant differences; but in this they consistently agreed. The Persians were absent.

“In face of such difficulty, it was eventually suggested that the destruction of Assyria by the Medes and Chaldeans [also with almost
no strata] in the years following 610 BC had been so total that the area was not reoccupied. Assyria, it was conjectured, must have been a wasteland during the Persian epoch. However, it had to be admitted that such a solution seemed to be contradicted by Herodotus, who informed us that the satrapy of Assyria [district governed by the Persians] was the wealthiest in the empire, contributing more in taxes than any other region. [Herodotus I, p. 192] [So it could not have been a wasteland, just the opposite: it had to be a rich state to provide the great tribute or taxes to Persia.]

“If the absence of the Persians in northern Mesopotamia was problematical, it was a complete enigma in the south. No claim could be made that Babylonia and Chaldaea was a wasteland in this age, for Babylon was well-known to have been the second city of the Achaemenid Empire—a fact that the classical authors never tired of repeating. . . . Yet, here again, no evidence of Persian occupancy could be found.”35

P. Lampl in Cities and Planning in the Ancient Near East echoes this problem and discusses the absence of Persian cities from strata in regions of Mesopotamia where they dominated other peoples:

“No substantial remains of cities from the Achaemenid [Persian] period have yet been discovered, and it is unlikely that any will be found, except perhaps of those cities which were conquered and taken over by the Persians, such as Ecbatana, Susa or Baby [sic]. It seems that the Achaemenids did not build cities in the true sense . . . Neither Pasargade nor Persepolis have the appearance of permanent ‘capitals’, and they could not have functioned as such. Both lack permanent residence palaces.”36

A.L. Oppenheim writes of the lack of Persian textual materials from strata in regions of Mesopotamia where they dominated other peoples:

“The scarcity of cuneiform texts bearing directly on the political and military events of this [Persian] period is a known fact. The

35 Sweeney, op.cit., p. 6
36 P. Lampl, Cities and Planning in the Ancient Near East (London 1968), pp. 117 ff
encounter between the Achaemenian [Persian] empire and Babylonia seems to have left a surprisingly insignificant impact on the latter . . . The flowering created by the contacts of Babylonia with Hellenism and the Parthian civilization respectively, stands in unmistakable contrast to the sterility and lack of interaction which seems to characterize the Achaemenian presence in Babylonia."

Here we have a series of civilizations residing in the same regions, but archaeological evidence is generally always found for one and not the other. Of the well-known Chaldeans and unknown Sumerians we have strata for only the unknown Sumerians. For the well-known Second Chaldeans and the unknown Neo-Sumerians we have strata only for the unknown Neo-Sumerians. With respect to the well-known Medes and unknown Mitanni there is almost nothing in the way of archaeological evidence for the Medes but archaeological evidence for the Mitanni. The same exists for the well-known Persians and the unknown Old or First Babylonians and the Neo-Assyrians. Because the strata of the Assyrian Empire were given to the Akkadians, the strata of the Neo-Assyrians dominated by Persia were said to be those of the true Assyrian Empire.

In almost every case the Mesopotamian civilizations that were known to the ancients failed to leave archaeological proof of their existence. In almost every case the Mesopotamian civilizations completely unknown to the ancients left nearly all the artefacts in the ground. This is immensely improbable.

William Stiebing, Jr., at the Hampton Virginia conference in the late 1980’s, attempted to get around this problem by offering the suggestion that destruction by wind, rain, etc., eroded away only the archaeological materials of the Chaldeans, Second Chaldeans, Medes, and Persians, but failed to do the same with those of the Sumerians, Neo-Sumerians, Mitanni, Old or First Babylonians, and Neo-Assyrians. Probability is, of course, one of the vital forms of dealing with evidence on a scientific footing.

In this respect, Stiebing has also raised the specter of probability as a scientific mechanism by which to determine the validity of a hypothesis:

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“Scientists are used to evaluating explanations of data in terms of probability, rather than possibility, so the evidence would have to be clear and unambiguous to convince them that a theory . . . is valid.”

Now it is not probable that erosion was so highly selective that it only wore away Chaldean strata but not those of the Sumerians, that it only wore away Neo-Chaldean strata, but not the Neo-Sumerian, that it only eroded nearly all Median strata but not those of the Mitanni, that it only eroded away Persian strata but not the Old or First Babylonian or Neo-Assyrian.

French philosopher of history, Marc Bloc, discusses this problem of probability:

“In linguistics, there is a famous example of the word *bad* which means the same thing in English and Persian, although—the term has not the faintest common origin in the two languages. Anyone who should pretend to found a connection upon this isolated agreement [as Carl Sagan did suggesting that *Dio* and *Teo* in Latin and Toltec derived from diffusionist contacts] would sin against the main law of all criticism of coincidences. ‘Only large numbers are conclusive’.”

Let us deal only with the 88 missing cities of the Chaldeans that have never been found. Failure to find one of up to ten of these cities is not too improbable. Failure to find any of 20 to 30 of these cities seems rather improbable. Failure to find one of 30 to 40 of these cities is highly improbable, and when we raise the failure to find one of 40 to 50 of these cities, the improbability is simply extraordinary. Then to fail to find one of 88 of these fortified Chaldean cities is simply too large a number of cities to have any probability at all. The probability of this occurring borders on the miraculous. Yet this miraculous aspect of archaeological research in Chaldean territory has never been comprehended by those who uphold conventional chronology. The possibility of such selective erosion or any other phenomenon being responsible for such a state of affairs transcends rationality.

For example, during a coin toss the chances are 50-50 of coming up heads or tails. If we call the Chaldeans tails and the Sumerians heads, then we would expect in 88 tosses for there to be some tails [Chaldeans] to come up as well as heads.

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39 Marc Bloc, *op. cit*, p. 132
What the absence of Chaldean cities requires is that 88 tosses of the coin come up all heads without a single tail. To have the coin come up heads a great number of times in a row suggests there are two heads on the coin, one on each side. That is, the coin has been counterfeited by those who made it. So, too, the conclusion that the Chaldeans never left evidence of their cities is a counterfeit form of logic as well as history.

This selectiveness is just what we found with the erosion on the Sphinx, discussed earlier. Erosion cannot erase only what the historians cannot find in the way of well-known empires, nor can one invent any other highly selective process with which to manipulate the data to support the conventional chronology. In this instance probability is against that chronology in no uncertain terms. To expect erosion to remove almost totally the archaeological strata in the tells throughout Mesopotamia of the well-established, well-documented, and well-known empires over a period of 2000 years, defies the coincidence of large numbers being conclusive.

The scientific facts of archaeology, subject to mathematical probability, are in total contradiction to the chronology that establishment historians and archaeologists have upheld for about a century. As Sweeney states:

"Thus the archaeologists, whose histories of the ancient world are housed in the great libraries of the world, disclosed to us the existence, in Mesopotamia, of peoples and kingdoms unknown to the Classical authors, but failed completely to add anything to our knowledge of the ancient peoples mentioned by those same authors. Only the Assyrians, it is held, are equally well represented in the archaeological and literary sources. Thus our knowledge of the previously unknown Sumerians and Akkadians [as well as Neo-Sumerians, Mitanni, Old or First Babylonians] comes entirely from the materials excavated by the archaeologists, whilst our knowledge of the Chaldaean and Persian [as well as Median and Second Chaldaean] epochs comes entirely from the Classical authors."40

But archaeological research had yet another way with which to determine the validity of Heinsohn’s thesis. That research will be presented next to illustrate how Heinsohn’s hypothesis was confirmed, and how the established chronology was refuted, in unmistakable terms.

40 Sweeney, op. cit., p. 7
CHAPTER 9
MESOPOTAMIAN STRATIGRAPHY

There are two approaches to Heinsohn and Sweeney’s chronology with regard to the stratigraphical record. The first was presented by Jan Sammer:

“Heinsohn’s is not an abstruse argument about the succession of ancient dynasties. If Heinsohn is right, the entire history of the development of civilization will have to be written anew. The validity of his scheme will emerge in short order, since the theory is highly falsifiable: at every stage the double existence of historical figures and events must match, allowing only for the vicissitudes of historical preservation. Once the overall historical scheme has been declared, not only are the kingdoms and dynasties identified with their historical doubles, but individual rulers within each dynasty, and the peculiar events of their reigns as well. A single fact, if sufficiently substantiated, could ruin the entire structure. An example may usefully illustrate this point. If the Sumerians are indeed the Chaldeans, it follows that Shulgi the greatest king of the so-called ‘neo-Sumerian’ period is the same as Nebuchadnezzar, the greatest king of the neo-Babylonian period [not the Old or First Babylonians]. There is nothing arbitrary about this identification; it is a logical deduction from the postulated premises. If any discrepancy between the two men, such as lifespan, length of reign or ancestry, is found and sufficiently established, the scheme proffered by Heinsohn would be disproved.”

In this respect Ev Cochrane, who cited Sammer as well as Clark Whelton, maintains that

“while no one would claim conventional history as we have it is completely secure or without difficulties, certain facts seem so well established as to approach certainty.”

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42 Ev Cochrane, “Heinsohn’s Ancient History,” AEON vol. V, no. 1 (July 1999), unpaginated
He further argues “The relative chronology of the pivotal figures [from ancient history] can be constructed in great detail from COUNTLESS ANCIENT DOCUMENTS AND ARCHAEOLOGICAL SOURCES . . . What, then, are we to make of a historical reconstruction [Heinsohn’s] which attempts to turn this intimately intertwined and precisely constructed chronology upside down and topsy-turvy?”43 [capitalization added]

And dutifully Cochrane does cite “certain [documented] facts” from this “Interlocking Web of History”44 to argue with Heinsohn. But this Interlocking Web of History was organized in defiance of many of the scientific and technological established facts that are a certainty and that were known even to the early historians like Petrie. Although their Interlocking Web of Historical Documentation was internally consistent with what they perceived as the proper chronology, it was not then, nor is it now, internally consistent with the known facts of science and technology. This indicates the conventional chronology is not intimately intertwined and precisely connected to the real facts.

This approach to Heinsohn’s thesis rests on a basic assumption, namely that the documented historical record is fundamentally sound. The fact of the matter, as was pointed out in the last chapter, is that the historical records of Mesopotamia are highly fragmented and corrupted. “Listers casually omitted kings whom they considered unimportant,” or “copied wrongly,” or “took dynasties which reigned simultaneously and listed them one under another instead of side by side.” “Moreover the names of years were often changed—in obedience to the whims of kings, or in deliberate falsification of history, or for whatever reason.”45

Turning to the historical record as confirmation naturally leads to inconsistencies with Heinsohn’s hypothesis. According to Heinsohn, the documents for a particular dynasty were written in two languages, perhaps three. The scribes writing of events in one language interpreted the events according to their understanding, knowledge and biases. The scribes writing of the same events in the other language also interpreted the events according to their understanding, knowledge and biases. Many of the records were copied, and falsifications would also have crept into the record; also, most records were lost.

43 ibid., next page
44 ibid.
45 Ceram, Secret of the Hittites, op.cit., pp 138-139
Amelie Kuhrt, in her two-volume work on the ancient Near East, corroborates this:

“Beyond the evidence of the year names the ‘history’ of the Ur III period is very scantily known save for its rise and fall—and even these events bristle with problems and their details remain obscure.”46

[Of the Old Babylonian and Old Assyrian period she states:] “It is not at present possible to present a connected political history of this period . . . other important political powers whose histories remain unfortunately fairly obscure . . . of these, the history of the powerful kingdom of Aleppo is perhaps the one most consistently illuminated by various documents. But none of the evidence comes from Aleppo itself, and it often serves simply to make us more aware of the depths of our ignorance. Ebla . . . has yielded rich archaeological material (palaces, temples, a strongly fortified city-wall, subterranean tombs, analogous to those found at Ugarit slightly later), which indicates its revival as an important urban centre. But very little textual material comes from Ebla in this period and its political relations with the large powers remain obscure.”47

“The reasons behind the secession of Larsa are completely obscure.”48

“What do we know about the great kingdom of Mitanni . . .? The answer is: disappointingly little. We can date its emergence only very tentatively; there is almost no archaeological evidence from the central area of Mitanni. . . . The line of Mitanni kings becomes dimly discernible at the turn of the fifteenth to the fourteenth century.”49

Therefore, Sammer and Cochrane’s arguments face a critical problem: since the documented history is sparse, fragmented, kings were omitted, it was copied wrongly, etc., then turning for evidence to refute Heinsohn from that record is ignoring that that record has these massive problems. Using that record to refute Heinsohn is circular reasoning. How do we know which kings were omitted or

47 ibid., p. 75
48 ibid., p. 78
49 ibid., p. 289
which kings’ records were distorted or copied wrongly, and so forth? Since we cannot know this with certainty, then that record cannot be called upon as clear evidence to argue against Heinsohn’s revision.

Lynn E. Rose has offered another approach to Heinsohn’s thesis that this author suggests better reflects the proper understanding of how the stratigraphical chronology should be interpreted:

“What I would call the ruthlessness of Heinsohn’s method lies in its almost exclusively stratigraphical approach. If something does not fit the stratigraphical evidence then it cannot be true; it is myth, fiction, poetry, error, or even fraud.

“Heinsohn insists that no matter what the documents and the monuments may say, ancient civilizations reveal their existence—and also their sequence—in the strata that they have left behind. The physical evidence, be it only debris, tends to lie above the layers of any earlier peoples and below the layers of any later peoples. Though technically the word stratigraphy means writing about strata, one could say that in a sense history itself is already ‘written’ in the strata. Accordingly, Heinsohn wants to reconstruct ancient history from the ground up (almost literally!).

“For better or for worse, Heinsohn’s method does remain ruthlessly stratigraphical, and is largely non-literary. Ancient texts and documents (Manethon and the various king-lists, for example) are not allowed to intrude into the process. Any literary evidence must accord with the underlying strata, or else it is to be disbelieved.”

Rose paraphrases Hume’s closing remarks in the Enquiry to show Heinsohn’s approach:

“If we take in our hand any volume; of . . . historiography, for instance; let us ask, Does it contain any abstract reasoning concerning civilizations and stratigraphy? Does it contain any experimental reasoning based on the materials actually found at excavation sites?

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50 Rose, Sun, Moon, and Sothis, op. cit., p. 202
No. Commit it then to the flames. For it can contain nothing but sophistry and illusion.”

Thus, there is really only one method, according to Heinsohn, by which one may evaluate and falsify his chronology. If the documentary evidence is basically sound then the stratigraphy should support it unequivocally. Those of us who have relied too strongly on historical documentation have failed to understand that these records should not only corroborate and confirm the stratigraphy, but, conversely, the stratigraphy should corroborate the historical records. This, I believe, is the crux of the issue.

What I have found most alarming over the past several years is the failure of Heinsohn’s critics to come to grips with the stratigraphical evidence of Tell Munbaqa in Syria. This evidence was presented by Heinsohn in 1993 in the very first issue of *The Velikovskian.* Nothing critical has been said in print about this evidence by Heinsohn’s critics, so far as this author knows.

To truly test not only Heinsohn’s chronology but that of establishment historians, what is demanded is a scientific empirical method that permits both to be falsified. As we learned earlier, Heinsohn equates the Akkadians with the Assyrians and the Mitanni with the Medes. According to Sweeney,

“A typical Assyrian stratigraphy (as for example at Tell Hamadiyah, Munbaqa, Barak, Balawat, and Nimrud) looks like this (starting with the top stratum and going down):

“Hellenists (after 330 BC)

Middle and Neo Assyrians (1350-610 BC)

MITANNI (1550-1350 BC)

AKKADIANS (2300-2200 BC)

Early Dynastic Sumerians (until c. 2300 BC)”

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51 ibid., p. 203
53 Sweeney, *op.cit.*, p. 6
Thus, according to the established chronology there is about a 750-year settlement gap separating the Akkadian strata from those of the Mitanni above them. However, Heinsohn, who claims the Akkadians are the Assyrians who were conquered by the Medes whose *alter ego* were the Mitanni, would not have a settlement gap between them. Since the Medes conquered the Assyrians there could never be a settlement gap of some 750 years between them in the strata. Here then was a fundamental way with which to determine the validity or falsification of either Heinsohn’s or the establishment’s chronology. If the establishment’s chronology is valid, an excavation at Tell Munbaqa will find a settlement gap of about 750-years in the ground between the Akkadians and the Mitanni. If Heinsohn’s chronology is valid, an excavation at Tell Munbaqa will find no settlement gap at all. Conversely, if the establishment’s chronology is false, it will be falsified by the finding that there is no settlement gap there, and if Heinsohn’s chronology is false, it will be falsified by the finding that a 750-year settlement gap exists. The simplicity of this test to determine the falsifiability of either Heinsohn’s or the establishment’s chronology could not be better suited to answering the question: Which chronology is correct and which is false? Heinsohn discusses this matter:

“The correspondence partners of the Mitanni are well known in Egypt’s New Kingdom. . . . Because of its highly peculiar pottery style (Nuzi Ware) and a wealth of written tablets listing its rulers also known from the Amarna correspondence [with Akhenaton in Egypt] there is little danger that archeologists . . . [will] not recognize a stratum of the Mitanni nation once they see it. Thus no other nation of antiquity is better suited for archeological cross-references than the Mitanni.”

Heinsohn goes on to discuss the various tells in which Mitanni and Akkadian strata are found:

“The excavators *believe* that all these sites suffered a settlement gap of 700 to 800 years or (in Rimah) of some 250 years . . . [after which] the Mitanni [came to] resettle the ruins.

“I say *believe* because, up to 1988, no archeologist ever put this hiatus [settlement gap] to the test. None of the excavation reports listed

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54 Heinsohn, “Ancient Near Eastern Chronology Revised,” *op. cit.*, p. 28
any hard evidence for a hiatus of 750 or 250 years. No dramatic discontinuity of pottery styles, [the shape of] tools, . . ., or building ground plans was reported. The same silence covered the fact that aeolic (windblown) layers usually required for a gap of so many centuries were missing. All the archeologists did was . . . apply their knowledge of textbook chronology: A tablet [in the ground] inscribed in Old Akkadian automatically brought the stratum in which it was found into the 24th century. Since these strata were found underneath Mitanni strata, whose Sothic Amarna textbook date was the 15th century, none of the archeologists failed to mention this settlement gap between the Akkadian and . . . Mitanni stratum. Thus, they proved that they had attended their history classes. From an archeological viewpoint, however, they had proven nothing.

“When I claimed the Old Akkadians, the Old Assyrians . . . not only to be stratigraphically synchronous but also . . . historically identical . . . [my] challenge was out that there was no hiatus [settlement gap] between the Old Akkadian . . . strata and the Mitanni strata. German archeologists who read my book first, immediately took up the challenge. They decided to refute me in a genuine scholarly manner by proving archeologically that the hiatus indeed did exist. After having accomplished this, they believed, they would have silenced me once and for all. The lead was taken by Professor Machule’s team of the Technical University of Hamburg at Harburg, excavating Tell Munbaqa/Syria. In 1988, for the first time, they devoted a special sounding [dig] to refute my claim of the gap’s non-existence. The sounding focused on architectural expertise and was the first attempt ever to scientifically prove that the famous hiatus between Akkadian and Mitanni strata existed [as conventional chronology predicted]. To the surprise of the archeologists they could not confirm the gap between c. 2250 and c. 1475 existed, but found clear-cut architectural continuity. Instantly understanding the far-reaching impact of their discovery they went back to Munbaqa in 1989 and this time devoted three special soundings to my claim. To go beyond the limits of their own expertise (architecture, pottery, crafts and arts) they asked Dr. U. Rösner [a geologist] from the University of Erlangen to join the team. She specializes in sediments and aeolic [wind-blown] layers; her preliminary draft with the result of her research dates from August 23, 1990. Her findings confirmed [those of] the archeologists. THERE IS NO GAP BETWEEN EARLY BRONZE (OR OLD AKKADIAN)
AND LATE BRONZE (OR MITANNI) IN MUNBAQA.”

Critics of Heinsohn have offered the view that since the report has not been published there can be no real evidence to support Heinsohn’s claim. Cardona has offered to translate any reports from German into English if such confirmatory papers exist. However, this is unnecessary because U. Rösner’s geological paper on the 1989 dig at Tell Munbaqa was fully published, in English, in 1995 in QUARTÄR, titled “Sedimentological Evidence to Archaeological Problems on Tall Munbaqa, Northsyrria.”

What Rösner found was that “[d]ue to a relatively fast burying by broken down wall remainders the aeolian sediment character was preserved. In 1989, the year of the investigation, the excavators [archaeologists] discussed an occupation gap between the Early [Akkadian] and the Late Bronze Age [Mitanni]. HOWEVER, NO SEDIMENTOLOGICAL INDICATIONS FOR A MIDDLE BRONZE OCCUPATION GAP WERE DISCOVERED.”

In her conclusion, she makes it quite clear that not only do the geological sediments prove that there was no settlement gap, but she claims the archaeological evidence also supports this conclusion:

“Detailed study of the sediments from Tall Munbaqa ALONG WITH EVALUATION OF ARCHAEOLOGICAL FINDINGS yielded evidence to events during the occupation as well as to chronological problems.

“The accurate investigation of a cultural level, which should theoretically represent a Middle Bronze Age occupation gap, could not prove an aeolian component. But to be certain it would have to be expected at an actual interruption of the occupation of about 600 years. This statement is sustained by the results of the sediment analyses above the oldest Late Bronze Age road. Therefore, including the first archaeological results, it is more likely that a continuous occupation has existed. LATER ON, THESE SEDIMENTOLOGICAL INTERPRETATIONS WERE CONFIRMED BY NEW ARCHAEOLOGICAL FINDINGS ON TALL MUNBAQA MADE

55 ibid., pp. 29-30
DURING THE EXCAVATIONS OF THE EARLY [NINETEEN] NINETIES (see D. Machule 1995).\textsuperscript{57} [capitalization added]

Rösner makes it quite clear that further archaeological work was carried out by Machule’s team in the early 1990’s that corroborates her geological sediment research. This Machule published in the same year as Rösner—1995—in Reallexikon der Assyriologie und Vorderasiatischen Archäologie.

In that article we are told that New Bronze Age material lies directly on top of Old Bronze Age material. No reference to a settlement gap is ever mentioned as if Rösner’s geological evidence did not exist. Instead, we are told that a Middle Bronze Age temple was uncovered in one area of the tell. How is the dating on this temple arrived at? It is made by pottery dating, etc. But that dating is self-referential (a circular argument). Rösner showed that there was no settlement gap between the Old Akkadians/First Babylonians and the Mitanni, proving that these two peoples ruled consecutively, one directly after the other. To interpret a temple in the strata as being of the Middle Bronze Age does not invalidate the geological evidence. It is an attempt at overcoming geological fact with historical interpretation. Since there is no settlement layer between the assumed Old Bronze Age Old Akkadians and the New Bronze Age Mitanni, as Rösner proved, there is no time intervening between the Old and New Bronze Ages. The entire concept that the historians and archaeologists are using regarding Old, Middle and New Bronze Ages in Mesopotamia cannot be valid. If that was the case, then it would require that a 700 to 800-year gap exists, and it simply does not.

Moreover, it was pointed out to this author [Birgit Liesching, personal communication] that if a small population came to settle on a tell of an originally larger population, they would dwell on only a part of it, but even in that case the unbuilt-up part would have to exhibit an Aeolian stratum as a settlement gap; but this is simply not the case here. Also, why would there be a large Middle Bronze Age temple in one area, if there isn’t a township nearby to serve it? Where would the worshippers for the gods have come from? The entire elaborate \textit{ad hoc} thesis invented to create a settlement gap where none exists makes no sense under any considerations!

Above and beyond all this, Rösner reports: “The excavation in ‘Sondage östlicher Steinbau 2’ . . . also yielded some scattered Middle Bronze Age ceramics, which on the other hand could as well be defined by some archaeologists

\textsuperscript{57} ibid., p. 218
as ending (still in use) Early Bronze Age ceramic or as early (already in use) Late Bronze Age ceramic’ (D. Machule, written, translated communication).” That is, Machule admits that the identification of the Middle Bronze Age ceramics which were found could not be solid proof of a Middle Bronze Age occupation because such ceramics were “(still in use) [during the] Early Bronze Age . . . or . . . (already in use) [during the] Late Bronze Age.” Turning to such evidence in order to dismiss Heinsohn’s prediction that there is not a settlement gap between these peoples is not historically plausible. That is, Machule is turning to pottery dating which was organized on the lines Petrie established. Since that dating method is fundamentally unsound in its present form, pottery dating at Tell Munbaqa is enormously problematic.

The geological evidence proves that the so-called Mitanni ruled directly after the so-called Old Akkadians. That is the heart of the matter and is the problem for conventional chronology.

One cannot move the Mitanni back in time by 700 to 800 years without completely overturning established Mesopotamian chronology; nor can one move the Old Akkadians forward in time by the same number of years without doing the same. The facts of geological research are a death knell to conventional chronology, and those researchers committed to it are incapable of dealing with that fact. Here is a copy of the letter by a researcher at the digs carried out at Tell Munbaqa; Wilfried Pape sent this to Gunnar Heinsohn from Berlin on November 22, 1988:

“Sehr geehrter Herr Heinsohn,

Bezugnehmend auf Ihr Buch “Die Sumerer gab es nicht”, möchte ich Ihnen folgendes mitteilen:


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58 *ibid.*, p. 217

Mit freundl. Grüßen

(signed) W. Pape

Translation of letter to Prof. Dr. Gunnar Heinsohn, University of Bremen, from Wilfried Pape, Architect, Berlin, dated November 22, 1988:

Dear Mr. Heinsohn,

With reference to your book Die Sumerer gab es nicht (The Sumerians Never Existed) I would like to inform you of the following.

I am an architect and have been working for nearly fifteen years in a “semi”-professional capacity in the field of archaeology. I have been to Turkey, Egypt, Yemen, and have been working in Syria for the last five years. I consider myself a reasonable judge both of archaeologists and their motives, and of stratigraphic materials. In the last five years I have been able to acquire rather precise stratigraphic knowledge of Tell Munbaqa on the eastern bank of the Euphrates. For two years I have made deliberate efforts to find evidence against your theses. According to official chronology, there should be about 700 years between the Early Bronze and Late Bronze strata that were found on our Tell. Based on my investigations and observations I have come to the conclusion that it is IMPOSSIBLE for a gap in occupation of +/-700 years to have existed on Munbaqa. It is not the purpose of my letter to go into detail but just to give you an indication that may contribute to the debate on chronology which I consider to be useful.
Unfortunately the archaeologists involved in this particular excavation have only limited ability for carrying on such a discussion. You probably know the reasons better than I do. They have not expressed any interest in reading your book, they prefer to maintain a hiatus of 700 years on Munbaqa, even without sufficient arguments in favor.

Yours sincerely,

(signed) W. Pape

[Emphasis in the original. These materials were kindly provided to the author by Birgit Liesching who has long been involved in reading and understanding the work of revisionist historical research. An attempt was made recently to contact Wilfried Pape to obtain more details regarding some of his remarks, but without success.]

Heinsohn reported on these findings at the Sixth International Congress of Egyptology in 1993:

“When, in February 1988, the author [Heinsohn] first published . . . his stratigraphy-based equation of Hyksos and Old Akkadians, the German archaeologist Wilfried Pape excavating at Tell Munbaqa/Syria was the first to take up the challenge [that claimed there was no 750-year settlement gap there]. . . . W. Pape devoted the first special sounding to this ‘gap’ but could not confirm the hiatus between ca. 2250 and ca. 1475 [B.C.]. On the contrary, he found clearcut architectural continuity. This observation was written to the author [Heinsohn] on November 22, 1988. In 1989, three special soundings were brought down to test Munbaqa’s Akkad-Mitanni gap. A geologist specializing in sediments and aeolic layers [U. Rösner] confirmed the work of the archaeologist. There is no hiatus between the Old-Akkadian and Mitanni/Hurrian strata at Munbaqa . . . Moreover, Old-Akkadian cylinder seals remained in use for business contracts in the Mitanni/Hurrian stratum . . ., another indication of the uninterrupted continuity between both periods, i.e. for the very absence of a hiatus of some 750 years.”

The archaeologists who discovered that there was no settlement gap at Tell Munbaqa will not discuss the evidence nor what it implies about Mesopotamian chronology, and they certainly will not publish this fact. The science of geology has declared that the settlement gap on that tell is an invention that was created in order to fit the established chronology with archaeological evidence.

Beyond all this is the problem of Egypt and the Mitanni. The kingdom of the Mitanni along with its people is supposed to have disappeared from history around 1360 B.C. Therefore they could not be involved with the Egyptians hundreds of years after their demise. The Libyan—22nd—Dynasty according to the established chronology reigned from 945 to 712 B.C. However, one of its kings, “Sosenk, referred . . . to the Mitanni.”

J.A. Wilson’s article “Egyptian Historical Texts” in Ancient Near Eastern Texts, edited by Pritchard, on pages 263-264 discusses this: “Mitanni as a nation had ceased to exist at least four centuries earlier.” Thus, the Mitanni/Medes are tied to the trunk of Egypt by a written document from the first millennium. If Egypt is the trunk of ancient Near Eastern chronology, that document makes the case for Heinsohn and for Velikovsky who make the Mitanni-Mede connection. However, in spite of this branch being grafted onto the trunk of Egypt, the historians will not accept this. This, of course, is the double standard of evidence that Fischer pointed out above. Evidence which should count best counts least when it contradicts the established chronology. Being tied to Egyptian history is valid in all cases but only if it does not disrupt that chronology. With such a standard it is clear there is no standard.

At the outset of this book (Chapter 1) there was a brief discussion of archaeological evidence. Fekri Hassan was quoted with the view that archaeologists work to uphold “the mandate of reason” and guard “against any usurpation of the concept of ‘fact’ . . . and uphold . . . canons of knowledge against the abandonment of reason.” However, what have we found when the establishment archaeologists discover that the evidence shows that there was no 700 to 800 year gap at Tell Munbaqa was a failure to uphold that mandate and a need to abandon canons of knowledge as well as reason. Hassan has further argued that these “experts in the academy” uphold “reason” . . . [as] opposed to beliefs based on fear, self-importance, authority, prejudices . . . trained scholars using standards and canons of description . . . [are] opposed to . . . dogmatism, obscurantism.” Yet in complete disregard of every vestige of honest scholarship, the archaeological researchers at Tell Munbaqa

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60 Velikovsky, Ramses II and His Time, op.cit., p. 178
have *thus far* presented only dogmatism and belief based on authority and prejudice. They have disregarded the very canons of reason, acting not “as guards against any usurpation of the concept of fact,” but as transgressors against that standard.

Heinsohn has further claimed that there are many false settlement gaps at other tells in Mesopotamia between civilizations other than the one found at Tell Munbaqa. Archaeologist W. Pape admits Heinsohn’s thesis was validated and the established chronology falsified there. Since the archaeologists now refuse to accept that evidence, why then would they or their colleagues carry out these many other digs after the shock of discovering Heinsohn was right? Surely with such confirmation of his thesis one would expect a concerted effort to test these other tells to confirm or deny the validity of either Heinsohn’s chronology or their own. There are, so far as this author knows, no such plans to test these numerous other sites, and this author believes the reason is patently obvious: the proponents of the established chronology have everything to lose if these further tests confirm Heinsohn’s thesis. Having proved Heinsohn correct at Tell Munbaqa, they dare not carry out these digs. They fully comprehend that to do so could mean the death of their established chronology.

On the other hand, they can also raise the cry that this is only one dig and needs confirmation from many others. But rather than explore the exciting challenge to where it clearly seems to lead, they will fail to test these other sites. By not following through to test these numerous other tells, they can then maintain the hypocrisy that they are still honest historians and archaeologists only interested in unearthing historical truth.

Furthermore, the dig at Tell Munbaqa fully proves that the original research which put the settlement gap there is false. Again they may claim this is only one site, but then fail to learn whether or not the settlement gaps at these other tells are also false. To test these sites could falsify all their historical chronology for Mesopotamia. The challenge to test these numerous other tells is still there but have the proponents of the established chronology the real qualities of true historians and archaeologists to meet it? Excuses for not doing so are easy to invent.

For example, after a complete excavation of a Babylonian site, J. Schmidt was dismayed by the supposed settlement gaps needed to explain the evidence; he wrote that a
“small group of rooms [were found, dating], from the oldest of the phases of the 1st millennium [which were] . . . in all probability Neo-Assyrian remains. These strata are situated directly above those of the 3rd . . . millennium [B.C.], in the present case above those of the Early [Sumerian] Dynastic Period. The fact which had been observed previously, that after the Early [Sumerian] Dynastic Period (c. 2400 BC) there was a total building gap which was only ended in the Neo-Assyrian-Neo-Babylonian Period [which begins about 700 B.C.] remains inexplicable.”

This may be a site where no settlement gap exists as at Tell Munbaqa and which therefore should be tested. Schmidt felt that a 1700-year settlement gap could not be explained.

The tragedy is not for Heinsohn’s chronology but for historical truth and for the historians themselves who appear to be too dogmatic to seek the evidence at these other digs. Someday, perhaps, others who are interested in finding that evidence may carry out these archaeological investigations at these other sites. Should these digs confirm Heinsohn’s chronology, those future historians and archaeologists will rightly regard the present generation—their teachers—as falsifiers of historical truth. The present generation will rightly deserve to be cursed by their pupils of that future time as well as for all time. They will then have been shown to all the world as no better than the churchmen who attempted to destroy the theses of Copernicus, Galileo, and Kepler.

After the defeat of Germany and Japan in World War II, textbooks in both countries, for both high school and university students of history, contained little or nothing of the events and actions committed by these countries. And to a large extent this period of time was suppressed. The historians who now fail to relate to their students the evidence that there is no settlement gap at Tell Munbaqa and teach that there is nothing amiss with respect to Mesopotamian chronology will be taking part in a deliberate falsification of historical truth, similarly to those who wrote those textbooks for German and Japanese students.

Based on the established chronology, the Mitanni are said to have ruled in northern Mesopotamia from around 1500 to 1360 B.C. The Old Akkadians or First Babylonians had to have ruled from about 700 to 800 years earlier which allows

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for the settlement gap. But since that settlement gap does not exist, the history of Mesopotamia organized over the last century or so, for that period of time, cannot exist. All the events presented in the history books for that time belong to some other time. All the cities said to have been built or inhabited during that time belong to some other time. All the pottery found in the strata for that 700 to 800 year period belong to, and must be assigned to, some other time. All the trade and communications by Mesopotamian nations with nations outside Mesopotamia assigned to that long era must be assigned to, and belong to, other times. Because these immense negations of their established chronology are so damaging, instead of squarely facing up to the evidence from Tell Munbaqa, silence has descended over the field. Rather than accepting the cold hard fact that 700 to 800 years do not exist in Mesopotamian history, archaeologists and historians are engaged in perhaps one of the greatest deceptions in the history of history.

To cite W.E.B. Du Bois, the great Black American Harvard sociologist and most important leader of the movement in the United States for civil rights during the first half of the 20th century, in “The Propaganda of History,”

“If history is going to be scientific, if the record of human action [and chronology] is going to be set down with accuracy and faithfulness of detail which will allow its use as a measuring rod and guidepost for the future . . ., there must be set some standard of ethics in research and interpretation.”

That standard has been abandoned by those who have pledged their lives to uphold it. Those involved in this falsification of history deserve a place in history but not one honor. To paraphrase Annette Gordon-Reed in the preface: It is my belief that those who are considered ancient history scholars will never make a serious and objective attempt to get at the truth of the matter. Their failure to look closely . . ., the too ready acceptance and promotion of the 700-800 year gap as a reality . . ., the reliance upon the established chronology in place of investigation and analysis, all indicate that ancient history researchers decided from the outset that Heinsohn’s thesis was not true and that if they had anything to do with it, no one would come to think otherwise . . . The goal has been . . . to restrict and falsify knowledge as a way of controlling the allowable discourse on this subject. This attitude betrays a basic disrespect for readers of history . . . Historians’ prejudices and individual desires to keep inviolate their particular image of that settlement gap

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62 W.E.B. Du Bois in Annette Gordon-Reed, op.cit., p. 78
prevented a fair, heard-headed, and thorough presentation and consideration of the facts of history to the world.

Eventually this falsification of historical truth will become known and future generations of historians may curse their teachers for what they were willing and capable of doing and may in time brand them as notorious liars. But in terms of the present moment, it is à propos to quote George Bernard Shaw’s play, *The Devil’s Disciple* (1901), Act 3, where Swindon asks, “What will history say?” To this Burgoyne replies: “History, Sir, will tell lies as usual.”
CHAPTER 10
IRON, DIORITE, AND THE SUMERIANS

“Gudea built the Temple . . .
With two hand-breadths of shining stone
He cased the brickwork
With diorite’s hand-breadth of bright stone”
Cylinder A Column
XVI of Gudea, third millennium B.C.

According to conventional chronology the Sumerians lived in Mesopotamia from about 2400 to 2024 B.C.\(^1\) They lived there about the time of the Old Kingdom in Egypt. This, of course, is the period when copper was supposed to have been the main metal used for tools. Gold and silver were too precious for such everyday work. The Mesopotamians did have meteoric iron, but as was pointed out earlier, this metal is only slightly harder than bronze and difficult to make into tools. It is far softer than granite and much, much softer than diorite. As we are already aware, soft metals such as copper, bronze, wrought iron or meteoric iron cannot work diorite. So the question is: Did the so-called Sumerians of the third millennium B.C. make statues of about the hardest of stones used in Mesopotamia, over a thousand years prior to the development of carburized iron—steel? The answer is quite clearly: Yes.

Harriet Crawford tells us that the kings of Sumer imported “diorite.”\(^2\) She goes on to show that “Diorite was the favorite stone of the Agade kings and was used for their royal statues.”\(^3\) “The later kings of the Dynasty also continued to be

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\(^1\) Encyclopedia Britannica, Micropedia, vol. IX (Chicago 1982), p. 667
\(^3\) ibid., p. 148
active . . . and to make their statues of black diorite.”

“After the fall of Agade Gudea of Lagash continued the tradition of making statues of black diorite.”

In his tome, Ancient Mesopotamian Materials and Industries: The Archaeological Evidence, P.R.S. Moorey fails to mention that diamonds or topaz were known to the early Mesopotamians; he does discuss:

“Corundum (Emery). In chemical terms this is aluminium oxide. It can be found in a variety of colours notably red (ruby) and blue (sapphire). It is extremely hard, registering 9 on the Mohs scale. Neither of these precious stones has been reliably reported in use as a gemstone in Mesopotamia before the final quarter of the first millennium B.C.”

As for beryl, he reports:

“The occasional reported uses of this stone in Mesopotamia before the Seleucid period [312-64 B.C.] need to be confirmed, since confusion [of it] with green feldspar and olivine is possible . . . They [beryl stones] have yet to be reliably reported in Mesopotamia before the fourth century B.C., as is the case in Egypt, where the stone naturally occurs.”

Therefore, as with the early Egyptians, the early Mesopotamians did not have diamonds, topaz, ruby, sapphire, beryl, or corundum-emery to cut stone.

However, the so-called Sumerians, a people without steel, were supposedly cutting diorite. With what did they do this, since they lacked steel and the abrasives that existed in much later times? All the keenly sharpened ad hoc hypotheses that may be advanced have been shown to be failures. All these ad hoc inventions that have been discussed earlier require that the process be replicated in a serious, practical, public demonstration to support such inventions. Without a demonstration to produce a statue of diorite, the ad hoc theories are again substanceless hand waving. If the historians truly had carved diorite with copper

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4 ibid.
5 ibid., p. 149
7 ibid., p. 82
into a statue comparable in size and workmanship to those produced in pre-Iron Age Mesopotamia they would have made their case. Since they have no case, via a demonstration, to offer the world, they have presented only wishful thinking.

Nevertheless, there is direct evidence that the so-called Sumerians did have iron as pointed out by E. Herzfeld:

“Against all expectations, the excavations of Alaja Oyük have brought to light a necklace of gold and iron and an astonishing iron dagger with gold handle of a pure Sargonic style from one of the tombs which may even go back to the pre-Sargonic period of the Royal Tombs of Ur.”

In *AEON*, Heinsohn discussed this find: “The so-called ‘Akkadian’ iron dagger with a golden handle from Alaça Hüyük—dated to approximately 1100 years too early [for the production of iron] even in the conventional chronology for the Iron Age.” In the footnote, omitted in *AEON*, Heinsohn points out “The remains of a blade [of iron], in a bronze hilt from the end of the Lagash stage [ED III. . . ] [or Early Dynastic III] in Eschnunna were made of terrestrial and not meteor iron.”

The so-called Sumerians did not have tin to make a bronze handle for a dagger in the third millennium. They also were not smelting terrestrial iron ore to make blades then. These products were made, according to the established chronology, at the end of the second and through the first millennium B.C.

There is, in fact, plenty of evidence of the knowledge of iron in Mesopotamia prior to the beginning of the Iron Age. For example, Heinsohn equates King Hammurabi, the sixth king of the Old or First Babylonian empire,

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9 Heinsohn, *AEON*, vol. I, no. 2, p. 21
10 For example, see P.R.S. Moorey, *op.cit.*, pp. 287-288.
dated before 1792 to 1750 B.C., with Darius the Great of the Persian Empire, dated to the first millennium B.C., a time when steel did exist. On the other hand, Sweeney equates Darius the Great with the Neo-Assyrian king Sargon the Great of the eighth century B.C. The question related to Hammurabi is the stone upon which his laws were carved. Lawrence H. Robbins explains:

“Next to the Dead Sea Scrolls, the law code of King Hammurabi of [Old or First] Babylon is probably one of the best known of ancient documentary finds. In it is the famous passage about ‘an eye for an eye, a tooth for a tooth.’ The Code of Hammurabi was written in cuneiform about 1750 B.C. on a black piece of diorite about seven feet long.”¹¹

The original site where it stood is unknown but the slab itself was discovered during the December 1901 to January 1902 excavation at Susa by the French archaeologist M. de Morgan. Today it is housed in the Louvre in Paris. The problem, once again, is: How did the Old or First Babylonians carve this code into diorite three hundred years before steel was supposedly invented? This strongly indicates that whoever Hammurabi is equated with, he did not reign prior to the invention of steel. That is, the Code of Hammurabi was carved into diorite after about 1400 B.C. and not 1790 to 1750 B.C. based on the established chronology.

However, Ernst Herzfeld makes it quite clear that iron was known. “In Babylonia it [iron] is first mentioned . . . in a contract of the Hammurapi period which speaks of iron produced in mines.”¹² Heinsohn mentioned this evidence in AEON vol. I, no. 2, p. 24, which unfortunately did not publish his reference to Herzfeld:

“The mention of iron mines in a contract of the Hammurabi period is too early by 600 years [for the production of iron products from ore extracted from mines] even in the conventional chronology—for the beginning of the Iron Age. On the other hand, the period of the Martu (and the preceding one as well) should have made sophisticated use of iron since otherwise it has to remain enigmatic how the impressive objects of diorite—an extremely hard stone—could be chiseled.”

¹¹ Lawrence H. Robbins, Stones, Bones, and Ancient Cities (NY 1990), p. 2
¹² E. Herzfeld, loc. cit.
In essence, since, according to Heinsohn, Hammurabi is the alter ego of Darius the Great who lived in the first millennium B.C., in the Iron Age, it is completely valid that there should be contracts with Hammurabi to obtain iron from mines and carburize it to make steel to cut diorite. On the other hand historians cannot place this contract into their chronology and do not discuss the enormous problem it poses for their chronology. And they certainly have never carved diorite and engraved it with copper, bronze, or meteoric iron. But as Garland pointed out above, steel saws will cut diorite, and it follows that steel chisels will engrave and shape the stone.

This is corroborated by Woolley:

“The earliest literary mentions of iron are in a letter from Mari mentioning an iron bracelet which came as a gift from the king of Carchemish (implying that the metal was a rare luxury) and in an Alalakh tablet in which King Ammitaku claims that he had captured from those who opposed him booty including four hundred weapons of iron. As Ammitaku ruled somewhere around 1750 B.C. the date was unexpectedly early."  

Those seeking a good background in geology and/or metallurgy should read Dayton’s book which discusses other minerals and metals that indicate unmistakably that the established chronology can have no standing.

There is again simply no escape from the obvious conclusion that only carburized iron—steel—will cut the diorite statues of the Sumerians and the diorite slab which contains the Code of Hammurabi. Only around 1400 B.C. or thereafter, based on conventional chronology, did steel become known and spread throughout the rest of the Near East. This requires that the so-called Sumerians did not live in the third millennium B.C. but in the latter part of the second, and that the Old or First Babylonians did not live in the earlier part of the second millennium B.C. but in the latter part of the second or even that both empires may have thrived in the first millennium B.C. And as we will see below even this date must be greatly lowered.

Thus, as with Egypt’s Old Kingdom, the carving of diorite, which requires carburized iron and probably abrasives, supports Heinsohn’s placement of the Chaldeans who were in fact the so-called Sumerians in the latter part of the

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13 Hawkes, Woolley, *op. cit.*, p. 559
second millennium B.C. in conventional chronology. This author will be perfectly willing to drop this conclusion as soon as a historian or archaeologist or anyone else takes a large block of diorite and publicly carves a statue of either an Egyptian or Mesopotamian king and inscribes on it either hieroglyphics or cuneiform script, with copper or bronze and sand. The sculptor may also use wrought iron or meteorite iron which existed in these ancient times.

I dare venture that no one will take up this fundamental crucial test, this challenge, because they understand full well that none of the historians’ sharply honed ad hoc hypotheses will ever do that job. Scientific proofs require scientific tests. There are no such fully carved diorite statues executed with soft copper and sand abrasives by modern experimenters. There is no scientific proof, no scientific test, only words, words, words from historians that cannot cut the mustard, let alone diorite!
CHAPTER 11
TIN BRONZES AND THE SUMERIANS

“Gudea built the temple bright with metal
He made it bright with metal . . .
With copper mixed with tin he built it”

Cylinder A Column XVI of
Gudea, third millennium B.C.

“Let me set out on the road to the tin ores,
Let me learn of their mines”
Inanna and Ebih

As with the development of steel in 1400 B.C. in Mesopotamia, the same problem exists with respect to tin bronzes. The Sumerians are supposed to have lived from about 2400 to 2024 B.C., but to produce bronze requires a source of tin. The Britannica shows:

“Tin, in the form of its alloy bronze, was widely used in ancient times. Bronze implements with tin content of up to 10 to 18 percent dating from about 3500 BC have been found in Ur (Iraq). Tin mining was reported in Caucasia as early as 1500 BC, and later in Persia (now Iran). . . . The Phoenicians are believed to have played an important part in spreading the early bronze culture by their trade in tin, which their ships brought to the eastern Mediterranean from Great Britain and Spain at least as early as 1100 BC.”

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The historians of the ancient world have looked to the geologists who understood the nature of how tin ore, cassiterite, is obtained. The ore is found as alluvial or stream deposits which come from it being washed from a vein or lode of tin from solid rocks. Heavy downpours of rain or floods and avalanches can dislodge

1 Encyclopedia Britannica, Macropedia, vol. 18 (Chicago 1982) p. 426
the ore and carry it into streams. The source of tin could not have been near Sumer, and B. Landsberger, in the *Journal of Near Eastern Studies*, vol. XXI, went so far as to suggest that Sumerian tin was imported from Burma, Thailand, and Malaysia. Others have suggested that Sumerian traders or other traders carried the tin ore from areas along the Danube and further north, especially the provinces which are today called Bohemia and Saxony. The theory of tin from the Caucasus has been found to be a false lead, as discussed earlier. Jacquetta Hawkes presents the thinking of her colleagues with respect to the source of tin ore:

“The advance [from copper] to the use of bronze [in Mesopotamia], however, was probably as tentative and uncertain for the ancient metal workers themselves as is the historical reconstruction of the process for us today. The sources of uncertainty are twofold. One is that copper ores usually contain impurities of various kinds, sometimes including tin. Metal containing 2 percent or less of tin is usually written off as representing a natural, unintentional adulteration of this kind. On the other hand, of course, the metal workers may have learned to prefer ores coming from regions where tin was present because they observed that they produced better results.

“The second source of uncertainty is past and present ignorance about tin itself. There are no ancient accounts to tell us anything about where it was obtained or how it was prepared. Nor is it ever found separately (as copper may be) in bronzesmiths’ ‘hoards’. Cassiterite or tinstone occurs in quartz veins in volcanic rocks, where it may be associated with gold. Both ores were often washed out and redeposited by streams. It seems that men seeking the bright specks of gold in the streams came across heavy dark nuggets of tin and recognized them as metallic. . . .

“The earliest Mesopotamian artefacts that can be called bronzes date from about 3200-3100 B.C. However, as they contain only about 4 percent of tin and also yield lead and iron, it is very probable that the tin was present as a natural impurity. It was in the Early [Sumerian] Dynastic period that the controlled and deliberate production of bronze really got under way.”

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2 Hawkes, *op.cit.*, p. 108
If lumps of tin were washed into the streams of Mesopotamia, they could be separated from the riverbed materials such as mud and gravel by washing them with water in a pan, a process known as panning, or passing them through a fine mesh. This method of washing, Hawkes and her colleagues believe, was carried on in the mountainous regions around Mesopotamia and is known as placer mining.

This process is ancient and the forty-niners of the American gold rush days were also placer miners. The problem is that in order to have nuggets in streambeds there has to be a tin source to be exploited by rains, floods, earthquakes, avalanches, etc. As we pointed out in our discussion of the tin bronzes in Old Kingdom Egypt, mineralogists have gone over the various source regions, assumed by the historians to have tin ore, and failed to find tin in or around Mesopotamia.

Hawkes further tells us about well-fashioned bronze objects:

“One of the most remarkable of all these unique and unexpected works that star the traditional course is the bronze head, believed to be that of Sargon himself, found a Nineveh. The complete mastery of technique as well as the creative power of the portraiture make this so exceptional a work of art that critics could not at first believe that it had been made in the twenty-fourth century B.C.”

3 ibid., p. 255

Not only does the style of the work indicate that it was made after 1100 B.C., the fact that it was a masterly work in bronze casting points to the very same fact. In discussing ancient India, Hawkes admits “Once again as in Mesopotamia and in Egypt, the source of tin is unknown.”

4 ibid., p. 269

Transported by Phoenician traders, tin does come into general use in Mesopotamia from European sources, Bohemia, Saxony, Spain, and Cornwall, only around 1100 B.C., based on the conventional chronology. So how could the so-called Sumerians produce true tin bronzes from 2400 to about 2040 B.C.? One does not have a Bronze Age without tin, in the same way that one does not cut diorite without having an Iron Age. Again, the so-called Sumerians could not have lived in the third millennium B.C. The digs at Tell Munbaqa also leave the Mesopotamian chronology in a shambles, a concept without a stratigraphical leg upon which to stand.

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3 ibid., p. 255
4 ibid., p. 269
CHAPTER 12
POTTERY DATING AND THE SUMERIANS

As we recall, Petrie found Late Helladic forms of pottery (800-700 B.C.), many with geometric designs, in Egypt’s 1st Dynasty site at Abydos. What is the nature of the pottery, and to what period is it properly dated, in so-called Sumerian strata? If the pottery and other artefacts were made in the third millennium B.C., they would not be similar in form and design to pottery made in the first or second millennia. Nevertheless, Dayton shows that this is indeed the case:

“In Mesopotamia where the connections or similarities with 1st Dynasty Egypt have been noted, even so far as to suggest sea travel between Ur and Egypt, again [the derived] chronology is based on Petrie’s Abydos pots.”¹

That is, the form and style of pottery found in the Sumerian city of Ur dated to the third millennium B.C. was so similar in type to the pottery that Petrie found at Abydos that the archaeologists could suggest that there was sea travel between Ur and Egypt. But of greater significance is that they attributed these Late Helladic pottery forms, as Petrie did, to the third millennium B.C. Since Petrie dated Late Helladic pottery to the Old Kingdom, the historians and archaeologists dated it to the third millennium when they found it at Ur. The chronology of Ur was again constructed on Petrie’s error. But not only did the pottery properly date to the first millennium B.C., so did many other artefacts at Ur tell the very same story:

“In fact, Woolley noted how the pins of Ur type at Alalakh [in Syria, dating from about 1780 to shortly after 1200 B.C. by conventional chronological reckoning] survived there over a thousand years later, while Hall and Woolley . . . noted that at Ubaid [in pre-Sumerian strata] the designs were ‘exactly paralleled on Late Minoan IIIb vases two thousand years later’! Again . . ., ‘the elaborate double kymation [a form of pottery] curiously resembles a Late Minoan II-III motif. . . .

¹ Dayton, op.cit., p. 216
“At Telloh [in the Sumer region] we find . . . [o]ne spouted jug has a curious extra side spout such as we see at Berbati in a Late Minoan I context . . . Trojan-type four-lugged [four-handled] storage jars of Troy IV type are common, as are single-handled drinking cups. . . .

“The closest parallels to the hatched chevrons and triangles [geometric designs of the first to late second millennium B.C.] of this pottery are to be found in the white painted wares of Cyprus.”

The various items of pottery, pins, etc., found in so-called Sumerian strata are directly correlated with forms that appear up to “two thousand years later,” meaning two thousand years closer to the present. The pottery and artefacts found require that the Sumerian strata containing them must be dated either to the end of the second millennium B.C. or to the first. Yet we are also expected to accept that the artefacts in the strata directly above this late second to first millennium, or claimed by the historians’ and archaeologists’ correlations from other sites, date to the early part of the second millennium. That is, Sumerian pottery and artefacts dated to 1200 to 600 B.C. are followed by pottery and artefacts above them or correlated with them that date from 2000 to 1200 B.C. It is obvious that materials dated from 1200 to 600 B.C. cannot be followed by civilizations that are dated from 2000 to 1200 B.C. The depth of contradiction to the established chronology is clearly destructive of that chronology. Yet this is exactly how historians and archaeologists have organized the pottery to fit into their Procrustean chronological bed.

Historian Richard J. Evans of Cambridge University discusses this form of analysis:

“What is a historical fact? Sir Geoffrey Elton, for one, had no doubts . . . A historical fact was something that happened in the past, which had left traces in documents [or other artefacts of history such as pottery] which could be used by the historian to reconstruct it in the present. In order to perform this operation successfully, the historian had in the first place to shed all prejudices and preconceptions and approach the documents [or other artefacts] with a completely open mind. ‘Ideological theory,’ Elton declared, ‘threatens the work of the historian by subjecting him to predetermined explanatory schemes and thus forcing him to tailor his evidence so that it fits the so-called

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2 *ibid.*, pp. 216-217
paradigm imposed from outside.’ He argued instead that the material left us must be read ‘in the context of the day that produced it . . . The present [paradigm] must be kept out of the past if the search for the truth of the past is to move toward such success as in the circumstances is possible.’ Thus the historian’s questions should be formulated not by some present theory but from the historical sources themselves.”

Just the opposite approach was applied to so-called Sumerian pottery. In order to tailor this pottery found in Sumer to the third millennium B.C., historians are pretending that the pottery, which is clearly Mycenaean, Aegean and Syrian of the second millennium, was produced in Egypt in the third millennium, and then traded with Sumer, in spite of the fact that no such trade is known between these early societies. They are employing an “ideological [chronological] theory” in order to “tailor evidence so that it fits the so-called paradigm imposed from outside.”

What then of tin faience and tin bronzes, were artefacts of this material found at Sumerian sites as they were at the Old Kingdom site at Abydos? On this question, Dayton also informs us: “At Telloh . . . anachronistic faience beads [were found]”

Dayton summarizes the research at Ur and Uruk in Sumer:

“From a technological viewpoint the writer has been unhappy for some years with the apparent discrepancy in chronology between the arrival of true tin bronze in Mesopotamia, particularly in the Royal Tombs of Ur which also have silver objects with tin in them, and the [conventional] date when tin bronzes and silver appear in Syria, the Aegean and Levant and Egypt—i.e., some time after about 1800 B.C. or even later. And yet we have tin bronzes, silver, and now also faience in splendid isolation in Mesopotamia, some eight hundred years earlier than these other areas. . . .

“It seems to the writer [Dayton], from a purely technological point of view, that eventually the Mesopotamian dates, especially for . . . Ur, will have to be drastically lowered to bring them into line with Egypt and the Levant. The silence of the technological gaps is too deafening to be ignored much longer.”

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4 Dayton, *op.cit.*, p. 217
5 *ibid.*, p. 220
In terms of tin bronzes, tin faïences—tin-glazed earthenware—as well as silver with tin, the so-called Sumerians have to be dated in conventional chronology a minimum of 800 years closer to the present or even more, based on the shape and style of their pottery.

Three dimensions of science all point quite unambiguously to the fact that the so-called Sumerians could not have lived in the third millennium B.C. Stylistic pottery dating (not a science) points unambiguously to this fact. Tin bronzes at Ur point unambiguously to this fact. Silver with tin does the same. Tin faïence leads to the same inescapable conclusion. When we add to the mix the problem of cutting diorite with soft copper and quartz sand, instead of carburized iron and abrasives that are only used in Greek times, the empirical facts override and destroy any conclusion that the so-called Sumerians could ever have lived in the third millennium B.C. Similar forms of evidence, as we well know, also apply to the Old Kingdom in Egypt. The science has spoken quite decidedly. Its evidence unrelentingly points to a highly shortened chronology for both Egypt and Mesopotamia.
“In any case, is Ginenthal still of the opinion that the Royal graves at Ur, as per Heinsohn, were actually those of Scythian princes despite all the evidence I had presented to the contrary? Why did he not, instead, attempt to refute that evidence?”

Dwardu Cardona

“A Return to the Two Sargons and Their Successors,” *AEON*, vol. V, no. 4 (July 1999), p. 33 as paginated by Ginenthal

According to Heinsohn, the Sumerians were the alter egos of the Chaldeans who were conquered by the Assyrians with the aid of Scythian warriors from Southern Russia. As a reward the Assyrians gave the Scythians control of the city of Ur as their vassals. When these governors or vassals died they were buried according to their custom in graves along with members of their court and animals. This places the Sumerians-Chaldeans in the first millennium B.C. and not the third. Cardona has raised several issues, which he claims “refute” this identification of Scythian graves with those found in the Royal Cemetery of Ur. As a method of testing this identification, one can turn to the historical evidence, as did Cardona, but one can also turn to scientific as well as technological evidence which refutes Cardona’s critique. As a basis for determining this question of Scythian-Sumerian burials, scientific and technological facts must be given the greatest weight, while that of historical evidence will be less so. Yet both forms of evidence must corroborate each other in terms of either Heinsohn’s thesis or the conventional chronology that Cardona appears to uphold. This author accepts Cardona’s challenge above and will employ all forms of evidence to respond to that challenge, holding that the scientific and technological facts greatly support Heinsohn, as do the historical facts. According to Heinsohn,
“The magnificent ‘Sumerian’ vassal graves of the so-called Royal Cemetery at Ur, which according to stratigraphy must be placed at the end of the early dynastic period, have an uncanny resemblance to the graves of the Scythian princes, dated 1500 years later.”¹

Here Heinsohn clearly maintains that the Scythians are not Sumerians, but vassals of the Assyrians who set them over Ur as local city rulers. In the spring and summer of 1999 a debate over Heinsohn’s identification of the Scythian graves with those at the Royal Cemetery was carried on in the Kroniatalk Internet discussion group. Clark Whelton took part in the debate and has generously provided this author with a copy which will now be presented.

Cardona: “What happened, then, to Heinsohn’s identification of the Scythians as the Sumerians?”

Whelton: “No idea. First I’ve heard of it. Where was the above ID made?”


Whelton: “Thanks for the clarification. As you know, Gunnar [Heinsohn] has long since abandoned his ‘two Sargons’ approach. I assume this is also true for the Scythians-Sumerians . . .”

Cardona: “I understand. It’s just that he’s on record for retracting his original ‘two Sargons’ identification. But he never said anything about his Sumerian/Scythian one. I was simply wondering whether he still adheres to it.”

Whelton: “Gunnar [Heinsohn] went on record last week as saying he never made any such Sumerian/Scythian identification.”

Cardona: “What, then, about the source I sent you? It came from the horse’s own mouth.”

¹ Heinsohn, “Did the Sumerians and Akkadians Ever Exist?”, op.cit, p. 18
Gunnar Heinsohn writes: “Just refer Dwardu [Cardona] to the 1998 Harvard/Yale poster text as my state of the art.”

Cardona: “And refer Heinsohn to his own words as they were published in ‘Did the Sumerians and Akkadians Ever Exist?’ AEON I:2 (February 1988), p. 41. It’s all there, as plain as day. How can Heinsohn say he never made such an identification?”

Whelton: “Please quote the relevant passage. I don’t recall it and have never heard Gunnar say Scythians were Sumerians. If you say he wrote it, then he must have, or perhaps he changed his mind. Please post it. Thanks.”

Jan Sammer: “Having leafed through Heinsohn’s 1988 paper in AEON I find that he constantly identifies the Scythians with the Quti or Guti (e.g., on p. 49); I don’t find any identification that he identifies them with the Sumerians. Or have I missed something in my cursory review of his 36-pages long article? Having followed the Heinsohn controversy from the very beginning, both in print and in personal discussion with the author, I do not recall him ever identifying Scythians with the Sumerians.”

Heinsohn: “I also wrote in 1988 that the typical Scythian stag and panther—sculptures and drawings—found in strata right above the Old Akkadians [whom Heinsohn equates with the Assyrians], i.e. in strata of the Ur III-Sumerian period, belong to Scythians (= [equated to] Qutheans = Guti . . . who helped the Chaldeans (= Sumerians) defeat the . . . Assyrians . . .”

Whelton: “. . . Gunnar answered Dwardu’s point about Scythians/Sumerians.”

Cardona: “He didn’t ANSWER. He DODGED.”

Whelton: “Dodged? It was a flat-out denial. . . . I have never heard Gunnar say the ‘Sumerians’ were really Scythians, unless he somehow meant that the Chaldeans were Scythians, which I have never heard him say, either. . . . Please post the relevant Scythian/Sumerian citation. Thanks.”
Cardona: “I already have—twice. It’s there as plain as day.”

Whelton: “. . . I don’t have that issue available. Will SOMEONE who has access to *AEON* I:2, p. 41, please post the passage or footnote or whatever where Gunnar Heinsohn equates Scythians with Sumerians. Thanks . . .”

Sammer: “The only relevant information on page 41 is under nos. 17 and 18. How anyone in his right mind can conclude from these statements that Gunnar equates Scythians with Sumerians is beyond me. Judge for yourselves.

[the verbatim citations on page 41:]

“(17) The mysteriously long-lived and treacherous Quti at the end of the ‘Akkadian’ empire are the treacherous Scythians at Nineveh at the end of the Assyrian Empire.

“(18) The apparently unique vassal graves at Ur are typical graves for Scythian princes. As co-conquerors of the south, on the side of the Assyrians, the Scythians gain ruling positions and consequently also the appropriate burial places.”

Sammer: “That’s it, folks. If you read the first sentence under (18) in isolation, you could be excused for concluding that Gunnar equates the Scythian GRAVES with Sumerian graves. The second sentence however shows that he does nothing of the sort.

“I must say that I am deeply disappointed in my friend Dwardu—not so much for making the assertion in the first place (a faulty recollection is understandable) but rather for insisting on its correctness and even citing a page that does not contain what he claims it does . . .”

Heinsohn: “I will not let Dwardu off the hook. Yet I assume I know why he got it all mixed up. I said in 1988 that the alliance of Scythians, Chaldeans and Medes which—according to Herodotus—brought about the downfall of the Empire Assyrians . . . were the same as the Qutheans, Sumerians and Mitanni of mainstream [historians]. Thus I—as do mainstream [historians]—spoke of an alliance between Qutheans
and Sumerians. I identified these two peoples with Scythians and Chaldeans of Greek historiography, and I still do. Dwardu, however, turned the alliance into an identification.”

Cardona: “I did not turn the alliance Heinsohn speaks of into an identification. It was Heinsohn himself who directly identified the Sumerians as Scythians when he identified the Sumerian Royal Graves at Ur and their contents as Scythian graves and THEIR contents. . . .”

The rest is of little value since Cardona never retracted his twisted logic wherein he claims that Heinsohn equates Scythians that were found in the graves in the Royal Cemetery at Ur with Sumerians. People can be buried in a so-called Sumerian cemetery who were not Sumerians at all. That is just what Heinsohn maintained. He never claimed that because the Scythians were buried in the so-called Sumerian cemetery, they were Sumerians. In this context, Whelton wrote:

Whelton: “Dwardu, your conduct here is shameless. Gunnar Heinsohn has NEVER believed the ‘Sumerians to have been Scythians’ to use your words. Only the most twisted logic could reach such a conclusion.”

Now if Cardona seems to suggest that the Sumerians are actually dated to the third millennium, he would have to prove that they carved diorite with copper or bronze and quartz sand, and he would have to prove that there were sources of tin to make tin bronzes. Neither of these fundamental scientific technological facts has ever been proven with unambiguously clear-cut evidence.

As with the availability of carburized steel that is required to cut diorite, Cardona’s criticism of Heinsohn’s thesis of the Scythians as vassals of the Assyrians being buried in the Royal Cemetery at Ur deserves a full response. In doing so this author will evaluate the evidence Cardona raised from historical as well as from scientific evidence as these impinge upon each other on these questions, the scientific evidence being fundamental to the answers.

Cardona states:

“The graves in question are those discovered by Woolley in the 1920s. The archaeological treasures that were retrieved from them astonished the world and were fast in becoming internationally famous.
Comparable in quality, if not in quantity, to Tutankhamon’s funerary equipment, they remain spectacular to this day. One can hardly open a work on ancient Mesopotamia without being confronted with pictures, often photographs, of these richly ornamented items in silver, gold, and lapis lazuli.

“These graves were spectacular in one other respect. Besides the royal personages, interred within their chambers, the richly dressed remains of soldiers, courtiers, and female attendants, as also draft animals with the wheeled vehicles they once drew, were discovered at the foot of the sloping ramps which led to them. These were all laid out in an orderly manner, grisly evidence that the members of the funeral cortege had allowed themselves to be sacrificed and buried with their suzerain. Some privileged individuals were even laid to rest within the burial chambers that held the royal remains.

“According to Heinsohn, these tombs are ‘paralleled only in the graves of Scythian princes’ or, as he later phrased it, at least bear ‘an uncanny resemblance’ to them. In any case, his conclusion is that these graves at Ur are ‘typical’ Scythian graves. As ‘co-conquerors’ of southern Mesopotamia ‘on the side of the Assyrians,’ the Scythians managed to ‘gain ruling positions and consequently also the appropriate burial places.’”

Here Cardona makes it quite clear that the knew that Heinsohn held that the Scythians were described as “co-conquerors” of southern Mesopotamia “on the side of the Assyrians” and were not identified by Heinsohn as Sumerians. After having written this statement he ignored it when he then accused Heinsohn of identifying the Scythians with the Sumerians.

But Cardona’s discussion does not explain why these so-called Sumerians were practicing this grisly form of burial as did the Scythians some 1500 years later. There are some indications that the Egyptians in their very early history may have sacrificed members of the royal court with their sovereign. Cardona writes about certain similarities between the Scythian and Sumerian graves but has failed to give us any evidence at all from Sumerian history, art, literature, religion, etc., for the Sumerians having ever been involved in this burial rite before this time or after. Jacquetta Hawkes states that “. . . this practice [of human and animal sacrifice] seems

2 Cardona, “The Two Sargons and Their Successors,” op.cit., part II, pp. 82-83
in itself to have been abandoned after the period of the Ur tombs (representing some two generations)...”\(^3\)

Though sacrifice of people, and animals, is known and was common, the only other known people outside Mesopotamia directly associated with its empires who practiced sacrificing the members of the king’s court and animals were the Scythians. Various theories have been advanced to explain this funerary rite but none has been generally accepted. In fact, the uniqueness of these burials at Ur is still being discussed and debated. However, if Heinsohn is correct, there is no enigma and the reason for these rites is quite obvious. Since this was an inherent practice of the Scythians in their homelands across south-eastern Europe and southern Russia to the Altai in southern Siberia, they would naturally take it with them it when they ruled elsewhere, say, at Ur. P.R.S. Moorey in an updated version of Woolley’s *Ur of the Chaldees*, in referring to the people buried in the royal tombs at Ur, provocatively asks:

“Who then were the people who received such rites?”\(^4\)

Moorey did not ask: “Why did the Sumerians practice these rites?” No, he wonders and asks “Who were the people” that did so! Paul G. Bahn in 2001 echoes this problem:

“Questions still remain about the Royal Cemetery despite the many details provided by Woolley. *Did the graves really contain remains of Sumerian royalty?* Who were the sacrificial victims? Why did they die? Although the special nature of the tombs is clear, there is only slender evidence to indicate conclusively royal interment. . . .

“The mortuary practices in the Royal Tombs are unparalleled elsewhere in Mesopotamia, and they seem to have been an aberration unique to Ur within Sumerian culture.”\(^5\) [emphasis added]

Bahn does not ask: “Who were these Sumerians?” He asks: “Did the graves really contain the remains of Sumerian royalty?” He says, “there is only slender evidence to indicate conclusively royal interment,” and calls these mortuary

\(^3\) Hawkes, *op.cit.*, p. 209
\(^4\) P.R.S. Moorey, *Ur of the Chaldees* (Ithaca NY 1982), pp. 89-90
practices “an aberration unique to Ur within Sumerian culture.” This problem echoes through the literature. Woolley himself stated:

“Now there had never before been found in Mesopotamia anything like these tombs; there was no archaeological parallel to the age, the wealth, the architecture and, above all, to the ritual which they displayed. Moreover in Sumerian literature there was no hint of human sacrifice forming part of a royal funeral, and such a practice seemed alien to all known Sumerian tradition.”

Georges Roux states, “The Royal Cemetery of Ur presents the historian with very difficult problems.” Susan Pollock writes:

“Many scholars have wrestled with the question of who the people were who were entitled to such extraordinary ritual attention at their death. The question is all the more intriguing in that nowhere else in Mesopotamia have tombs with ‘attendants,’ who were killed for the funerals of other people, been found.”

Roux, in fact, considers the Royal Tombs at Ur a “Great Enigma.”

Woolley, as do all the others, makes it undeniably clear that one can find nothing in archaeological research in Mesopotamia like these tombs. They are “alien,” “unparalleled,” “an aberration” for Sumerian culture. The architecture of the tombs is also unparalleled and alien to Sumerian culture, as is the rite itself. But of course, none of these practices were alien, unparalleled, or an aberration to the Scythians. This anomaly stands against any consideration that the people who engaged in this ritual were Sumerians. A ritual of such momentous import to its followers does not come into being out of nothing and then abruptly end for no reason. The Egyptians, as is well known, placed carved and painted figures in the tombs of their pharaohs to assist them in the afterlife, after they may have abandoned this sacrificial practice. If the so-called Sumerians abandoned this ritual, they didn’t do as the Egyptians did. This suggests that this was not a Sumerian rite. When Woolley used the term “alien” to describe these mortuary practices, he meant that

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6 Woolley, _The Excavations at Ur_ (NY 1963), pp. 77-78
7 Roux, _op.cit._, p. 136
8 Susan Pollock, _Ancient Mesopotamia_ (Cambridge 1999), p. 211
these forms were outside of, and foreign to, Sumerian culture. And foreign is what they were. It well supports Heinsohn’s suggestion that these were the tombs of Scythian princes.

Cardona has noted that there were only sixteen such burials in the royal cemetery. Yet Woolley provocatively asks: “But in the graveyard at Ur, which must have been in use for many generations, there are only 16 ‘Royal Tombs.’ Did men only 16 times in all those years take steps [to bury sacrificial victims] to secure good harvests [or for any other reason]?”

This ritual was carried on for 40 or 50 more years which indicates it was indigenous to the people and culture that practiced it. It was not like the rites carried out in Egypt in terms which represent an anomaly here and there in time, but a rite that appears to be a culturally developed burial rite. For the Scythians this was part and parcel of their culture which can explain why it lasted for two generations or more at Ur when the Scythians ruled there.

According to Roux:

“Up to now, only two other cemeteries with human sacrifice are known in that country: one at Kish . . . the other at Ur itself—but they are very different from the cemetery known to be ‘royal.’ In cemetery ‘Y’ at Kish (datable to around 2700 BC), the pit tombs contained only chariots, the animals that pulled them, the driver and groom. As for the cemetery known as that ‘of the Second Dynasty of Ur,’ at Ur, it comprised only five pit tombs, relatively poor in grave goods and totally anonymous; ‘the victims’ (if that is what they were) were buried separately, individually, in the earth infill of the pits.”

Above and beyond all this, the so-called Sumerian texts about death paint a picture of the future life as dismal, as S. Moscati explains:

“However, the future life, as the Sumerians conceive it, is in strong contrast with that of the Egyptians [and Scythians], for it is wretched and unsubstantial, with poor possibilities and over shadowed with gloom. One text makes this very clear:

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10 Cardona, *op.cit.*, p. 83
11 Woolley, *loc.cit.*
‘I am no more to enjoy life,
The place of my resting is in the dust of the earth; I lie among the wicked
My sleep is anguish; I dwell among foes
O my sister, from where I lie I can no more arise!’

“All the literature, and the hero epic [of Gilgamesh] in particular, expresses this same mood.”13

The Royal Cemetery graves with all their victims and rich furnishings indicate that the afterlife of the people who were buried was not one of gloom, anguish, and unhappiness. All the materials left for the dead suggest that the afterlife would be one of comfort and joy, with servants to make eternity a happy one. There is no way that these graves reflect the so-called Sumerian culture; rather, they fully suggest the culture of the Scythians who practiced this burial rite as preparation for their eternal rest. The Royal Tombs at Ur are totally antithetical to so-called Sumerian culture. They are fully compatible with Scythian culture which also fully conforms with Heinsohn’s chronology. True, commoners’ graves were left with artefacts for the next life, but the Royal Tombs with sacrifices are quite a different matter.

Cardona appears to be unaware that this problem exists and that historians are still at a loss to explain this anomaly from Sumerian literature, history, religion, art, etc. This aspect of the enigma is really a question for Cardona and not for Heinsohn to elucidate. To say that since these tombs are found at Ur, the people buried in them are Sumerians, does not answer the question. All the evidence to date has not resolved this central issue at the heart of the discussion. The various theories to solve the problem with their counter-evidence can be found in Moorey’s book14 and Roux’ article.15

Cardona states:

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14 Moorey, *Ur of the Chaldees*, *op.cit.*, p. 91 ff
“Thus the cylinder seal of one occupant [of a grave at Ur] identifies him by name as Meskalamdug, designating him as *lugal*, that is a ‘king.’ The cylinder seal of another occupant is even more specific. It names the interred person as Akalamdug, designating him in no uncertain terms as ‘King of Ur.’ And that, in itself, is enough to put Heinsohn’s hypothesis on this matter to rest. But before he claims this King of Ur to have been a *Scythian* king—even though he only allowed for Scythian *princes* in his reconstruction—let us look at the rest of this evidence.”

That is, only two names suggest kingship.  Heinsohn did claim that Scythian princes rose to seats of power at Ur and were merely vassals. This is exactly what the literature tells us, as Woolley explains:

“Here we have the names of kings [at Ur], and the fact that they do not appear in the Sumerian king-lists is only to be expected. The king-lists enumerate only those rulers whose authority was believed to have extended over the whole country; . . . the kings . . . who were buried there were kings not of Sumer, but, as A-Kalam-dug says, of Ur; they were local city-kings, vassals of whatever State was at that time suzerain.”

The rulers buried in the “royal tombs” were like governors, petty rulers over a small domain. However, Cardona suggests that since Meskalamdug and Akalamdug are titled “king,” they were Sumerians. Nevertheless, the research on this point is rather negative to what Cardona firmly puts forth. Let us recall that Bahn claimed “there is only slender evidence to indicate conclusively royal interment.” There were no sacrificial victims found in Meskalamdug’s tomb. As Moorey writes:

“If there is good reason to think that the Meskalamdug buried in a tomb lacking sacrificial victims was a king of Ur, why are they absent, if they really were indicators of a royal burial? Does it mean that not all the rulers of Ur in Early Dynastic times were given such burials and if not, why not?”

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16 Cardona, *op.cit.*, p. 83
17 Roux, *op.cit.*, p. 136
18 Woolley, *op.cit.*, p. 80
19 Moorey, *op.cit.*, pp. 89-90
Cardona maintains that Meskalamdug is a Sumerian king, but since kings were buried along with many sacrificial victims and he was not, then he might not be royalty. The furnishings found in his tomb are quite rich but without the sacrificial entourage found in the other tombs, suggesting that he was merely a wealthy person but not one of high rank. There is no evidence that Meskalamdug is a king because the inscription with his name actually found in his grave does not contain the title *lugal*. That name and title were found in an unnamed woman’s grave, as we will see. He was probably related to those in the royal tombs and was probably an important member of this group, but hardly a chieftain or a king.

Furthermore, the riddle posed by this grave only deepens when we examine the facts related to it:

“For although the grave had the marks of a private burial—. . . it was, curiously, one of the richest at Ur. . . crammed with ‘objects found elsewhere only in royal graves.’ Among those in the coffin were two gold bowls and a gold lamp, each inscribed with the name Meskalamdug (but lacking a title, such as *lugal* for king); . . . Was he a king buried like a commoner—or a private citizen with the riches of a monarch? A queen’s non-royal lover, perhaps, or a warrior heaped with wealth for his deeds? The mystery is further complicated by a seal found in an unnamed royal woman’s grave. The seal is inscribed ‘Meskalamdug lugal’—King Meskalamdug. Woolley believed this was a different man. Others are not so sure.”

We are further told: “Other scholars have speculated that the tombs were of priests, not royalty.”

In fact, some historians suggest that Meskalamdug was a prince and not a king. For example, Hans Baumann states, “He was Meskalamdug the hero of the good Land. Presumably he was a prince and not a king, for there was no sign of royalty either there or on the cylinder seal with his name that was found in a queen’s grave.” Brian Fagan also claims that “Woolley uncovered a brilliant masterpiece, Prince Mes-kalam-shar’s gold helmet in the form of a wig [in the Royal cemetery].” It is clear that certain historians do not accept that this individual was

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21 ibid., p. 116
22 Hans Baumann, *In the Land of Ur*, transl. Stella Humphries (NY and Oxford 1969), pp. 73-74
a king but consider him a prince, as Heinsohn suggests. With respect to MeskalAMDUG, Baumann wonders “why was it that . . . the body of the king appears to be missing from the grave [with the high status lady where his name and description of king was found]?”24 or “where had the king gone, since he was no longer in his vault.”25 These are serious discrepancies that cannot be dismissed.

What then of Cardona’s firm assertion that a cylinder seal names Akalamdug a “King of Ur” “in no uncertain terms”? Does the historical evidence support this translation of that cylinder seal? Again, the evidence does not truly support Cardona’s assertion, as Moorey further points out:

“Only one seal inscription from these tombs mentions both a king and his wife, and she is not titled nin [queen]. This inscription has been read to mean either: ‘Akalamdug, King of Ur; A. Shusikildigir is his wife’ or ‘To A., King of Ur, A., his wife has dedicated this seal.’”26

There is not only one translation of the cylinder seal. We do not know who A. was other than a King of Ur, not necessarily a so-called Sumerian king. It is very slender evidence upon which to rely. The evidence regarding MeskalAMDUG and Akalamdug strongly undercuts what Cardona suggested was firmly established and in no way can be construed as other than “slender evidence to indicate conclusively royal interment.” In these cases conclusiveness is needed but it is lacking.

H.W.F. Saggs states succinctly of the royal Cemetery:

“No burial, however, is directly identified with a royal name, and in the one case the name of a known king occurs in the burial chamber of a female corpse. It is therefore possible that the royal names did not identify the person buried but were there because royal persons had made dedications at some ritual vital to Ur.”27

If the burials were not those of kings, as Saggs suggests, then they could have been those of Scythian princes or other Scythian high-ranking people who had dedications offered to them by so-called Sumerian kings.

24 Baumann, op.cit., p. 77
25 ibid., p. 78
26 ibid., p. 89
27 H.W.F. Saggs, Peoples of the Past: Babylonia (Berkeley CA 2000), p. 65
As Susan Pollock states:

“The excavator of the Royal Cemetery, Sir Leonard Woolley, argued that the tombs were the burial places of royalty accompanied in death by members of their court. . . . He based this contention on the unusual nature of the funerary rites as well as the presence in several of the tombs of inscribed artifacts that mention the Sumerian word for ‘king’ and ‘high status lady,’ or ‘queen.’ However, other scholars have pointed out that because none of the inscribed artifacts were found in unequivocal association with a principal occupant of a tomb, they may represent gifts from survivors rather than possessions of the dead individuals. . . . Although we may never be able to specify the exact social positions of the dead in the Royal Tombs, it seems safe to say they were a small group of top-ranking members of the political and [or] religious establishment of the day.\textsuperscript{28}

The Scythians were surely top-ranking political members of the day: Cardona’s statement that the word “king” on a cylinder seal “puts Heinsohn’s hypothesis on this matter to rest,” is not only not supported by the evidence but placed in great doubt by the evidence.

One aspect of the burials is that the Scythian high-born or important women were often buried with horses and other symbols of power. Although horses were not found by the tomb of the woman in the Royal Cemetery (to be discussed below), this is a common aspect of Scythian burials.\textsuperscript{29} The correlation between Scythian burials of high-born or important women with those found in the royal cemetery is hardly fortuitous. It is exactly what one would expect to find. Not only would princes or chieftains be buried with their courtly members, but princesses or notable women would be accorded the same burials with victims and valuables just as in Scythia. The burial rite does not conform with so-called Sumerian practice, while it does with the practice of the Scythians, but the way important women were buried conforms with the Scythian practice as well. Finding both practices at one site, the Royal Cemetery at Ur, lends strong support to Heinsohn’s thesis that both Scythian men and women were buried in the royal tombs at Ur. Nowhere else in so-called Sumerian civilization were important women buried with sacrificial victims.

\textsuperscript{28} Pollock, \textit{op.cit.}, p. 211

\textsuperscript{29} See for example the NOVA program, “Ice Mummies: Siberian Ice Maiden,” shown on PBS at 8 p.m. on Tuesday, Oct. 15, 2002
rich adornments and artefacts. But this is just how important women were buried by the Scythians.

Cardona states, talking first about Chinese graves which he feels are more likely to be called similar to the Royal Graves at Ur:

“Needless to say, these [burial] similarities are merely superficial. We are comparing two entirely different peoples who only exhibited a certain amount of similarity in the manner in which they buried their royal personages. The actual construction of the tombs as, naturally enough, the nature of the funerary items, are entirely dissimilar. And so, also, are those associated with Scythian burials.

“The burial chambers of the tombs at Ur were constructed of stone and brick with a corbelled vaulting, a technique that was entirely unknown to the Scythians. The latter merely stretched wooden beams across the top of the shafts which were then covered with thatch to form a roof.”

A corbelled vault is built to appear like a fairly steep arch. It is constructed by arranging blocks of stone or brick in a room from one, two, three, or four sides in successive layers such that the layer of stone above projects beyond the layer below. The next layer above projects beyond that below it, and so on until the final projecting blocks meet or are so close to one another that a top block will span and cover the opening.

Cardona tells us this corbelling technique “was entirely unknown to the Scythians.” He has given no citation to support his absolute assurance regarding this point. Cardona has merely used his own authority to argue for this. Unfortunately he is quite in error on this matter of corbelled vaulting among the Scythians.

George Rawlinson pointed out that the Scythians were well acquainted with, and built, corbelled vaults. He did so in 1881. In discussing the tombs of the First Monarchy of the Chaldeans—the so-called Sumerians—he writes:

“The brick [burial] vaults are found chiefly at Mugheir. They are . . . composed of sun-dried brick . . . and exhibit a very remarkable form

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Cardona, op.cit., p. 83
of construction of the arch. The side walls of the vault slope outward as they [first] ascend; and the arch [is then] formed, like those in Egyptian buildings and SCYTHIAN TOMBS, by each successive layer of bricks from the point where the arch begins, a little overlapping the last, till the two sides of the roof are brought so near together that the [topmost] aperture may be closed by a brick.”

Why did Rawlinson claim the Scythian tombs employed corbelled vaulting over one hundred years prior to Cardona claiming this form of construction was “entirely unknown” to them?

This fact was known at least since 1864. At the Kurhan Velyka Blyznytsia on the Taman Peninsula in the Ukraine, one of the great burial mounds of the Scythians was excavated from a few sides. One particular side was dug about two-thirds of the way toward the center. Near that point and to one side of the excavation an impressive vault of stone was discovered. The archaeologists carefully removed many of these stone blocks from one side of the vault but, in the interest of showing the building’s structure, they left a sufficient number of them standing. Thus the vault could be seen in perspective within the mound. F. Gross made, as clearly as possible, a detailed drawing of the opened mound with its central vault. This can be seen in the book Scythian Gold, edited by Ellen D. Reeder (NY 1999), page 88. As Figure 1, below, shows, this author has enlarged the drawing, which for all its graininess clearly exhibits a Scythian corbelled vault.

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Figure 1 Scythian corbelled vault.
It is impossible not to recognize the corbelling technique that went into the construction of the vault. In *The Treasures of Scythian Art*, by Mikhail Illarionovich (NY 1969), figure XIII and the drawing on the next page also exhibit the same corbelling technique at a Scythian mound, nineteen years prior to Cardona emphatically stating that it was “entirely unknown to the Scythians.” See Figures 2 and 3.

*Figure 2 (Scythian vault)*
Figure 3 (Scythian vault)
The corbelling technique used by the Scythians stands in stark contrast and contradiction to what Cardona tells us does not exist. It does exist and one would have to be blind not to understand that the Scythians had knowledge of the corbelling technique. This construction is “unique,” as stated in *Scythian Gold*, page 88.

But in a sense, this structure is not at all unique. The Scythians lived in tents which were carried from place to place in wagons. Little clay models of these tents have been found in Scythian lands and elsewhere. The “Scythians habitually used tents, living in the covered wagons only whilst on the move. The decoration of the burial chambers at Pazirik tends to confirm this suggestion for it was clearly designed to transform the tomb into a tent.” Rice suggests this tent structure for tombs does not exist on the European steppes but may have in the past. But this is not correct because a tent-like structure was common in southern Russia where “the dead man had been placed in the burial chamber beneath a specially constructed tabernacle . . .” as shown in the following figure.

See Figure 3A and compare it to Figure 3.

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32 Tamara Talbot Rice, *The Scythians* (NY 1957), pp. 60-61
33 *ibid.*, p. 61
34 *ibid.*
35 *ibid.*, p. 102
Clearly, this tabernacle is an effort to create a tent to house the Scythian dead for eternity. According to my old Funk & Wagnall’s New College Dictionary, a tabernacle is a “tent or similar structure . . . fixed or portable.” This is what is seen in Figure 3A. When there is little wood available, as in southern Mesopotamia, one builds with brick to form the tent-like structure as in Scythia.

Rice describes these tents:

“A shaft, often of impressive dimensions, had to be sunk into the virgin soil. Great wooden props were used to strengthen the sides . . . the shaft [was] turned into a chamber by the erection above it of a gabled roof set on massive posts resembling columns.

“Once the structure had been completed, its decoration was begun. In southern Russia, the walls of the main chamber were hung either with wicker or rush matting, birch bark, thatch or rugs, whilst at Pazirik felt was used. At Karagodenashkh in the Kuban, on the other hand, frescoes were preferred, and the figure of a deer at pasture was still visible to the excavators who uncovered the burial. The ceilings of the main chambers generally received the same treatment as the walls, thus giving the effect of a chamber or tent, or possibly even the cubicle in the covered wagon in which the defunct had spent many of his living hours. Within this chamber a smaller, roofed construction or tabernacle was often erected. In . . . southern Russia, this inner structure often seemed to follow the lines of the outer chamber, but two centuries later a dressed stone construction, ascribed . . . to Greek influence, surmounted by a wooden roof, was at times preferred.”

Interestingly, at the royal tombs Woolley found a similar construction:

“It was particularly exciting . . . because the top of the [corbelled] dome had been built over a centering [temporary wooden framework] supported by stout beams which ran right through the stonework.”

Surely, this is just like what was found in the tombs of the Scythians. This suggests that the royal tombs at Ur are really those of Scythian princes.

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36 *ibid.*, pp. 95-96
37 *Sumer: Cities of Eden, op.cit.*, p. 91
However, there is even more related to so-called Sumerian architecture and decorative arts that suggests these people lived in the first and not the third millennium B.C. After discussing corbelled vaulting, Woolley revealed the following point: “It is astonishing to find that at this early period the Sumerians were acquainted with and commonly employed not only the column, but the arch, the vault, and (as may be argued from the apsidal ends [having an apse or recess added to a room] of the chambers) the dome, architectural forms which were not to find their way into the western world for thousands of years.”

Surely this evidence of these architectural forms produced thousands of years prior to their common use in the ancient Near East strongly suggests that these forms in so-called Sumerian civilization place it thousands of years closer to the present in correlation with and corroboration of Heinsohn’s chronology. It is hardly thinkable that arch, vault, and dome could have been developed and then not been utilized repeatedly by the cultures that followed this supposed earlier one for thousands of years, especially in view of the evidence that these people influenced all the subsequent civilizations in Mesopotamia.

In terms of the sculpture at the Royal Cemetery at Ur, Woolley states:

“The mastery of material is shown in the more complex field of sculpture: a silver cow’s head and a pair of silver heads of lionesses from the grave of Queen Shub-ad combine a dignity of conception and a delicacy of treatment which could only be attributed to a craftsman whose skill of hand did justice to his imagination; even more striking perhaps is the solid-cast electrum ass which decorates the rein-ring of the queen’s chariot pole, for here there is a frank realism as in more hackneyed subjects had been ousted by the resolving of curves into planes, which was only recovered by the Greek masters of the fifth century B.C.”

Again, the so-called Sumerians had developed skills in sculpture that only came into general use in the first millennium. Just as with the architecture, this sculptural technique is developed but not commonly used for thousands of years. But since these are common expressions of first millennium architectural and sculptural development, it is more sensible to place them and the society which employed them in the late second to first millennium rather than the third millennium B.C. Woolley cannot explain this but suggests:

39 *ibid.*, p. 43
“The principles of architecture understood by the early tomb builders were not forgotten and the use of the vault, the arch and the dome seem to have been continuous, and stone sculpture certainly did advance beyond the crude efforts of the First [so-called Sumerian] Dynasty, but for the rest the evidence that we have would seem to show a steady decline both in imagination and craftsmanship . . . the worker lost his skill . . . But in 3500 B.C., Sumerian art stood at a level seldom reached in the ancient world and it must have had behind it centuries of growth and experience.”40

Placing these people in the first millennium B.C. means their arts and skills are not lost and belong to the period where these skills and forms belong.

Cardona states:

“No sloping ramp led to these [Scythian] pits.”41

Undoubtedly the so-called Sumerians built sloping ramps fairly deep to transport the ruler’s body and wagon, etc., to the vaults in which they were laid to rest. As with his statement regarding corbelled vaulting, Cardona presents nothing to back up this further pronouncement. The reader is simply asked to accept Cardona’s authority that the Scythians did not use ramps when burying their rulers with court, animals, furnishings, wagons, pottery, etc. And again, as with Cardona’s claim about corbelled vaulting, this further proclamation is indisputably false. Tamara Talbot Rice, an archaeologist and art historian, has written extensively on the art and archaeology of the Scythians. In her book The Scythians she discusses the method by which the Scythians constructed a grave site for their rulers:

“The construction of an important burial place required much effort. First a clearing had to be made in the steppe, next a sloping trench of varying length was dug and at its far end a shaft, often of varying dimensions had to be sunk in the virgin soil. Great wooden props were used to strengthen the sides.”42 [emphasis added]

40 ibid, pp. 44-45
41 Cardona, loc.cit.
42 Rice, op.cit., p. 95
From the main burial chamber subsidiary chambers as at Ur were prepared for the chieftain’s companions.43

A “sloping trench” is a ramp. Funk & Wagnall’s New College Standard Dictionary defines a “ramp” as “An inclined passageway or roadway, as between floors or different levels ...” The New Century Dictionary, vol. 2 (NY 1959) defines a “ramp” as “A slope or ascent . . . a sloping way or passage connecting two different levels . . .” Cardona seems never to have asked how the Scythians were able to place wagons, horses, etc. in these tombs, without a ramp leading down from 6 to 45 feet depths. 45 feet is the height of a 4 1/2 story building. Rice explained how this was done. The ramp led down to the burial chambers, steep slopes then led down into the floors of the burial chambers. Rice also tells us: “The cart which had carried the dead chieftain to all parts of his territory was placed either in the burial chamber or in the passage leading to it.”44 That passageway was the ramp dug to the grave level.

On page 72 of E.D. Phillips’ book, The Royal Hordes—Nomad Peoples of the Steppes (NY 1965) one can actually see the drawn model of a Scythian ramp in the center running down into the tomb. See Figure 3B.

Figure 3B. The arrow points to the ramp

43 Rice, “Scythians,” Encyclopedia Britannica, Macropedia, vol. 16 (op.cit.), p. 441
44 M.I. Artamonov, The Splendor of Scythian Art (NY 1959), p. 17
The Scythians were thoroughly familiar with ramps in tombs in spite of Cardona’s forceful assertion, “No sloping ramp led to these [Scythian] pits.”

Cardona states:

“Moreover, huge mounds of earth were then shovelled on top of the Scythian tombs, sometimes as much as 60 feet high, ‘the height of a six-story building,’ a major feature that was entirely lacking at Ur.”

How do we know that there was no mound over the Royal Tombs? Cardona has merely stated this as a fact. How then did Woolley know where to dig outside the city of Ur to find these tombs? The answer is simply that there was a huge mound over it. Werner Keller explains:

“‘The graves of the kings of Ur’—so Woolley in the exuberance of his delight at discovering them, had dubbed the tomb of . . . nobles whose truly regal splendor had been exposed when the spades of the archaeologists attacked a 50 FOOT MOUND south of the temple and found a long row of superimposed graves [above the Royal Tombs].”

A fifty foot high mound is as high as a five-story building! So not only did these people at Ur carry out sacrifices for their dead leaders or high-born people, have corbelled vaulted burial chambers, use sloping ramps to bury their rulers, but they also constructed a very large mound over their tombs, just as the Scythians did. A part of this mound can be seen in P.R.S. Moorey’s book, Ur of the Chaldees (1982), p. 54.

One may argue that sixteen major tombs are not found in one mound in Scythia and therefore these are not Scythian tombs. However, the Scythians did bury additional bodies after the original mound was completed, as Renate Rolle explains:

“If for some reason the Scythians wished to bury another body in a tomb which had been filled in, they dug a second shaft and tunneled through from the bottom until—with luck—they came to the original

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45 Cardona, *op.cit.*, pp. 83-84
burial chamber. Here, without disturbing the first burial, they laid the body of the person who had died later.”47

The Scythians at Ur, of course, were in a strange country and may have built a huge mound to place all their dead leaders close to one another, perhaps to communicate with or be near their kinsmen.

If Cardona or anyone has doubts about this mound, Keith writes of the Royal Tombs at Ur: “The remains of this group were found in graves under the ‘Tomb Mound’ of Ur . . .”48 Lawrence H. Robbins reports that “one of Woolley’s workmen was excavating below the Royal Cemetery of Ur in an effort to get to the bottom of the deeply stratified mound or tell (about sixty-four feet thick).”49 Leonard Cottrell writes: “In 1926, when workmen [at Ur] were digging deep shafts in the mound, they came upon the remains of a cemetery.”50

But Cardona has also failed to discuss or recognize a further similarity between the Scythian royal tombs and the royal tombs at Ur. At both the people who constructed the graves generally dug fairly deep pits into the earth to bury their leaders. Rather than address the evidence of earthen mounds at Ur, he should have probed down below the surface and dealt with the fact the Scythian graves tend to be dug anywhere from 6 to 45 feet down, and a comparable condition exists at Ur. There is no process that can explain away the fact that both people dug deep pits with subsidiary rooms in the earth to bury their dead leaders and their courts. This again is yet another similarity that supports Heinsohn’s hypothesis.

After discussing the ritual sacrifice of horses, found in Scythian tombs, which we will turn to below, Cardona states:

“We know from Herodotus, one year following the burial of a king, another sacrificial ritual was held at the site of the [Scythian] tombs.”51

Cardona probably derived this from his source, Trippett, or ultimately Herodotus, who discusses the length of mourning at Scythian burials:

49 Robbins, op.cit., p. 219
50 Cottrell, Lost Cities, op.cit., p. 69
51 Cardona, op.cit., p. 84
“Prolonged and demonstrative grieving followed the death of every Scythian tribesman. Forty days of processionals and ceremonial feasting were the standard prelude to laying a body in the grave. But such effusive expressions of sorrow were just a small part of the year-long series of rituals that followed the passing of the king. In tribute to the lost leader, members of every tribe in his domain joined the solemn march to the burial site, and participated in prescribed rites of mourning, sacrifice and purification.”  

Trippett talks of “processionals” and of a “solemn march” related to these rituals. The Royal Standard of Ur, among other pictorial elements, exhibits a processional, a solemn march. Surely, this cannot be fortuitous. But there is more. Fagan also points to extensive periods of ritual held at the royal tombs at Ur:

“Elaborate rituals must have extended over a considerable period of time, and to judge from more or less identical finds in several Ur tombs, were conducted at many royal funerals.”

Moorey tells us of the rituals carried out at Ur:

“The filling up of this [royal tomb] building was done by degrees; clay was brought [down to the grave below ground] and trampled hard to make a floor over which offerings were spread and on which was laid the body of a human victim sacrificed in these later rites; earth buried these, and another floor was made and more offerings placed in order and another victim did honour to the dead below, and this went on till the top of the walls was nearly reached; then half of the building [below ground level] was roofed in with a vault of mud-brick.”

Similar layering in tiers was found inside the Scythian mounds: “The soil of a Scythian warrior mound is raised in layers.” This is not exactly the same but is similar.

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53 Fagan, *op.cit.*, p. 261
54 Moorey, *op.cit.*, pp. 85-86
55 *Scythian Gold, op.cit.*, p. 96
Rituals of sacrifice and processions seem to have been conducted at the royal tombs at Ur and at the royal tombs of the Scythians, prior to, during, and after the burial which lasted a long time. Both the Scythians and the so-called Sumerian royalty carried out elaborate sacrificial rituals as well as processions over extensive periods of time. This too supports and corroborates Heinsohn’s theory.

Cardona states:

“Also one need not be an art historian to notice the striking difference between the objects found at Ur and those of Scythian workmanship. Granted both sets of items are, in their own way, simply exquisite. . . . Even so, there is not a single item from one heritage that resembles anything, in execution or appearance, from the other.

“And then, for all their richness and fine workmanship, the items from Ur betray an unmistakable naivete in the depiction of the human figure, especially the facial features. Compare now the artistic realism with which the Scythian craftsmen depicted their race and their lavish bestiary. Where, if I may ask, is the similarity?”

Cardona’s description of Scythian art is itself naïve and unrealistic. What we usually see of Scythian art are its most beautiful objects, rarely the simple, naïve and unrealistic ones. Scythian art objects in a great many instances fall into the latter category. For a fair representation of some of these all that is needed is to peruse Georges Charrière’s *Scythian Art Crafts of the Early Eurasian Nomads* (NY 1979) or Karl Jettman’s *Art of the Steppes* (NY 1964). Cardona’s suggestion that Scythian art is not often simplistic, naïve and unrealistic is a statement based on ignorance. Again he gave us no citations to establish this fact, but presented an opinion that is simply false. Much of Scythian burial art is highly realistic and sophisticated, but much of that Scythian art is also naïve and unrealistic.

Cardona speaks of “Scythian craftsmen” as though they made all the fine artefacts found in their graves. What he has failed to inform his readers of is that many of these fine pieces of workmanship are distinctly Greek in character. This is problematic in view of the fact that his own source Trippett reports:

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56 Cardona, *op. cit.*, p. 85
“Herodotus . . . the historian indicates that the Scythians maintained close contact with the Greek cities on the shores of the Black Sea . . . And here in the graves of these long-forgotten people were pieces of Greek craftsmanship and origin.”

“Herodotus had indeed been telling the truth . . . the lavishness of the grave goods established beyond doubt that the Scythians had been men of power who must have had control of the extremely profitable trade that passed through their domain.

“As they waxed rich, the Scythians indulged a passion not just for golden objects but also for Greek works that portrayed them in various familiar activities.”

“Some of the individual Greek works [in the Scythian tombs] have achieved special celebrity among students of art and archaeology; a quick-label reference to them without description in scholarly texts is sufficient to conjure up a vivid and detailed image in the minds of the experts . . .

“[These] Greek-made pieces are . . . graphic—and . . . beautiful.”

Rice also claims that the Scythians bought Greek artefacts for their tombs:

“In their heyday the Scythians were a prosperous people, obtaining much of their wealth from trade, more especially from trade with Greece, for even in those far distant days Hellas was already unable to feed her mainland population without importing basic necessities from far afield. Scythia served as one of Greece’s granaries, and in southern Russia the corn grown by the settlers was transmitted by the nomadic overlords to the Greek colonists of the Pontus, who in their turn acted as middlemen in selling it to Greece. The Scythians in the Kuban [between the Black and Caspian Seas], on the other hand, traded directly with the masters of vessels coming to their ports from

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57 Trippett, *op.cit.*, p. 104
58 *ibid.*, p. 105
59 *ibid.*
Ionia. In addition, the Scyths as a whole supplied Pontic Greeks with valuable consignments of salt, sturgeon and tunny-fish, with honey, meat and milk, hides and furs, and not least important, with slaves. . . . In return for this merchandise the Scythians received Greek jewellery, metal-work and pottery of the finest quality.”

There is little doubt that Cardona’s own source directly told him that the finely crafted work that he described came from Greek and not necessarily Scythian craftsmen. At the very least he should have reported this and explained his reason for claiming that Scythians made these objects of art. Onyshkevych reports:

“While it is likely that the Scythians did make some of their own metallurgical objects, since their culture does include metal work predating contact with the Greeks . . . clearly a considerable percentage of their gold jewelry, vessels, weapons, and other items . . . was made in workshops in the Greek cities along the northern coast of the Black Sea. The source of the gold used in these pieces is not known; there are no natural gold sources in Scythia or in modern Ukraine.”

Trade may have enabled the Scythians to obtain gold, but this is not known. What is known is that one cannot with certainty show that the fine artistic works found in Scythian tombs were produced by the Scythians, especially those with all the realistic, elegant characteristics that Cardona discussed. These elegant works clearly look like Greek craftsmanship to anyone who views them.

Not only did the Scythians near and around the Black Sea obtain fine golden works from Greeks, but those even farther east were also able to obtain these works:

“Of all the many elements which made themselves felt in Scythian art the strongest was perhaps Ionian. It penetrated to Eurasia from several directions. In the first instance it reached the nomads from Persia where Ionian workmen were employed [in] Darius’ great palace at Susa, but it was also brought direct from Ionia by the merchants trading with the towns on the eastern shores of the Black Sea and it was

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60 Rice, *The Scythians*, op.cit., pp. 50-51
in addition disseminated throughout southern Russia by the Greek artists.”

The reason that Scythian art throughout southern Russia was elegant and realistic was that it was clearly made by Greek craftsmen. Then what of the Scythian contingent that came to high positions at Ur in southern Mesopotamia? Did they have access to Ionian art in that more remote region, cut off from their homelands, to the north, by about 1000 miles? There appears to be no evidence at all that the Assyrians were trading with the Ionian cities or with Greece. In *The First Civilizations*, Jacquetta Hawkes devotes an entire chapter to Mesopotamian trade, and for the Assyrians there is no trade with Greece mentioned. On page 57 mention is made of Queen Shub-ad in the royal cemetery having unguent jars but these did not come from Greece. In essence there was no trade, or practically no trade, between Assyrian Ur and Greece.

This author is fully aware of the difficulties and dangers of trying to prove a negative. But, as J.G. Macqueen tersely reports: “There is no evidence at all of Mycenaean goods penetrating far inland and passing through Levantine territory to be received in Assyria.”

That being the case, Scythian art at Ur would largely reflect the workmanship of the so-called Sumerian craftsmen. There would not, and could not, be a stylistic similarity between the Greek artistic works found in Scythian tombs in Russia and so-called Sumerian works executed at Ur by its craftsmen. If the Scythian craftsmen made them, that does not mean that they possessed the requisite skills to do fine works as those of the Greeks. The stylistic difference derives, most probably, from this difference in location and lack of trade and from the use of Chaldean craftsmen who probably made most if not all these artistic relics.

Yet if the two people were Scythians, though they received their funeral artefacts from different craftsmen using different techniques, they would still desire to have ornaments that exhibit the same motifs and themes. Thematically the Scythian and so-called Sumerian funeral artefacts should be quite the same. That is just what is found. In *Scythian Gold* (NY 1999), p. 99, we find that the main themes of their art are:

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“depictions of animal combat, scenes that are full of movement tension, and the confrontation of life and death . . .

“Images of Scythians at war . . . and images of Scythians at peace and performing ceremonial rites.”

The very same themes are dominant in the art found in the royal tombs. “One or other of two designs appears most often: contest scenes and heroes and animals with animals, or banquets.”\textsuperscript{64} Bahn states that the “Royal Standard of Ur is decorated with scenes of warfare and ritual celebration.”\textsuperscript{65}

The battle scenes between animals and animals, heroes and animals, heroes and heroes, banquets, and ritual scenes are major thematic materials of both the Scythians and the so-called Sumerians. That two different people separated by about 1500 years in time and about 1000 miles distant from one another should practice the same sacrificial form of burial and in the same way incorporate the very same thematic motifs in their funeral art surely supports Heinsohn’s hypothesis that the 1500 year separation between these people does not exist. Furthermore, Moscati informs us “. . . in Mesopotamia little importance is attached to the tomb.”\textsuperscript{66} But the Royal Tombs indicate that the people who buried their dead in them attached “great importance” to the tomb, contrary to the custom of the ancient Mesopotamians.

Cardona states:

“We note, next, the very appearance of the two peoples compared. On the one hand we have the Sumerians, again as portrayed on items found in the tombs in question: clean shaven up to their head in most instances and wearing their cumbersome kaunakes, or petaled skirts—and was not this the very natural costume of the Sumerians? On the other we have the Scythians, . . . clad in their modern-looking trousers and heel-less boots.”\textsuperscript{67} [emphasis added]

How do we know that the individuals buried in the royal tombs actually dressed in petaled skirts, their “cumbersome kaunakes”? One would assume that because the Royal Standard depicts the people dressed in these costumes that is just

\textsuperscript{64} Moorey, \textit{Ur of the Chaldees, op.cit.}, p. 91
\textsuperscript{65} Paul G. Bahn, \textit{The Atlas of World Archaeology, (London 2002)} p. 68
\textsuperscript{66} Moscati, \textit{op.cit.}, p. 52
\textsuperscript{67} Cardona, \textit{loc.cit.}
what they wore. But in Greek works found with the Scythians in their tombs, they are draped in clothing that for all the world looks Greek.\(^{68}\) Does that mean that since this piece of art was found in a Scythian grave, this is how those buried dressed? Of course not. The same applies to the royal tombs at Ur.

Reeder suggests that the “flexibility necessitated by their [nomadic] lifestyle may have nurtured, or simply been another manifestation of, the openness to foreign influences that is such a hallmark of Scythian art and culture.”\(^{69}\) That is, wherever the Scythians went, they were influenced by various alien cultures. They adopted the art and other aspects of these cultures, and thus could have adapted the dress of the so-called Sumerians to their own needs. But even this explanation is unnecessary because the depictions of the clothing described by Cardona, as seen in the art of the royal cemetery at Ur, have nothing to do with the actual dress of the people there.

Harriet Crawford of the Institute of Archaeology, University of London, explains:

“It is interesting to note that the archaeological evidence for dress . . . does not agree well with the evidence of the pictorial art, where men are frequently shown with bare torsos, fleecy skirts and shaven, uncovered heads.”\(^{70}\)

Here is how the people in the cemetery were actually clad as per Crawford:

“The evidence from the cemetery also allows us to make some deductions about the clothes these people wore. Three types of cloth were identified, a fine weave, a coarse weave and a cloth with long threads to one side . . . Many of the bodies in the richer graves seem to have worn clothes decorated with beads and rings of shell or metal, which may have served as fasteners. The court ladies seem to have worn long-sleeved jackets opening down the front, with bands of beads at the wrist, but no evidence was discovered for their skirts or trousers. They also seem to have worn a fine veil, or head covering beneath their magnificent head-dresses with gold ribbon and sometimes with great

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\(^{68}\) See *Scythian Gold*, pp. 12-13

\(^{69}\) Reeder, “Scythian Art”, *op.cit.*, p. 44

\(^{70}\) Crawford, *op.cit.*, p. 115
combs like a Spanish mantilla comb. Men wore a front-opening shirt or jacket, with buttons and a belt supporting a dagger or a knife. On their heads they wore something like a modern kaffir or headcloth, with a string of beads or metal ribbon to hold it in place. Cloaks, fastened at the shoulder with a long pin, were common to both sexes.”

On page 27 of *Scythian Gold* there are depicted two Scythian women, one in a “long-sleeved” dress, “opening down the front with beads at the wrist.” They are both wearing veil-like pieces of material over “their magnificent headdresses, with gold ribbon.” Both women’s dresses are decorated with metal. The dresses are not identical with those described by Crawford, but the Scythians were not inflexible respecting their costume. As Reeder suggests, “flexibility necessitated by their lifestyle may have nurtured, or simply been another manifestation of the openness to foreign influences that is such a hallmark of Scythian art and culture.”

When the Scythians came into contact with various cultures, these influenced them. They adopted and adapted the art and other aspects of these cultures and thus did the same with the dress of the people they lived among at Ur.

As for the dress of the Scythian men, Rice describes those items found still in good condition in Siberian tombs:

“Various items of clothing were unearthed . . ., many of which were in excellent condition. No trousers were found with any of the outfits with which the dead were provided for use in the next world, but tailored woolen tunics survived in good condition. Both were skillfully shaped at the waist and flared at the hips . . . The tunics were worn like shirts beneath jackets or jerkins.”

In addition, these shirts were of “fine” quality. In this respect, as with those in the cemetery at Ur, Scythians wore a front-opening shirt or jacket with a belt. Again the dress is not exactly alike but very little adaptation is required to make the clothing of the Scythians conform to that of the people at Ur. Furthermore, Rice has told us that at the well-preserved sites in Siberia, those buried did not wear

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71 *ibid.*, pp. 114-115
72 Reeder, *op.cit.*, p. 44
73 Rice, *The Scythians, op.cit.*, p. 64
75 See *Scythian Gold*, p. 28
pants. This being part of Scythian ritual, one could hardly expect to find Scythians buried at Ur to have worn pants or a skirt, as Cardona suggests.

There is a further reason for the Scythians to adapt their clothing when they came to Ur. That has to do with climate. While the southern Russian steppes have short summers where temperatures can rise above 100°F, the rest of the year it is quite cool to bitterly cold. Under such conditions leather or felt pants and heavy woolens are a necessity. This to a great extent is what they wore in their homelands.⁷⁶ Leather, felt, and heavy woolens provide good insulation on the open plains of the Russian steppe.

On the other hand, southern Mesopotamia around Ur is extremely hot most of the year:

“From May onward . . . air currents come from land areas . . . so that for several weeks or even months the sun beats down, producing extremely high temperatures. At Baghdad [north of Ur] July and August daily temperature means are about 95°F (35°C) and 88°F (31°C), respectively; mean daily maxima are about 110°F (43°C). Some drop is apparent close to water surfaces, but the effect is hardly improved because of the greater relative humidity.”⁷⁷

Even winter temperatures in the southern lowlands of Iraq around Ur can reach above 81°F (27°C).⁷⁸ Under these conditions the Scythians would have shed their leather, felt or heavy woolen pants and jackets, etc., and worn lighter summer wear which suited the environment. Adapting their summer clothing to one similar to the people at Ur was not so much a stylistic necessity but a basic requirement for the Scythians to live there. Granted nights in deserts are quite cool, but so too are they generally on the Russian steppes. While summers are short in Scythia, winters are short in southern Mesopotamia.

Crawford suggests that since the clothing actually worn by those in the tombs does not match the depictions of them, “Perhaps the explanation is to be found in the functions which the men and women are generally performing when they are represented in the art. Here they seem to be specifically undertaking ritual tasks for

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⁷⁶ Trippett, op.cit., p. 74
⁷⁷ Encyclopedia Britannica, Macropedia, vol. 9 (op.cit.), pp. 873-874
⁷⁸ ibid., p. 874
which special clothes may well have been worn.”\textsuperscript{79} Whatever the reason, the artistic representations do not reflect what those in the tombs were wearing, despite Cardona’s claim.

Instead, Scythians wore clothing quite similar in many respects to that of the so-called Sumerians. Neither in Ur nor in Scythian Siberia did those buried wear trousers. The women wore clothing open at the front, with long sleeves and beads at the ends, as well as metal artefacts sewn into their clothing. They also wore elaborate head-dresses with veils just as did the Scythians. So, too, is the men’s dress similar to that of those buried at Ur with front-opened shirts and jackets. The climate conditions also required an alternation in Scythian dress and these people, we were told, were flexible in their art and culture, suggesting their clothing would be adapted to new cultural influences. Again, there is no problem here. Only by assuming rigidly that Scythians were themselves rigid about such matters can one dispute Heinsohn’s thesis.

Cardona states:

“The Sumerians, again as portrayed . . . [are] clean shaven up to their head in most instances . . . [while] the Scythians [are] invariably bearded and with long flowing hair . . . \textsuperscript{80}

How do we know that the Scythians were “invariably bearded”? Cardona has not presented a single citation to support this claim. And again, as with so many other claims he made, this one is inherently false. In fact, his own source, Trippett, presented photographs of Scythian pottery and other items that show them without so much as the hint of a beard. These can be seen on pages 50, 68, 74, 76, and 103 of \textit{The First Horsemen}. Figure 4 is a tracing of the electrum \textit{kumis} jug from Chertomlyk found in Trippett’s book, showing three Scyths without beards, which this author has enlarged from another source.

\textsuperscript{79} Crawford, \textit{op.cit.}, p. 117
\textsuperscript{80} Cardona, \textit{loc.cit.}
On page 76 of *Scythian Gold*, a *gorytos* from Chertomlyk shows four Scythian men without beards.

In Rice’s book, p. 67, is a drawing of a silver vase from Voronezh in the northern Ukraine, also showing a beardless Scythian. See Figure 5
Having seen these beardless faces in Trippett’s book, why did Cardona suppress this evidence and offer the *ex cathedra* statement that the Scythians “invariably” wore beards? The literature also categorically contradicts him, as Rice explains:

“Another intriguing problem hinges on whether the Scythians wore beards. Some classic writers suggest that degeneracy and disease had made many of them beardless, but their representations on such comparatively late, yet first-hand documents as the Kul Oba, Chertomlyk and Voronezh vessels quite clearly indicate the contrary. The Pazirik excavations complicate the issue instead of elucidating it, for they show that whereas the majority of the tribesmen either plucked or shaved their faces, the Mongoloid chieftain who was buried in Mound 2, though by nature beardless, had been provided in his tomb with a sham beard, which was placed beneath his head-rest. It was made of real hair cut fairly short and dyed black, and was mounted onto a strip of leather, the ends of which met at the back of his head. . . . It would seem that the Pazirik people expected their chief to appear in a
beard, at any rate on ceremonial occasions, and that, since this particular ruler, possibly because of his Mongol origin, may have been unable to grow one, he had to be provided with a counterfeit which would enable him to appear correctly turned out in the next world. . . . If the beard cannot be accepted as a symbol of office in either the political or religious field, should it perhaps be regarded as a sign of rank or an emblem of cast, beards of this particular shape being worn by leaders whilst the ordinary freemen of the tribe, that is to say the warriors and hunters, had the pointed beards shown on the metal-work of the period.” 81 [emphasis added]

One of two germane points about the Scythians is that classic writers suggested that “many of them [were] beardless.” These statements cannot just be dismissed as having no validity. The other point is that the “majority of the [Scythians buried] tribesmen either plucked or shaved their faces.” The burial evidence itself shows that the majority of men plucked and shaved their beards. These facts simply contradict Cardona’s view that “Scythians invariably wore beards.”

Had Cardona paid the least attention to his source, Trippett’s own statements, he would have known that some Scythians were beardless because Trippett wrote:

“In the case of some of the [Scythian] men [that were buried], false beards were provided.” 82

Trippett told Cardona that “some of the [Scythian] men . . . [had] false beards,” which means they were beardless. Thus, not only did he fail to report the pictures he saw of clean shaven Scythians, but also the statement that “some” Scythians needed “false beards,” and hence were beardless. In fact, Trippett also directly informed Cardona that the Scythians were sometimes “PARTIALLY OR COMPLETELY SHAVED.” 83 [bold and capitalization added] Such a failure to report these facts beggars description.

However, Cardona has also told us that Scythian hair, unlike that of the so-called Sumerians pictured in art in the royal tombs at Ur, was “long flowing hair.”

81 Rice, The Scythians, op.cit., pp. 78-79
82 Trippett, op.cit., p. 114
83 ibid., p. 112
What is his evidence that when Scythians were buried with their chieftain they still wore their hair long? Again, all we have for this absolute assurance is his own authority. But this, as so much of his criticism, is contradicted by the very facts that he read in Trippett. When a Scythian chieftain died and was to be buried, Trippett writes: “Men of the dominant tribe would crop their hair.”

In this regard, Phillips tells us of the funeral procession containing a “great crowd of cropped . . . mourners . . .” Clearly the Scythians cropped their hair for the funeral of one of their chieftains.

The chieftain ruled the dominant tribe and those who were interred with him cut their hair short. This also Cardona has failed to report to his readers. Why?

But what of the so-called Sumerians, did they wear their hair short, as Cardona reports, or did they wear it long? Samuel Noah Kramer is one of Cardona’s sources in “The Two Sargons and Their Successors,” p. 18. Kramer states that they were clean shaven or wore beards (just like the Scythians) but that they had “long hair parted in the middle.” Thus, the people portrayed at Ur, following Cardona’s criticism, could not be the so-called Sumerians because if they were, they would be depicted with long hair parted in the middle. Having read this statement by Kramer, why did Cardona fail to report it or discuss its relevance when he raised the issue of short hair? This repeated failure on his part to deal with evidence he himself read or saw is totally disingenuous and dishonest. Cardona is dead wrong on every point he presented in this regard.

Again, if Cardona had only done his homework he would have found that the gorytos from Chertomlyk in Scythian Gold, page 76, not only shows beardless men but shows some of them with short hair.

Furthermore, Cardona stated that the Sumerians were “clean shaven up to their heads in most instances,” meaning that they shaved their heads. The Sumerians, so far as we know, had long hair, and did not practice this rite. Yet the Scythians did, as S.I. Rudenko points out that in Siberia

“the men’s and women’s heads in barrows 2 and 5 . . . are relatively well preserved, and more or less shaven . . . it is still not clear

84 ibid., p. 24
86 Kramer, The Sumerians . . ., op.cit., p. 99
whether the shaving of the head, albeit partially, was a normal part of the burial custom.”

This evidence certainly undercuts Cardona’s claim on this point. But worse still, his own source, Trippett, also informed Cardona that the Scythians were shaved “up to their heads.” Trippett writes “The heads [of the Scythians] were partially or completely shaved.” Yet again Cardona failed to inform his readers of this evidence or even discuss it! How does one explain such a blatant disregard for evidence?

Therefore, when the Scythians were leaders at Ur, the majority, as in the royal tombs in Pazirik, would have been beardless. As Cardona has stated “in most instances,” the people “portrayed [are] clean shaven.” And they would not have, as Cardona stated, “long flowing hair,” since their tradition required that those closest to the dead leader, and buried with him, cropped their hair short. The Scythians, contrary to Cardona’s pronouncements, were not “invariably” bearded when buried with their rulers. The majority would be beardless and have short hair. This, once again, is not evidence that discredits Heinsohn’s thesis, rather the facts are as clear as the beardless faces of the Scythians, which again supports and corroborates that thesis.

Cardona states:

“And, . . . then there is the matter of embalming. The bodies of Scythian kings were embalmed and ‘stuffed with a preparation of cypress, frankincense, parsley seed and anise seed.’ No evidence of such a concoction was discovered in any of the skeletal remains unearthed in the Royal Tombs at Ur.”

The evidence from the field of geology and climatology clearly explains this seeming disparity. The reason for finding preserved bodies in Scythian tombs in Siberia as opposed to the skeletons in the royal tombs at Ur is based on facts of science. It was in the Siberian tombs that the bodies of the Scythians were discovered to be intact or largely intact because in that region circumstances allowed the ground in which they were buried to freeze. Trippett explains, as do others, that when grave robbers entered the Scythian tombs in Siberia, they created air shafts:

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88 Trippett, op.cit., p. 112
89 Cardona, loc.cit.
“The temperature inside the rock-covered log tombs had dropped below freezing, and moisture entering through the shafts dug by the grave robbers had turned to ice. Thus the tombs had become frozen storehouses, in which the bodies of the chieftains, and their women, horses and possessions of perishable fur, fabric, leather and wood were preserved for all time.”

Temperature plays a large role in how long flesh in the earth takes to biodegrade. The frozen bodies of mammoths in the Siberian tundra speak eloquently of this fact. If, as at Ur, the temperature of the soil is high and there is little or no moisture in it, that will remove nearly all the moisture from bodies and they will be preserved. This process occurred in the Tarim Basin in Northern China, as described in Chapter 4. In regions such as the Ukraine, with soil temperatures between those of Siberia and Ur, one would expect lesser rates of disintegration of biodegradable materials.

However, there is another soil phenomenon that determines the length of preservation of organic matter. Highly acidic soils will, like any strong acid, decompose most organic matter rather rapidly and fairly completely. This is exactly the situation that exists with the soil in the area of Ur. For example, Woolley discovered what were the filmy remains of a wooden statue of a goat at Ur lying in the earth.

“It was in a sorry state. The wooden head had decayed to nothing, the animal was a mere silhouette, the shell and lapis-lazuli inlay lay in the earth around the decayed wood. All he could do was pour paraffin wax into the remains, lift the statue intact, and then restore it in the laboratory.”

Moorey reports on the manner in which wood and matting are decomposed in the acidic soil at Ur:

“It is a strange thing that in soil wherein so much that might be thought enduring rots away completely. . . . Material such as wood or matting, though it lose all its substance, yet retains its appearance and its texture and with care can be exposed in such conditions that a photograph of it looks like the real thing whereas it is but a film which

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90 Trippett, op.cit., p. 104
91 Fagan, op.cit., p. 26
a touch of the finger or even a breath obliterates more easily than it
dislodges the plumage of the wing of a butterfly.”

Woolley writes:

“Of the sixty-eight women in the [Royal Cemetery] pit, twenty-eight wore hair ribbons of gold. At first sight it looks as if the others had nothing of the kind, but closer examination showed that many, if not all, had originally worn exactly similar ribbons of silver. Unfortunately silver is a metal which ill resists the action of the acids in the soil, and where it was but a thin strip and, being worn on the head, was directly affected by the corruption of the flesh, it generally disappears altogether.

“Another thing that perishes utterly in the earth is cloth, but occasionally on lifting a stone bowl which had lain inverted over a bit of stuff that has protected it from the soil one sees traces which, although only of fine dust, keep the texture of the material, or a copper vessel may by its corrosion preserve some fragment which was in contact with it. By such evidence we were able to prove that the women in the death-pit wore garments of bright red woolen stuff . . .”

Even in Kiev and Poltava, where the Scythian barrows are smaller, about seven feet on average, “ornaments of wood were probably used there, but very few have withstood the rigours of the climate.” The situation pertains in the Ukraine. Renate Rolle specifically informs us that “not one embalmed Scythian body has survived the ravages of the centuries.” While frozen bodies and vegetable embalming items survived in Siberia, none of these survived in Scythia.

There is absolutely nothing to support Cardona’s contention that the vegetative materials used to embalm the Scythians, as found in Siberia, should have survived in the soil at Ur. There is direct geological and chemical evidence that explains why such vegetative matter had to be destroyed by the acids in the soil there.

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92 Moorey, Ur of the Chaldees, op.cit., p. 55
94 Rice, The Scythians, op.cit., p. 106
95 Rolle, op.cit., p. 28
In the Ukraine there would be less destruction of organic materials than at Ur, but more destruction than in Siberia. But Trippett, on page 23, exhibits a photograph of a female skeleton from a tomb in the Ukraine, whose grave apparently had not been disturbed by thieves. What that photograph shows are her bones, gold artefacts, but no clothing, no tissues, and nothing of the embalming vegetative matter which had once filled her body cavity. In the Ukraine the climate and soil conditions were not as good at preserving these organic materials as they were in Siberia. However, the fact that this photograph showed Cardona that the lady’s embalming matter had been biodegraded away was never mentioned at all. Why did he not discuss or explain this contradiction to everything he was claiming about the preservation of embalming vegetation? Since that vegetation was apparently missing it deserved an explanation. What is one to do when evidence here and elsewhere is suppressed in such a cavalier manner?

Cardona states:

“[There are] illustrations of chariots depicted on objects, such as the Royal Standard, retrieved from the tombs at Ur. No further references are necessary since these objects are too well known. As already stated, the Scythians did not use chariots so that this, in itself, makes for one more dissimilarity between the two people. . . .

“True, the Assyrians, whom Heinsohn claims to have held power in Mesopotamia at the time, did use chariots. The Assyrian vehicles, depictions of which are also plentiful, were streamlined engines of destruction, with a pair of spoked wheels, drawn by swift horses. Compare the Sumerian chariots to them: bulky cart-like conveyances, with four solid wheels . . .”

It is not true, as we will see, that the Scythians did not have chariots in their homelands around and in the Ukraine; they also had men ride wagons chariot-style:

“Both in Scythia and at Pazirik the carts were fitted with a central pole some ten feet long with special bow-shaped attachments or cross-pieces. . . . The horses were yoked to these cross-pieces in pairs by means of straps. Four horses was the usual number used for pulling the

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96 Cardona, *op.cit.*, p. 84
carts, but in southern Russia, six often figured at important burials, and at times the number even rose to eight. The driver controlled them from a wooden platform erected at the front of the cart.”

Therefore, the Scythians were quite familiar with riding wagons in a chariot-like fashion. When they came in contact with the Assyrians, this concept that such a wagon could be used as a weapon of war probably had a profound effect on them and they may have imitated their Assyrian overlords when they lived at Ur.

The comparison Cardona made between the wheels of the Scythian chariots and those of the Assyrians is really not at issue. What is at issue is whether or not the chariot wheels seen on the Royal Standard are similar to, or totally different from, the wheels known to have been used on Scythian wagons? The drawing of these wheels in Trippett’s book came with a caveat. The drawings are based on “recent archaeological evidence [which] expands the basis for the paintings on . . . [these] pages.” See Figure 6

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Figure 6 Scythian wagons

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97 Rice, The Scythians, op. cit., p. 104
98 Trippett, op. cit., p. 117
These drawings are the best interpretations of what Scythian wagons were like though they are not definitive. The Scythians, having seen the chariots of the Assyrians, would have imitated them. To do so, given their level of technology, they would have converted their smallest wagons into chariots. Were one to remove the hay from the four-wheeled wagon in Figure 6, as well as the upper structure and replace it with a chariot structure, one would have a chariot like those seen on the Royal Standard. See Figure 7

The difference in the wheels of the Scythians and those seen on the Royal Standard may be due to using innovations learned in Mesopotamia. But the fact that the Scythian wheels and those represented on the Royal Standard are both bulky, wooden affairs, is not a contradiction to Heinsohn’s thesis. Similarities between Scythian and so-called Sumerian wheels are not problems for Heinsohn.

It must be understood that these heavy, bulky wagons and chariots were of little use in ancient so-called Sumeria. They could only be utilized for very short times over short distances. Discussing King Enantum of Lagash of this region,
Hawkes writes of the “Vulture Stele” as well as the “Standard” of Ur that these chariots could be used in battle. “The chariots are represented as they begin to advance at speed, the leading animals already at the gallop . . . an indication of the experienced skill and control that would be necessary to manoeuvre these clumsy, solid-wheeled vehicles with their teams of four onagers.” For a short battle this type of vehicle could be utilized. But for transportation the ox-wagons make no sense as Woolley explains that these heavy carts

“. . . had to be drawn by two oxen, . . . Obviously the ox-carts could have been employed for draught also, slow as they were, for short hauls but for short hauls only; for [use on] a long trek across country they would have been useless. For the . . . decisive objection to wheeled traffic was the absence of roads. In the Euphrates valley itself the heavy carts could move about, making use of the canal banks as highways; but even there could with difficulty risk going on [and sinking deeply into] the softer soil of the watered fields and for a long journey through wooded or mountainous country [because of their weight and bulkiness] they were manifestly impossible.”

Woolley claims “that two such wagons [drawn by oxen] were found in a king’s grave [in the Royal Cemetery at Ur; then add] . . . it would seem that they were used for ceremonial purposes not for carrying merchandise.” As we will discuss below, the Scythians tended to sacrifice old, lame or worthless horses; it seems evident that they used these ox-carts in lieu of horses to bury with their princes. The ox-carts used by the Scythians with heavy, bulky, solid wheels, as pictured above, are the same type of ox-cart in the Royal Cemetery. Further, if ox-carts were of little real use in so-called Sumerian times as transport, why would they even have made them? And if they were of use and were necessary for transport, why destroy them? There is, as we have pointed out, nothing to suggest in their literature, religion or burial practices elsewhere that these so-called Sumerians even practiced such a funeral rite. The Scythians possessed such heavy, solid-wheeled vehicles which they did use and therefore there is nothing terribly anomalous with their burying them with their king. The people of that region also had such solid-wheeled chariots but more importantly, the Scythians used ox-carts with these heavy, solid wheels.

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99 Hawkes, *op.cit.*, p. 166
100 Hawkes and Woolley, *op.cit.*, pp. 615-616
101 *ibid.*, p. 165
By not evaluating this clear similarity Cardona has merely begged the question. The fact that the Scythians did not drive chariots until they came into contact with the Assyrians makes perfectly good sense. They probably only learned of these war wagons after becoming involved with Assyrians and then may have imitated the model.

There is evidence that the people of the Russian steppes did use spoke-wheeled chariots. The dating may be incorrect and may indicate that these were employed after these people came into contact with the chariot builders of Mesopotamia as reported in 1994:

“In ancient graves on the steppes of Russia and Kazakhstan, archeologists have uncovered skulls and bones of sacrificed horses and, perhaps most significantly, traces of spoked wheels. These appear to be the wheels of chariots. . . .

“The discovery sheds new light on the contributions to world history by the vigorous pastoral people who lived in the broad northern grasslands, dismissed as barbarians by their southern neighbours.”

It is assumed that these graves date from about 2000 B.C. However, these may actually date to the time of the Scythians and therefore confirm the concept presented by this author that after the Scythians had contact with the chariot-riding people of Mesopotamia they built spoke-wheeled chariots. The article suggests that these chariots were first invented in southern Russia; if that was the case, then the Scythians would have known of this invention and employed spoke-wheeled chariots from their earliest beginnings. That they did not, suggests that the dating of this innovation is incorrect. It would seem to make far more sense if these people imitated the Assyrians after contact. The dating offered by the archaeologists creates clear problems for the evolution of spoke-wheeled chariots. As pointed out, “If the ages of the burial sites are correct,” said Dr David Anthony, who directed the dating research. . . ." But no one has explained fully why after inventing the chariot the people of southern Russia to a large extent abandoned its use.

Nevertheless, down-dating the so-called Sumerian civilization to the end of the second or early first millennium B.C. allows the use of wheeled carts. They would be commonplace since wheels have been known earlier.

102 The Science Times Book of Archaeology, Nicholas Wade, ed. (NY 1999), p. 40
103 ibid.
Correcting the dating of ancient history may well resolve this question and the many others discussed above.

Cardona states:

“The animals drawing the Sumerian chariots are shown to be onagers, or asses, lowly beasts that no Scythian warrior would have been caught associating with. In fact, let’s face it, the horse was totally unknown in Mesopotamia at the time of the burials in question. . . . Not a single character on the funerary items at Ur is shown riding Scythian fashion astride a horse, or even an ass.”

How do we know that the Scythian warriors would not be caught associating with asses, or donkeys? Cardona, once again, has only given his opinion on this matter as though it were a fact. He has presented nothing in the way of support for this assertion, and has merely invented a Scythian prejudice unknown as a fact to make this point. How big was the average Scythian horse compared to, say, an ass? The fact of the matter is that they were about the same size. Juliet Clutton-Brock, one of the world’s leading authorities on horses, claims that the equides seen at Ur “would have been quite as large as early domesticated horses.”

The Britannica claims that the “finest Scythian riding horses were of the Fergana [or Ferghana] breed . . . the majority were Mongolian ponies.” Ponies by definition are small horses.

We are specifically told that

“Horses, domesticated somewhere in southwestern Russia . . . arrived in the Near East . . . after true civilization had risen there. They were about the same size as an onager or wild ass. . . .”

Be that as it may, riding donkeys was never considered beneath anyone’s dignity in the ancient Near East. According to Stuart Piggott,

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104 Cardona, loc.cit.
106 Encyclopedia Britannica, Macropedia, vol. 16 (op.cit.), p. 439
107 Jonathan Norton Leonard et al., The First Farmers (NY 1973), pp. 89-91
“The beginnings of regal horse-riding were tentative. In the ancient Near Eastern tradition the king, if he did not appear in a chariot, might on occasion ride on a mule or a donkey [a domesticated ass]: in Archaic Egypt from 3000 BC ritual burials of donkeys accompanied tombs at Tarkhan and Helwan, and singly or in pairs at Tell el Dab’a in the late seventeenth century BC and at a similar date in the Hittite Osmanskayasi cemetery. In the early second millennium a well-known letter to Zimri-Lin, King of Mari, gives him advice on this matter—‘Let my lord not ride horses. Let him mount only chariots or mules and honour his kingly head.’ A thousand years later, Absalom, son of the Israelite King David . . . is in command of an infantry unit with a chariot corps, but himself rides a mule, while traditional Jewish law put a prohibition on horse-breeding by the king: ‘he shall not multiply horses to himself, nor cause the people to return to Egypt to the end that he should multiply horses’ (II Sam. 18:9; Deut. 18:6). In the Messianic aspirations of Zechariah ‘thy king cometh unto thee . . . lowly and riding upon an ass and upon a colt the foal of an ass’ (Zech. 9:9) and this was taken up as fulfilled prophecy and used verbatim of Christ’s entry into Jerusalem in John 12:4, the original assertion of kingship in ancient Semitic tradition, but it would only be seen as demeaning by the provincial Greco-Roman world of the first century AD, where the horse was now firmly established as the steed of majesty and authority, the donkey [or ass] as the lowly beast of burden.”

This in no way is meant to suggest that horses were despised at that time, although there is a statement to this effect, quoted above. Piggott, quite significantly, points out that mules were so highly valued throughout the Near East that they were buried in tombs or near them. If the Scythians disdained the “lowly ass,” as Cardona claims, then they did so several centuries prior to the time when this prejudice came into vogue.

Nevertheless, the question at hand is: were the equides seen on the Royal Standard at Ur onagers (wild Asiatic asses), ponies, mules (crosses between horses and asses) or donkeys (domesticated African asses)? Cardona forthrightly claims to know that the equides displayed on the Royal Standard (Figure 7) are “onagers.” Again, as with so many of his statements, he has given no citation, no

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109 *ibid.*, p. 40
evidence, only his bald assertion that this is a fact. Breasted was himself not ready
to give his full endorsement to the Royal Standard as depicting onagers:

   “An Anglo-American expedition at Ur had . . . found . . . a . . .
   plaque [on] which [are seen] the chariot and the bodies of animals
   (probably wild asses, possibly horses).”¹¹⁰

   Woolley in his final report of 1934 called the equides depicted at the
   royal tombs “donkeys,” “wild asses” and “onagers.”¹¹¹ An identification of the
   equides as mules would require that horses were available to mate with asses, there
   at Ur. Juris Zarins in his monumental work The Domestication of Equidae in Third
   Millennium B.C. Mesopotamia, lists the identifications of these equides, as depicted
   at Ur, by the various zoologists and other experts:

   “Van Buren said that the short ears and flowing tails indicated
   the depiction of a horse. Werth . . . stated that the depiction of short
   ears recalled the mule, but that he preferred the onager identification.
   Salonen thought that the short ears, erect mane and diagnostic tail and
   body indicated a hinny [a cross between a male horse and female ass],
   Hancar felt it depicted a hemione [half ass], as did Lundholm, Noble,
   Brentjes, and Frankfort. Kinal favored the identification of horse.
   Potratz trod the middle road by saying that pure asses were not depicted
   but that the mosaic [of the equides conformation] could be a breed of
   horse unknown to us. It goes without saying that such a diverse range
   of opinions means that the depictions on the [royal] standard . . . are
   inexact.”¹¹²

   Such a divergent set of opinions by experts as to the identification of
   these depictions of equides at Ur makes it quite clear that Cardona’s absolute
   assurance that these “are shown to be onagers” cannot be sustained.

   There is a further reason for holding that these equides, shown pulling
   chariots, were not onagers, based on biology. Jared Diamond, in his recent
   bestseller, Gun Germs and Steel gives the reason. These Asian asses called onagers

¹¹⁰ James Henry Breasted, Ancient Times—A History of the Early World, 2nd ed. (Boston 1944),
p. 150
¹¹¹ Sir Leonard Woolley, The Royal Cemetery (London 1934), pp. 267, 268, and 272
   (Chicago 1976), p. 273
have never been domesticated except in the minds of historians and those that say they were. They are inherently incapable of being controlled to pull wagons or to be ridden:

“Closely related to the North African ass [which is domesticated], is the Asiatic ass, also known as the onager. Since its homeland includes the Fertile Crescent, the cradle of Western civilization and animal domestication, ancient peoples must have experimented extensively with onagers. We know from Sumerian and later depictions that onagers were regularly hunted as well as captured and hybridized with donkeys and horses. Some ancient depictions of horselike animals used for riding or for pulling carts may refer to onagers. However, all writers about them, from Romans to modern zookeepers, decry their irascible temper and their nasty habit of biting people. As a result, although similar in other respects to ancestral donkeys, onagers have never been domesticated.”

The fact that “onagers have never been domesticated” indicates quite undeniably that the equides depicted on the Royal Standard and elsewhere at Ur pulling carts or chariots are simply not onagers, but may be hybrids of horses or other equides. What historians and others who have made this identification claim, without a shred of biological proof, based solely on their belief system, is that somehow the so-called Sumerians domesticated onagers, but then dispensed with them when the horse was introduced. But all of this is assumption piled on assumption. As Zarins explains, the idea that onagers are

“... domesticated comes from either cultural-historical data [such as the Royal Standard] or merely from unsupported statements. For example, Noble states that the hemiones ‘appear’ as draft animals from ‘Sumerian levels.’ Reed comments the Equus hemionus hemippus [a particular type of Asian ass] was ‘probably’ domesticated in Mesopotamia by 3000 B.C. Hatt states that Equus hemionus hemippus was ‘broken to harness’ by the Sumerians prior to the advent of the horse. Flannery provides a similar statement that the hemione was ‘temporarily domesticated’ by the ancient Sumerians but later abandoned in favor of the horse. It can only be said that none of the references mentioned in this paragraph cite any evidence for their statements. If the horse took over the role of the domesticated hemione,
why then is the hemione consistently identified in faunal collections from Mesopotamia even after 2000 B.C., when the horse is said to have been introduced?"\textsuperscript{114}

The general literature is replete with statements by historians that these equides were onagers. Hawkes succinctly writes: “Sumerians succeeded in harnessing that intractable beast the onager. Then at the beginning of the second millennium the onager began to be replaced.”\textsuperscript{115} The Britannica authoritatively reports, “By about 3000 BC the yoke was employed in Mesopotamia with onagers (Asian asses) to draw light wagons and chariots; and by about 2000 BC it was applied to horses, then newly arrived from Central Asia.”\textsuperscript{116}

This is the form of argument that seems to dominate every aspect of history, science, etc. Highly regarded members of a profession state something as a fact repeatedly, without a shred of real evidence to sustain that statement. It is an innocent but destructive propagandist form that permeates all fields of study and one of the most perniciously difficult problems to overcome.\textsuperscript{117}

A striking example of the persistence of conventional wisdom regarding the horse was pointed out by Able; he cites Meyer Shapiro who examined the artist Géricault’s painting, *Horse Race*, ca. 1820:

“The galloping horses are shown with all four feet off the ground and pointed in opposite directions [*le galop volant*]. No horse can do this naturally, the Lippizaner horses of Vienna perform it after years of intensive training. But Géricault was unconsciously copying certain English hunting prints, which had copied the engravings of Charles Cochin (c. 1750), who had been influenced by the introduction from China into France of porcelain and Chinoiserie. In China, the *galop volant* was found as far back as the Han dynasty (206 B.C.-A.D. 220) . . . And in all these centuries no horse did it.”\textsuperscript{118}

Fischer calls this error by historians

\textsuperscript{114} Zarins, *op.cit.*, p. 98
\textsuperscript{115} Hawkes, *op.cit.*, p. 102
\textsuperscript{116} Encyclopedia Britannica, *Macropedia*, vol. 8 (Chicago 1982), p. 657
\textsuperscript{117} See, for example, this author’s discussion of Columbus and the Flat Earth in *The Velikovskian* vol. V, no. I, “Propaganda and Scientific History,” pp. 81-109
\textsuperscript{118} Able, *op.cit.*, p. 37
“The fallacy of argument ad nauseam [which] might serve, tongue in cheek as a tag-name for a form of serious error, in which a thesis is sustained by repetition rather than by reasoned proof [or scientific proof]. This strategy was a favorite of Lewis Carroll’s immortal Bellman, in *The Hunting of the Snark*.

“‘Just the place for a Snark!’ the Bellman cried
As he landed his crew with care;
Supporting each man on top of the tide
By a finger entwined in his hair.

“Just the place for a Snark! I have said it twice:
That alone should encourage the crew.
Just the place for a Snark! I have said it thrice
What I tell you three times is True.’

“The absurdity of this ludicrous device appears merely in its description. But it should not be lightly dismissed. Its popularity among advertising executives, public relations specialists, and professional propagandists is sustained by the solid fact that it works. Recent research in psychology has suggested something of the subliminal mechanisms by which repetition erodes critical resistance to the most absurd assertions.”

In the same vein with which historians cut diorite with words, and produce tin bronze with words from tin sources that they cannot find, etc., only words have now domesticated the onager. Zarins, citing Herre, adds

“that no zoological collection [of bones in Mesopotamia] leads one to the conclusion that the hemiones were domesticated. ‘According to all zoological data, the domestication of the hemione must be considered unlikely’.”

He goes on to cite those who attempted to domesticate them. “Miller says he examined a tamed kulan [type of Asian ass] at Tian Shan . . . it could not be

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119 Fischer, *op.cit.*, p. 302
120 Zarins, *op.cit.*, p. 99
broken to the saddle.”

“Bannikov also comments on the attempts to domesticate the young, who acclimatized themselves well to captivity when fed diluted cow’s milk and hay but remained intractable to the harness.”

Juliet Clutton-Brock has also come to the conclusion that the onager was not depicted on the Royal Standard, based on an examination of textual evidence about equides from supposed third millennium cuneiform tablets:

“It seems clear from the texts that the domestic donkey was the common agricultural draught animal, while small numbers of onagers were kept for cross-breeding with donkeys. The hybrid offspring were large powerful animals which were yoked in teams and were used to draw chariots.”

Elsewhere Clutton-Brock succinctly explains the problem of assuming domestication of the onager:

“For the last thirty years it has been assumed by archaeologists that [the onager] was this equid that was domesticated by the ancient civilizations of the Fertile Crescent. Some authors have gone so far as to suggest that domestication of the horse only followed after the peoples of the north had gained experience of using onagers for riding and as draught animals.

“There is one rather large flaw in this supposition. If the onager had been domesticated, why is it not the common domestic equid of Asia today?”

There is not a single shred of real evidence to support Cardona’s contention that the equides depicted at Ur pulling chariots are onagers. Even the writers of the Bible understood that the onager could not be domesticated. In Genesis 16:12, Ishmail, the founder of the Arab people, is referred to: “He shall be a wild ass of a man, his hand against every man and every man’s hand against him.”

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121 ibid., pp. 21-22
122 ibid., p. 23
123 Clutton-Brock, op.cit., p. 89
124 Juliet Clutton-Brock, Domesticated Animals from Early Times (Austin TX 1981), p. 98
But worse than what Clutton-Brock says is the lack of logic to her suggestion that onagers were crossed with donkeys. Where, one must ask, did these donkeys come from, since the only donkey, in the chronology employed by Clutton-Brock, was the African domesticated ass? There were no African donkeys in Mesopotamia in the third millennium B.C. so far as this author has read. So if there were donkeys available to the so-called Sumerian civilization, they were introduced much closer to the present.

Let us now turn to Cardona’s statement, “let’s face it, the horse was totally unknown in Mesopotamia at the time of the burials in question.” Again, what is this based on other than Cardona’s word? But he can turn to innumerable statements in the literature which will also back up his claim. Nearly every book which this author has read by reputable authorities has made the very same claim, ad nauseam, but each and every one of them, so far as this author has found, has ignored a thoroughly contradictory piece of evidence that denies these assertions.

Based on the conventional chronology that nearly all historians and archaeologists accept, “Tell Barsib [in Mesopotamia] has been dated . . . to 2200 B.C. . . .”125 This is around the same period as the Sumerian civilization when horses are forbidden existence in Mesopotamia. But at the top of page 86 in Dayton’s book, Figure 25, one can clearly see that a rein ring depicts horses. See Figure 8, found at Tell Barsib.

Figure 8 Horses

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125 Dayton, op. cit., p. 330
The manes, thick tails and conformation of these equides are those of horses, contrary to what Cardona claimed.

Furthermore, John Noble Wilford reported in *The New York Times* in January 1993:

“A craftsman 4300 years ago molded the clay figurine of a horse, only five inches long and three inches high, but exquisite in detail . . . uncovered in northern Syria by archaeologists who call it the oldest known sculpture of a domesticated horse and one of the finest ancient representations of a horse ever discovered. . . .

Juris Zarins, an anthropology professor at Southwest Missouri State University in Springfield and an expert on the role of horses in early Mideast civilizations, said the figurine, along with recent discoveries in the Ebla texts, showed that the domesticated horse was well established in Mesopotamia in the last half of the third millennium B.C. . . .”

The reason that no horses were found or depicted at the Royal Cemetery at Ur will be explained below.

Cardona states:

“Scythian men ate, lived and all but slept on horseback. The horse was their most prized possession and the herds they raised more than anything else denoted the wealth of their tribes. It is therefore not surprising that horses proliferated among the items buried with Scythian kings and warriors. The remains of these horses have been found neatly arranged in circles, or laid out in orderly rows, around the central burial chamber.

“. . . one year following the burial of a king, another sacrificial ritual was held at the tomb. At that time, as many as fifty of the late king’s attendants were strangled and fifty horses butchered. Stakes were run lengthwise through he [sic] bodies of the horses which were then supported on posts in upright positions with their legs dangling in

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126 *The Science Times Book of Archaeology, op.cit.*, pp. 69-70
the air. . . . These being the most tell-tale signs of Scythian burials, where are the horse remains in the tombs of Ur?”

The inference one might draw from Cardona’s statement is that large numbers of horses were sacrificed at all Scythian burials. The Scythians did practice horse sacrifice and interred horses with the person buried, but the number of animals sacrificed appears to have varied greatly, most probably in proportion to the number of horses available and to other factors. As Rice states,

“All the Pazirik tombs contained horse burials, the number varying from seven in Mound 3 to fourteen in some of the others. The animals were those which had belonged to the dead lord in the course of his life, and they included his outworn mounts, and those he was using at the time of his death, as well as some two- and three-year-olds which he had probably selected for his future use. Although professor Vitt has established that the bulk of the ponies used at Pazirik were wild Mongolian stock of Przewalski descent, and thus similar to those used throughout Scythia, each Pazirik burial contained as well at least one thoroughbred of real quality, a horse of the much prized Ferghana breed.”

That is, the “bulk of the ponies” buried in Siberia as well as “throughout Scythia” were wild Przewalski equides. Like onagers, the Przewalski horse is a “wild horse that has never been domesticated.” The Scythians sacrificed many horses of little value at their tombs apparently because this saved many others that were valuable and useful. Where great herds of horses were available, sacrifices may “often run into hundreds. In the Poltava and Kiev districts on the other hand, it is rare to find more than one horse to a burial.” But they also tended to sacrifice the old, infirm and faulty horse as well:

“Recent discoveries show that the orthopedically faulty animals were sometimes killed off in Hungary, and a proportion of those buried in Altaian graves suffered from similar defects.”

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127 Cardona, op. cit., p. 84
128 Rice, The Scythians, op. cit., pp. 70-71
129 Olive W. Burt, The Horse in America (NY 1975), p. 17
130 Rice, op. cit., p. 71
131 ibid., p. 92
The Scythians sacrificed horses that were wild, undomesticated, infirm, lame, etc. It seems that if the number of their stock was small they did not kill off their good horses to the very last animal. Like all people they conserved their wealth and sacrificed horses only to the degree that these would not endanger the numbers which provided them with their livelihood. In fact, some Scythians did not sacrifice horses at all. “Thus virtually every Scythian burial is a horse burial. Although Professor Artamonov found that this did not apply to the Scythians who were buried in shaft graves in the Crimea, during the 4th century B.C.”

Nevertheless, if the Scythians reigned at Ur under Assyrian rule, they would have kept horses if they could maintain them in this desert region during the first millennium B.C. But Ur and the region around it is a desert where very little grows except in the river valley along the Euphrates. And that is the problem.

To maintain thousands of horses at Ur requires large areas of pasture near the city and sufficient grass for hay, or other vegetation, for fodder over the eight months of almost rainless summer, whereas in southern Russia this forage is needed for winter and grows sufficiently to supply large herds. The same does not exist around Ur. Robert McC. Adams discusses this problem for southern Mesopotamia:

“The maintenance of large herds, principally of sheep and goats, cannot be understood without reference to pasturage available for much of the year in immense, semi-arid and even desert tracts that were not used by cultivators. Meat and dairy products from these herds may not have been an absolutely vital source of proteins and other nutrients in most circumstances, in view of the fairly general availability of fish. But, without the wool for textiles to be traded for natural resources that were wholly lacking in the alluvium, it is difficult to believe that Mesopotamian civilization could have arisen. . . . And at least equally important was the aspect of herds as a food resource that could be held in reserve, not subject to the same set of natural hazards as the crops, and capable of being moved from one location to another.”

Since “rain is virtually absent from late May through early October, . . .
conditions for plant-life are ‘most rigorous’.”\textsuperscript{134} E. Guest explains that

“. . . little growth can be made until the short spring season—just as the rains are beginning to cease. During the long summer months there is no surface water available, while the intense heat and dryness of the air create conditions of extreme desiccation. Thus the only plants able to survive in the lower plains of Iraq [where Ur is located] are the ephemeral annuals (which can rapidly complete their life cycle in the spring and then lie dormant in the form of seeds for the remainder of the year) or such deep-rooted and highly xerophytic perennials as are sufficiently protected to withstand the rigors of summer while drawing on underground sources of water. This is reflected in the paucity of plant species over the greater part of lower Iraq, since only a limited number of species come within the above two categories.”\textsuperscript{135}

Thus, there is a major problem related to maintaining large herds of horses in and around Ur at the time the Scythians lived there. There is very little in the way of forage for horses which must consume sufficient vegetation year-round to live. In southern Iraq this must lead to overgrazing of what little grows around Ur.

“Given the omnipresent fact of climatic variability, it follows that extensive overgrazing must occur almost everywhere with great frequency, even if the flocks and the supply of natural vegetation remain aggregatively in rough balance across the region as a whole. To this destructive force must be added the more selective activities of the fuel-gatherers.”\textsuperscript{136}

Cardona’s own source reports, in the British edition:

“The country has summer temperatures ranging from 110 to 130 degrees in the shade, and eight months in the year without rainfall. By the end of the dry season, the rivers are reduced to sluggish brown meanders in a waste of dried mud. Then comes the winter, . . . bringing intermittent rainstorms. But the rivers do not receive their full volume

\textsuperscript{134} \textit{ibid.}
\textsuperscript{135} \textit{ibid.}
\textsuperscript{136} \textit{ibid.}, p. 14
of water until spring . . . Then come the spring floods which, a generation ago, were considered practically uncontrollable and all through history represented an ominous threat to the inhabitants of the lower plain. Paradoxically also, this happens between April and June, which from an agricultural point of view is too late to water the main crop, usually harvested in April.

“This was the climatic regime and seasonal fluctuation with which the ancient farmers of southern Mesopotamia were faced: rain in adequate quantities at the wrong time; river water, also at the wrong time, and concentrated around the river-bed in almost unmanageable quantities. [Especially on the Tigris River] . . . the canals themselves very rapidly filled up their beds with silts, and consequently required repeated dredging.”

The nomadic herders, therefore, have had to travel over great distances almost constantly to find sufficient vegetation throughout the year to maintain their flock. But then add to the sheep, goats, donkeys, and cattle, thousands of horses, and the conditions for the life of all these animals become quite precarious. Even on the Russian steppe, horses need winter (rather than summer) forage to survive, as Trippett explains:

“In the wild horses had to rely on forage alone for sustenance; even after being domesticated they were at first left to fend for themselves, and often starved during the long winter months on the [Russian] steppes. And those that survived were too skinny and weak to be of much use in spring. So man learned to supply some of the animals with grain . . .” [to maintain them].

Moving great herds of horses into the desert and semi-arid desert surrounding Ur might allow the horses to survive, though the evidence is against this possibility, as we will soon discover, but the need for water for the horses becomes even more problematic. The nomads would not readily inform their foreign rulers of their water holes if that threatened the survival of their flocks.

On the other hand, if one kept the horses in and around Ur itself, that would require that considerable land irrigated to feed the city’s population must

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138 Trippett, *op.cit.*, p. 49
instead be used to grow fodder for these herds. This, too, would be an unreasonable expectation. A great part of the population would have to starve or migrate to allow for this. But even this does not solve the problem. Sedentary farming at Ur on the Euphrates is dependent on labor-intensive irrigation. If one removes a considerable part of that population, the labor force necessary to maintain the irrigation system becomes insufficient. There is less land that can be cultivated.

This, to a large measure, is what happened to southern Mesopotamia around the time of the Assyrian conquest of the region, when the Scythians came there.

“The constant resistance of the tribally organized part of the [Chaldean] population to Assyrian pressure is striking. As a result of it, according to Assyrian claims, a total of more than 450,000 persons were forced into exile over little more than a forty-year period in the latter part of the eighth century [B.C.]. Even allowing for some duplication, this surely testifies to the massiveness of the assault that was directed primarily against the Chaldean and Aramean country. . . .

“In its general outlines, the picture . . . is strikingly similar to conditions obtaining in southern Iraq during the last centuries of Ottoman rule. . . . Hence it is the generic features of political instability—the decline of irrigation agriculture, urban-rural polarization . . . [etc.].”

Once one removes a large element of the farming population and the irrigation canals are neglected, with sand filling the canals, then wind erosion comes into play, adding to the destruction of agricultural processes; “the action of the wind is better considered another of the diverse forces that act not to concentrate riverine [or irrigation] sediments or their derivatives in a few loci, but to disperse them very widely.”

As pointed out above, erosion is a powerful agent at Ur. Adams has told us the conditions in southern Mesopotamia were similar to those in the last few centuries of the Ottoman Empire. Adams and Hans J. Nissen describe these conditions as being subsistence agriculture, producing the bare minimum to maintain

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139 McC. Adams, *op.cit.*, p. 154
140 *ibid.*, p. 10
only the immediate population with little or nothing for export. They state that at Uruk, north of Ur, the people were forced to adopt “subsistence alternatives ranging from fully sedentary to seminomadic.” “For all the renowned productivity of Mesopotamian agriculture, the attainment of even the bare margin of subsistence was always precarious.”

This suggests that large herds of horses could not possibly be maintained when the Scythians came to Ur. And it may be that the Assyrians planted the Scythians there to destroy their dangerous allies. During the past century the development caused by the oil industry in that region has ameliorated the conditions there, bringing pipelines of water to oil pumping stations and cities in Iraq. Nevertheless, Zarins has presented maps of Iraq made by its Ministry of Agriculture for 1952-1953. A census was taken of horse, mule and donkey populations, restricted to those who were sedentary, therefore excluding nomadic peoples. What the census shows for Ur and the immediate vicinity is that there were about 200 donkeys near Ur, but no mules or horses there or in the surrounding areas. Only to the north and to the east did conditions allow for horses and mules. And this census is made under better conditions than existed during the last centuries of the Ottoman Empire.

Rawlinson, writing of southern Mesopotamia in the 1800’s—the time near the end of the Ottoman Empire—mentions that flocks of grazing animals constitute the chief wealth of the people who have nearly forsaken . . . agriculture, which anciently gave Chaldea its pre-eminence, [they] have [presently] relapsed very generally into a nomadic or semi-nomadic condition. The insecurity of property consequent upon bad [Ottoman] government has in great measure caused this change, which renders the bounty of Nature useless . . .”

In the 1920’s, Woolley described the region around Ur:

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142 *ibid.*, p. 73
143 *ibid.*, p. 83
144 Zarins, *op.cit.*, pp. 233-235
145 Rawlinson, *The Five Great Monarchies . . .*, *op.cit.*, p. 41
“Standing on the summit of the mound” [of Ur] “one can distinguish along the eastern skyline the dark tasselled fringe of the palmgardens on the [Euphrates] river’s bank, but to the north and west and south as far as the eye can see stretches a waste of unprofitable sand . . . nothing relieves the monotony of the vast plain over which shimmering heat-waves dance and the mirage spreads its mockery of placid waters.”

That is, even after the Ottoman Empire fell at the end of World War I, southern Mesopotamia was a desert unfit to raise crops or horses in appreciable numbers.

Little land was available for agriculture and in order for the people to survive they were forced to follow a nomadic or semi-nomadic subsistence life. Only by being constantly on the move could their meager herds find sufficient vegetation upon which to live. This is not the environment in which thousands or even hundreds of horses could be kept settled in one small area. The Assyrians, like the Ottomans, created an environmental catastrophe by removing the population. It is this region they apparently gladly gave into the hands of the Scythians whom they quite probably distrusted as allies.

The reason that the donkey, a domesticated wild African ass, can do well in such an environment is that deserts are its natural habitat:

“Wild asses are desert dwellers, some populations inhabiting regions in which vegetation and fresh water are too scarce for any other large animals” [such as the horse].

The environment around Ur was quite probably too arid to support the Scythian herds confined to it and its nearby surrounding area. Under the better conditions of the 1950s, horses of nomads may have existed but they were probably very few in number and only survived because the nomads were constantly on the move. Great herds of thousands of horses are unlikely to have been kept at Ur for long periods under the conditions that existed in first millennium Assyrian times, when the Scythians lived at Ur.

\[146\] Woolley, Ur of the Chaldees in Leonard Cottrell, Lost Cities (NY 1963), p. 41
Cardona may argue that in so-called Sumerian third millennium times conditions were different. But this does not address the issue of the Scythians of the first millennium. Cardona has also maintained that there were no horses in Mesopotamia at the “time of the burials in question,” which of course refers to the third millennium. This was addressed earlier but needs to be enlarged upon as refutation to this claim. Speaking of northern Pakistan and Persia, Jacquetta Hawkes claims that “domesticated horses were found . . . as early as the mid-fourth millennium B.C. Asian pastoralists were already herding on horseback.”

Elizabeth Wayland Barber further informs us that

“Two big breakthroughs of importance to the [Russian] steppes occurred around 4000 B.C. . . . One, first noticed and worked out in the 1980s by the English prehistorian Andrew Sherratt, has been dubbed the Secondary Products Revolution. Sherratt noticed evidence that prior to about 4,000 B.C. people used their domestic animals only as a supply of meat and hides, killing all but a small breeding flock of yearlings. But then archaeological finds show a change: people get the idea of harvesting useful things from their living animals, so they keep them alive to ripe old age. Vessels for milk and milk products appear (food), processing of wool begins (clothing), and use of draft animals to pull a more efficient plow commences (energy). . . .

“The second change was the domestication of the horse, which we know first from a Ukrainian horse jawbone of about 4000 B.C. that shows special tooth wear from chewing on a man-made bit. As people grew more skilled in controlling horses, they could use these splendid beasts to manage . . . flocks.”

Therefore, if the horse was domesticated in the Ukraine around 4000 B.C., it would have over time escaped captivity and run wild into the surrounding regions. Mesopotamia is only some hundreds of miles south of the Ukraine, not southern Mesopotamia but its northern end. While the horse spread into Europe and east Asia thousands of miles to the east and west in less than 500 years, we are expected to believe that getting through the Caucasus mountains to Anatolia and the Fertile Crescent took the horse 2000 years. Horses can and do survive well in northern Mesopotamia today as Zarins’ maps clearly indicate. There would be no obstacle to halt their spread there around 4000 B.C.

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148 Hawkes, *op.cit.*, p. 54
149 Wayland Barber, *op.cit.*, pp. 189-190
Evidence does exist that proves wild horses were able to thrive in northern Mesopotamia since that “panthers, lions and a species of wild horse roamed in the northern Mesopotamian steppe is proved by animal remains from the excavations of Tell Barak and elsewhere.”\textsuperscript{150} Austen Henry Layard, the excavator of Nimrud, also commented on this:

“Assyria [in northern Mesopotamia] and particularly that part of the empire which was watered by the Tigris and Euphrates, was celebrated at the earliest period for its horses, as races of Arabia. The Jews probably obtained horses for their cavalry from this country . . . At a later period the plains of Babylonia furnished horses to the Persians.”\textsuperscript{151}

This condition exists because northern Iraq, where Nimrud or Kalhu is situated, has a different climate to that of the south:

“In the north east [of Iraq] the summer is shorter than the [southern] lowlands . . . and winter considerably longer. The summer . . . average temperatures are 5 to 10 degrees (Fahrenheit) cooler than those of lowland Iraq. In Mosul [north of Nimrud] . . . January temperatures range between 24°—63°F (-4° and 17°C) . . .

“In the foothills of the northeast annual rainfall of 12 to 22 inches, enough to sustain good seasonal pasture, is typical. Precipitation may exceed 40 inches in the mountains, much of it falls as snow.”\textsuperscript{152}

Speaking of the region of western Asia, Gordon T. Bowles reports that it was a grazing steppe:

“‘As implied by its descriptive pseudonym, ‘the land of the five seas,’ western Asia . . . is surrounded on all sides by oceans and inland seas. It shares with the southern coast of Europe the cool, moist Mediterranean winds and coastal vegetation along part of its western littoral. . . .

\textsuperscript{151} Austen Henry Layard, “Excavations at Nimrud,” in C.W. Ceram, Hands on the Past, op.cit., pp. 243-244
\textsuperscript{152} Encyclopedia Britannica, Macropedia, vol. 21 (Chicago 1998), p. 973
“The narrow coastline [of Syria] is humid; the hills are forested; and the wadis of the mountain perimeter are tilled and sharply differentiated from those of the hot, arid interior. Inland, the northern half of the peninsula forms a dry desert steppe grazing zone . . . The southern half is largely a vast desert more than six hundred miles broad. . . .

“Northward, the grazing grounds extend to include all of the Syrian desert. . . . The interfluvial section between the mountainous and forested Mediterranean section of the Fertile Crescent and the twin river alluvial plains of Mesopotamia . . . The upper courses of the Euphrates which receive permanent tributaries only from the Anatolian plateau have been significant to Syria by providing a steppe hinterland.”

In the Americas horses brought by the Spaniards did escape and become wild after 1500 A.D. It did not take these horses 2000 years to cross deserts and mountains to inhabit all the North American environments suited to them. It took less than 200 years. Exactly the same situation pertains to the Ukraine and Mesopotamia. If the horse thrived on the Ukrainian steppe in 4000 B.C., by around 3800 B.C. it would have overrun practically all of Mesopotamia where habitats suited to its needs existed. If, as Cardona claims, the horse was unknown there prior to about 2000 B.C. then the chronology of the horse’s wanderings suggests it came into the Ukraine not 4000 B.C. but about 1800 years closer to the present. On the basis of the established chronology, if Mesopotamian history really begins around 3500 B.C., it should have been almost immediately well acquainted with the horse.

Let us examine the question from yet another area. “Depiction of horses in ancient art suggests that horses of Arab type lived in the Arabian peninsula as long ago as 2000-3000 B.C.” This Arabian-type horse was living adjacent to southern Mesopotamia around 3000 to 2000 B.C. but, as with the horses of the Ukraine, failed to migrate to the steppes of central and northern Mesopotamia. Arabian horses are well suited to desert and semi-arid desert environments and should have migrated into Mesopotamia by 3500 B.C. Again, if Mesopotamian history extended back to 3500 B.C., its people should have been well-acquainted with the horse. The implication is that Mesopotamian history cannot reach back to 3500 B.C. If it did so, it would have known, and been involved with, horses. There

153 Gordon T. Bowles, The People of Asia (NY 1977), pp. 32-33
is simply no way around this evidence. This problem has not, so far as I have read, been explained by historians.

Why did it take the horse 2000 years to migrate a few hundred miles from the Ukraine into northern Mesopotamia or a few hundred miles farther into southern Mesopotamia from Arabia, while in America the horse migrated thousands of miles across the continent in a few hundred years? Are we to assume that wild modern horses walk and run rapidly while ancient horses walked and ran about ten times more slowly?

The horses which the Scythians brought to Ur either died there during the first year or were sold, eaten or whatever. The reason that no horses were found in the royal tombs seems to be that there were none left to bury. That is probably why donkeys were employed to pull chariots. They could survive in and around Ur where the horse could not. But Cardona is right to claim that since the Scythians knew of the horse as did the people they ruled over, the horse should have been depicted at Ur. Now we do have on record several authorities identifying the animals pulling the chariots as horses, mules, donkeys, etc., but these depictions are too inexact to take any solid stand. Yet there is another reason that will account for this lack of depiction. The Scythian mound graves number into the thousands all across southern Russia while at Ur we have only sixteen in one place. Indeed, both groups of sites have been plundered by grave robbers, but what can survive these predations in thousands of graves will be far greater and more highly diverse in nature than that which will survive in only sixteen. Roux informs us that “all but two tombs had been plundered in antiquity.”155 One of these was that of a woman.156

Pollock further points out that graves in Mesopotamia were often re-used or looted to obtain artefacts to be place with those buried after the earlier ones.

“But the practice of reusing graves declines after Early Dynastic II, while the disturbance of graves shows no sign of doing so. Graves continue to be reopened, apparently with the express intention of removing some of the objects buried with the dead person. Archaeologists have typically labeled this behavior ‘grave looting.’ . . .

“But by the later part of the Early Dynastic period the removal of goods from circulation through their placement in burials had reached

155 Roux, Ancient Iraq, op.cit., p. 136
156 Sumer: Cities of Eden, op.cit., p. 91
unprecedented proportions that were not sustained in the Akkadian [in Heinsohn’s theory: Assyrian] period . . . To be sure, some of the removal of objects from graves may have been robbery . . . some of the reexcavation and removal of objects may have been part of a more or less accepted practice of reclaiming goods (one’s inheritance?) after ‘a decent interval’ . . . this does not explain why some [2] of the . . . burials in the Royal Tombs were untouched, although those of the principal occupants often were.”

The lack of horses in the royal tombs of Ur is explicable on climatic grounds, while their absence from artefacts is explicable in terms of the fact that these were most probably looted. What might have escaped looters of the thousands of Scythian graves will surely be incomparably greater than that of only sixteen specially since this cemetery was located at a city, available to generations of thieves to raid for its treasures.

The difficulty of keeping horses at Ur imposed on the Scythians by the Assyrians may very well have led to the early dissolution of their alliance, causing the Scythians to later join with the Medes against their former allies. Roux calls the Scythians “doubtful allies” of the Assyrians. He also shows that the Scythians had been enemies of the Assyrians. Distrust by the Assyrians of their Scythian allies may well have caused them to place these doubtful people in an environment where they probably understood their horses could not survive.

Cardona states:

“Personally, I would have been less surprised had Heinsohn attempted to compare these tombs with those discovered at His-pei-kang, near Hou-chia-chuang in northern China, and dated to the Shang dynasty of the second millennium B.C. At least the Shang tombs, like those at Ur, were located at the bottom of pits with similar sloping ramps leading to them. Items of bronze, jade, marble, silk, and pottery, not to mention ‘entire armouries of elaborate weapons’ and ‘enough musical instruments for an entire orchestra,’ were buried with Shang monarchs. Moreover, similar to the rituals that must have taken place

157 Pollock, op.cit., pp. 214-215
158 Roux, Ancient Iraq, op.cit., p. 136
159 ibid, p. 336
at Ur, human sacrifice, ‘sometimes on an awesome scale,’ was often part and parcel of the Chinese burials.”\textsuperscript{160}

Although this point, made in jest, is not made definite it is worthwhile exploring because it will help us to determine what type of people are buried at the Royal Cemetery. If by some stroke of luck Cardona had made a good guess, then the skeletons and especially the skulls and cranial features of those buried at the royal tombs at Ur would exhibit Mongoloid characteristics which a physical anthropologist could determine. Roux explains:

“Another point should be made quite clear; there is no such thing as a Sumerian ‘race’ neither in the scientific nor in the ordinary sense of the term. The skulls from Sumerian graves that have been examined are either dolicho- or brachycephalic and indicate a mixture of the so-called Armenoid and Mediterranean races, the latter being somewhat predominant. As for the physical features depicted on monuments, they are largely conventional and have therefore no real value. The big fleshy nose, the enormous eyes, the thick neck and flat occiput long considered to be typical of the Sumerians also belong to the statues of individuals bearing genuine Semitic names found in the almost exclusively Semitic district of Mari, while more realistic portraits, such as those of Gudea, the Sumerian governor of Sumerian Lagash, show a short, straight nose and a long head.”\textsuperscript{161}

This naturally rules out the Chinese of the Shang Dynasty. But let us briefly examine those of Scythia as Rice reports:

“Rudenko has succeeded in establishing that the majority of the skulls found at Pazirik and at such allied burials as Shibe, Tuekt, Kurai and Katanda were European in type. This bears out Jettmar’s view that, at any rate until the fifth or fourth century B.C., the inhabitants of western Siberia were a fair-haired people of European origin, and that it was after that date that an influx of Mongoloids resulted in a very mixed type of population. At Pazirik the burials contained sub-brachycephalic, brachycephalic, mesocephalic and dolichocephalic skulls, which suggests a considerable admixture. From the representations on the Kul Oba, Chertomlyk and Voronezh vessels the

\textsuperscript{160} Cardona, \textit{op.cit.}, p. 83  
\textsuperscript{161} Roux, \textit{op.cit.}, p. 81
Scythians seem to have resembled to a striking degree peasants of pre-revolutionary Russia. Most scholars are however convinced that no racial links exist between the Slavs and the Scythians . . .”162

There does seem to be a similarity between the so-called Sumerians and the Scythians. In this respect Sir Arthur Keith states of the people around Ur. “They certainly belong to the same racial division of mankind as the nationalities of Europe; they are scions of Caucasian stock.”163

Yet this does not tell us precisely who those interred at the Royal Cemetery are, only that they are closely related to the Caucasian race. However, if the Scythians came to Ur and stayed there for a generation or two, they may have some distinct aspect of their skeletal structure that might betray this short sojourn. In this respect teeth do give us a considerable amount of information.

Ancient people who inhabited desert regions had a great problem keeping sand and grit out of their food. This grit is extremely damaging to teeth. It cuts the enamel and grinds down the teeth far more rapidly than those of people who do not live under these conditions. If the person is wealthy enough to have honey in his diet—a sugar—this will lead to dental caries and lesions far earlier than in those who cannot afford such a luxury. This is explained by Joyce Tyldesley, a renowned Egyptologist:

“Teeth, usually the best preserved part of any corpse or skeleton, form a highly specialized branch of mummy studies. Teeth can provide information on age, health, diet and even family resemblances . . .

“The unfortunate Egyptians suffered greatly from toothache. Decay, or caries, caused by excessive sugar consumption, was a disease limited to the wealthy. Sugar was only available in the form of honey and this was a luxury beyond the reach of most Egyptians. However, the standard daily diet was high in bread made from stone-ground cereal that incorporated a large quantity of wind-blown sand. This eroded the teeth and gradually wore them down to the gums. This extensive wear

162 Rice, The Scythians, op.cit., p. 77
on the teeth of adult Egyptians can make it hard to estimate age at death
on the basis of teeth alone.”\textsuperscript{164}

Therefore, if the Scythians did not live all their lives in the great desert
of southern Mesopotamia, but only stayed there for a generation or two, their teeth
would not have been as severely affected by the sand and grit as those who lived
there all their lives. Speaking of the people of al-‘Ubaid which “is only four miles
to the west of Ur,” Keith states:

“I have never seen human teeth worn down to the degree found
in the al-‘Ubaid people. In eight of them—four men and four women—
the upper incisors were worn down almost to the level of their sockets;
in one woman, the roots of the molar teeth have been exposed and used
in mastication. As will be seen from the census of the teeth . . . dental
abscesses were common, but caries was almost entirely absent.”\textsuperscript{165}

As for the people at Kush or Kish, 150 miles northwest of Ur, Keith
states: “Dr Buxton reports the \textit{extreme degree of wear to be seen in the teeth of the
Kishites} of the latter part of the third millennium [B.C.].”\textsuperscript{166} [emphasis added] That
is, the Kishites were about contemporary with the people in the Royal Cemetery.
Thus all around Ur the people living at that time had teeth that were severely eaten
away by sand and grit in their food, especially in bread. Therefore, if the people in
the royal tombs at Ur had lived there all their lives, their teeth should also exhibit
great wear. However, if these were Scythians who only stayed there a generation or
so, the degree of wear of their teeth would be quite small compared to those living
there from birth. If the people buried in the Royal Cemetery exhibited much less
wear of their teeth, it would clearly indicate that they were not native to that region
but came there from a region where there was much less sand and grit available to
get into their food. This, in fact, is just what was found. The people under the ‘Tomb
Mound’ of Ur excavated by Woolley clearly show “an absence of aged individuals
in the Ur group—also much less tooth wear . . .”\textsuperscript{167}

One could argue, of course, that since these people appear to be
younger, they naturally should show less tooth wear. But this argument fails as Keith
clearly explains:

\begin{flushright}
\begin{itemize}
\item \textsuperscript{164} Joyce Tyldesley, \textit{The Mummy} (Dubai 1999), pp. 118-119
\item \textsuperscript{165} Keith, \textit{op.cit.}, p. 217
\item \textsuperscript{166} \textit{ibid.}, p. 239
\item \textsuperscript{167} \textit{ibid.}, p. 219
\end{itemize}
\end{flushright}
“"I have never seen in any race, ancient or modern, teeth worn to the degree . . . unearthed at al-‘Ubaid; the wear, making all allowances for the age of the individuals, was much less in men and women of Ur who belonged to a later period in the history of Mesopotamia."" [emphasis added]

Various *ad hoc* hypotheses have been advanced to explain this problem. Keith admitted the teeth of the residents of Ur did not have harder enamel and suggests the grain had not been cleansed sufficiently to inhibit the wear. Crawford, almost 75 years later, stated:

“Professor Keith who studied the bones [in the Royal Cemetery] remarks on the good condition of the teeth, and attributes this to a ‘civilized diet’ . . . More accurately, it probably indicates that grain was being well processed, without too much stone debris in the flour, and that the diet was low in sugar.”

But this requires a process that will remove the grit. This cannot be done by a screening of the grain. Grit can be as small as the powder of flour and will pass through the finest mesh. Grinding the flour finely also grinds the grit finely. Fine grit is as destructive as coarser grit. And how does one keep the blowing sand from getting into the food while it is being prepared or eaten? There is no process discussed in the historical literature to support these *ad hoc* assumptive assertions. In the same manner that the historians cut diorite only with words, or have found sources of tin only with words, they only have words with which to separate the grit from their food. As with all their *ad hoc* hypotheses, they will probably never show how the process actually works and that it was in fact used at Ur.

Supposedly, we have a population of so-called Sumerians living all their lives in the same environment as all their other countrymen, eating the same food that grew near a desert, but the sand and grit in their food failed to wear away their teeth to the same extent as everyone else’s. The contradiction speaks volumes against the historical presumption that those buried in the Royal Cemetery were indigenous to that country.

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168 *ibid.* p. 239
169 *ibid.*
170 Crawford, *op.cit.* p. 114
The most significant point is that the people living in Mesopotamia or Egypt at the time these regions were deserts must have had sand and grit get into their grain and thus into their bread and these would erode the enamel on their teeth. If, however, people lived in these regions prior to their becoming desert and prior to the agricultural revolution that developed grains and baked bread, then their teeth would not exhibit the great wear seen on the teeth of the ancient peoples. That the teeth of those in the Royal Cemetery while living in Mesopotamia during the period when it was a desert and eating grains and bread ground on rock which produced grit and infiltrated by wind-blown sand, show very little such wear compared to others, implies that these people did not live all their lives in Mesopotamia but came there from some forested, or steppe region for only a relatively short period of time. This most certainly would apply to the Scythians when they stayed at Ur.

This evidence fully supports Heinsohn’s thesis. Their teeth were not worn down as greatly by the sand and grit in their food because they came to that region long after their teeth had well developed and were exposed to these destructive elements for only a generation or so. Many of them probably died in less than a generation, others somewhat later. Their teeth reflect a short-term exposure to the elements that were so destructive to those who were exposed far, far longer.

If one wishes to see how well the teeth of the Scythians were maintained in adults, both men and women, figures 43 and 44 in Rudenko’s book Frozen Tombs of Siberia will make these facts quite clear. The Scythians did not spend much, if any, of their lives in the deserts to the south of their homelands and therefore their food would contain extremely little sand or grit compared to those who did live there. Their teeth would therefore not be terribly worn down and this is reflected by the fact that those found in the Royal Cemetery had teeth that were in a far less eroded state than the teeth of the people around them who had experienced this destructive element their entire lives.

Cardona states that if these people “dated to the Shang dynasty of the second millennium BC . . . like those at Ur . . . [they would have] items of bronze, jade . . . not to mention ‘enough musical instruments for an entire orchestra’ . . .”\textsuperscript{171}

Among the musical instruments found in the Royal Cemetery were lyres, harps, and wind instruments. If the people buried in the royal graves were Scythians of the first millennium B.C., they would have had contact with the Greeks

\textsuperscript{171} Cardona, \textit{op.cit.}, p. 83
and would have known of their musical scale—the heptatonic-diatonic scale. In this respect Zecharia Sitchin states:

“A team of scholars from the University of California at Berkeley made news in March 1974 when they announced that they had deciphered the world’s oldest song. What professors Richard L. Crocker, Anne Kilmer and Robert R. Brown achieved was to read and actually play the musical notes written on a cuneiform tablet from circa 1800 B.C., found at Ugarit on the Mediterranean coast (now in Syria) [a region with contacts to Greece].

“We always knew,’ the Berkeley team explained, ‘that there was music in the earlier Assyro-Babylonian civilization [which in Heinsohn’s chronology would be Persian times of the first millennium B.C.], but until this deciphering we did not know that it had the same heptatonic-diatonic scale that is a characteristic of contemporary Western music and of Greek music of the first millennium B.C.’ . . .

“There can be no doubt that the music and song must also be claimed as a Sumerian ‘first.’ Indeed, Professor Crocker could play the ancient tune only by constructing a lyre like those which had been found in the ruins of Ur.”

Professor Crocker told this author the lyre he constructed was from the Royal Cemetery at Ur.

If the so-called Sumerians, and not the Greeks, invented the heptatonic-diatonic scale, since in terms of the established chronology the scale was used supposedly in post-Sumerian times, then Mesopotamian wind instruments which cannot be tuned like lyres or harps from these later cultures should also be capable of playing that scale. If such wind instruments could indeed play the heptatonic-diatonic scale then it would prove the so-called Sumerians invented this scale. If, however, such wind instruments have been constructed and cannot play this scale, it would indicate that the Greeks of the first millennium invented the scale and the Scythians learned it from them and brought this knowledge to Ur when they ruled there. The fact is that no such wind instrument is known from Sumerian times onward that can play this scale. Lawergren and Kilmer comment in a recent edition

172 Zecharia Sitchin, The 12th Planet (NY 1976), pp. 45-46
of Musik in Geschichte und Gegenwart, p. 149, that the only evidence of wind instruments of post-Sumerian times are pictorial representations of pipes with enough finger holes to play the octave. But representations are not reproductions of the sound needed nor of the Greek scale.

Herodotus in Book IV of his History describes the death of a Scythian for taking part in a ritual to Cybele playing the tympanon. He was killed for worshipping an alien god, not for playing music. So the Scythians knew of Greek music. Until the archaeomusicologists reconstruct a wind instrument based on Heinsohn’s chronology prior to Greek times that can play the heptatonic-diatonic scale, his theory does fit well with the musical instruments in the Royal Cemetery. For now, this evidence suggests that these instruments were those of the Scythians of the first millennium B.C.

Cardona states:

“The worst part of all this is that Heinsohn could not be unaware of some of these dissimilarities. No one who has delved into ancient history could have failed to come across illustrations of the items found at Ur as well as those retrieved from Scythian burials. All of which leads one to suspect that Heinsohn can be very heedless of the facts at his disposal.”

Heedless? The worst part of all this, to paraphrase Cardona, is that Cardona was fully aware of many of these similarities but failed to report or discuss them from the sources he read. Having done so he fully and openly misrepresented and suppressed facts at his disposal. Not only did the written sources contradict many of his ex cathedra statements, but the illustrations he saw flew in the face of these same utterances regarding Scythian and Ur burials. None of this behavior can be described other than as indifference to evidence. Cardona’s abject failure to present anything resembling adequate or competent scholarship on many of these points is what must be called “heedless”.

Cardona states:

“We can see therefore that there was absolutely nothing similar about the two modes of burial. The two peoples concerned looked

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173 Cardona, op. cit., p. 85
differently, they dressed differently, their burial chambers were constructed differently, the bodies of their royal deceased were prepared differently, their funerary furniture was different, as were their method, equipment, and mounts of warfare. Can Heinsohn still claim these to be one and the same people?"  

After all the denials of similarities with respect to art forms etc. which have been discussed and answered above, one can with confidence show that there is a great deal of evidence that clearly indicates that these are indeed “one and the same people.” The Scythians and the royal personages buried at Ur exhibit an identical form of burial with sacrificial victims, that in turn is clearly different from the burial practices used by the so-called Sumerians. The persons buried, including women, are not kings, nor known to be kings, but are clearly vassals who ruled over the city and country which fully fits the Scythians while at Ur, and are even called “princes” by some historians. The Scythians and royalty buried at Ur employed ramps and corbelled vaulting in their tombs. Both peoples dug deep pits with subsidiary rooms to bury their leaders. Both peoples had long elaborate period in which rituals to the dead were observed. The ground over both burials was raised in layers. The themes of the artistic relics left at the graves of both peoples are the same. The dress of both peoples in their tombs is clearly similar. Both peoples are shaved up to the heads before burial. As for embalming, the soil at Ur would destroy the embalming materials used.

The wheels of the wagons of both peoples are clearly alike and the problem of spoke-wheeled wagons in Scythia is a problem for the historians’ chronology and not Heinsohn’s. Onagers were never used by either people and many zoologists have suggested that the Royal Standard of Ur exhibits horses, though this author disagrees with them. The two peoples had teeth with much less erosion by sand than the indigenous population of Ur. This clearly makes excellent sense with Heinsohn’s thesis and contradicts the established view that these were indigenous people. Indigenous people would have very highly eroded teeth. The musical instruments at Ur clearly exhibit evidence of first millennium construction with respect to the scale they played. Both peoples left harps, lyres, and pipes, in their tombs. Both peoples left cooking cauldrons of which the Chertomlyk vessel is an example, as well as dishes, cups, and other cooking utensils. This is corroborated for the Royal Cemetery at Ur: “From the Royal Cemetery at Ur, no less  

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174 Cardona, loc. cit.  
175 Rice, The Scythians, op. cit., pp. 93–95
than 118 types of metal vessel are recorded from simple bowls and dishes to jars, cauldrons . . . [etc.]  

This large number of identities between the tombs of Scythians and those at Ur cannot be dismissed as coincidence. Other tombs in Mesopotamia may have one, two or more similarities, but the totality of the evidence for both places argues against this possibility of coincidence.

But now we must go to Cardona’s argument that directly contradicts the placement of the Royal Cemetery at Ur in the third millennium B.C., pointing to a placement in the first millennium. This concerns the bronze artefacts recovered at the royal tombs mentioned by Cardona. As we have already shown, bronze could not be made in Mesopotamia in the third millennium because the sources of tin did not exist there until the second millennium at the very earliest. But Jacquetta Hawkes makes it thoroughly clear:

“The earliest Mesopotamian artifacts that can be called bronzes date from about 3200-3100 B.C. However, as they contain only about 4 percent tin and also yield lead and iron, it is very probable that the tin was present as a natural impurity. It was in the early dynastic period that the controlled and deliberate production of bronze really got underway. . . .

“By about the middle of the third millennium, however, bronze smiths had realized that about 10 percent of tin was the ideal proportion for most purposes. They now probably were able to control this proportion more exactly by reducing the tinstone in the presence of smelted copper. By this time craftsmen could cast very fine objects indeed—well represented by the many splendid vessels and other bronzes from the Royal Cemetery of Ur (c. 2600 B.C.).”

To get around the problem of tin, Woolley offers that:

“Sumer imported [copper] from Oman . . . Oman copper ores contain both nickel and tin, so that the smelted metal is a natural bronze,

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177 Hawkes, op.cit., pp. 108-109
objects from the royal cemetery at Ur showing as much as 14 percent
tin and . . . up to 2 percent of nickel . . .”

He goes on to add that:

“with the conquest of Sargon of Akkad, which put an end to the
Early Dynastic Period, bronze disappeared and vastly inferior hammer
axes of unalloyed copper replace the splendid bronze castings of the
royal cemetery. Perhaps Oman veins were exhausted. More probably
political reasons cut the conqueror off from the country’s old source
of supply.”

But this is all conjecture; why would Sargon who led the first great
military empire allow the supply of bronze to be halted for any reason? And
Woolley and Hawkes well knew this source did not run out. These are typical
conjectures to deal with the problem.

Nevertheless, Dayton explains that Oman copper is of two types,
neither of which contains “nickel”, which proves “Oman could not have been the
source of nickel-rich Sumerian copper.” As one can see, even the most respected
historians of ancient history will offer gossip and hearsay as evidence to support
their chronological belief system in order to indoctrinate (not educate) others who
trust them because of their prestige and influence.

In addition to all this is the evidence discussed earlier of two daggers—one with a gold handle and iron blade, the other with a bronze handle and iron blade,
and a necklace of gold and iron found at Alaka Oyuk of pure Sargonic style “which
may even go back to the Sargonic period of the Royal Tombs of Ur.” Then there is
the statement by Herzfeld and Christian that these blades were derived from
terrestrial ores and were not of meteoric iron which indicates the so-called Sumerians
were a first millennium society. Neither tin nor terrestrial iron was used in
civilizations in Mesopotamia in the third millennium to fashion daggers. These
daggers thrust deep into the heart of the established chronology and have dealt it a
mortal wound.

As Roux admits:

178 Hawkes and Woolley, op. cit., p. 555
179 ibid.
180 Dayton, op. cit., p. 74
“Thus more than half a century after its discovery, Ur’s ‘royal cemetery’ still keeps its secret. One day, perhaps, some chance excavations in other cemeteries with human sacrifices . . . will be able to tell us who these dead people were surrounded by so much gold and will solve this baffling enigma.”

But there are excavations of a great many other cemeteries in which leaders and their court and animals and much gold are found. These cemeteries are located in the Scythian homelands which clearly will inform those open to evidence that these dead people surrounded by such wealth were Scythians and the baffling enigma is resolved.

As this author has maintained all along, scientific evidence must override historical interpretive analysis, clearly pointing to the fact that the dating of the royal tombs of Ur cannot belong to the third millennium. The historical interpretive analysis must be made to conform with the science, not the other way round. Until Cardona or anyone else can produce reliable facts that indicate that the people of Ur had sources of tin and knew how to smelt iron in the third millennium other than assumptive arguments, they cannot be taken seriously.

In addition the mere fact that diorite was supposedly cut by these same, so-called Sumerians about 1000 years or more prior to the introduction of steel to that region must also be counted as a fundamental scientific negation to the dating of the royal tombs. Placing these tombs in the first millennium not only agrees with the evidence from technological science, but allows the various forms of historical interpretive analyses given above to corroborate, and conform to, the science. In the final analysis science and history confirm and corroborate each other for a first millennium B.C. placement of these Scythian burials at Ur and are negated and fully out of connection with one another if one places these burials in the third millennium B.C. To paraphrase Cardona: in any case, is Cardona still of the opinion that the Royal Graves at Ur, as per Heinsohn, were actually not those of Scythian princes despite all the evidence this author has presented to the contrary? Why does he not, instead, attempt to refute all this evidence with citations from the literature?

But let us now turn to another aspect of science that categorically denies that the so-called Sumerian cities and farmlands and pasturelands survived, as we are told by the historians, for thousands of years, first with the so-called Sumerians

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and then by the later Akkadian conquerors who exacted tribute from the great cities in southern Mesopotamia.
CHAPTER 14
AGRONOMY AND CLIMATOLOGY

“Water has profoundly affected the course of human history. Its abundance has helped societies to flourish, its scarcity has caused them to wither. No consideration of history or of the fate of societies, past or present, can ignore its role.”

Daniel J. Hillel

*Out of the Earth—Civilization and the Life of the Soil* (NY 1991), p. 34

According to conventional chronology, the so-called Sumerian civilization, located in the southern Mesopotamian desert plain between the Tigris and Euphrates rivers (present-day southern Iraq), was established around 2900 B.C. There it reigned for almost a millennium and then was ruled in turn by the Akkadians and Babylonians, paying tribute to its conquerors. Prior to its rise it apparently had a rich prehistory during the Ubaid period that covered the fifth millennium B.C. and the Uruk period covering the fourth millennium B.C. The cities in that southern area are believed to have been well populated for almost 3000 years; they were highly productive with respect to agricultural crops and supported large herds of sheep whose wool brought wealth to the region.

In terms of the revisionist historians Heinsohn, Rose, and Sweeney, this great length of continuous civilization in southern Mesopotamia is clearly not possible. In order to analyze this chronological question we must turn our attention to agronomy and climatology as these relate to agriculture. In this way, science can be employed to determine whether or not the oldest so-called civilization of southern Mesopotamia could have maintained its great cities and large populations with agriculture and pasturage for about 3000 years.
Jacquetta Hawkes describes the manner by which southern Mesopotamia was able to grow its great crops by irrigation:

“Irrigation on the Plain was of a distinctive kind. Natural conditions determined that it should be perennial. By the time the floods began in late March or April the winter-sown crops were already greening the fields and the summer sowing was done. It was, therefore, impossible to allow the water freely on to the land as could be done in Egypt. Some of the flood water could be immediately used under strict control, while some was stored in reservoirs, natural depressions and in the irrigation system itself. This could be used during the following summer months. Probably rather more important was the water that could be led off from the rivers themselves all through the year. The silt brought down from the hills and mountains by the floods was deposited when the current slackened on the Plain, and gradually built up the bed and banks until the river flowed well above the level of the fields. This was particularly true of the Euphrates, always of far greater importance than the Tigris for the watering of the Plain. To prevent disastrous breaching at flood time, the banks had to be perpetually strengthened and repaired. This was immensely laborious, as was also the building up of the main canals to a level corresponding with the river bed in order to avoid an excessive rush of water when the sluices were opened. On the other hand the high level of the river made it possible, even in summer time, to lead water over long distances and with a minimum of hoisting. Such perennial irrigation enabled the farmers of the Plain to raise two crops a year, reliably brought to harvest beneath the summer sun.”

Samuel Noah Kramer, in his chapter “The First ‘Farmer’s Almanac’,” discusses a cuneiform document that was restored containing 109 lines of instruction by a farmer to his son on how to insure a bountiful crop. Of greatest importance to the discussion before us is the number of times the fields were to be irrigated. During the first irrigation,

“Care must be taken that their water does not rise too high over the field; when the water subsides, the wet ground must be carefully

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1 J. Hawkes, *op.cit.*, pp. 93-94
guarded against trampling oxen and other prowlers; the field must then be cleared of weeds and stubble and fenced about.”

Kramer goes on to describe further periods during the growing season in which irrigation was utilized:

“When the barley had grown sufficiently to fill the narrow bottoms of the furrows, he was to water it; and when it was dense enough to cover the fields, like the ‘mat in the middle of a boat,’ he was to water it a second time. A third time he was to water the ‘royal’ grain. Should he then notice a reddening of the wet grain, it was the dread samana-disease that was endangering the crops. If the crop showed improvement, he was to water it a fourth time, and thus get an extra yield of 10 per cent.”

The process of holding flood waters to irrigate the southern Mesopotamian plain at least three to four times a year for a summer crop, or six to eight times for a summer and winter crop, will undoubtedly enable the growth of rich crops at first, but this process also leads to the destruction of the soil. Daniel J. Hillel discusses this problem:

“In many of the older countries, where human exploitation of the land began early in history, we find shocking examples of once-thriving regions reduced to desolation by man-induced soil degradation. Some of these civilizations succeeded all too well at first, only to set the stage for their own eventual demise. Consider, for example, the southern part of Mesopotamia . . . which, as every school child knows, was a great ‘cradle of civilization.’ We need only fly over this ancient country, now part of Iraq, to observe wide stretches of barren, salt-encrusted terrain, criss-crossed by remnants of ancient irrigation canals. Long ago, there were fruitful fields and orchards, tended by enterprising irrigators whose very success inadvertently doomed their own land.”

David and Joan Oates point out that in southern Mesopotamia “in this very flat land . . . intense summer heat lead[s] to high evaporation which . . . brings the salts to the surface, where they can sometimes be seen as a white crust. No

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2 Samuel Noah Kramer, History Begins At Sumer (Garden City, N.Y., 1959), p. 66
3 ibid., p. 67
4 Hillel, op.cit., p. 4
cereals will tolerate such salinity.”\textsuperscript{5} They are stating directly that when salt becomes visible as a white crust on the surface of the land, cereals cannot be grown nor, quite probably, any other cultivated foods.

Let us understand how this occurs. Hawkes has told us that due to the silt it carried, the Euphrates in southern Mesopotamia raised the river bed; to prevent the river banks breaching, levees were created naturally so that the river was above the adjacent plain. But this condition involves seepage from the river through the ground. “The problem is general. Elevated rivers continually cause seepage into the groundwater and thus tend to raise the water table.”\textsuperscript{6}

Nyle C. Brady and Ray R. Weil in their college text, *The Nature and Properties of Soil*, explain what must ensue when one irrigates an alluvial plain in a desert region:

“Irrigation not only alters the water balance by bringing in more water, it also brings in more salts. Whether taken from a river or pumped from the groundwater, even the best quality freshwater contains some dissolved salts. . . . The amount of salt brought in with the water may seem negligible, but the amounts of water applied over the course of time are huge. . . . pure water is lost by evaporation, but the salts stay: (1) the water available from rivers . . . is relatively high in salts because it has flowed through dry-region soils which typically contain large amounts of easily weatherable minerals, and (2) the dry climate creates a relatively high evaporative demand, so large amounts of water are needed for irrigation. An arid-region farmer may need to apply 90 cm [36 inches] of water to grow an annual crop. Even if this is good-quality water relatively low in salts, it will likely dump more than 6 Mg/ha (3 ton/acre) of salt on the land every year.”\textsuperscript{7}

One need not dwell on the destructiveness of salt on crops used by man. There are plants that do grow in saline soils, known as halophytes, but these are not the ones mankind has typically grown as his food crops.\textsuperscript{8}

\textsuperscript{5} D. and J. Oates, *op.cit.*, p. 18
\textsuperscript{6} Hillel, *op.cit.*, p. 82
\textsuperscript{8} Hillel, *op.cit.*, p. 45
“Plants that grow best in moist but aerated soils, generally in semihumid to semiarid climates, are called mesophytes. Most crop plants belong in this category. Mesophytes control their water economy by developing extensive root systems and optimizing the ratio of roots to shoots, and by regulating the aperture of their stomates [leaf pores] to curtail transpiration [water vapor loss out of the leaf pores] during periods of water shortage. The latter effect, however, necessarily entails restriction of photosynthesis. Moreover, curtailment of transpiration reduces the evaporative cooling of the plants, so that they tend to warm up under the sun’s radiation, and as they warm up their rate of respiration rises. In effect, therefore, a thirsty crop plant consumes its own reserves and further reduces its growth potential. For these reasons, thirsty crop plants are generally much less productive than plants that are well-endowed with water throughout their growing season.”

This then is the double bind that the farmers of southern Mesopotamia faced. If they irrigated their crops regularly during the growing season they would at first have abundant harvests, but they would be adding salts to the soil and over time would have made the soil so saline that it would not grow their crops. If they did not irrigate their crops regularly they would not generate saline conditions but their crops would be stunted and have little, if any, value. The salinization process works in the following manner:

“In arid regions natural rainfall is generally insufficient for annual leaching [the salt at or near the surface]; irrigation must hence be applied in excess of crop water requirements so as to remove harmful salts by downward percolation beyond the root zone. Initially, for quite some years or even generations, the processes of groundwater salinization and water table rise are invisible and go unnoticed. Then, when the water table comes within a few feet of the ground surface, a secondary process of capillary rise comes into play. The rising groundwater evaporates at the surface [rising in the atmosphere but leaving behind in the soil or near the surface] precipitating . . . salts which are always present in the groundwater, especially in arid regions. This process infuses the topsoil with salt. As the salinization process advances, an irrigator might try to irrigate more and more in a desperate and frantic attempt to flush out the salt with fresh water, as before. But

\[9 \text{ibid., pp. 45-46}\]
in so doing he is merely accelerating the rise of the water table and thereby causing waterlogging [drowning the roots with too much water] as well as salinization. After each irrigation the salt reappears [because the water table is so close to the surface that capillary action readily carries it up and the sun’s heat causes the water to evaporate and leave more salt]. It rises from below and blossoms out in mockingly beautiful floral patterns, more of it each time, until the soil is rendered sterile.

“Working in tandem, therefore, silt and salt can destroy an entire region’s irrigation-based agriculture.”

The problem endemic to all irrigation systems in such arid areas is that too much water from the irrigation ditch network seeps down into the soil to the ground water before it reaches the crop plants. As Brady and Weil explain:

“A simpler measure of water-use efficiency, sometimes termed the water application efficiency, compares the amount of water allocated to irrigate a field to the amount of water actually used by the irrigated plants. In this regard, most irrigation systems are very inefficient, with as little as 30 to 50 % of the water that is taken from the source ever reaching the plant roots.

“Much of the water loss occurs in the canal and ditch system used to deliver the water to the irrigated field. If conveyance is by an open unlined ditch, [as was used in southern Mesopotamia] water moves rapidly downward under the ditch and into the ground water . . . and horizontally into the soil alongside the ditches. Substantial volumes of water are also lost by evaporation from the surface of water in canals and ditches. . . .

“Commonly, more than half of the water actually delivered to the field is not transpired [absorbed by the roots, carried to the leaves and evaporated through the leaf pores] by the [irrigated] crop. . . .”

There are certain factors that contribute to this salinization process. First, there is the fact that generally the southern Mesopotamian region is a plain. In

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10 ibid., p. 83
11 Brady, Weil, op.cit., p. 263
mountainous or fairly hilly regions the water table will generally be deeper below the surface than in the lowlands where it will be closer to the surface. This would allow tillage on the mountains and hills to continue without the threat of salinization for a very long time compared to that in the valley.

This specifically occurs north of the plain where the higher land is like a great hill or like a plateau where the water table is at a greater depth than the plain and salinization is not a problem for farmers. As Thorkild Jacobsen explains:

“In Middle and Northern Babylonia, on the other hand, the watertable is on the whole considerably lower and a much larger zone of dry soil separates it from the root zone above. This minimizes in no small degree the dangers of the salt waters of the watertable rising to the surface and allows, should such a rise happen, more latitude for leaching the upper soil layer [by rain or irrigation to] free the salts again. Here, therefore, it is understandable that the danger from salt could be kept relatively successfully under control over more than 5000 years of continued irrigation agriculture down to Abbasid times.”

Another factor contributes to the lowering of the water table, namely the speed of the current. Rapidly flowing rivers may indeed be muddy like the Colorado River in the Grand Canyon of the American southwest. Such rivers do not deposit silt on the riverbed and build levees along their shores. Instead, they tend to either lower the riverbed and the adjacent water table associated with it or reach an equilibrium base at a considerable depth where the water table varies during the year, rising in pluvial periods or falling during dry ones. This is true of the Tigris River and northern parts of the Euphrates. As Hillel shows, the Tigris

“cuts into the alluvium to a depth of several meters over most of its course, making it difficult to divert water [for irrigation] from it. Only along its southernmost stretch [near the Persian Gulf] does it rise above the level plain. The Euphrates, because it travels a much greater distance from its sources, loses almost half of its water through evaporation and seepage in the Syrian desert, so it arrives on the southern Mesopotamian plain at a much lower speed. Because of its more sluggish pace and greater load of silt [gathered over its longer

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12 Thorkild Jacobsen, *Salinity and Irrigation Agriculture in Antiquity* (Malibu CA 1982), p. 56
journey] the Euphrates deposits more sediment and tends to run above the plain rather than cut into it, and it forms high levees.”

Floods can and do leach salt out of the soil but are only effective in doing so where the river tends to stay well below the land surface. This is precisely the case with the Nile River:

“The irrigation-based civilization of Egypt survived and continued in the same location. In contrast the civilizations of Mesopotamia—Sumer, Akkad, Babylonia, and Assyria—each in turn rose and then declined and disappeared [sic—as we will see, the cities in the southern plain of Mesopotamia did not decline or disappear over its supposedly long history], as the center of population and culture shifted gradually from the lower to the central to the upper parts of the Tigris-Euphrates valley.

“In the words of Herodotus Egypt is the gift of the Nile. There are actually two gifts: silt and water. The Delta and the narrow flood plain . . . are deposits laid down by the mighty Nile, whose water and sediments are derived from central and eastern Africa. The silt comes mainly from the steep and rugged volcanic highlands of Ethiopia, lashed each summer by the torrential monsoonal rains rolling in from the Indian Ocean. The downpours scour the slopes, scraping off their loose mantle of mineral-rich brown soil and splashing it into the boisterous annual flood of the Blue Nile. The good fortune of Egypt is thus derived from the misfortune of Ethiopia. Added to that silt is the humus contributed by the White Nile from its jungle and swampy sources. When in ancient times the gathering annual flood reached Egypt proper, it would overflow its banks and deposit an amount of silt estimated to have been about one millimeter thick on the flood plain. This amount was not so excessive as to choke the irrigation canals or cover young seedlings, but it was fertile enough to add nutrients to the land and nourish its crops.

“Whereas in Mesopotamia the inundation usually comes in spring, and summer evaporation tends to make the soil saline, the Nile [flood] begins its rise in the middle of August and attains its maximum height at the beginning of October. In Egypt the inundation comes at a

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13 Hillel, op.cit., pp. 83-84
much more favorable time: well after the spring harvest, and after the summer heat has killed weeds and aerated the soil.

“The narrow floodplain of the Nile (except in the Delta), as well as the deep-cut nature of the riverbed, made it impractical to divert and convey water in long canals as was done in Mesopotamia. Thus there was no widespread raising of the water table. The water table was controlled by the stage of the river, which over most of its length, normally lies below the level of the adjacent land. When the river crested and inundated the land the seepage naturally raised the water table. As the river receded and its water level dropped, it pulled the water table down after it. This all-important annual pulsation of the river and the associated fluctuation of the water table under a free-draining floodplain created an automatically repeating self-flushing cycle by which salts were leached from the irrigated land and carried away by the Nile itself.”

Although the Nile valley water table has been rising very very gradually, so too has the land adjacent to it because of the silt left on it. Thus, the water table has been low enough to halt salinization. Neither the regions of northern Mesopotamia nor the Nile Valley have the severe problem of salinization that the southern plain of Mesopotamia suffers from, where the supposedly most ancient civilizations of so-called Sumer and later Akkad were situated.

The second problem associated with salinization of the southern Mesopotamian plain is that its soils are made of very tiny alluvial silts which tend to hold its water. Water is cohesive in that it tends to hold together and forms drops on surfaces. It is also adhesive in that it tends to adhere to materials with which it comes into contact. This can be seen at the edge of a glass partly filled with water. Around the edge where the water meets the glass, it rises slightly. Therefore, in a plain made up of tiny alluvial particles, adding water by irrigation will raise the water table fairly rapidly since the soil will tend to retain its water. Soil made up of large particles with large spaces between them, on the other hand, is highly permeable, that it, it permits the water to seep away:

“"The pattern of deposition [of silt] is such that the coarser particles settle closer to the [upland river’s] source and form more permeable soil, whereas the finer particles are carried farther and settle

14 ibid., pp. 90-91
downstream to form less permeable soil. The latter is, of course, more difficult to drain.”

In the final analysis, farming in southern Mesopotamia (Iraq) is not likely to continue for even a thousand years. As Adams comments:

“Salinization is a generic problem for Iraq [and southern Mesopotamian] agriculture. Among contributing factors are high evapotranspiration rates [loss of water in crop plants through leaves] caused especially by extreme summer temperatures, limited surface [water] runoff owing to very low alluvial gradients, [which cause irrigation water, rather than flow away, to sink into the soil and thus raise the water table] and even more limited groundwater as a result of fine sediment size and poor structure. Thus in a very short time, a few decades at most, the land will be unable to produce crops.”

The question of chronology now rears its head. How long could the farmers of southern Mesopotamia have maintained their fields to support the populations of the cities found there and which supposedly flourished there for about 3000 years? This we will soon see is quite impossible. How then do the historians explain away this problem? Hawkes deals with it thus:

“There was one very serious problem of soil conservation [for agriculture in southern Mesopotamia]. The silt [from irrigation] fertilized the fields, but the water also carried salt and gypsum. If it was allowed to lie in stagnant pools and evaporate, it [seeped into the soil and] caused salinization of the soil that greatly reduced its yield. Drainage channels were therefore needed to lead away surplus water, a process made very difficult by the high water table.”

If one digs a channel down below the water table and grades it away, the groundwater will flow down the channel, away from the fields, and lower the water table. This has been done with modern irrigation systems and is quite effective. However, although Hawkes admits that “drainage channels were . . . needed,” she does not say that the farmers of southern Mesopotamia actually built these drainage channels. The literature, nevertheless, is replete with statements by

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15 ibid., p. 85
16 Adams, op. cit., p. 5
17 Hawkes, op. cit., p. 94
the authorities that drainage channels were not employed by these ancient people. Jacobsen states:

“Extensive drainage systems to lower the watertable . . . as far as the sources allow one to judge were not known and used by the ancients.”

Michal Artzy of the Department of Archaeology at the University of Haifa, Israel, and Daniel Hillel of the Department of Plant and Soil Sciences at the University of Massachusetts at Amherst state:

“The only way to break this vicious cycle of temporary leaching [via irrigation] followed by resalinization is to introduce a system of artificial groundwater drainage . . . This can be achieved by digging and maintaining open ditches, or by installing sectioned or perforated pipes in the subsoil. In either case, an outlet must be provided for disposing of the excess groundwater and thus preventing the water table from rising . . . Where the topography does not facilitate disposal by gravity, pumping must be provided.

“Even where a drainage system is installed, there remains the problem of how to dispose of the brackish [salty] drainage water. Dumping it back into the river merely salinizes the water supply for users downstream. Ultimately, the brackish drainage water must be led to the sea (which may be far away), or to tracts of land specially designated to serve as evaporation basins. Altogether, artificial groundwater drainage is a complex and very expensive operation, requiring precise and simultaneous control over the dynamics of surface waters, soil moisture and groundwater. . . . That control is difficult to achieve even today, and was practically impossible in ancient times. That is why we find that large areas which once were productive farmland are now salt-encrusted barren deserts.”

The authors go on to show that in one area irrigation did operate, though for very special reasons, but not in southern Mesopotamia:

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18 Jacobsen, op.cit., p. 56
“One example where the ancients apparently did manage to avoid salinization is in the Mandali region where the site of Choga Mami is located. . . . That area is situated between two rivers where sharp differences in land levels exist. The geographical configuration allowed one river, the one placed higher, to be used for the irrigation of the fields, whilst the lower river could act as a natural drainage outlet. . . . What is true in the Mandali area, however, does not hold in the low-lying parts of the Tigris-Euphrates Valley. In this connection, the authors note that until very recently no artificial drainage facilities had existed in Iraq at all.”

The problems with drainage are manifold. The slope of the plain in southern Iraq is “a very slight slope (of less than 1:1000)”21. That means for every kilometer [3,280 feet] of horizontal distance away from these rivers the land drops one meter [3.28 feet]. For every mile (5,280 feet) the land drops 5.28 feet. Such a slope, even if it existed, would hardly carry much groundwater away. To drain the water back to the Euphrates is improbable because one would have had to pump the water uphill.

The other method of lowering the watertable is fallowing and leaching. This concept was presented in M.A. Powell’s paper, “Salt, seed and yields in Sumerian agriculture: A critique of the theory of Progressive Salinization,” in Zeitschrift der Assyriologie, vol. 75 (1985), pp. 7-38. Artzy and Hillel summarize it as follows:


“Powell mentions two methods which may have been used to combat soil salinization: fallowing and leaching. He quotes text DP 573 wherein a field is actually named ‘salt ground field’ (GÁNA kímum), which is described by the term ‘dag giš-bar’ that Jacobsen . . . had identified as some type of fallow. He then poses the question: Why

20 ibid., p. 237
21 ibid.
would that field be in fallow? And why bother to survey it at all if the soil was so salty as to merit the name ‘salt ground’? Powell proceeds to answer that the land had been reclaimed and therefore text DP 573 ‘. . . does not support the theory that Sumerian agriculture was a helpless victim of progressive salinization’ (Powell 1985, p. 37). He then introduces the term ‘kidurus’ which apparently refers to land that has been soaked in water [to leach out the salts] before plowing. Powell believes that ‘the usage of this term resembles so closely the antisa
ilinization process known as ‘leaching.’ The Sumerians, he therefore claims, must have known how to cope with salinization, as indicated by a pre-Sargonic field called DÛNUH in which some parts of the field were recorded as saline and in the same year some parts were flooded, plowed and planted with emmer (Powell 1985, p. 38). However, we are not told what the yield was for that year. Nor does Powell tell us just how the Sumerians could have leached their salt-affected fields in view of the inexorable water table rise which must have resulted from the flood-irrigation of low-lying fields in the absence of adequate natural or artificial drainage.”  

The fallowing-leaching process is supposed to work in the following way: after a field has been planted, irrigated and harvested, it is left fallow for a full year. During that time phreatophytes, which are native weeds that send down deep roots to the water table, transpire water through their leaves during the year and thereby lower the water table by five to six feet. This done, water cannot be carried upward sufficiently by capillary action to enhance the salinization process. The next year, the field is flooded and the water leaches the salts of the past year down to the water table, so that the crop can grow. This fallowing-leaching process, according to Powell, will allow the ancient farmers of the southern Mesopotamian plain to successfully deal with the salinization of their soil.

Unfortunately, this process has very little to do with reality, as Hillel explains:

“In ancient times, however, practically the only method for dealing with salinization was a system of alternate-year fallowing. At the end of an irrigation season, the water table is typically within a half-meter [20 inches] to a meter [40 inches] of the surface. If the land is kept fallow for a year, it is normally invaded by phreatophytes—the
native weed capable of sending down roots to the water table and drawing water from it. These plants can lower the water table to a depth of perhaps two meters, thus lessening the capillary rise of water and the migration of water-borne salts toward the surface. Drying out the subsoil in this manner facilitates the downward leaching of salts from the soil by rainfall or subsequent irrigation.

“But while the ancient practice of alternate-year fallowing may have retarded salinization, it could not prevent it. At each alternate-year irrigation season, more salt was added to the subsoil and the groundwater. In the absence of sufficient natural or artificial groundwater drainage, the fallowing system gradually lost its effectiveness. The inevitable result was progressive soil salinization.”

The problem is that the salt in the groundwater and subsoil from previous irrigations does not drain away. It remains in the subsoil and groundwater making both more and more saline. Therefore, when irrigation waters flood the field, the water table is raised to a level where the more saline groundwater added to the more saline subsoil rises close enough to the surface that capillary action carries these greater concentrations of salt water upward. Again, the water is heated by the sun and evaporates away, leaving behind a greater amount of salt. Alternate-year fallowing only slows the process somewhat; it does not, however, remedy the problem of salinization!

How long, then, could the so-called Sumerians and Akkadians have carried on irrigation agriculture? According to Adams, “in a very short time, a few decades at most, the land will be unable to produce crops.” Artzy and Hillel state:

“We can illustrate the intrinsic nature of the soil salinization process under irrigation by citing a few typical figures. If the quantity of irrigation is, say, no more than 500 millimeters [20 inches] (equivalent to 5000 cubic meters per hectare per year), and the water contains no more than 200 parts-per-million of salt (both these figures are very conservative), then the amount of pure salt added annually to the field totals 1000 kilograms (1 ton) per hectare [about half a metric ton per acre]. If there is no natural or artificial drainage, this salt remains in the soil, concentrates the soil solution until it becomes too

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23 Hillel, op.cit., p. 86
24 Adams, op.cit., p. 5
saline for most crops, and builds up over the years to enormous quantities. If mixed uniformly in the root zone, the accumulation can constitute 1% of the soil mass within 50 years. If, however, the salt rises to the surface, as it does by capillary action when the water table is very high, the same amount of soil can form a crust several millimeters thick, enough to make the soil in effect sterile by preventing seed germination. In actual situations, to be sure, some natural drainage does occur, so the salinization rate is not likely to be so rapid.

“However, whether the time span of salinization is 50 years, or 100, or even 500, the result is always the same: sooner or later, the soil begins to lose its intrinsically high productivity and eventually it becomes completely unproductive.”

Again, about 3000 years of continuous farming to feed the population of the various cities, towns, and villages in southern Mesopotamia is not possible. Using conservative figures Artzy and Hillel can conceive of no more than 500 years. The only fairly accurate estimate this author has found for the salinization of the soil for southern Mesopotamia was reported by David and Joan Oates. They cite J.C. Russell’s paper, “Tillage Practices in Iraq” (Abu Ghraib, 1957; mimeographed).

“Russell (1957) has calculated that the ancient fallow and idle land system with the aid of legumes whose deep roots remove water, such as shok and agul, enable farmers to use land for about 450 to 500 years. Then salt concentrations reach a point where the land to be reclaimed [for agriculture] must be abandoned for a very lengthy period during which the ground-water tends to fall to a depth of five to seven metres [16 1/2 to 23 1/3 feet] and further extensive leaching [of salt] becomes possible. . .

“[This] type of weed-fallow cultivation that we have just described is applicable only in the more northerly parts of the alluvial plain.”

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25 Artzy, Hillel, *op.cit.*, p. 236
These citations suggest that in a few to several generations the land became too saline and the farmers had to move to new areas to repeat the process. Thus over a few hundred years the entire region would become too saline to carry on irrigation agriculture.

Oates and Oates go on to explain what is done nowadays to reclaim saline soil in southern Iraq where the salt has made the land sterile:

“In the marshy regions of the river delta, where the land gradient is virtually nil and ground-water always high, a method of ‘isolating’ salt areas is now employed. Extra irrigation water is applied to cultivated shitiue crops in order to leach salt from the surface soil.”27

In essence, only about 450 to 500 years of cultivation are possible in the northern part of the plain before the land must be abandoned “for a very lengthy period” so that the water table can drop to depths of 16 1/2 to 23 1/3 feet. But on the plain itself the water table cannot fall to such depths because, as we are told:

“Badly salinized lands [in ancient Mesopotamia] frequently must have gone out of production almost indefinitely, for even in modern times their reclamation depends on slow, expensive, carefully controlled methods of drainage and flushing.”28

Artzy and Hillel make it quite clear that the ancients in ancient southern Mesopotamia could not reclaim land that had become sterile because of salinization.

“When first noticed, soil salinization can be dealt with somehow, by substituting more tolerant crops . . . and by increasing the frequency and quantity of irrigation. But the incessant rise of the water table and the upward migration of the salt-bearing water in response to the high evaporative demand (characteristic of the arid climate) inevitably win out in the end. After irrigation is abandoned, the land may recover slowly if there is sufficient rainfall and natural drainage. However, where there is insufficient rainfall and especially where drainage is impeded, soil salinization is practically irreversible, unless an intensive program of land reclamation is instituted.”29

27 ibid.
28 Adams, op.cit., p. 152
29 Artzy, Hillel, op.cit., p. 236
All of this had to occur south of Baghdad where the civilizations of the so-called Sumerians, Akkadians, and Babylonians were located. “The salt problem is most serious below Baghdad, where the valley of the Euphrates and Tigris widen to form a single, vast plain.” And, there is also no doubt that in this region there is “very low rainfall” to allow for leaching salts from the soil, and very poor drainage.

Once all the soil in the region, after a period of 300 to 400 years, became sterile, it could not be reclaimed unless the ancient farmers had modern methods to do this, which, of course, they did not. Yet we are expected to accept as a reality that in southern Mesopotamia these lands somehow did not become sterile from salinization for about 3000 years or more. That is, in Uruk times (fourth millennium B.C.) and Ubaid times (fifth millennium B.C.), irrigation was carried out even before the birth of the so-called Sumerian civilization. As Oates and Oates report:

“We have no direct information concerning the early ‘Ubaid economy, but there can be little doubt that the inhabitants of the settled villages of this phase cultivated wheat and barley with the aid of simple irrigation techniques. This is not as surprising as it once seemed since we know that the Samarran farmers of central Mesopotamia had been well practiced in such techniques for some centuries and, by the time of the earliest ‘Ubaid villages, were capable of constructing and maintaining true canals four to six meters [13 to 20 feet] wide and a number of kilometers in length.”

These irrigation systems grew through the Ubaid and Uruk periods as the villages, towns, and cities grew in population and more and more land had to be cultivated via irrigation to sustain these growing populations. Thus, we have about 3000 years of irrigation farming in southern Mesopotamia.

What must be emphasized is that irrigation was carried out around villages, towns, and cities throughout these 3000 years. These were the same villages, towns and, most importantly, great cities that supposedly remained viable through all this time because of irrigation agriculture that existed around them. But if irrigated farming was concentrated about these cities, etc., then these lands would

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30 ibid., p. 237
31 ibid.
32 ibid.
have been the first to be destroyed by salt and this destructive process should have radiated outward and away from the cities as they became bigger and required more crops to sustain their people and in part also their domesticated animals. Salinization should have, under these circumstance, begun to destroy these lands even before the on-set of the dynastic period.

The ubiquitous nature of these canals throughout southern Mesopotamia is well known:

“The construction of the main canals, that might be twenty-five yards [75 feet] wide and run for many miles, was indeed so great a task that it was a royal responsibility. Most of the kings of Sumer and Akkad who had any claim to greatness built or restored canals. . . .

“The first recorded canal in all history is still in partial existence. This is the Al-Gharrif, an apparent branch of the Tigris at Kut. It was in fact cut by a governor of Lagash before the middle of the third millennium B.C., as we know from a surviving account of his project. For a later example it is recorded under the ninth regnal year of Hammurabi [around 1780 B.C.] that ‘The canal [called] Hammurabihegal [was dug]’; under the twenty-fourth year, ‘he redug the ‘Flowing-Vase’ canal for [the god] Enlil, and also [the bed of] the Euphrates’, while the entry for the thirty-third year reads, ‘He redug the canal [called] Hammurabi [spells] abundance-for-the-people, the Beloved-of-[the gods]-Anu-and-Enlil, [thus] he provided Nippur, Eridu, Ur, Larsa, Uruk and Isin with a permanent and lasting water supply.’”

And with these great canals in operation over millennia we are expected to believe that such immense irrigation projects did not render the land sterile with salt through all this time.

In fact, there exists a map of the ancient farming community, supposedly dated to 1300 B.C.:

“A 1300 B.C. map on a clay tablet . . . depicts farmland outside the Mesopotamian city of Nippur. At the direction of a king, a scribe . . . diagrammed the area’s fields and irrigation canals . . . The man-

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34 Hawkes, *op.cit.*, p. 93
made irrigation system, a huge ‘V’ with offshoots, enclosed the king’s own field” . . . 35 (see figure 9)

Figure 9 Irrigation system

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35 Norton Leonard, *op.cit.*, p. 132
Since salt will destroy all agriculture on the plain in about 300 to 400 years, we can get some idea as to when this occurred from its earliest mention. Jacobsen writes:

“Shortly before the Proto-imperial Period . . . Excavations at Girsu . . . have recovered an extensive archive dating to the Proto-imperial Period . . . [these] show conclusively that salinity at that time was already a problem and had rendered parts of the temple’s fields unusable for cultivation.”\textsuperscript{36}

Jacobsen goes on to point to the same problem at other sites for this period.\textsuperscript{37} Therefore, one would expect to learn that irrigation agriculture ended a few hundred years later in southern Mesopotamia. But this is not the case. The Proto-Imperial Period extends to perhaps 2400 B.C.:

“The earliest mention of salt as an agency destructive of cultivation occurs in documents from Girsu as early as 2400 B.C. and continues to \textit{ca.} 2100 B.C. when the evidence available suggests sporadic salinity to have been present over most of Southern Babylonia from the Euphrates in the West to the Tigris in the East. The salt involved (\textit{mun}) seems to be the white nondeliquescent sodium chloride.

“After 2100 B.C. for some eight hundred years the sources fall silent about salt and salinity.”\textsuperscript{38}

Therefore, based on the established chronology, all the land should have become sterile from salinity soon after 2400 B.C. But some of the great cities lasted, based on the same chronology, to about 1700 to 1600 B.C. What we have is the assumption by the historians that the so-called Sumerian and Akkadian civilizations located on the southern Mesopotamian plain were able to carry on agriculture long after about 2500 to 2400 B.C. when their land had gone out of use. We have them carry on agriculture to about 1700 B.C. That is, for 700-800 years after the land had lost its fertility by becoming salinized, these ancient farmers were still farming their land.

This is made explicit by Adams who states:

\textsuperscript{36} Jacobsen, \textit{op. cit.}, p. 9
\textsuperscript{37} \textit{ibid.}
\textsuperscript{38} \textit{ibid.}, pp. 12-13
“Badly salinized lands [in ancient Mesopotamia] frequently must have gone out of production almost indefinitely. . .

“This means loss of the formerly cultivated area [which] intensified the pressure [to utilize and irrigate] less affected districts and thus must have extended the [salinization] problem. Serious as was the decline in average yield by the Ur III period, it subsequently went on to become catastrophic. Jacobsen has shown that by 1700 B.C., shortly before an extensive abandonment of southern Babylon, yields around ancient Larsa had slipped . . . this represents less than a fifth of the expected yields eight hundred years earlier. . .”

As one can clearly understand the ancient people carried on irrigation agriculture all through the Ubaid period of the 5th millennium B.C., through the Uruk period of the 4th millennium B.C., through the Proto and Pre-dynastic periods of the early 3rd millennium B.C., and through dynastic Sumerian and Akkadian times, down to 1700 B.C., a length of around 3000 years or more.

Woolley tells us about the city of Ur

“Much of the old city remains still untouched, but the Sacred Area with its temples and various sites within the town walls have been cleared and the work carried down through layer after layer of historical remains, in some cases to virgin soil; the buildings and the graves which we have found represent successive stages in a life-history of not less than four thousand years.”

Of course, the question of how they were able to do so must be explained by those involved with ancient history. The historians know that, at best, irrigation agriculture on the southern plain can last no longer than 300 to 400 years, but they also maintain that this form of agriculture was carried on for 3000 years. There is a great contradiction here that demands an explanation.

How do the historians answer this question? They simply refer back to the chronology they began with and evade the impossibility of their chronology by

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39 Adams, *op.cit.*, p. 152
40 Woolley, “Ur and the Sumerians,” *The World of the Past, op.cit.*, p. 343
clinging to that chronology. Adams, who understood all of this, states that in southern Mesopotamia

“irretrievable losses of [agricultural] productivity are not suggested by either the historical or archaeological record—even in the absence of the massive drainage programs on which modern agricultural development increasingly relies.”

Today where agriculture in Iraq and Pakistan is employing massive irrigation projects, the land has begun to become saline in less than a century.

“On entering Iraq, the Tigris and Euphrates contain about 30 parts per 100,000 of salts; in their lower courses this proportion trebles. It was estimated in 1949 that 60 percent of all irrigated land in Iraq had become salinated, and an overall assessment is that 1 percent of the total cultivated area is abandoned each year because of salinity.”

That is, in about a century nearly all the cultivated land of Iraq, both in the north and especially in the south, would be abandoned unless drainage and other urgent measures were employed to reclaim the soil. These methods did not exist in ancient southern Mesopotamia. This implies that with less irrigation in ancient times, the entire region of so-called Sumer would be destroyed in a few hundred years.

Without massive drainage systems and other controls, all these modern lands would become sterile in several decades to a century or two at most. But in southern Mesopotamia, after thousands of years of irrigation without massive drainage systems to lower the water table—the very same conditions—the same type of land miraculously did not become sterile over millennia! The science proclaims, in undeniable terms, that the ancient so-called Sumerian and Akkadian civilizations had to have begun their development well over a thousand years closer to the present. The historians, in uncompromising terms, have overlooked or denied the evidence of science to uphold their scientifically impossible chronology. To paraphrase John W. Gardner:

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41 Adams, op.cit., p. 5
A historian who understands but ignores the evidence that irrigation farming in the so-called Sumerian civilization will destroy its soil, and with it its agriculture, in less than 400 years, but who still exalts and maintains the established chronology, has gone beyond the bounds of science. While the ancient irrigation canals surely did hold water, the historians’ chronology of the oldest civilization on Earth does not. That chronology has been falsified by well-reasoned and well-established scientific facts. The historians who scorn these facts are figuratively walking on irrigated water.

Let us for the moment assume that the ancient so-called Sumerians could maintain their soil against the destructive elements of salinization for thousands of years. This, of course, is based on a process that is totally unknown today. But if they could miraculously do this, one must wonder why the agricultural base collapsed from the salinization of the soil in the first place. Almost 3000 years of irrigated agriculture in southern Mesopotamia is unrelated to reality, but even the physical anthropologists who have dealt with this matter have failed to see what their own research into the salinization of soil clearly states. They have failed to explain how millennia of irrigation in southern Mesopotamia could have gone on without destroying the soil in about 300 to 400 years. But even if we allow a thousand years before the destruction of crop agriculture, the established chronology simply fails, and we do not allow even this length of time as a possibility.

The point that must be emphasized is that ancient salt-blossom patterns are well observed from airplanes above the plain, as Hillel pointed out earlier. Had there actually been a natural process that could leach away the salts from these blossom patterns, say, leaving the fields fallow for centuries or millennia, to permit the water table to drop so that the scant rainfall gradually leached out and removed these patterns, they would not be there presently. But quite obviously, they still persist, and this demonstrates that once the soil was ruined for crop agriculture because of irrigation salinization, nothing could restore it to its former fertility.

As we indicated above, Artzy and Hillel made it quite clear that “where there is insufficient rainfall and natural drainage and especially where drainage is impeded, soil salinization is practically irreversible.” This position is also maintained by R. McC. Arnon in *Crop Production in Dry Regions*, vol. I. M.P. Charles discusses this in the *Bulletin of Sumerian Agriculture*:
“Arnon (1972) p. 236 expressed doubts about the possibility of maintaining irrigation agriculture permanently.”

In the same manner that Adams told us above that “irretrievable losses of [agricultural] productivity are not suggested by either the historical or archaeological record—even in the absence of massive drainage programs . . .”, Charles argues: “Yet irrigation agriculture is seen to have continued . . . despite all [the] soils are saline, most of them even strongly saline and large areas are out of production.”

This is not science. This assumes all that must be proven. By turning to the established chronology as their reason these researchers have ignored and evaded the issue of soil salinization.

The southern plain of Iraq has lain fallow for a great length of time. If leaving fields fallow allows the soil to be leached and regenerated, then this region today would be arable and crops could be grown there via irrigation. But the vast stretches of this region with salt blossoms, in spite of long-term fallowing, except for the delta marshes on the Persian Gulf, are not arable. The land is a desert incapable of supporting crops of any significance. If the fallowing method was valid then the land would be arable and under cultivation today.

The only explanation this author has found is that as the land became salinized the people moved to new land. As Seton Lloyd states: “When this [salinization] happens to land in an ‘extensive economy’, the farmer simply transfers his cultivation to new ground and starts irrigation all over again.” This would only make sense if this evidence was supported and corroborated by all the earlier evidence presented in this book. It does not. If the so-called Sumerians and Akkadians lived 4500 to about 1700 B.C., then they could not have had steel to work diorite. The fact that they did work diorite means that they did not live in that region for such a long a time. If they lived there from 4500 to about 1700 B.C., then they would not have had tin to make tin bronze. The fact that they did make tin bronzes means that they did not live in that region for such a long time. The fact that they used pottery that Dayton showed, by cross-reference to Egypt, needs to be dated to the first millennium, again contradicts that they lived in that region for such a long

45 ibid.
46 Lloyd, *op. cit.*, p. 19
time. The fact that they owned iron daggers before the development of iron points to the same condition. All these other scientific and technological facts contradict the long chronology with respect to how long the so-called Sumerians irrigated their homeland farms. All these other scientific and technological facts fully support and corroborate the short chronology with regard to the length of time they carried out irrigation agriculture. The totality of this scientific and technological evidence is what must be answered; it is not enough simply to state that the so-called Sumerians took over new lands. The totality of this evidence has never been dealt with. The reason this chapter was placed at the end of all this other material rather than at the beginning was to raise just this point. All these facets of science and technology are a remarkable observation that contradicts the concept that these so-called Sumerians merely moved when salt ruined their land.

No one, so far as this author has read, has explained away this remarkable observation staring every researcher of this problem in the face. If the soil could be reclaimed in ancient times and up to the present, why are these salt-blossom patterns still spread across the southern Mesopotamian plain? Neither the historians nor the physical anthropologists have any real evidence with which to confront this observed contradiction to their belief (and it is only a belief) that the ancients could maintain irrigation agriculture for millennia in the face of rampant salinization that must ensue all across the region in about 300 to 400 years. The only explanation that I have read claims that the millennia-long chronology of so-called Sumerian civilization demands that these ancient people, for some completely unknown reason, were capable of doing so. But then, why did their civilization collapse if they could still raise crops because they knew how to or because nature corrected the salinization problem?

If present-day Iraq with massive modern intervention techniques is losing one percent of its arable land each year, meaning its soil will be completely salinized in a century, it becomes clear that in ancient times this process without massive intervention would destroy all its arable land in a few centuries.

What we have here, in terms of irrigation agriculture in southern Mesopotamia going on for millennia, is what Fischer terms the

“fallacy of false extrapolation [which] is a statistical series which is stretched beyond the breaking point. It occurs in a variety of forms.
The most clear and simple is a generalization from a true series A, B to a false A, B, C.”47

In our case the true series A, B is that A equals the period in southern Mesopotamia prior to irrigated farming, while B equals the 300 to 400-year period after A when all the soil becomes too salty to grow crops. The false A, B, C generalization allows C to maintain that even after the ancients could not grow crops in that salt-ridden soil, they did! The extrapolation is false and even absurd. A comical specimen of the fallacy of the false extrapolation was presented by Mark Twain who stated:

“The Mississippi between Cairo and New Orleans was twelve hundred and fifteen miles long one hundred and seventy-six years ago. It was eleven hundred and eighty [miles long] after the cut-off of 1722. It was one thousand and forty after the American Bend cut-off. It has lost sixty-seven miles since. Consequently, its length is only nine hundred and seventy-three miles at present.

“Now, if I wanted to be one of those ponderous . . . [historians] and ‘let on’ to prove what had occurred in the remote past by what had occurred in late years, what an opportunity here! Geology never had such a chance, nor such exact data to argue from! Nor ‘development of species,’ either! In the space of one hundred and seventy-six years the lower Mississippi has shortened itself two-hundred and forty-two miles. That is on average a trifle over one mile and a third per year. Therefore, any calm person, who is not blind or idiotic, can see that in the Old Oölitic Silurian Period, just one million years ago next November, the lower Mississippi River was upward of one million three hundred thousand miles long, and stuck out over the Gulf of Mexico like a fishing-rod. And by the same token any person can see that seven hundred and forty-two years from now the lower Mississippi will be only a mile and three-quarter long.” 48

Stretching the length of the Mississippi is no different than stretching the length of the Sumerian and Akkadian civilizations from their earliest prehistoric period of irrigation farming in the fifth millennium B.C. By the same token any calm person who is not blind or idiotic can see that the extrapolation made by the

47 Fischer, op.cit., p. 120
48 ibid., pp. 121-122
historians and physical anthropologists is a fishing rod held out over the Euphrates which has landed a whale. It is a fish story. It can only be accepted by swallowing a desert of salt.

In the final analysis, one can clearly see that the oldest civilizations of Mesopotamia—the so-called Sumerian and Akkadian—could never have carried on irrigation agriculture for millennia as is required by the established chronology. The short chronology of Heinsohn and Sweeney has the Chaldean/Sumerian civilizations reigning for only a part of the first millennium B.C. Therefore, their chronology has, it seems, no problem related to irrigation agriculture. When we add all the other forms of scientific and technical evidence discussed above to this material regarding salinization, we discover that all these forms of evidence speak quite forcefully for their thesis. The scientific and technological facts support their chronology; they certainly do not support the long-established chronology.

**ADDENDA**

Based on the short chronology that suggests that the major civilizations began some time after 1400 B.C., probably 1200-1100 B.C., we still have to account for an Ubaid and Uruk period as well as a proto- and pre-dynastic period, not of millennia but of some hundreds of years before the rise of the Sumerians whom Heinsohn and Sweeney equate with the Chaldeans. Even on the basis of this short chronology these lands would have become unavailable to agriculture by around 1000 to 900 B.C. Thus we are still faced with the problem of maintaining agriculture via irrigation well into the 1st millennium B.C. The short chronology, too, must explain how irrigation agriculture was able to last in that region for perhaps 800 years and not destroy the soil. It is the same problem facing the established chronology that needs an answer.

The only way for ancient southern Mesopotamian civilization to last longer than 300 to 400 years is for it to operate under different climatic conditions.

The position taken by this author, based on Velikovsky’s work (to be presented below) is that around 1500 B.C. the rotational axis of the Earth had moved from an eight or nine degree tilt to about fourteen to eighteen degrees. This would place the ancient river valley civilizations about ten and a half to five and a half degrees farther north than they are at present. They would be situated at a distance of about 730 to 380 miles north of where they are located today. After 800 B.C. an
axial tilt caused by the Mars event would bring the tilt of the Earth to its present position.

Under this condition, the regions adjacent to these river valleys would not be anywhere as arid and desiccated as they are at present. This would require an adjustment in the lives of the people dwelling in such areas. Hunter-gatherer methods would not have sufficed to sustain the population because the land was not as fruitful as it had been prior to the 1500 B.C. axial change. Forests would be reduced greatly. Animals dependent on forest-type environments could only survive by reducing their numbers through starvation, and the various herbs, roots, and berries accessible to the hunter-gatherers would either have disappeared or have been greatly reduced. With these conditions the people would be forced to migrate to river valley regions or quite near to them and adopt an agricultural way of life to sustain themselves. Rainfall in these more northern latitudes would naturally be greater prior to the eighth century B.C. than after that time. Sometime after the eighth century B.C., all these regions would have been moved to their present latitudes and the great desiccation would have set in, creating the problem of salinization.

In this respect, Velikovsky has presented evidence from astronomy, temples and obelisks, and a water clock, to prove that prior to the eighth century B.C. the conditions just described actually occurred. He presented this evidence in his chapter “Poles Uprooted” in Worlds in Collision:

“According to Seneca the Great Bear had been the polar constellation. After a cosmic upheaval shifted the sky, a star of the Little Bear became the polar star.”

Hindu astronomical tablets composed by the Brahmans in the first half of the first millennium before the present era show a uniform deviation from the expected position of the stars at the time the observations were made (the precession of the equinoxes being taken into consideration). Modern scholars wondered at this, in their opinion inexplicable, error. In view of the geometrical methods employed by Hindu astronomy and its detailed method of calculation, a mistake in observation equal to even a fraction of a degree would be difficult to account for.

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49 J. Bentley, A historical View of the Hindu Astronomy (1825), p. 76.
In \textit{Jaiminiya-Upanisad-Brahmana} it is written that the center of the sky, or the point around which the firmament revolves, is in the Great Bear.\textsuperscript{50} This is the same statement we found in \textit{Thyestes} of Seneca.

In Egypt, too, “the Great Bear played the part of the Pole Star.”\textsuperscript{51} “The Great Bear never set.”\textsuperscript{52} Could it be that the precession of equinoxes shifted the direction of the axis so that, three or four thousand years ago, the polar star was among the stars of the Great Bear?\textsuperscript{53} No. If the earth moved all the time as it moves now, four thousand years ago the star nearest the North Pole must have been $\alpha$-Draconis.\textsuperscript{54} The change was sudden; the Great Bear “came bowing down.”\textsuperscript{55} In the Hindu sources it is said that the earth receded from its wonted place by 100 yojanas,\textsuperscript{56} a yojana being five to nine miles. Thus the displacement was estimated at from 500 to 900 miles.

The origin of the polar star is told in many traditions all over the world. The Hindus of the \textit{Vedas} worshiped the polar star, Dhrura, “the fixed” or “immovable.” In the \textit{Puranas} it is narrated how Dhrura became the polar star. The Lapps venerate the polar star and believe that if it should leave its place, the earth would be destroyed in a great conflagration.\textsuperscript{57} The same belief is found among the North American Indians.\textsuperscript{58}

The day on which the shortest shadow is cast at noon is the day of the summer solstice; the longest shadow at noon is cast on the day of the winter solstice. This method of determining the seasons by measuring the length of the shadows was applied in ancient China, as well as in other countries.

We possess the Chinese records of the longest and shortest shadows at noontime. These records are attributed to \textsuperscript{-1100}. “But the shortest and the longest

\textsuperscript{50} Thibaut, “Astronomie. Astrologie und Mathematik,” p. 6.
\textsuperscript{53} Wainwright in the \textit{Studies} presented to F.L. Griffith, pp. 379-380.
\textsuperscript{54} Cf. H. Jeffreys, “Earth,” \textit{Encyclopedia Britannica} (14th ed.).
\textsuperscript{55} Wainwright, \textit{Journ.Egypt.Archaeol.}, XVIII, p. 164.
\textsuperscript{56} J. Hertel, \textit{Die Himmelstore im Veda und im Avesta} (1924), p. 28.
\textsuperscript{58} The Pawnee Mythology (collected by G.A. Dorsey; 1906), Pt. I. p. 135.
The length of the longest day in a year depends on the latitude, or the distance from the pole, and is different at different places. Gnomons or sundials can be built with great precision.

The Babylonian astronomical tablets of the eighth century provide exact data, according to which the longest day at Babylon was equal to 14 hours 24 minutes, whereas the modern determination is 14 hours 10 minutes and 54 seconds.

“The difference between the two figures is too great to be attributable to refraction, which makes the sun still visible over the horizon after it has set. Thus, the greater length of the day corresponds to latitude 34° 57’, and points to a place 2\(\frac{1}{2}\)° further to the north; we stand therefore before a strange riddle [vor einem merkwürdigen Rätsel]. One tries to decide: either the tablets of System II do not originate from Babylon [though referring to Babylon], or this city actually was situated far [farther] to the north, about 35° away from the equator.”

Since the computations of the astronomical tablets did refer to Babylon, there is a possible solution that Babylon was situated at a latitude of 35° from the equator, much farther to the north than the ruins of this city.

Claudius Ptolemy, who, in his *Almagest*, made computation for contemporaneous and ancient Babylon, arrived at two different estimates of the

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61 A gnomon (277 feet high), built by Toscanelli in 1468, during the Renaissance, for the cathedral in Florence, shows midday to within half a second. R. Wolf, *Handbuch der Astronomie* (1890-1893), n. 164.

longest day at that city, and consequently of the latitude at which it was located,\textsuperscript{63} one of his estimates being practically of the present-day value, the other coinciding with the figure of the ancient Babylonian tables, 14 hours 24 minutes.

The Arabian medieval scholar Arzachel computed from ancient codices that in more ancient times Babylon was situated at a latitude of 35°0′ from the equator, while in later times it was situated more to the south. Johannes Kepler drew attention to this calculation of Arzachel and to the fact that between ancient and modern Babylon there was a difference in latitude.\textsuperscript{64}

Thus Ptolemy, and likewise Arzachel, computed that in historical times Babylon was situated at latitude 35°. Modern scholars arrived at identical results on the basis of ancient Babylonian computations. “This much, therefore, is certain: our tables [System II and I also], and the astronomers mentioned as well, point to a place about 35° north latitude. Is it possible that they were mistaken by 2° to 2\( \frac{1}{2} \)°? This is scarcely believable.”\textsuperscript{65}

As there was but one Babylon, its location, at some historical time, at 35° north latitude signifies that at the longitude of Babylon the earth since then has turned toward the south, and the direction of the polar axis, or its geographical location, or both, have undergone displacement.

Some of the classic authors knew that the earth had changed its position and had turned toward the south; not all of them, however, were aware of the real cause of this perturbation. Diogenes Laërtius repeated the teaching of Leucippus: “The earth was bent or inclined towards the south because the northern regions grew rigid and inflexible by the snowy and cold weather which ensued thereon.”\textsuperscript{66} The same idea is found in Plutarch, who quoted the teaching of Democritus: “The northern regions were ill temperate, but the southern were well; whereby the latter becoming fruitful, waxed greater, and, by an overweight, preponderated and inclined


\textsuperscript{64} J. Kepler, \textit{Astronomi opera omnia} (ed. C. Frisch), VI (1866), 557: “Et quia altitudinem poli veteri Babyl. assignat 35° 0′, novae 30° 31′.”

\textsuperscript{65} Kugler, \textit{Die babylonische Mondrechnung}, p. 81.

\textsuperscript{66} This is a translation by Whiston in his \textit{New Theory of the Earth}. The modern version of L.D. Hicks differs greatly.
the whole that way.” Empedocles, quoted by Plutarch, taught that the north was bent from its former position, whereupon the northern regions were elevated and the southern depressed. Anaxagoras taught that the pole received a turn and that the world became inclined toward the south.

As we have seen, Seneca in Thyestes correctly ascribed the displacement of the pole to a cosmic catastrophe.

**Temples and Obelisks**

In classic authors references can be found to the fact that the temples of the ancient world were built facing the rising sun. Orientation toward the sun is, at the same time, orientation toward the visible planets, as all of them travel through the signs of the zodiac or in the ecliptic. The sun changes the point of its rising and setting from one day to another, and the ecliptic makes a corresponding slow swing from one solstice to another. Therefore, for the purposes of accurate observation of whether the terrestrial pole shifted in a sudden way, it was necessary to build the temple observatories, not simply facing the east and the west, but with a device that would permit checking the position of the sun on the days of the vernal and autumnal equinoxes, when the sun rises exactly in the east and sets exactly in the west.

The Tractate Erubin of the Jerusalem Talmud records “the Surprising fact” that the Temple of Jerusalem was so built that on the two equinoctial days the first ray of the rising sun shone directly through the eastern gate; the eastern gate was kept closed during the year, but was opened on these two days for this very purpose. The first ray of the equinoctial sun shone through the eastern gate and into the very heart of the Temple.

There was no sun worship in this arrangement, it was dictated by the events of the past, when the position of the earth, in relation to the rising and setting points of the sun, was moved in world catastrophes. The fall equinox was observed as New Year’s day. This ceremony with the equinoctial sun was old. The

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69 *Jerusalem Talmud*, Tractate Erubin V, 22c.
Babylonian temples, also, had “the gate of the rising sun” and “the gate of the setting sun.”

With the growing belief that there would be no more changes in the world system, a belief expressed also by Deutero-Isaiah (66:22), the eastern gate of the Jerusalem Temple was closed forever: it will be opened in Messianic times.

Although unaware of these ancient practices and literary references to the orientation of the temples, a writer of the end of the nineteenth century came to the conclusion that the temples of the ancient world faced the sunrise. He found considerable evidence in the position of temples, but he wondered also that there were deliberate changes in the orientation of the foundations of some older temples.

“The many changes in direction of the foundations at Eleusis revealed by the French excavations were so very striking and suggestive” that the author asked “whether there was possible astronomical origin for the direction of the temple and the various changes in direction.”

Further investigation by other authors revealed the fact that generally only the temples of a later time faced the east, and that earlier temples, built before the seventh century, had their foundations purposely directed—the same orientation can be traced in a number of archaic foundations—away from the present east.

Knowing by now that the earth repeatedly shifted the direction of the sunrise and sunset, we understand the changes in the orientation of the foundations as the result of changes in nature. Thus, we have in the foundations of the temples, like that of Eleusis, a record of the changing direction of the terrestrial axis and the position of the pole; the temple was destroyed by catastrophes and rebuilt each time with a different orientation.

Besides the temples and their gates, the obelisks also served the purpose of fixing the direction of east and west, or of sunrise and sunset on equinoctial days. As this purpose was not perceived, the object for

72 Winckler, Keilinschriftliche Bibliothek, III, Part 2 (1890), 73
73 Lockyer, The Dawn of Astronomy.
74 Ibid., p. viii
75 H. Nissen, Orientation, Studien zur Geschichte der Religion (1906); E. Pfeiffer, Gestirne und Wetter im griechischen Volks glauben (1914), p. 7. See also F.C. Penrose, Philosophical Transactions of the Royal Society of London, CLXXXIV, 1893, 805-834, and CXC, 1897, 43-65.
which the obelisks were built seemed enigmatic: “The origin and religious significance of the obelisks are somewhat obscure.”

Two pillars were erected before the Temple of Solomon, but their purpose is not revealed in the Scriptures.

In America, obelisk-pillars were built, too. Sometimes a ring was set on the vortex of the pillar for the sun’s rays to pass through. “The solstices and equinoxes were carefully observed. Stone pillars were erected eight on the east and eight on the west side of Cuzco, to observe the solstices. . . . At the heads of the pillars there were discs for the sun’s rays to enter. Marks were made on the ground, which had been levelled and paved. Lines were drawn to mark the movement of the sun. . . .

“To ascertain the time of the equinoxes there was a stone column in the open space before the temple of the sun, in the center of a large circle. . . . The instrument was called inti-huatana, which means the place where the sun is tied up or encircled. There are inti-huatanas on the height of Ollantay-tampu, at Pissac, at Hatuncolla, and in other places.”

The Egyptian obelisk could serve as a gnomon, or shadow clock. The length of the shadow and its direction would indicate the hour of the day. Obelisks placed in pairs served as a calendar. On the vernal and autumnal equinoxes their shadows would be continuous for the length of the day, the sun rising exactly in the east and setting exactly in the west.

That the purpose for which the obelisks were erected was to check on the shadow of the sun (and the position of the earth) can be plainly seen from this passage of Pliny:

“The obelisk [of Sesothis, brought from Egypt) that has been erected in the campus Martius [in Rome] has been applied to a singular purpose by the late Emperor Augustus: that of marking the

76 R. Engelbach, *The Problem of the Obelisks* (1923), p. 18
77 1 Kings 15.
shadow, projected by the sun, and so measuring the length of the days and nights.” There then follows this remark: “For nearly the last thirty years, however, the observations derived from this dial have been found not to agree: whether it is that the sun itself has changed its course in consequence of some derangement of the heavenly system; or whether that the whole earth has been in some degree displaced from the center, a thing that, I have heard say, has been remarked in other places as well; or whether that some earthquake, confined to this city only, has wrenched the dial from its original position; or whether it is that in consequence of the inundations of the Tiber, the foundations of the mass have subsided.”

The passage indicates that Pliny envisaged every possible cause, not excluding the one known to have occurred in earlier times when, in the language of Plutarch, “the Pole received a turn or inclination,” or in the words of Ovid, “Earth sank a little lower than her wonted place.”

The Shadow Clock

The poles changed their locations; all latitudes were displaced; the axis changed its direction; the number of days in the year increased from 360 to 365\(\frac{1}{4}\), a fact demonstrated in a following section: the length of the day probably also altered. Of course, a sundial or shadow clock from before —687 can no longer serve the purpose for which it was devised, but it might well be of use in proving our assumption.

Such a clock, originating from the period between circa —850 and —720, was found in Faijum in Egypt at latitude 27°. A horizontal slab with hour marks has at one end a shadow-casting, vertical hob.\(^\text{80}\) This shadow clock cannot show correctly the change of time in Faijum or elsewhere in Egypt. A scholar who investigated its working came to the conclusion that it must have been kept with its head to the east in the forenoon and to the west in the afternoon, and several scholars agreed that this was the way to use the clock. But this arrangement by itself did not make it possible to read the time. “Since all actual hour


\(^{80}\) The Egyptian day was divided into hours that represented equal portions of time between sunrise and sunset, independently of the length of the day.
shadows lie substantially closer to the hob than the corresponding marks of the instrument, the shadow-casting edge must have been higher over the shadow-receiving plane than we find it to be. The upper edge cannot be the shadow-caster of the instrument; it must have been on a parallel line above this edge.”

“The marks were also not made on the basis of actual observations, but must have been taken from some theory or other.”

But as a critic remarked, “this theory implies that at no season of the year did the clock denote the hours correctly, without an hourly alteration of the height of that part of the instrument which cast the shadow.”

As the clock has no device to adjust the height of the head, it is improbable that this hourly manipulation took place. Besides, in order to change the height of the head every hour, in itself an impractical method, it would have been necessary to have another clock to show the hours without any manipulation, thus indicating the exact moment when the first clock had to be adjusted. But if there was a clock that could show the hours correctly without adjustment, what purpose did the shadow clock serve?

Another explanation has therefore been offered for the manner in which the Egyptian sundial was used. The author of the new idea supposes that at some early date (the precession of the equinoxes being taken into consideration) the shadow clock was used at some latitude in Egypt on the day of the summer solstice. He admits: “Account has, however, not been taken of change in the declination of the sun between sunrise and sunset. . . . For other seasons of the year it would be necessary at each hour or each clock reading, either to alter the height of the hob, or tilt the st’t [clock] or both. Indeed when the sun had south declination, and even when it had slight north declination, it would always be necessary to do both. The inference is, therefore, that the clock was originally used at or near the time of the summer solstice.”

The problem of adjustment for each reading once more crops up in this

82 Ibid., p. 15.
84 Ibid.
explanation, again requiring some better means of knowing the exact time. The conclusion at which the author of this explanation arrives—that originally the clock was built for a single date in the year—is rather odd and defies the very purpose for which clocks are constructed.

And even if a clock were to be read only once a year, the author of this theory could not make the specimen found in Faijum work, but only a similar clock that had been found broken in pieces; and this he could do only by having recourse to the precession of the equinoxes and by referring the clock to a period many hundreds of years earlier than chronologists assume.

The shadow clock found at Faijum, built under the Libyan Dynasty, between about—850 and —720 before the present era, may help us to learn the length of the day, the inclination of the pole to the ecliptic, and the latitudes of Egypt in that historical period. A change in any of these three factors would have made the clock obsolete as an instrument for time reading, and probably, all three factors did change.

We do not possess the sundial of King Ahaz, but we do have the shadow clock used in Egypt in the period before the last catastrophe of —687 and possibly before the catastrophe of —747.

The Water Clock

Besides the gnomon or sundial, the Egyptians used the water clock, which had the advantage over the former of showing time during the night as well as during the day.

A complete example was found in the Amon Temple of Karnak (Thebes), 25.5° north of the equator. This water clock dates from the time of Amenhotep III of the Eighteenth Dynasty, father of Ikhnaton. The jar has an opening through which water flows out; marks are incised on the inner surface of the jar to indicate the time. Since the Egyptian day was divided into hours which changed in length with the length of the day, the jar has different sets of markings for the various seasons of the year. Four time points are prominently important: the autumnal equinox, the winter solstice, the vernal equinox, and the
summer solstice. The equinoxes have equal days and nights in all latitudes. But on the solstices, when either the day or the night is the longest of the year, the length of the daylight varies with the latitude: the farther from the equator, the greater is the difference between the day and the night on the day of the solstice. This difference also depends on the inclination of the equator to the plane of the orbit or ecliptic, which is at present $23^{1/2}\degree$. Should this inclination change, or in other words, should the polar axis change its astronomical position (direction), or should the polar axis change its geographical position with each pole shifting to another point, the length of the day and night (on any day except the equinoxes) would change, too.

The water clock-of Amenhotep III presented its investigator with a very strange time scale. Calculating the length of the day of the winter solstice, he found that the clock was constructed for a day of 11 hours 18 minutes, whereas the day of the solstice at 25° north latitude is 10 hours 26 minutes, a difference of fifty-two minutes. Similarly, the builder of the clock reckoned the night of the winter solstice to be 12 hours 42 minutes, whereas it is 13 hours 34 minutes—fifty-two minutes too short.

On the summer solstice, the longest day, the clock anticipated a day of 12 hours 48 minutes, whereas it is 13 hours and 41 minutes, and a night of 11 hours 12 minutes, whereas it is 10 hours 19 minutes. On the vernal and autumnal equinoxes the day is 11 hours and 56 minutes long, and the clock actually shows 11 hours and 56 minutes; the night is 12 hours 4 minutes long, and the clock shows exactly 12 hours 4 minutes.

The difference between the present values and the values of the day for which the clock is adjusted is very consistent: on the winter solstice the day of the clock is fifty-two minutes longer than the present day of the winter solstice in Karnak, and the night is fifty-two minutes shorter; on the summer solstice the day is fifty-three minutes shorter on the clock and the night fifty-three minutes longer.

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85 L. Borchardt, *Die altägyptische Zeitrechnung* (1920), pp. 6-25.
The figures on the clock show a smaller difference between the length of daylight on the solstices or between the longest and the shortest days of the year than is observed at Karnak at the present time. Thus the water clock of Amenhotep III, if it was correctly built and correctly interpreted, indicates that either Thebes was closer to the equator or that the inclination of the equator toward the ecliptic was less than the present angle of \(23^{1/2}\)°. In either case the climate of the latitudes of Egypt could not have been the same as it is in our age.

As we find from the present research, the clock of Amenhotep III became obsolete in the middle of the eighth century; and the clock that might have replaced it at that time would have been made obsolete in the catastrophes of the end of the eighth and the beginning of the seventh centuries, when once more the axis changed its direction in the sky and its position on the globe as well.

The implications in terms of climate for an axial tilt of from about fourteen to eighteen degrees are very clear. First, it requires that southern Mesopotamia was moved farther north into the temperate zone. But secondly, of even greater importance, a fourteen to eighteen degree tilt of the Earth’s axis would change the climatic conditions of this region. Summers in the southern Mesopotamian plain would have been cooler because the axial tilt was smaller than at present, and winters would have been warmer (see *The Extinction of the Mammoth*, by this author, page 205, for a clearer explanation of this phenomenon). Thirdly, this would have affected the rainfall pattern of the region.

The farther *this region* near the latitude of where the deserts exist is moved northward by *this axial tilt*, the more rainfall it will experience. The weak monsoon that arrives around October and lasts till about May would be stronger and last longer. There would be considerably greater rainfall throughout the year in this region. The summers would not only have been cooler, they would have been shorter as well. Therefore, southern Mesopotamia, prior to around the eighth century B.C., would not develop salinization problems for the following reasons.

The greater amount of rainfall during a year that experiences cooler and shorter summers would cause the Tigris and especially the Euphrates rivers to flow more rapidly. Greater rainfall added to these rivers increases their volume in generally the same-sized beds. The greater the volume of water in a river, the higher the water level and the more rapid the flow of its current. Cooler and shorter
summers significantly reduce the amount of evaporation so that less water is lost to the atmosphere which permits these rivers to retain even more water.

When rivers have greater volumes of water, they must flow more rapidly, and this would cause them to remove silt from their beds and carry it to the ocean. Such rivers do not form levees. Instead of the Euphrates flowing in a raised bed inside levees above the southern Mesopotamian plain, it would have the characteristics that it has farther to the north. Because the river level is below the plain, far less water will flow into the water table along its length. A raised river must permit a great deal of water to seep down into the ground at both sides, and raise the water table considerably, all along its course. All this was pointed out by Hillel.

Jacobsen has explained that under such a condition in Middle and Northern Babylonia the water table is “considerably lower and a much larger zone of dry soil separates it from the root zone above”. This, of course, greatly reduces the possibility of saline water rising to about 20 inches below the surface. With cooler, shorter summers, even with occasional floods raising the water table and capillary action then carrying the water near to the surface, the amount of evaporation will be greatly reduced and very little salt will be added to the soil. Greater rains will leach these salts away. As Jacobsen stated above, “therefore, it is understandable that the danger from salt could be kept relatively successfully under control over more than 5000 years of continued irrigation agriculture.”

If this indeed was the case, then there should exist clear forms of evidence, other than Velikovsky’s, that support this analysis. Greater rainfall on the southern Mesopotamian plain spread over a longer period of time would enhance the growth of grasses. Domesticated sheep, goats, and cattle would thrive in such an environment as they do in Northern Iraq today.

“[The northern foothills region of Iraq] enjoys a more generous rainfall [than the south] of up to 15 inches (355 or 381 mm) a year, colder winters, cooler summers . . . these lands produce abundant grain and support grazing.”

Today “annual totals vary from year to year, but mean annual rainfall in the lowlands [of Iraq] ranges from 4 to 7 inches (100-180 millimeters); about 90

percent of this rainfall occurs between November and April.” That is, more than twice the amount of rain falls in the northern and central parts of Iraq than on the southern plain.

If prior to the 8th century southern Mesopotamia had much greater rainfall than today, then it would be able to support significantly greater numbers of sheep, goats, and cattle than it does at present. Adams outlines the evidence:

“[in] the Iraqi agricultural census of 1952-53, it is instructive to consider the number of sheep and goats then held within the four [southern] provinces more or less corresponding to the ancient [Sumerian] heartland of alluvial settlement. At that time [1952-53] the total was 1,536,752, including just under 90 percent sheep and the remainder goats.”

With respect to cattle, Adams reports:

“Cattle, presently only a more limited number (in the order of 10 percent of the number of sheep and goats), must have been kept closer at hand [to the cities], because the sparse textual references to milk are almost entirely to cows’ milk. Goats’ milk, apparently obtained from flocks generally kept too far from the cities to be imported before spoiling . . .”

Milk was not the only product derived from cattle. Cheese and butter were made from raw milk. In a tablet dated to about 2000 B.C. was found an “accounting of the growth of productivity of a herd of cattle over 10 years in the time of King Sulgi.” It shows that at the end of that time the daily production was “butter 45 quarts, cheese 67 1/2 quarts.”

Thus, with 1,536,752 sheep and goats plus the ten percent cattle, we find that modern Iraq would have 1,640,427 sheep, goats, and cattle. But for ancient so-called Sumer we learn from Adams:

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88 Adams, op.cit., p. 148
89 ibid., p. 142
90 Norton Leonard, op.cit., p. 133
“Even without considering herds kept especially for meat and sacrifice [instead of for wool], the total during the Third Dynasty of Ur as hypothesized above was 53 percent greater [than the 1952-53 census]. Making generous allowances for segments of the royal herds kept across the Tigris and elsewhere, does this imply an Ur III magnitude of land use very little different from that of modern Iraq?”

Therefore, using the 53 percent greater number of sheep and goats, without cattle, we find that while today the Iraqis raise 1,536,752 animals, the ancients raised 2,351,230 sheep and goats. How could they do it on the same area of land with the same amount of rainfall as today? Under these same conditions, this is not possible, and Adams’ explanation is:

“To begin with, the contemporary pattern in which the bulk of the herds depend in the main on pasturage obtained within the perimeters of cultivation would not have been possible [for 53 percent larger numbers of animals in ancient times]. Most of the [ancient] herds instead must have spent most of the time on outlying steppelands and seasonally watered depressions, and the numbers are so large in relation to the subsistence potentialities of these types of terrain that many of the pastures were remote from the broad band of settlement and cultivation running down the center of the [river’s] alluvium. An entirely different class of settlements thus was made necessary, as yet unattested in either the archaeological or the textual record, to produce temporary or semi-permanent shelter for the herdsmen and perhaps their families.”

The fact of the matter is that there is not the slightest bit of evidence that the people took their flocks beyond the heartlands of their country. As Adams has clearly told us, the same lands with the same rainfall and pasturage as today could not maintain 53 percent greater numbers of sheep and goats. Undoubtedly, with greater rainfall (probably much greater than double that which falls presently) these ancient people could have raised far greater numbers of sheep, goats, and cattle.

If the axis was tilted less than today, as stated earlier, then the growing season would have arrived earlier, prior to 800 B.C. The growing season for crops

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91 Adams, op.cit., p. 148
92 ibid., p. 149
is not only dependent on warmth and water but, importantly, on the amount of sunlight each day. Photoperiodism can also tell us about the tilt of the Earth’s axis by the harvest time of ancient crops in that general region, and it shows that this clearly is the case. Although the following quotation is based on the accepted chronology, it is significant in terms of the short chronology:

“Another type of record of the early historical period are the inscriptions on Babylonian tablets during two periods; 1800 to 1650 B.C. and 600 to 400 B.C. Ninety tablets dated to the latter period give an indirect indication of the time of the barley harvests. Barley was used as currency in exchange for goods; the tablets detail who paid how much barley during what transaction. Interest rates, names of witnesses and the reigning king, and the date and place where the deal was made are all included in the tablets.

“Tablets for the earlier Babylonian period (1800-1650 B.C.) reveal the harvest dates more directly; they include receipts for harvest done on a certain day. Also recorded were receipts for advance payment for harvest work. Hence, researchers can compare the harvest dates of these two periods with each other and with those of the present. Providing there has been no significant change in the time barley varieties need to mature.

“Israeli researchers Jehuda Neumann and R. Marcel Sigrist have deduced from the tablets that the harvest during the period from 1800 to 1650 B.C. began late in March or early in April; during the newer Babylonian period, 600 to 400 B.C., it began about one month later. It is known that today in what was once central and northern Babylonia, and is now Baghdad, harvest begins in the second half of April.”

Prior to the 8th century B.C., harvests (as well as the sowing associated with them) came a month earlier, but after that time they arrived a month later. This of course is for the winter crop. But that changes nothing. The winter crop develops and grows well into spring which requires that it be planted so that the longer days of winter allow seeds to germinate and grow. It is exactly the same as if spring came earlier than it does today, prior to the 8th century B.C. and later after that time.

93 Stephen H. Schneider, Randi Londer, The Coevolution of Climate and Life (San Francisco 1984), p. 108
Hence, we have three forms of evidence that indicate that the tilt of the Earth’s axis was smaller prior to the 8th century B.C. than it is presently. A water clock points to that fact; the greater number of sheep, goats, and probably cattle living then than there are today, and the time of planting and harvesting, all tell the same story. But there is more.

How can one grow a winter crop without some source for water? As Seton Lloyd has told us above:

“Then come the spring floods which . . . paradoxically . . . happens between April and June, which from an agricultural point of view is too late to water the main crop, usually harvested in April.

“This was the climate regime and seasonal fluctuation with which the ancient farmers of southern Mesopotamia were faced: rain in adequate quantities at the wrong time; river water, also at the wrong time.”

Since this is so, it suggests that rainfall played a significant role both for the growing of crops as well as for irrigation. If the Tigris and Euphrates carried greater volumes of water than presently, they could more readily be tapped for irrigation since the level of the river (though lower than raised, leveed ones) would have been high all year. That is, instead of being low, sluggish streams in summer, these rivers would flow more fully throughout the year, enabling irrigation to be pursued.

The other aspect is that rainfall would have been much more significant for the growth of the winter crop. Four to seven inches could hardly have sufficed for an abundant harvest, if any at all. Therefore, rain was a necessity to grow those ancient winter crops, and the literature should contain statements that can illustrate that it was important to these ancient farmers. A tablet from King Gudea, the so-called Sumerian sovereign, addresses the gods during a period of drought:

“When Gudea the faithful shepherd puts his right hand to Eninnu my royal house, it will cry to heaven for wind and water; then will there come to thee from heaven abundance that shall triple the land. When the foundation of my house is laid, abundance shall come. Enlarged

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94 Lloyd, loc. cit.
fields shall bear for thee. . . . In Sumer . . . wool shall be weighed in abundance. . . . When thou shalt set thy right hand to my temple, I will set my foot upon the mountain where the storm dwells; from the dwelling of the storm, from a very high peak, from the holy place, abundant rain shall pour for thee, it will give the heart’s life to the land.”

Gudea did not ask for flood waters to irrigate the land. He cried for rain to give life to the land. This surely shows that rainfall was significant for the growth of crops—especially the winter harvest—in ancient times. And then, what constitutes a drought in southern Mesopotamia where there is only four to seven inches of rainfall on average? The land is always in drought with so little rain that it implies that rainfall had to be greater to generate triple abundance for the land.

In the Second Chapter, which dealt with erosion on the Sphinx, etc., it was shown that Nilometer records of early Egyptian times were “twenty-two feet higher than the highest level of today” at Samnen. This would be unimaginable if Egypt had the same climate then as it has now. This suggests, based on the short chronology, that for a relatively short period, perhaps 400 to 500 years during the second millennium, as with the evidence in Mesopotamia, there was a great deal of rainfall. Since Egypt and southern Mesopotamia are at relatively similar latitudes, this provides further evidence that to about 800 B.C. rainfall was greater in both regions.

All this evidence suggests that prior to the 8th century B.C., the poles were tilted somewhat less than they are today. Therefore, ancient so-called Sumeria could have thrived until this time, but after the poleshift described by Velikovsky, it required irrigation to maintain agriculture. By about 500 B.C. irrigation would have led to salinization of the soil and the land had to be abandoned. All this, I believe, does support the shortened chronologies of Heinsohn, Rose, and Sweeney. The historians and archaeologists all maintain that the climate of southern Mesopotamia in historical times was just what exists presently. Based on soil salinization that chronology cannot be sustained. The shortened chronology even with this addenda can.

If, as the historians maintain, there was no poleshift, then the climate in southern Mesopotamia had to be similar to that of the present, and agriculture would have collapsed because of salinization in early to pre-dynastic times.

95 Moscati, *op.cit.*, pp. 23-24
This evidence must not be taken in isolation from the other forms of evidence presented in this larger work. Those who would do so cannot be taken seriously. It is the correlations and corroboration of all the other evidence as well that point to a highly shortened chronology and that must be addressed by any thoughtful, honest critic. To answer this chapter only is a failure to deal with the overall evidence and structure of the book in which it is contained.

On the basis of this evidence it seems clear that prior to around 800 B.C. the climate of the Near East was more moderate and hospitable to man and thereafter it became desiccated and inhospitable. This, in terms of the shortened chronology, indicates that one of the great migrations that occurred happened sometime after this date.

Therefore, it appears evident that much of the lamentation literature of ancient Mesopotamia and elsewhere as well as the climatological research by modern climatologists, etc., that describe this period of desiccation, must therefore refer to the time after 800 B.C. The laments have been ascribed by historians to conquests by outside forces but the occurrence of a poleshift is clearly supported by the evidence. The goddess Ningal laments that at Ur:

“In the river of my city dust has gathered, truly they have been made into fox-dens;

In their midst the foaming waters no more flow, the workmen have deserted them;

In the fields of the city there is no more grain, the farmer has departed .. .

My palm groves and vineyards, that abounded with honey and wine, have brought forth the mountain thorn . . .

Woe is me, my house is a ruined stable, I am a herdsman whose cows have been scattered . . .”

Because Egypt had the Nile floods every year, this problem did not exist in the same way. As Moscati explains: “. . . Egyptian lyrical poetry must be ended

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96 Moscati, op.cit., p. 41
with the remark that the lamentation, in the strict sense of expressing sorrow over a destroyed city, which was a distinct literary genre in Mesopotamia, and will be again in Israel, is not found in Egypt.”\textsuperscript{97} Climatologists and historians have sometimes remarked on this. With regard to Syria, we learn:

“The question has often been asked whether the climate of Syria has undergone any radical changes since antiquity. The presence of large numbers of \textit{tells}, those flat-topped mounds which testify to the ruin of ancient towns many times rebuilt and at last abandoned, and the long lines of mounds which betray the course of ancient canal systems, points to the desiccation of once fertile and well-watered lands, and the great forests of cypress, cedar, and pine which once supplied timber for the shipwrights of Egypt and the architects of Assyria and Babylonia, have almost entirely vanished. That panthers, lions and a species of wild horse roamed the North Mesopotamian steppe is proven by animal remains from the excavations of Tell Barak and elsewhere, and elephants and aurochs were hunted in Syria . ..; the presence of such large animals implies plentiful water and a far richer vegetation than the sparse undergrowth and almost treeless conditions of the area today.”\textsuperscript{98}

This sort of evidence can be applied to Greece after 800 B.C., as described by Critias:

“What are now her mountains were lofty, soil-clad hills: the so-called shingle-plains of the present day were 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 . . . the country produced boundless pasture for cattle. The annual supply of rainfall was not lost, as it is at present, through being allowed to flow over the denuded surface into the sea, but was received by the country, in all its abundance, into her bosom . . . The shrines to

\textsuperscript{97} \textit{ibid.}, p. 135
the present day on the sites of extinct water supplies are evidence for
the correctness of my present hypothesis.”

The same condition also applies to the region of the Hittites, as O.R. Gurney writes:

“The Plateau of Asia Minor is in a sense a continuation of the
Russian steppe, and its climate is hard. Bitter winds from the north
bring heavy falls of snow during the winter months, and after the brief
but delightful spring, the country is scorched during the summer by a
relentless sun. The rain-clouds spend themselves for the most part on
the slope of Taurus or on the hillsides bordering the Black Sea. Thus,
the central plateau is a parched steppe-land, and it is only in the valleys
that enough water and shelter can be found for human habitation. One
may travel for hours over the bleak, undulating plain until one is
looking down into a well-watered valley many hundreds of feet below.
. . . In the Hittite homeland to the north . . . the streams and valleys are
more numerous and the country somewhat less bleak. Here almost
every acre is within reach of a village and is assiduously cultivated. . . .
The absence of trees on the higher ground is striking, and there is no
shelter there from the biting winds of winter. . . .

“Conditions may have been more temperate in the Hittite period.
That the people were as today mainly devoted to agriculture, is
confirmed by the texts. . . . Hittite society [was] . . . an agrarian
economy . . . We . . . have lists of fields and elaborate title-deeds
containing inventories of estates which were obviously of considerable
size.”

The same condition must now also be applied to ancient Harappan
civilization. With civilization beginning some time around 1400 to 1100 B.C., the
climate evidence meshes with the scientific and technological evidence.

With regard to the chapter on the Scythians, Cardona or others may
argue that since the climate would permit the so-called Sumerians to maintain horses
prior to the 8th century B.C., this contradicts the climate evidence presented. But

99 A.J. Toynbee, Greek Historical Thought from Homer to the Age of Herodotus (Boston 1950),
pp. 169-170
100 O.R. Gurney, The Hittites, 2nd ed. (London 1990), p. 65
the material related to horses occurs after the 8th century B.C.—in terms of Heinsohn’s thesis. Cardona has already claimed, in *AEON*, that there was no Velikovskian pole shift or other catastrophe in the 8th century to affect climate. Furthermore, all the other evidence regarding the Royal Cemetery at Ur as well as in Mesopotamia and Egypt, namely the scientific and technological evidence, are in stark contradiction to the established chronology. Until the scientific and technological evidence is refuted, there is no basis to argue one point—climate—while disregarding all the other material that speaks against the long chronology. That is a method by which one can appear to dispose of climate evidence and thus leave the impression that one need not address all the rest since the climate would have permitted thousands of horses to thrive around Ur in the 3rd millennium. They could not do so some time after 800 B.C., and this is corroborated by all the scientific and technological materials discussed above.
CHAPTER 15
DARK AGES BASED ON DARK SCHOLARSHIP

“I have myself . . . been deeply aware that the calendar dates for ancient Greece in the time-range 1000-600 BC are based on a very odd line of reasoning. Greek pottery in this period is dated by finds of such pottery made in Italy (in the Greek colonies in the west), and is based on the ‘foundation dates’ for these colonies given by such respected historians as Thucydides. But no one has ever explained to me why we should pin our faith on dates given by Thucydides for events some centuries before his own time. Nor has it been shown that the Greek pottery found on these sites was not imported (by trade) long before the colonies were officially founded there.”

Colin Renfrew
“Foreword,”
Centuries of Darkness,
Peter James et al.
(New Brunswick, NJ 1993), p. XIV

Renfrew has placed his finger on a very basic problem for dating a period of Greek history known as the Greek Dark Age. This period of time from around 1200 to 800 B.C. seems to have left almost nothing in Greece itself dating from these centuries. What Ceram says of the Dark Age of the Hittites in large measure applies to that of Greece:

“Hence historians were . . . confronted with a gap of some two hundred [in Greece up to 500] years, a period apparently an utter blank. Not a single document, not a single inscription, not a single artifact could be found which belonged to those years. . . .
“Could this possibly have been the case? Could a great and growing power have ‘relapsed into provincial insignificance’—and have remained in that condition for . . . hundred[s of] years?

“Suppose, for example, that we blanked out a similar period in the history of the western world—let us say, the years 1500 to 1700. In that case there would be no transition between the Middle Ages and modern times. Historians would find themselves surveying what appeared to be two entirely different cultures . . .

“What a task for historians to fill in by pure guesswork the possible happenings. . . . There were those two hundred years during which darkest night appeared to have descended . . . Those missing pages, which seemed to have been torn out of history, prompted them [historians] to make the wildest conjectures.

“All their theories were false.

“. . . Yet it remains astonishing that no one thought of subjecting the established chronology of events . . . to a searching criticism. Surely someone should have guessed, even if he had not been able to prove it at once, that what had gone wrong was the whole system of dating. Surely it should have occurred to someone that a people’s history cannot stop dead for two [to five] hundred years?”

Clark Whelton explains:

“Most events in the Hellenic and Hellenistic periods are firmly anchored in time. However, the Helladic period [1400-1200 B.C.]—the time of the heroes of Homer’s Iliad and Odyssey—. . . has been cut loose from the anchor by the intervening [500-year] ‘Dark Age.’ . . .”

Whelton cites Velikovsky on this problem:

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1 Ceram, The Secret of the Hittites, op.cit., p. 130-131
“In the light of or—better to say—in the darkness of the [500-year] Homeric Question . . . [Scholars apply] two time tables simultaneously to the past of Greece, one built on the evidence of Greece itself, the other on relations with Egypt. Against this set-up, the Homeric Question grew to even greater proportions. Thus, instead of any new discovery reducing the question to smaller confines, every new discovery enlarged the confines and decreased the chances of finding a solution.”

Homer composed the *Iliad* and *Odyssey* supposedly in the earliest Helladic age of Greek history in superb language in written form—although it is argued these books were committed to memory for 500 years and only thereafter committed to writing. But this is incorrect since the Helladic Greeks of that time, known as the Mycenaeans, could and did write. Peter James *et al.* explain:

“The major Mycenaean centres employed skilled bureaucracies to keep administrative records—such as inventories of available goods, notes of taxes and lists of offerings made to the gods. Their script, known today as ‘Linear B’, was used not only on clay tablets but also in brief inscriptions on pots.”

In spite of the complexity of decipherment, in 1952 an English architect, Michael Ventris, proved to the astonishment of Greek scholars that the language of Linear B was indeed Greek. It had been assumed that the Mycenaean Greeks spoke a language that was a branch of Minoan from Crete, which was not Greek. Therefore, the earliest Greeks did have a written language in 1200 B.C. and should have maintained records right through to 800 B.C. Yet this is not what is found. According to Vincent Desborough, “. . . the art of writing is forgotten.” But after 500 years the Greeks are again writing and strangely maintain a considerable number of elements from the earlier period.

We were told that the writing of Linear B “was used not only on clay tablets but also in brief inscriptions on pots.” As Velikovsky explained in *Ramses II and His Time*, page 75, although defenders of the established chronology suggested that “Perishable material (papyrus) was probably used for writing . . . the Mycenaean writers used mostly clay until —1200, and the Greek inscriptions of the

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3 *ibid.*, p. 105
4 James *et al.*, *op.cit.*, p. 81
5 *ibid.*, p. 72
seventh century [B.C.] are also in clay or on stone . . . Had Greek inscriptions existed from —1200 to —700 ‘we must have found some trace of them’ [Carpenter, American Journal of Archaeology, XXXVII, 26-27].”

What we have is quite similar to the unknown Sumerians and well-known Chaldeans who also wrote on clay and stone. We have not a real shred of Chaldean writing which is almost completely impossible, but we have Sumerian writing. Is it also conceivable that the Mycenaean Greeks who wrote on clay and stone failed to leave a single inscription in the earth for 500 years, and afterward they resumed writing on clay and stone material? As with the missing strata and writings of the Chaldeans there should be a great many inscriptions to represent this half millennium period of the Greeks.

“Recovery was a very long time in coming. However, when the recession finally ended the new Greek society displayed many ‘old’ Mycenaean features . . . preserved across [this] ‘Dark Age’ . . .

“It is remarkable that most examples of continuity occur in luxury goods of the sort one would have expected to disappear during the cultural Dark Age generally envisaged. As Bernhard Schweitzer remarked: ‘. . . the real media in which Mycenaean art survived were jewellery, utensils of gold, silver, bronze and ivory inlay . . .’

“In Mycenaean times there was a flourishing industry of ivory-carving which, although originally derived from the East, came to have a strong influence on Levantine schools. At the end of the Late Bronze Age [the end of the Helladic period], ivory manufacture ceased in Greece, reappearing only around 850 BC.”

Various theories have been put forth to explain these discrepancies but they are really ad hoc in nature. The same problem exists with painted pottery motifs. Painted pottery made in the 13th and 12th centuries B.C. bears “striking resemblances . . . [with] that of the mature Geometric (late 9th-8th centuries BC) and the 7th century ‘Proto-Attic’ styles.” J.L. Bronson summarized the analyses of various authorities in this continuity of pottery styles:

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6 ibid., pp. 72-73
7 ibid., p. 74
“... various scholars who in actual fact have worked quite intensively with Attic Late Geometric and Proto-Attic pottery . . . have found and recorded evidence for the continuing and recurring influence of the Mycenaean tradition (not specifically in figure style) . . . and others have explored aspects of the influence of that same tradition on figure style itself. In a quite different frame of inquiry a recent investigator of the symmetrical principle in Greek pictorial composition has been forced to recognition of a persistent substratum of Mycenaean [Helladic] influence on early Greek art [500 years later] . . . Yet another investigator found the origin of nearly all motifs of Rhodian [from the island of Rhodes] Orientalizing to be Mycenaean [500 years earlier] and had to postulate their survival on textiles and metal ornaments [so that they could be copied after the Dark Age].”

The same problem exists with chariots which were used in the Mycenaean-Helladic period but then disappear for 500 years to reappear in the 8th to 7th centuries B.C.9 It also exists with a certain type of shield to protect warriors10 and with bronzes, especially tripods.11 With respect to palaces, fortifications, houses, and temples, James et al. report:

“The architectural history of the Greek Dark Age amply demonstrates the post-Mycenaean collapse. On the mainland south of Thessaly, as well as the Aegean islands, construction in stone and brick almost entirely ceased. In Desborough’s words ‘the extreme rarity of any stone construction for a period of centuries after LHIIIC [Late Helladic 3C levels] strongly supports the view that the skill had been lost . . .’

“There is very little new building construction during [this period] . . . on the grand scale of the preceding period.”12

The level of architectural skills almost completely deteriorates.13 As Desborough comments:

8 ibid.
9 ibid., pp. 75-76
10 ibid., pp. 76-77
11 ibid., pp. 78-81
12 ibid., p. 85
13 ibid., pp. 85-88
“... the changes that come about are little short of fantastic. The craftsmen and artists seem to vanish almost without trace: there is very little new stone construction of any sort, far less any massive edifices; the metal-worker’s technique reverts to the primitive and the potter, except in the early stages, loses his purpose and inspiration; and the art of writing is forgotten. But the outstanding feature is that by the end of the twelfth century [B.C.] the population appears to have dwindled to about one-tenth of what it had been little over a century before.”

If we move the Late Helladic Mycenaean Greeks of the 13th and 12th centuries forward in time to about 800 B.C., all these problems disappear. The events described by Velikovsky as Martian catastrophes would make the climate worse throughout the Near East and the rest of the Eurasian continent as described in the literature. His 1500 B.C. Venusian catastrophe would cause the poles to shift several degrees, bringing desert and near desert conditions to the desert belts of the Earth. The 8th century B.C. catastrophe would shift the pole even more, causing the great aridity which presently exists across this belt.

In much less than a century, mankind would be able to rebuild and advance all its various previous developments; and these would be clearly linked with those that preceded. That is, there was no great Dark Age, just a few decades or less, a pulse of recovery. If this is the case, then materials now dated to the Late Helladic Mycenaean period would survive to be copied in the period that followed it.

As Velikovsky explains:

“It cannot be denied that there was some interruption between the Late Bronze and Early Iron Ages in Greece and elsewhere; no smooth and evolutionary transition took place from [the] Mycenaean to the Ionian Age. There were great migrations in the eighth century [BCE] and in the first part of the seventh. . . .

“These upheavals of nature . . . were responsible for the break in continuity that is found in Greece, in Asia Minor and in many other places. There was a disruption in [the] occupation of lands and a discontinuity in civilizations. But there was no Dark Age and the four

14 ibid., p. 72
centuries inserted between the Mycenaean and Greek periods are unreal.”

James et al. report:

“Ironically, many scholars of the 19th and early 20th centuries would have seen the above difficulties in Greek archaeology as quite illusory, the products of an artificially long chronology. They saw no need to accommodate a Dark Age between the Mycenaean and Geometric eras. William Ramsey, A.S. Murray, Cecil Torr and many others were happy, from the evidence at their disposal, to date the end of the Mycenaean civilization as late as the 9th or even 7th century BC! Ramsey, for example, saw a connection between 9th to 7th-century Phrygian rock carvings and the sculpture of the Lion Gate at Mycenae. Murray was a staunch supporter of a low dating for Mycenaean civilization, associating it with the period of colonization and the early tyrants, during the 8th and 7th centuries BC. Torr drew attention to, amongst other things, the close resemblance between the carved gemstones of the Mycenaean and Geometric ages.”

Snodgrass, one of the foremost historians of the Dark Age of Greece, was forced to ask:

“. . . why did it come about that some four centuries elapsed during which Greek material culture appears to have changed so little? Why did it take so long for literacy, representational art, monumental architecture, and other attributes to appear, or reappear, in the form in which they eventually did?”

This brief resume of James et al.’s book, Centuries of Darkness, does not do justice to the immense amount of evidence they have arrayed to show that there was no Greek Dark Age. The book, generally aimed at those with a good deal of historical knowledge, should be read.

Furthermore, why didn’t the Phoenicians exploit this market by selling the Greeks fine jewelry, ivory carvings, chariots, etc.? If no Greek products were

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15 Whelton, op.cit., pp. 112-113
16 James et al., op.cit., pp. 93-94
17 ibid., p. 94
manufactured, there should have been found imported luxuries made by the Phoenicians who made the products for trade.\textsuperscript{18}

Velikovsky in his \textit{The Dark Age of Greece} explored this question, citing Professor Ekrem Akurgal of the University of Ankara:

“Today (1961), despite all industrious archaeological exploration of the last decades, the period from 1200 to 750 [B.C.], for most parts of the Anatolian region [Turkey], lies still in complete darkness. The old nations of Asia Minor, like the Lycians and the Carians, the names of which are mentioned in the documents of the second half of the second millennium, are archaeologically first noticeable about 700 [B.C.] or later. . . . Hence, the cultural remains of the time between 1200 and 750 [B.C.] in central Anatolia, specially on the plateau, seem to be quite irretrievably lost for us.”\textsuperscript{19}

In \textit{The Dark Age of Greece}, which Whelton considers one of Velikovsky’s best historical works, there is also a great deal of evidence that contradicts the conventional chronology. How did this archaeological-historical gap come into being? It was created by the dominance of Egyptian history which demanded that the civilizations around Egypt correlate with it. Since it was taken as indisputable fact that Egyptian history was extremely long, materials found elsewhere, especially pottery, could be cross-reference-dated with Egypt. As Whelton, citing Velikovsky, delineates:

“The confusion and discord created by the hegemony of Egypt was the source of bitter debates and backbiting among archaeologists. Velikovsky reviews the fight to the death between Wilhelm Dörpfeld and Adolf Furtwängler. Both were outstanding scholars. Their field work was suburb. But they feuded endlessly and even vilified each other on their deathbeds. Why? Because Dörpfeld insisted the evidence showed that Geometric Greek and Dorian ware, usually placed in the first millennium, was really from the same time period as—and even preceded—Mycenaean ware of the second millennium. Since the Mycenaean Age is contemporaneous with the Eighteenth Dynasty in Egypt, Dörpfeld reasoned, and Geometric ware is found in

\textsuperscript{18} see Lionel Casson, \textit{The Ancient Mariners} (NY 1967)
\textsuperscript{19} Whelton, \textit{op.cit.}, pp. 107-108
the same strata as the Mycenaean ware, then Geometric ware belongs back in the second millennium too.

“Furtwängler scoffed. He called Dörpfeld an *ignoramus* and pointed out that Geometric ware could not possibly be assigned to the second millennium since it was found in the necropolis near the Dipylon Gate at Athens along with porcelain lions from the 26th Egyptian Dynasty [of the first millennium B.C.] . . . The debate raged. Both men excavated the Temple of Hera at Olympia. Both unearthed evidence to support their ideas. Dörpfeld [who claimed a first millennium age for the very early Greeks] saw clearly that Geometric and Mycenaean ware are found together, not only at Olympia but also at Troy and Tiryns. Furtwängler replied that iron tools were found at Olympia with Geometric ware. The Mycenaean Age is the Late Bronze [of the second millennium]. The Geometric period is in the Iron Age [the second into the first millennium B.C.]. Therefore the Geometric and Mycenaean periods must be separated by ‘eine ungeheure Kluft’—a tremendous chasm.

“The scales tipped in Furtwängler’s direction. A chasm opened in the history of Greece and swallowed Dörpfeld whole. He fought to the end, but in isolation. His students abandoned him. His theories were called ‘wild’.”

The problem of course was the reliance on Egyptian chronology and cross-reference to it. Velikovsky, who sides with Dörpfeld on this matter, explains:

“The archaeological work that brought him to his theories was impeccable. His theories were wild mainly because he did not make the final step and free Greek archaeology from the erroneous Egyptian timetable.”

Velikovsky clearly elucidated how the archaeological evidence is handled to support the established chronology even when the facts that are uncovered are diametrically in opposition to that chronology. This Velikovsky published in *Pensée* and it is part of his *The Dark Age of Greece*. Because of the archaeological and historical excellence of this material it is presented in full below.

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20 ibid., pp. 110-111

21 ibid., p. 111
The Scandal of Enkomi
by
Immanuel Velikovsky

The lengthening of Egyptian history by phantom centuries must have as a consequence the lengthening of Mycenaean-Greek history by the same length of time. On Cyprus, Aegean culture came into contact with the cultures of the Orient, and particularly with that of Egypt, and unavoidably embarrassing situations were then in store for archaeology.

In 1896 the British Museum conducted excavations at the village of Enkomi, site of Alasia, the ancient capital of Cyprus, not far from Famagusta, with A.S. Murray in charge (1). A necropolis was cleared, and many sepulchral chambers investigated. “In general there was not apparent in the tombs we opened any wide differences of epoch. For all we could say, the whole burying-ground may have been the work of a century.”

“From first to last there was no question that this whole burying-ground belonged to what is called the Mycenaean Age, the characteristics of which are already abundantly known from the tombs of Mycenae . . . and many other places in the Greek islands and in Egypt.”

So far so good. But the pottery, porcelain, gems, glass, ivory, bronze, and gold found in the tombs all presented one and the same difficulty. From the Egyptological point of view many objects belong to the time of Amenhotep III and Akhnaton, supposedly of the 15th to the 14th centuries. From the Assyrian, Phoenician, and Greek viewpoint the same objects belong to the period of the ninth to the eighth or seventh centuries. Since the objects are representative of Mycenaean culture, the excavator questioned the true time of the Mycenaean Age. But as the Mycenaean Age is linked to the Egyptian chronology he found himself at an impasse.

We shall follow him in his efforts to come out of the labyrinth. He submitted a vase, typical of the tombs of Enkomi, to a thorough examination. The dark outlines of the figures on the vase are accompanied by white dotted lines, making the contours of men and animals appear to be perforated. This feature is very characteristic. “The same peculiarity of white dotted lines is found also on a vase from
Caere [in Etruria] signed by the potter Aristonothos, which, it is argued, cannot be older than the seventh century B.C. The same method of dotted lines is to be seen again on a pinax from Cameiros in the [British] Museum, representing the combat of Menelaos and Hector over the body of Euphorbos, with their names inscribed. That vase also is assigned to the seventh century B.C. Is it possible that the Mycenae and Enkomi vases are seven or eight centuries older?"

Analyzing the workmanship and design of sphinxes or griffins with human forelegs on a vase, the archaeologist stressed "its relationship, on the one hand, to the fragmentary vase of Tell-el-Amarna (see Petrie, *Tell-el-Amarna*, Plate 27) and a fragment of fresco from Tiryns . . . and on the other hand to the pattern which occurs on a terracotta sarcophagus from Clazomenae, now in Berlin, a work of the early sixth century B.C."

The connection between the Mycenaean and Aristonothos vases caused “a remarkable divergence of opinion, even among those who defend systematically the high antiquity of Mycenaean art.”

The problem of pottery which belongs to two different ages is repeated in ivory. The ivories of the Enkomi tombs are very similar to those found by Layard in the palace of Nimrud, the ancient capital of Assyria. There is, for example, a group of a man slaying a griffin, “the man being remarkable for the helmet with chin strap which he wears. It is a subject which appears frequently on the metal bowls of the Phoenicians, and is found in two instances among the ivories discovered by Layard in the palace at Nimroud. The date of that palace is given as 850-700 B.C.”

An oblong box for the game of droughts, found in Enkomi, “must date from a period when the art of Assyria was approaching its decline,” five or six centuries after the reputed end of the Mycenaean age.

“A among the Nimroud ivories (850-700 B.C.) is a fragmentary relief of a chariot in pursuit of a lion to the left, with a dog running alongside the horses as at Enkomi, the harness of the horses being also similar.” The style of the sculpture (of Nimrud) “is more archaic than on the Enkomi casket.” But how could this be if Enkomi dates from the 14th or 15th century?

A bronze of Enkomi repeats a theme of the Nimroud ivories, representing a woman at a window. “The conception is so singular, and the similarity of our bronze to the ivory so striking, that there can hardly
be much difference of date between the two—somewhere about 850-700 B.C.”

“Another surprise among our bronzes is a pair of greaves . . . . It is contended by Reichel (2) that metal greaves are unknown in Homer. He is satisfied that they are the invention of a later age (about 700 B.C.).”

Bronze fibulae, too, were found in the Enkomi tombs, as well as a large tripod ‘with spiral patterns resembling one in Athens, which is assigned to the Dipylon period,’ and a pair of scales of a balance like the one figured on the Arkesilaos vase. But such finds are separated by a wide span of time from the fourteenth century.

The silver vases of the Enkomi tombs “are obviously Mycenaean in shape.” “On the other hand,” there were found two similar silver rings, one with hieroglyphics and the other engraved on the bezel “with a design of a distinctly Assyrian character—a man dressed in a lion’s skin standing before a seated king, to whom he offers an oblation. Two figures in this costume may be seen on an Assyrian sculpture from Nimroud of the time of Assurnazirpal (884-860), and there is no doubt that this fantastic idea spread readily westward.”

Next are the objects of gold. Gold pins were found in a tomb of Enkomi. “One of them, ornamented with six discs, identical in shape with the pin which fastens the chiton on the shoulders of the Fates on the François vase in Florence [sixth century B.C.].” A pendant “covered with diagonal patterns consisting of minute globules of gold soldered down on the surface of the pendant” was made by “precisely the same process of soldering down minute globules of gold and arranging them in the same patterns” that “abound in a series of gold ornaments in the British Museum which were found at Cameiros in Rhodes” and which were dated to the seventh or eighth century.

Among the pottery of “the ordinary Mycenaean and pre-Mycenaean type,” gems were found. A scarab “bears the cartouche of Thi [Tiy] the queen of Amenophis [Amenhotep] III, and must therefore be placed in the same rank as those other cartouches of her and her husband, found at Ialysos [on Rhodes] and Mycenae, which hitherto have played so conspicuous a part in determining the Mycenaean antiquities as being in some instances of that date (fifteenth century).” (3)
As for the porcelain, it “may fairly be ranked” with the series of Phoenician silver and bronze bowls from Nimrod of about the eighth century. A porcelain head of a woman from Enkomi “seems to be Greek, not only in her features, but also in the way in which her hair is gathered up at the back in a net, just as on the sixth century Greek vases of this shape.” Greek vases of this shape “differ, of course, in being of a more advanced artistic style, and in having a handle. But it may fairly be questioned whether these differences can represent any very long period of time.”

Murray surveyed the glass. “In several tombs, but particularly in one, we found vases of variegated glass, differing but slightly in shape and fabric from the fine series of glass vases obtained from the tombs of Cameiros, and dating from the seventh and sixth centuries, or even later in some cases. It happens, however, that these slight differences of shape and fabric bring our Enkomi glass vases into direct comparison with certain specimens found by Professor Flinders Petrie at Gurob in Egypt, and now in the British Museum. If Professor Petrie is right in assigning his vases to about 1400 B.C. (4), our Enkomi specimens must follow suit. It appears that he had found certain fragmentary specimens of this particular glass ware beside a porcelain necklace, to which belonged an amulet stamped with the name of Tutankhamen, that is to say, about 1400 B.C.”

Murray comes to the conclusion that “Phoenicians manufactured the glass ware of Gurob and Enkomi at one and the same time.” Consequently “the question is, what was that time? For the present we must either accept Professor Petrie’s date (about 1400 B.C.) based on scanty observations collected from the poor remains of a foreign settlement in Egypt, or fall back on the ordinary method of comparing the glass vessels of Gurob with those from Greek tombs of the seventh century B.C. or later, and then allowing a reasonable interval of time for the slight changes of shape or fabric which may have intervened. In matters of chronology it is no new thing for the Egyptians to instruct the Greeks, as we know from the pages of Herodotus.”

With this last remark, the excavator at Enkomi came close to the real problem, but he shrank from it. He did not dare to revise Egyptian chronology; all he asked was that the age of the Mycenaean period be reduced. How to do this he did not know. He quoted an author (Helbig) who thought that all Mycenaean culture was really Phoenician culture,
the development of which had remained at a standstill for seven centuries.

“In 1896 there was found in a tomb at Thebes in Egypt a bronze patera [a shallow vessel] which in shape and decoration has so much in common with the bronze Phoenician bowls from Nimroud that we feel some surprise on being told that the coffins with which it was found belong unmistakably to the time of Amenophis [Amenhotep] III or the first years of Amenophis IV [Akhnaton]. It is admitted that this new patera had been a foreign import into Egypt. Equally the relationship between it and the bronze Phoenician bowls is undeniable, so that again we are confronted with Helbig’s theory of a lapse of seven centuries during which little artistic progress or decline had been effected.”

It was necessary to assume a state of hibernation of almost seven hundred years.

The “endeavor of the excavator of Enkomi” was directed toward bringing the Mycenaean Age closer in time by five or six hundred years, so that there would be no chasm between the Mycenaean Age and the Greek Age. As curator of Greek and Roman antiquities of the British Museum, he constantly had before him the numerous connections and relations between Mycenaean and Greek art, which could not be explained if an interval of many centuries lay between them. He tried to disconnect the link between Mycenaean and Egyptian archaeologies and chronologies, but he felt that this was an unsolvable problem.

The proposal to reduce the time of the Mycenaean Age was rejected by the scholarly world.

Sir Arthur J. Evans, the explorer of the Minoan Age on Crete, came out against Murray’s work, “so full of suggested chronological deductions and—if its authors (5) will pardon the expression—archaeological insinuations, all pointing in the same direction,” namely, “a chronology which brings the pure Mycenaean style down to the Age of the Tyrants” and makes it “the immediate predecessor of the Ionian Greek art of the seventh century B.C.” (6). The age of the Tyrants, following Evans, was the eighth century.

Evans had to admit that “nothing is clearer than that Ionian art in many respects represents the continuity of Mycenaean tradition,” but he built his argument on the manifold connections of Mycenaean art with Egypt of the Nineteenth Dynasty. Are not the flasks of the Enkomi
tombs almost as numerous in Egyptian tombs of the Eighteenth Dynasty? A fine gold collar or pectoral inlaid with glass paste, found in Enkomi, has gold pendants in nine different patterns, eight of which are well-known designs of the time of Akhnaton (Amenhotep IV), “but are not found a century later.” The metal ring of Enkomi, with the cartouches of the heretic Akhanaton, is especially important because “he was not a pharaoh whose cartouches were imitated at later periods,” and so on.

One of the silver vases of Enkomi, Evans wrote, “is of great interest as representing the type of famous gold cup of the Valphei tomb (7). These cups, as their marvelous repoussé designs sufficiently declare, belong to the most perfect period of Mycenaean art.” This should establish that the theory of the latency of Mycenaean art for six or seven centuries after its flowering in the second millennium cannot help to solve the problem of Enkomi; the Enkomi finds date from the apogee of the Mycenaean Age.

Evans insisted that the material supplied by these Cyprian graves “takes us back at every point to a period contemporary with that of the mature art of the glass as seen in the Aegean area,” and this despite his own admission that a number of objects from Enkomi point to a later age, like the porcelain figures, “which present the most remarkable resemblance, as Dr. Murray justly pointed out, to some Greek painted vases of the sixth century B.C.” He concluded with regret that “views so subversive” should come from so high an authority in classical studies.

Two scholars clashed because one of them saw the close connection between Mycenaean art and the Greek art of the seventh century, and the other saw the very same Mycenaean objects disinterred in the Egypt of the fourteenth century.

The Mycenaean Age has no timetable of its own independent of that of Egypt. I have already referred to this question in the chapter dealing with Ras Shamra in Ages in Chaos I.

If Evans had had some evidence, independent of Egypt, on which to calculate the ages of the Minoan and Mycenaean cultures, we would have needed to take into account all Minoan and Mycenaean chronological material, as we did the Egyptian. But there is none.
“The chronological scheme depends ultimately upon Egyptian datings of Aegean pottery” (8), wrote H.R. Hall, who served as curator of Egyptian and Assyrian antiquities at British Museum.

“Using this Egyptian evidence as his guide, and checking the results of excavation with its aid, Sir Arthur Evans finds that the Bronze Age pottery and with it the general culture of Crete divides itself into three main chronological periods: Early, Middle, and Late, each of which again is divided into three sub-periods.” (9) The Mycenaean Age started at the same time as the Late Minoan III.

Dr. Murray’s case was lost. He had built its defense on two points, one strong, the other weak. The strong point was this: he analyzed and made clear the close interrelation between Mycenaean culture and the early Greek culture of the seventh century. His weak point was his anxiety to disregard the connection between Mycenaean culture and the Egyptian world of the end of the Eighteenth Dynasty. But in el-Amarna of Akhnaton scattered heaps of Mycenaean ware were found.

It was asked, Which fact should be given greater weight by an unbiased judge: the close relation between Mycenaean and Greek cultures or the fact that Mycenaean ware was found in the city of el-Amarna (Akhet-Aton), which was built and destroyed in the 14th century?

The verdict in the matter of the age of Mycenae was unanimous: it dated back to the 15th, 14th and 13th centuries. “This [Mycenaean] ware did not appear in large quantities in Egypt until about 1375 B.C., and little of it was received in the coastal countries after the middle of the thirteenth century. Therefore, whenever a piece of it is found in place in an ancient city, it dates the context between about 1375 and 1225 B.C.” (10). In the present-day conventional chronology —1375 is the first year of Akhnaton (11).

The verdict with regard to Enkomi was, in the language of Hall, as follows:

“Excavations of the British Museum at Enkomi and Hala Sultan Tekke (near Larnaka on Cyprus) have brought to light tombs filled with objects of Minoan or Mycenaean art, now mostly in the British Museum, most of which cannot be later in date than the fourteenth and thirteenth centuries B.C. The objects found with them are
demonstrably of this date, and not later, being all of the late Eighteenth and the Nineteenth Dynasties. Rings of Akhenaten [Akhnaton] and a scarab of Queen Teie [Tiy, mother of Akhnaton] have been found here as at Mycenae, and fine Egyptian necklaces of gold also, which, from their style, one would adjudge to the Eighteenth or Nineteenth Dynasty. Probably, too, the greater part of the treasure of gold-work found in the tombs and now in the British Museum is of this early date. The golden tiaras and bands certainly seem to connect with those of the Mycenaean shaft-graves. But at the same time there are many objects of later date, such as bronze tripods . . . which are demonstrably of the Dipylon period, and cannot be earlier than the tenth or ninth century” (12).

Thus, in effect, the excavator of Enkomi is accused of having been unable to distinguish between burials of different ages in a grave (13). He denied that the graves of Enkomi had been re-used.

Somewhere I came upon the expression, “the scandal of Enkomi.” I ask: Was the excavator to be blamed for something that was not his fault?

The graves of Enkomi belong to the period that begins about — 870 with Assurnasirpal and Amenhotep III, Shalmaneser III and Akhnaton and Tutankhamen. The necropolis was still in use during the eighth century and apparently also during the seventh.

We learn from this case the fact which both sides admitted: the Greek culture of the seventh century has many interrelations with Mycenaean culture. The chronological gap was called the Dark Age.

“Cyprus no less than Greece itself passed through a long and tedious Dark Age . . . Cyprus withdrew into herself, and life during this transitional age dull . . . and dim” (14).

REFERENCES
3. Since the beginning of the present century, the conventional date of the reign of Amenhotep III has been reduced to the end of the 15th and the first quarter of the 14th century.
4. Sir W.M. Flinders Petrie, *Illahun, Kahun and Gurob* (London: 1891), Plate 17. “Compare also Plate 18 with two identical glass vases which are assigned to Rameses II.” Murray, “Excavations at Enkomi,” in Murray, Smith, and Walters, *Excavations in Cyprus*, p. 23, note. Since the above evaluation of the time of Tutankhamen by Petrie, the conventional date of this king, son-in-law of Akhnaton, has been reduced to ca. —1350.

5. The reference is to A.S. Murray and his collaborators.


7. Vapheio gold cups were found in a bee-hive tomb in the neighborhood of Sparta, with designs representing men hunting bulls.


9. *ibid.*, p. 3.


11. Since the time of the Murray-Evans controversy, the age of Akhnaton and Tutankhamen has been reduced by a few decades.


[Immanuel Velikovsky “The Scandal of Enkomi”, in *Pensée*, vol. 4, no. 4, winter 1974-75, pp. 21-23 h]

What is obvious from the foregoing is that the established chronology of Egypt is the only acceptable paradigm into which evidence must be made to fit no matter how contradictory to that chronology it is. Velikovsky sums up the situation:

“In the 1880’s, when Hellenists were coerced, upon the evidence presented by Egyptologists, to introduce those five dark centuries, they
did it only after a period of protest and resistance. But now that three
[as of 2000, six] generations of historians have lived with those dark
centuries as a historical reality, it is even more difficult to part with
them. [The Dark Age has] engendered and continues to engender an
ever-growing scholarly literature. If it can be shown that the Egyptian
timetable is off its hinges, the bondage of these studies and their
dependence on Egypt may terminate.

“The removal of the Dark Age from the historical sequence
unshackles what was for centuries shackled, and releases the scholarly
endeavor from traveling on the same circular paths with no exit, the
modern version of the Cretan Labyrinth. Moreover, it rehabilitates
scholars accused of ignorance or negligence, their having been guilty
only of not perceiving that the problems they dealt with were not
problems at all, as soon as unreal centuries are stricken out.” 22

Velikovsky presented the concept that the great Venusian cataclysm,
dated to around 1500 B.C., together with other such cataclysms, so devastated
humanity that it created in their minds such emotional pain that they repressed it.
He called this “collective amnesia.” For proposing this psychological mechanism
he was roundly criticized and lampooned by historians and others. Yet when we
come to the Dark Age of Greece, the historians have failed to recognize that in those
centuries the ancient Greeks appear to have suffered from various forms of
“collective amnesia.” They postulate that collectively these early Greeks forgot or
almost forgot how to write, how to paint fine pottery, how to build large structures
of stone, how to make fine jewellery, how to make fine utensils of gold, silver,
bronze, and ivory inlay. And their “collective amnesia” lasted for centuries. The
psychological behavior which the historians ridiculed when Velikovsky presented
it, is considered unreal when they criticize Velikovsky. However, when they
indirectly introduce this psychic phenomenon to explain the collective failure of the
Greeks to remember nearly all their former fine artistic, literary and architectural
abilities, they fail to apply the same standard that they applied to Velikovsky. Dark
Ages of this type simply cannot exist without a monumental event to destroy the
Greek population and all those societies that surrounded them.

According to their chronology, neither all of Mesopotamia nor Egypt
went into similar Dark Ages and the Phoenicians were trading all across the
Mediterranean region. Wouldn’t these traders have exploited the Greek market by

22 Whelton, op. cit., pp. 115-116
selling them fine goods from the societies that were still producing them? Did the Phoenicians also develop a “collective amnesia” with regard to their trade with the Greeks for centuries? Did they forget this part of the world existed while they were trading elsewhere? Where did all the Greek craftsmen and population move to during this period? For this we have no answers but ad hoc conjectures. As Ceram so cogently stated, “Surely it should have occurred to someone [especially historians] that a people’s history cannot stop dead [or nearly stop dead] for two [to five] hundred years?”

When we look at the stratigraphy at Troy, the same Dark Age problem emerges, as James et al. report:

“Despite numerous excavations, no strata have yet been discovered representing the period between [the] Troy VIIb [level], usually linked with 12th-century [B.C.] Mycenaean imports . . . and the beginning of Troy VIII, dated by Archaic Greek imports to 700 BC. (C.W. Blegen et al., Troy IV:1 (Princeton NJ 1958), pp. 249-250). The classical scholar Denys Page remarked on the strange gap which results from this chronology:

“There is nothing at Troy to fill this huge lacuna. For 2000 years men had left traces of their living there; some chapters were brief and obscure, but there was never yet a chapter left wholly blank. Now at last there is silence, profound and prolonged for 400 years; we are asked, surely not in vain, to believe that Troy lay ‘virtually unoccupied’ for this long period of time.’ (D. Page, ‘The Historical Sack of Troy’, Antiquity 33, (1959) 25-31)

“Yet despite the apparent lapse of several centuries, there is every indication of continuity between Troy VIIb and VIII. The excavator, Carl Blegen, could detect no sign of a break in occupation. Furthermore, the local pottery of Troy VIII was the same distinctive, lustrous grey ware used during Troy VIIb. (Blegen et al., op.cit., 10, 147, 251) He therefore supposed that the inhabitants of Troy VIIb abandoned it for a nearby refuge

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23 Ceram, op.cit., p. 130-131
where they continued to produce this ‘Grey Minyan’ pottery for 400 years before returning.”

However, after 25 years of digging in the area, nothing was ever found to support Blegen’s theoretical refuge. Various efforts have been expended to close the gap, but it still stands in contradiction to conventional chronology. With respect to another site at Mycenae, P. John Crowe informs us:

“. . . British Museum based scholar, H.R. Hall, was totally convinced that some of the items from Mycenae Grave Circle A belonged to c[irca] 900 [B.C.] or later. He therefore suggested that priests opened up early D[ynasty] 18 graves after an interval of some 600 years, stole nothing, but piously inserted later items. This rather incredible idea not surprisingly received little support but it illustrates the huge pressure being placed upon archaeologists and art historians, once they were forced to accept Egyptian dates for the Late Helladic period, to invent an explanation for these anachronisms.”

We have seen the lengths to which historians and archaeologists are forced to go to explain a historical gap that does not exist. In the Fall, 1973, issue of *Pensée*, Professor William H. Stiebing, Jr., wrote of Velikovsky’s reconstruction as “not in harmony with the mass of archaeological evidence presently at our disposal.” In particular Stiebing mentioned Mycenae, Alalakh, Ras Shamra (Ugarit), Tiryns, and Troy. Each of these cities also suffered from a Dark Age and, according to Stiebing, archaeology supports the conventional chronology that claims there was a 500-year period of occupation between 1200 and about 700 B.C. but that during these five centuries the materials in the ground are datable to Egypt, Palestine, etc. He claimed that there are no sterile gaps at these sites between Mycenaean times 1200 B.C., and Hellenic times, 800 B.C. Israel M. Isaacson, a pseudonym employed by Eddie Schorr, actually did the research for each site and reported it in *Pensée*.

With respect to Mycenae in Greece, first excavated by Heinrich Schliemann in the 1870’s, Schorr pointed out that Velikovsky and Lewis M. James et al., *op.cit.*, pp. 61-62

25 *ibid.*, p. 62

26 P. John Crowe, “The Revision of Ancient History—A Perspective,”

http://www.knowledge.co.uk/SIS/ancient.htm, pp. 11-12


Greenberg demonstrated that the Lion Gate “now ascribed to ca. 1250 B.C., ... [was] originally dated half a millennium later, in the 8th century B.C.”

J. Boardman wrote: “More than five hundred years were to pass before Greek sculptors could [again] command an idiom which would satisfy these aspirations in sculpture and architecture.”

In terms of the strata, “The 11th layer, in addition to ‘12th century ... pottery, contained a significant number of fragments of Orientalizing ware’ (i.e., 7th-6th century B.C.).” The thickness of the stratum, which represented about 500 years, was only $\frac{1}{6}$ of that beneath it representing 200-250 years.

Circle graves at Mycenae, according to N.G.L. Hammond, exhibit “astonishing similarities” to circle graves from Albania. ... But when it comes to these Albanian graves, the archaeologists “have continued to assign them 500-600 years later than Hammond.” ... “[H]ow can 11th century graves be the ‘antecedents’ and models for graves 500 years older at Mycenae?” ... “The scenes of hunting and battle depicted [at Mycenae] ... remind one very much of Neo-Hittite relief sculptures supposedly six-seven centuries later in date.”

“One author judged [at Mycenae] the golden breast plates, diadems, sword handles, sword belts, and patterned gold discs from the graves to be products of the Geometric Age [500 years later]. ... The same author described animal depictions on the gold objects as ‘identical’ in style to the 7th-6th-century examples.”

This is merely a drop in the bucket of evidence that Schorr presented from Mycenae to show that the archaeology fully supports Velikovsky’s and others’ view that there was no 500-year Dark Age. Schorr does the same for Alalakh, Ugarit, Tiryns, and Troy, and his work should be read by those interested in the revised chronologies. The problems in archaeology for the Dark Age run into the dozens and dozens. For having the courage and honesty to thoroughly refute Stiebing and conventional chronology, Schorr paid a high price as summarized by Crowe:

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29 *Pensée* (Winter 1973), pp. 26-31
30 *ibid.*, p. 6
31 *ibid.*
32 *ibid.*, p. 7
33 *ibid.*
“Schorr was cruelly punished by the Establishment for his temerity in refuting their Dark Age dogma. As was the custom among research graduates, and in his turn, he duly recounted his findings [as published in Pensée] at a Graduate Seminar lecture. His reward was to have his lecture interrupted, his audience sent home, and be summoned to the Professor's office. There he was told he must recant his heretical beliefs, or he would not be allowed to finish his doctorate. This, in all conscience, he could not do, so he left the university, his ambitions dashed, and his career hopes in ruins.”\(^{34}\)

The English Romantic poet, Percy Bysshe Shelley, aptly delineated the nature of what was just exposed in Queen Mab, III:

“Power like a desolating pestilence,
Pollutes whate’re it touches; and obedience
Bane of all genius, virtue, freedom, truth
Makes slaves of men, and, of the human frame
A mechanized automaton.”

One will almost never find this form of intellectual suppression being discussed by sociologists, or historians of science and history. The idea that powerful individuals will destroy the career of someone who challenges their system of knowledge has been fairly well suppressed. It does go on in almost every field of human endeavor; but despite it being so prevalent, there still is no excuse for this failure of those in academia and the press to address and expose it. So long as they fail to directly repudiate this behavior, it must continue, and these acts of intellectual violence will deaden the heart of academia to innovations and explorations of new theories and new knowledge.

For all their learned development, those academics have never understood that they are the repository not only of historical analysis but historical justice and have failed to take the oath that Thomas Jefferson took:

\(^{34}\) Crowe, op.cit., pp. 14-15
“I have sworn upon the altar of God, eternal hostility against every form of tyranny over the mind of man.”

Look at what transpired: Stiebing presented a false analysis of archaeology as it relates to the Dark Age of Greece. Schorr cleared up the record with archaeological evidence, and showed that it supports Velikovsky’s revised chronology regarding that period. Schorr, who proved that the evidence for a Dark Age was non-existent, was driven out of his university. Stiebing, who falsified history, was never even censured. This dichotomy of punishment between what occurred to Schorr and Stiebing will undoubtedly be quietly forgotten by those who wish to maintain the status quo. Many academics, on the other hand, will be aghast and dismayed that such dismal behavior can be enacted in the institutions of learning to which they have earnestly devoted their lives. But despite this they will do absolutely nothing to expose it in their classrooms, in their journals, and in discussions with their colleagues. Beneath all the rhetoric of their works exists this cancer of intellectual suppression that they have failed to extricate from their fields. In this respect their silence, in the face of blatant fascism, is quite similar to the good Germans who despised Hitler but never raised their voices to oppose him. It is tragic to realize that academia has so many good Germans.

At this point it is well to keep in mind the words of Lord Acton, himself a historian and proponent of Christian liberal ethics, who wrote to his friend Mandell Creighton, the Anglican bishop of London, an eminent historian of the time:

“I cannot accept your connor that we are to judge Pope and King [and academics, historians, scientists, etc.] unlike other men with a favourable presumption. If there is any presumption, it is the other way against the holder of power, increasing as the power increases. HISTORIC RESPONSIBILITY HAS TO MAKE UP FOR WANT OF LEGAL RESPONSIBILITY. Power tends to corrupt, and absolute power corrupts absolutely. . . . There is no worse heresy than that the office sanctifies the holder of it. That is the point at which . . . the end learns to justify the means.”35 [capitalization added]

Acton stressed the role of the historian as judge rather than as objective observer. He exhorted his students and colleagues “never to debase the moral currency but to try others by the final maxim that governs our lives and to suffer no man and no cause to escape the underlying penalty which history has the power to

35 Robert L. Schaeffer, Lord Acton (La Salle IL 1975), p. 6
inflict on wrong.”

In writing this condemnation in this book, the arbitrary abuse of power exercised by Schorr’s professor is preserved for history. Acton argued for “ideas, not force [as] the spiritual property that gives dignity and grace and intellectual value to history.” But this author believes Acton’s words carry little weight with that professor and many, many of his colleagues. They simply cannot call him to task to suffer the shame and opprobrium for his actions.

What the behavior of this professor reflects is best exemplified by John Stuart Mill who wrote: “we can never be sure that the opinion we are endeavoring to stifle is a false opinion; and [even] if we are sure stifling it would be evil still.” Mill also condemns this behavior with these words:

“If all mankind minus one were of one opinion, and only one person were of the contrary opinion, mankind would be no more justified in silencing that one person than he, if he had the power, would be justified in silencing mankind.”

Since there is no or little discussion of this in the literature, this author wishes to make a modest proposal and term this form of reaction by establishments to challenges as the Fallacy of the Club, which is to maintain the Club or the paradigm of the Club as unassailable and not allow it to be subjected to criticism within the confines of the club. To be a member of the Club requires that one surrenders one’s human faculty of questioning the foundations of any concept held by the Club as inviolate. Those who do question these foundations are to be expelled. If they cannot be expelled they shall be boycotted by the rest of the members. This means, no publication of their works in the Club’s journals or other outlets, no financial support for them to carry on research, no invitations to them to speak at conferences, and in many cases no discussion with them even on the social level. Since the violator has renounced the paradigm of the Club, efforts to vilify and misrepresent him in the classroom, journals, and via the academic grape-vine are permissible and encouraged. These misrepresentations may then become the history presented to the rest of academia and the public by which to insure that the Club and its paradigm will continue to survive and prevail. Those outside the Club may be treated in a similar manner when and where such opportunities present themselves. Vilification may be liberally employed as part of all these efforts. All

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36 ibid., p. 59
37 ibid., p. 160
38 John Stuart Mill quoted in Leo Rosten’s Carnival of Wit (NY 1994), p. 351
39 ibid., p. 352
these practices are instituted by the Club because those inside the Club can no longer imprison or burn heretics at the stake. The tools of the Inquisition have changed but the Inquisition—the Club—has not.

To paraphrase ‘Twain’: “For obvious public relations reasons academics must present to the world the outward signs of sanity.” Or, as Kurt Vonnegut wrote:

“Fedor Mikhailovich Dostoevski, the Russian novelist, said one time that, ‘One sacred memory from childhood is perhaps the best education.’ I can think of another quickie education. . . , which in its way, is almost as salutary: Meeting a human being who is tremendously respected by the adult world, and realizing that that person is actually a malicious lunatic.”

The 500-year Dark Age of Greece running from about 1250 to 750 B.C. is clearly lacking in archaeological support. But the Homeric Age is also clearly linked with the time of the 18th Dynasty, supposedly running from 1570 to 1293 B.C. Rather than forcing Greek history to follow that of Egypt, the relationship between the two countries can be harmonized by moving the 18th Dynasty into the first millennium, as did Velikovsky. Therefore, there is no Dark Age for Greeks, and Mycenaean Greece is linked directly with the 18th Dynasty. This arrangement again suggests that several Egyptian dynasties co-reigned in different areas during the first millennium.

If this is the case, then there should exist certain types of scientific, technological, and historical evidence as support. That is, the latter part of the 18th Dynasty should be located in the Iron Age. As one may recall, the Iron Age came to Egypt around 800 B.C. Prior to that time iron products were absent.

As was pointed out in Chapter 7 by Rickard, “the quality of iron found in the Pyramid and the tomb of Tut-ankh-Amen appears to be excellent.” An iron dagger and other iron items were recovered from this 18th Dynasty pharaoh’s tomb. According to Howard Carter who opened Tutankhamen’s tomb, besides the iron dagger there were “six miniature implements fixed with hard, dark-grained wooden handles [which] proved to be iron.” There were altogether “nineteen separate

[iron] objects” in the tomb.\textsuperscript{42} However, as “a matter of fact, from his reign onward, we find special amulets made of that [iron] material.”\textsuperscript{43} Because of this number of iron implements, Carter concluded that “the historical value of these particular [iron] specimens is, I think, more from the point of view of the introduction of iron in Egypt, than the actual use of the metal by the Egyptians.”\textsuperscript{44}

According to Arnold C. Brackman, in the tomb of Tutankhamen:

“Carter and his assistants found a headrest made of iron. To the present, Egyptologists have regarded this object with special reverence. Like the blade of Tutankhamen’s dagger, the headrest conclusively demonstrated that Egypt of the Eighteenth Dynasty had crossed the threshold of the Bronze Age into the Iron Age, and all that that implies. But Egyptologists have been so engrossed with this spectacular discovery that they have failed to distinguish the forest from the trees.

“In Carter’s The Tomb of Tut-ankh-amen, Volume II, tucked away in Appendix II, which was written by his chemist friend Lucas and is appropriately entitled ‘The Chemistry of the Tomb’, there is an innocent, short paragraph about the discovery of iron implements in the hypogeum. ‘The iron [Lucas’ italics] pieces are only three in number,’ he wrote, ‘namely a dagger-blade, part of an amuletic bracelet and a miniature head-rest, all of wrought iron.’”\textsuperscript{45}

It is assumed that these wrought iron items were sent by the Hittites. Herzfeld cites a letter by Hattusilis II, “Regarding the smelted iron about which you wrote to me”.\textsuperscript{46} This letter was sent to an Assyrian king, according to Elizabeth Wayland Barber in The Mummies of Ürümchi (NY 1999), p. 185. However, would a Hittite king send an Egyptian headrest rather than one (if such items existed in his kingdom) fashioned with Hittite glyphs, designs etc.? One cannot prove with any degree of certainty that these wrought iron objects in Tutankhamen’s tomb came from the Hittites except by circular reasoning that the established chronology demands it. As was stated earlier, circular reasoning dominates this field.

\textsuperscript{42} ibid., p. 92
\textsuperscript{43} ibid.
\textsuperscript{44} ibid., p. 93
\textsuperscript{45} Arnold C. Brackman, The Search for the Gold of Tutankhamen (NY 1976), pp. 169-170
\textsuperscript{46} Herzfeld, loc.cit.
Tutankhamen reigned from 1334 to 1325 B.C., according to the established chronology. That is, about 500 years before iron was first introduced to Egypt, this pharaoh had iron implements. This is clearly not possible. According to the established chronology, iron had supposedly been only invented around this time. It is taken that the Hittites who first smelted this metal may have sent these items to Tutankhamen. But as we will see in the next chapter, the Hittites also suffer from a long Dark Age and are taken by Sweeney to be alter-egos of the Lydians. Tutankhamen having iron objects in his grave indicates that he reigned at least 500 years closer to the present.

There is also the problem with the production of fine glass items found at New Kingdom sites covering the 18th, 19th, and 20th Dynasties which suddenly ceases for five to six centuries before glass ware is again fashioned. In this respect Heinsohn informs us:

“The finest pieces of glassware came from Akhenaton’s [18th Dynasty] (—1363 to —1347) capital Akhetaton where ‘ancient Egyptian glass reaches its highest development.’ [P. Fossing, Glass vessels before Glass Blowing (Kopenhagen, 1940), p. 18] . . . It is then at least five or six centuries before glass vessels begin to show up in Egypt. When these glasses reappear they are produced in the same way as before the long break and artistically they seem to evolve out of designs of the 14th century BCE. . . .

“. . . the technology disappeared in the late 2nd . . . millennium only to reappear in the 7th/6th century: ‘About the middle of the VI Century [B.C.], i.e. during the long reign of King Amasis [—589 to —526], these rare glass objects suddenly became a frequent component of the archaeological material found in Mediterranean coastal countries. Though the forms to some extent were Greek in style, and for the most part were found together with Greek products, especially Attic Vases, there is such a strong likeness between the glass objects of this period and the corresponding products of the New Empire glass industry eight to nine hundred years before, that in all probability the glasses of the VI-V Centuries were also Egyptian products’.” (P. Fossing, op. cit., p. 134; cf. also Fossing’s pictorial presentation of the two periods in Figure 10 below) 47

Figure 10 Glassware
As with 18th Dynasty iron items displaced by several centuries, we also have a glass Dark Age for five to six centuries in Egypt associated with the 18th, 19th, and 20th Dynasties. Heinsohn’s piece presents additional evidence related to glass which requires that the 18th Dynasty be placed in the middle of the first millennium B.C.

If the Egyptians lost the ability to make glass, is it possible that the Phoenicians also forgot this process for 500 to 600 or more years? According to David Frederick Grose:

“Although there is no specific proof that glass was manufactured in the Canaanite area along the Syro-Palestinian coast or on the islands of Cyprus and Rhodes during the late Bronze Age, there is literary and archaeological evidence that these lands enjoyed a brisk trade both in raw glass and finished products. The cuneiform diplomatic correspondence found at [18th Dynasty] Tel el-Amarna, for example, speaks of shipments to Egypt of what is thought to have been raw glass from the [Phoenician] kings of Tyre and their neighbors in the Levant. Also, a Bronze Age merchant ship that sank off Ulu Burum near Kas (Turkey) was carrying over a score of cobalt-blue cylindrical glass ingots intended for remelting... as well as a Canaanite jar filled with glass beads.”

According to L. Casson, the Phoenicians “shipped out fine Asia Minor wines...[and] expensive glassware...” This is the same problem we encountered with the Greeks during their supposed Dark Age.

Since the Phoenicians were able to supply Egypt with glass during the 15th century B.C., what caused them to stop supplying their former trading partners after 1000 B.C.? Trade was the very core of Phoenician merchants’ life and a product that was so valuable surely would have continued to be produced for other trading partners as well as for Egypt. Looking through a glass darkly, a Glass Dark Age in Egypt and Phoenicia simply makes no sense. But moving the 18th, 19th, and 20th Dynasties into the first millennium makes the dark glass age into a clear, translucent glass age. As one can see, glass products of the mid to latter part of the second millennium B.C. look just like those of the mid first millennium B.C. and, in-between, we are asked by the advocates of conventional chronology to believe

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48 David Frederick Grose, *Early Ancient Glass* (NY 1989), pp. 54-55
that the technology was forgotten. If it was truly forgotten, then the glassware forms from the two periods would not resemble each other so closely. All this technological evidence is unresolved and will remain so as long as these Dark Ages blind historians to the evidence.

Another element of the discussion has to do with the art of ivory carving. According to Velikovsky,

“Thutmose III [of the 18th Dynasty], on plundering Megiddo [in Palestine] in his campaign of conquest, took, according to his annals, ‘six large tables of ivory and six chairs of ivory’ besides other spoils. The pharaoh brought from Jerusalem ‘the great throne of ivory, overlaid with pure gold’ . . . In the eighth campaign Thutmose III took tribute including ‘vessels laden with ivory’. . . . After enlarging his fleet with the fleet of the Phoenicians, he sent his ships to collect tribute and, . . . used the maritime route for transport of ivory from Palestine to Egypt.

“The art of working ivory was transferred at that time to Egypt. In the tomb of Rekhmire, a vizier of Thutmose III, workers from Palestine are portrayed ‘making chests of ivory’—these were imported craftsmen.

“Samaria [in Palestine], built a few decades later, became a manufacturing center of ivory, working mainly for Egypt. Among the ivories excavated in Samaria, there are many unfinished pieces with Egyptian patterns.

“Color effects were produced by staining the ivory with pigments. Colored ivory was also found in the tomb of Tutankhamen. In an el-Amarna letter of Burraburiash we read about colored ivory. . . .

“In the last century [1800’s] inlaid plaques with Egyptian subjects were found in Mesopotamia. When the ivories of Samaria were unearthed they were acclaimed as closely related to those found previously in the palace of Nimrud [in Assyria] and in other places: ivories which ‘may have come from the same workshop as the Samaria ivories, were found by Layard in the northwest palace at Nimrud; a few stray examples come from other sites.’ (J.W. Crowfoot and G.M. Crowfoot, Early Ivories from Samaria (London 1938), p. 9)
“Ivories similar to those of Samaria have been found in a number of different places, sometimes together with Egyptian objects of the Eighteenth Dynasty. One of these places is Megiddo. Though the ivories of Samaria and Megiddo show the same patterns and the same workmanship, they were ascribed to two different periods. [see G. Loud, *The Megiddo Ivories* (Chicago 1939)] Similarly, other discoveries of ivory were attributed by their finders either to the period of the Eighteenth Dynasty (the fifteenth and fourteenth centuries) or to the period of the kings of Samaria (the ninth and eighth centuries). [ibid.] The second period is thought to have been the age of imitation of old Egyptian styles and the renaissance of an old craft.

“They were but one and the same period, and the ivories of the Samaria of Ahab and of the Thebes of Tutankhamen are products of one and the same golden age of ivory art.”

With regard to Ramses II, Velikovsky writes of “The Speedy Scribe”,

“Among the texts composed in the time of Ramses II there is a letter written by a scribe named Hori to a scribe named Amenemope. Hori was insulted by Amenemope and charged with being ignorant; Hori replied in a sarcastic letter, proved his own erudition, and exposed the ignorance of his opponent. The field of knowledge in which he thought himself an authority was Palestinology. It is possible that the letter was written in Palestine.

“The letter mentions many geographical names; they are spelled in a easily legible and recognizable form: Kiryath-n-b is Kiryath-anab.

“Even more impressive than the list of Palestinian cities is the use of numerous Hebrew words by the scribe. Thus ‘flour’ is called kemakh, ‘bramble’ is koz, ‘quiver,’ ashep, and even an entire Hebrew sentence is inserted in the letter: ‘Avadta kmo ari, mahir noam.’ [You have perished like a lion, said the speedy scribe.”] . . .

The conclusion that the population of pre-Israelite Canaan not only spoke Hebrew but must also have had speedy scribes writing in

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this language has been avoided [by historians], though it follows from Hori’s text: Hori used the Hebrew word sofer yodea for a learned scribe, and mahir for a speedy scribe. . . .

“. . . a ‘speedy scribe,’ or one who could write down words as they were spoken, is a late development of skill in the art of writing.”  

Sweeney’s comment on this evidence explains the stunning disagreement with the established chronology:

“Now, since Ramses II, and therefore the scribe Hori, is placed in the early 13th century BC by orthodox chronology, we must expect that the Hebrew words and phrases used in Hori’s letter would display obvious archaisms [because it is so old]. Actually, it is assumed that since this is the 13th century there cannot have been any Hebrews in Palestine at all, and the Hebrew in Hori’s letter is the language of the Canaanite natives of the mid-second millennium. Yet, the astonishing thing is that Hori’s Hebrew is identical, in minute detail, to the Hebrew employed in Palestine during the period of the later monarchies, say around the time of Isaiah [8th to 7th century B.C.] . . .

“Textbook chronology therefore requires us to believe that the language of Palestine remained completely unchanged for the six or seven hundred years separating the time of Hori from the epoch in which scholars would expect to have found written the advanced Hebrew that he used, i.e. the 8th/7th century BC. It is the 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 the textbooks by historians without comment.”

If, as the evidence suggests, the 18th Dynasty existed in the first millennium B.C., should not the 19th Dynasty which followed it exhibit evidence that it also existed in the mid- to later part of that millennium? In this respect, Velikovsky presents evidence directly associated with Ramses III of the 19th Dynasty which also places it there, based on Greek:

51 Velikovsky, Ramses II and His Time, op.cit., pp. 61-62
52 Sweeney, op.cit., pp. 43-44
“Over ninety years ago the Swiss Egyptologist Edouard Naville excavated [at Tell el-Yahudiya] the ruins of a palace of Ramses III. Tiles, colored and glazed, once adorned its walls. . . . On the reverse side of these tiles are found incised signs: these are apparently the initials of the craftsmen who produced them, inscribed before the tiles were fired.

“There was no doubt that the signs on many tiles in the palace of Ramses III at Tell el-Yahudiya were Greek letters. ‘The most noticeable feature is that several of the rosettes have Greek letters at the back, evidently stamped on during the process of making,” wrote T.H. Lewis [in “Tell-el-Yahoudeh,” Transactions of the Society of Biblical Archaeology, vol. VII 1881, (1882), p. 182], orientalist and art expert, to whose judgment the tiles were submitted.

“But how could Greek letters have been used in the days of Ramses III, early in the twelfth century before this era? The Greek alphabet was derived from the Phoenician or Hebrew much later; no traces of it have been found in Greece, on the islands, or in Asia Minor before —750. The problem of the Greek letters on the tiles of Ramses III 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 the early Greek letters of the seventh century but like the classical letters of the age of Plato [who lived from about 428 to 348 B.C.].

“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 [ibid., p. 189]; and the forms of some other letters also indicate that they are of a late century. Thus sigma was designated C 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 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; but the matter becomes more difficult if we ask who the author of this work was.’ So wrote Emil Brugsch. [‘On et Onion,’ Recueil de travaux relatifs à la philologie et à l’archéologie égyptiennes et assyriennes, VIII (1886) 5] . . .

“The dilemma was very clear but it had no answer: the Greek letters could not have been written at the time of Ramses III early in the twelfth century; they could have originated only in the last decades of the Egyptian Kingdom or during the following age of the Ptolemies. But the tiles must have been manufactured by laborers of Ramses III, and the royal name of the pharaoh adorns the front of the tiles. Would it be possible to separate the tiles and to ascribe part of them to Ramses III and another part to a later epoch?”

Velikovsky explains that the problem created by these supposedly twelfth century tiles with fourth century Greek letters cannot be explained by assuming that these were hieroglyphics that looked like Greek letters. “This presupposes that the Egyptians, who used hieroglyphics, also had an alphabetic system . . . presumably . . . known in Egypt for a thousand years or thousands of years . . . [but which] was never used to write down an Egyptian text.” And, “Despite the turning of ten thousand signs in different directions to facilitate a comparison, they still do not look like Greek letters.”

“The problem of the classical Greek letters on the tiles of Ramses III of the twelfth century, and of older Greek letters dating from . . . earlier, has never been solved, and is handled very much as though it were a parapsychological phenomenon. ‘Light will be thrown on the question someday,’ one of the quoted scholars wrote resignedly when he realized the scope of the problem and became aware of the impasse. [Griffith in Naville, The Mound of the Jew, p. 41] But for . . . generations now scholars have been turning away from the problem with no promise of a solution.”

53 Immanuel Velikovsky, Peoples of the Sea (NY 1977), pp. 7-8
54 ibid., pp. 9-10
55 ibid., p. 10
56 ibid., p. 11
In the final paragraph on this topic, Velikovsky uses a clear analogy that defines the solution to these Greek letters on Ramses III’s tiles:

“If, digging in the countryside, you find in the ground several pieces of ancient armor with heraldic emblems of an early king who died eight centuries earlier, but on the inside of the armor you discover a clear trade mark of a Sheffield manufacturing firm of Victorian days, and if you are certain of not being the victim of a practical joker, and the best experts assure you that the armor was wrought for Richard the Lionhearted, and other experts, equally good in their field, assure you that the trade mark is genuine and the Sheffield firm that used it was not in existence before the days of the Hanoverian Dynasty in Great Britain, then you, too, would say like Griffith, who subsequently became one of the great names in Egyptology, . . . “The question involves a great difficulty.” However, the chances are that you would be inclined to follow the opinion of the experts on the Sheffield steel trade mark.”

Dayton has also come to the conclusion that the Greek Dark Age never existed and supports this conclusion with several other forms of evidence. Peter James and John Bimson summarize his views:

“Finally, we would like to note that John Dayton, a field archaeologist in the Near East and specialist in ancient glazing techniques, has arrived independently at similar conclusions to those of Velikovsky. In his massively documented Minerals, Metals, Glazing and Man . . . Dayton . . . concluded that the dates for the Late Bronze Age are centuries too high, and that the nebulous ‘Dark Ages’ of the Ancient Near East (from c. 1200 to 800 B.C.) are a chronological fiction. Dayton describes the currently held model of Ancient Near Eastern chronology as ‘a card house of interrelated “facts”,’ built on tenuous evidence but with an unfortunate tendency to be self-perpetuating . . . it is not sufficient, as Stiebing has done, to simply cite ‘evidence’ from elsewhere in the ‘card house’.”

The view that there was a 400 to 500 year Dark Age in Greece requires one to accept that a literate people forgot how to read and write, how to make fine

57 ibid., p. 12
58 Bimson, James, op.cit., p. 77
pottery, jewelry, artefacts of gold, silver, and bronze, how to erect large stone buildings, how to carve ivory, etc. That view also requires one to believe that after this lengthy Dark Age these people not only recommenced their earlier achievements, but, in so doing, employed all these literary, artistic, and architectural modes largely in the same forms that they had used 400 to 500 years earlier. One must also accept that 90 percent of the population of Greece vanished or migrated, but to where they migrated is a mystery. Surely, had they moved to a new land, that region would exhibit all the literary, artistic, and architectural forms they practiced. And it requires that the Phoenicians of these centuries stopped trading with both Greece and Egypt for no reason whatsoever.

One must accept that the 18th Dynasty had iron 500 years prior to that metal coming to Egypt. One must accept that fine glass vessels were produced for 500 years and then the Egyptians forgot how to make such glass ware for 500 to 600 years and then re-made the same forms that they had forgotten. One must accept the dichotomy that if ivory dated to the 18th Dynasty, *circa* 1500 B.C., is found in foreign strata conventionally dated to 1500 B.C. it corroborates the established chronology; however, when the same forms and workmanship are found in 9th and 8th century contexts with 18th Dynasty objects, these were imitations or heirlooms. And on top of all of this the 19th Dynasty of Egypt, dated to the 12th century B.C., wrote Hebrew of the 8th to 7th century B.C. as well as making tiles with Greek letters that only come into existence around the 5th to 4th centuries B.C.

On the other hand, if both the Mycenaean Age of Greece and the 18th Dynasty of Egypt are lowered to the 9th to 8th centuries B.C., the Greeks do not forget how to write, do not forget the arts of making fine pottery, jewelry, and architecture, do not forget their skills in ivory carving. They do not vanish or migrate to an unknown and undiscovered country. All their achievements simply continue to develop in the normal manner of civilized growth, and the continuity of their history is attested to by all their achievement.

When the 18th Dynasty is moved down to 800 B.C., Tutankhamen has iron artefacts in his tomb because the Iron Age had begun in Egypt. There is a continual development of glass manufacture with similar styles going forward in time, just as with the artistic developments in Greece. Ivory artefacts of the same style and workmanship belong to the same period in Egypt and elsewhere. 8th to 7th century Hebrew in a 19th Dynasty letter is no mystery but belongs to the 19th Dynasty. 4th century Greek letters on 20th Dynasty tiles are not a mystery but belong to them because the 20th Dynasty existed in the 4th century B.C. The
Phoenicians never stop trading with Greece or Egypt and continue to ply their trade throughout the first millennium B.C. There are no problems, no anomalies, no *ad hoc* inventions to prop up the chronology. The only anomaly all this evidence generates is for the established chronology. One must invoke that chronology to argue against these rather clear-cut and obvious correlations or brush these correlations aside and ignore them.

But the existence of the Iron Age dagger and items in Tutankhamen’s tomb, several centuries prior to Egypt having iron, and Hebrew words and advanced terms from the first millennium in addition to Greek letters on 19th Dynasty tiles, rather calls for historical feats of legerdemain on the part of the historians. That the Phoenicians stopped trading for 500 years makes the mind boggle. The old capitalistic, Marxian economic motivation to make money disappears from history during this period if the Dark Age existed, but survives and continues to flourish if there is no Dark Age.

If there is indeed a case for a Greek Dark Age and a shortening of Egyptian history, should not the same problem of a Dark Age exist in Mesopotamia from about 1200 to 800 B.C.? Peter James, *et al.*, point out that this is precisely the case. They discuss “The gap in Assyrian archaeology”:

> “While it has been claimed that ‘Assyria was the one power in western Asia that survived the upheavals at the end of the Bronze Age’ [by Hallo and Simpson, 1971, p. 124], it is also agreed that it underwent a serious cultural and political recession at this time . . . described [by Roux, *Ancient Iraq*, p. 251] as the ‘Dark Age of Mesopotamia’. . . .”

> “Thus for over 250 [500] years, from the death of Tukulti-Ninurta I in 1208 BC to the renaissance . . ., Assyrian history is an almost complete blank—apart from the interlude around the time of Tiglath-pileser I. The gap in documentation extends to all kinds of literature. . . .”

> “Developments in art are also difficult to trace. Not only is there a dearth of material, but styles [as with the Dark Ages of Greece] on either side of the gulf . . . are curiously similar. . . .”

> “. . . even where better archaeological investigations have been carried out, the published reports are still disconcertingly silent about
material remains from the Dark Age. The temple built by Tiglath-pileser I at Assur is a striking exception.”

That is, many of the very same problems discussed above respecting the Greek Dark Age are also found around the same time for the Assyrians. The very same Dark Age problem also exists for Babylonians whom James et al. discuss in their subchapter “Babylonia the illiterate?” They cite John Brinkman, “Babylonia c. 1000-748 B.C., in CAH vol. III no. 1 (1982), p. 282:

“Babylonian history during the first quarter of the first millennium B.C. may be characterized as a period of obscurity or ‘dark age’, with the land frequently overrun by foreign invaders and with the central government often unable to assert its jurisdiction in many areas. Little source material has survived from these turbulent times, and this little is sometimes quite difficult to date.”

James, et al., go on to explain:

“The term ‘Dark Age’ seems like an understatement when the archaeological remains from Babylonia . . . are examined. Even the most important cities show little trace of activity over this long period. . . . the documentary record becomes a complete blank over a period of something like 350 years; archaeological remains are equally elusive. Ur returns to well-documented history and archaeology only at the end of the 8th century BC. Similarly, at Uruk, which survived as a major religious center . . . there is nothing to fill the gulf between the 12th century BC . . . and the renaissance of the city . . . [in] the 8th century BC . . .”

“An equally bleak picture is related by Brinkman [in his CAH article cited above, p. 284] for the whole country:

‘Archaeological sources are . . . meagre. Architectural remains which may belong to this time are usually minor repairs on older structures, with no inscription left to record the identity of the repairer. (In
fact, no buildings have yet been excavated in Babylonia which can be dated with certainty to the time of any ruler [during this period].

“. . . The sheer exasperation which the search for remains of [these] times produced can be judged from the following comment by the leading fieldworker Robert [McC] Adams [op.cit., p. 174]:

‘. . . [it] remains a little-known intercalary period, with attitudes towards its limited cultural attainments aptly summarized by casual reference to it among fieldworkers as the “V.D.” (Various Dynasties) period.’

“The later Babylonian lists enumerating the ‘various dynasties’ of this period provide little consolation. For many kings there is no contemporary evidence even to substantiate their existence . . .

“Are we really to believe that the Babylonians wrote almost nothing for some 250 [or more] years? Or have the records from this time simply perished? This possibility can be discounted completely. By its very nature cuneiform was never written on perishable materials such as parchment or papyrus. Clay is the only really suitable medium for the script, written by impressing a series of wedge shapes into wet clay with a stylus. Baked tablets can last almost indefinitely. . . .”62

The theoretical possibility that the records were deliberately destroyed by Nabonassar who collected and destroyed the records of the kings who lived before him, is also easily discounted:

“Even if Nabonassar (747-734 BC) had attempted to erase the evidence of his predecessors, it is inconceivable that he could have destroyed not only their royal inscriptions, but also every economic and private document mentioning them from the whole of Babylonia. . . .

“The scarcity of the documents from [this] period poses yet another riddle. Given the tiny number of surviving texts, how could literacy have been preserved at all? Babylonia, from the 8th century

62 ibid., pp. 279-281
BC onwards, was widely respected by its contemporaries (including the Assyrians, Hebrews and Greeks) as a centre of literature, possessing an immense corpus of written knowledge from mathematics and astronomy to medicine and philosophy. Writing . . . formed the very basis of the carefully ordered Babylonian society, dependent on its day-to-day records of business transactions, sales of lands, wills, loans of money etc. How the complex Babylonian administrative or commercial systems could have survived for so long with so few written documents is simply unfathomable.”

As with the Greeks we are expected to accept that, during the Dark Age in Mesopotamia, Assyrian and Babylonian society came to a halt. They stopped keeping cuneiform records. They stopped building. They stopped producing pottery and other art forms. And then, when the Dark Age ended, they recommenced writing, building, and production of pottery etc., in the same manner as prior to the Dark Age. The probability that all these societies all experienced a Dark Age at the same time defies reason. James et al. produce evidence of a similar Dark Age in Iran where they cite René Labat, “Elam and Western Persia c. 1200-1000 B.C.,” *CAH* II:2 (1975), p. 503: “This period was followed by a dark age of three centuries, during which there are no native texts . . . Elam was not to be mentioned again until 821 BC . . .” They further cite Elizabeth Carter who could not accept that there was a Dark Age in Iran: “Yet she was at a loss to provide an alternative explanation.”

In the next chapter, we will encounter this Dark Age yet again. Removing the Dark Age resolves all the absurdities of history created by its imposition by the historians for this period.

Lastly, the 18th, 19th and 20th Dynasties as well as those back to the 12th Dynasty all fall within the period of which Rose has shown on the basis of astronomical dating that it does not permit these dynasties to be placed in the second millennium B.C. but that they need to be placed in the first millennium B.C. The fact that this time chasm is closed in Egypt and that there is a similar time chasm in Mesopotamia as shown on the dig at Tell Munbaqa in addition to all the other evidence discussed in this chapter and elsewhere makes it not improbable, but impossible to resurrect the now discredited long chronological history of the ancient Near East.

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63 *ibid.*, pp. 281-282  
64 *ibid.*, p. 285  
65 *ibid.*
Above and beyond all of the foregoing is the fact that the Mycenaean Greeks from the 16th through the 12th centuries were producing bronze items, as Velikovsky discussed. Tin only came to this region after 1100 B.C. As John Chadwick points out66, “It is the source of the tin which is the puzzle. . . there are objects of bronze at Knossos. . .”. At Pylos, “there is a long series of documents listing the smiths . . . and the amounts of bronze issued to them.” Chadwick presents a long list of bronze artefacts found in Mycenaean centers including wheels described simply as “of bronze”. On page 161 is displayed a suit of “Bronze armour and helmet from Dhendra.”

That means that for hundreds of years prior to the time tin is imported into this region, the Mycenaean Greeks were manufacturing tin bronzes. But as with all the rest of the evidence, if we move the Mycenaean in the first millennium B.C., the problem of the sources of their tin vanishes.

So, too, with the Bronze Age Egyptians of the 18th and 19th Dynasties, running from 1570 B.C. to 1185 B.C. in the conventional chronology. They also were using and making tin bronzes for hundreds of years before tin was available and by moving these dynasties into the first millennium B.C., the problem of the sources of their tin also disappears.67

The technological data related to bronze forces us to place these dynasties in the first millennium B.C. Linguists of Hebrew and Greek demand the same placement for these dynasties. The totality of the evidence against the Dark Age of Greece can only be denied by ignoring these facts or by ad hoc inventions.

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As the reader may recall, at the very beginning of this book we referred to Ramses II’s epic encounter with the Hittites at Kadesh, an event of which we possess both a Hittite and an Egyptian record. On the basis of this unimpeachable connection, the Hittites were joined to the trunk of Egyptian history and placed in the latter to middle part of the second millennium B.C. However, in the previous chapter we looked at several forms of evidence that would instead place Ramses II and his time in the first millennium. The evidence of iron production and metallurgy in general is incompatible with a second millennium location, as indeed is the Dark Age of Glass in Egypt between 1000 and 600 B.C., as well as artistic carvings of ivory, advanced Hebrew words as well as Greek letters on Ramses III’s tiles. There is of course altogether an enormous body of evidence pointing in the same direction, some of which is outlined in Velikovsky’s *Ages in Chaos* series. What has been highlighted here are merely a few interesting items from the literature that speak against placing the 18th, 19th, and 20th Dynasties in the second millennium B.C.

How strong is the basic research that supports the chronological placing of the Hittites in the mid-second millennium B.C.? According to Amelie Kuhrt, as late as the mid-1990’s:

“It is only from c. 1650 [B.C.] on that we can begin to reconstruct Hittite history—although problems of chronological sources loom large, with occasionally totally blank periods . . . Similarly, although one of the most important texts for Hittite institutional and early history date to c. 1500 (the Edict of Telepini . . .) very little is known about Telepini himself. Again the period from his reign down to c. 1430 (1420) . . . is veiled in total obscurity, not helped by intense scholarly disagreement as to which and how many of a number of Hittite royal names might be assigned to this time span. . .

“The very approximate chronology of the Hittite kings depends entirely on correlations with material [such as pottery] outside Anatolia, and yields only unsatisfactory results. No precise regnal years are
preserved for any Hittite king, and there is no Hittite ‘king list’ of the type preserved in Babylonia, Assyria and Egypt. The later Hittite kings, begin [ . . . ] with Suppiluliuma I of c. 1370-1330 (or 1344-1322 . . . ). After Suppiluliuma I, the Hittite kings usually introduce text by tracing their genealogy back to him, which helps to establish at least the succession of the kings and their relationship to each other.”¹

Thus what we encountered earlier, with regard to dating via ancient documents, is also the case with the Hittites. More significantly, however, the dating of the Hittite civilization falls (in part) in the period between the Mitanni and the Old Akkadians, a period which the dig at Tell Munbaqa proves never existed. That is, dating the Hittites back as far as 1900 B.C. places them into a period of 550 years which simply does not exist. Is it any wonder that their chronology, based on the methodology of joining them to Egypt or other cultures for this non-existent time, has to be loaded with totally blank periods that are “veiled in total obscurity” so that their approximate chronology depends on correlations with materials outside Anatolia with dates that are unsatisfactory? Hittite chronology has nothing truly resembling a solid foundation in spite of the great and growing research into it.

The position taken by Emmet J. Sweeney in his Ramessides, Medes and Persians (Forest Hills 2000) is that the Hittites were actually the kingdom of the Lydians and thus must be placed in the first, rather than the second, millennium B.C. If this is indeed the case, then the reign of Ramses II, or the 18th Dynasty of Egypt, which is tied to the Hittites, also falls in the first, rather than the second, millennium. This chapter will explore Sweeney’s thesis.

The Hittite Empire is conventionally dated from about 1400 to 1190 B.C., while it is thought that Old Hittite texts from about 1650 to 1595 B.C. are preserved in copies from the empire period. They are believed to be the oldest texts in an Indo-European language thus far discovered.² Because of the insertion of the Hittites in the second millennium, their history, like that of the Greeks and others, also displays a considerable Dark Age. Of this Velikovsky reports:

“The industrious digging of the last decades, the period from —1200 to —750 for most parts of Anatolia still lies in complete darkness.’

¹ Amelie Kuhrt, op.cit., p. 229
“These are the words of Ekrem Akurgal, a prominent Turkish archaeologist who carefully surveyed large regions of Asia Minor. The area contains no relics of art or industry, no remains of human culture or even habitation, for a full span of four hundred and fifty years. [Akurgal goes on to say:]

“‘It follows that any relics of a culture between —1200 and —750 in central Asia Minor, especially in the highlands, are lost for us beyond retrieving. . . . [And Akurgal adds:]

“‘Also in the southern part of the peninsula the early Iron Age, or the period between —1200 and —750, is enshrouded in darkness.’

“To come to such a conclusion and still to adhere to the accepted chronological timetable, a scholar needed to be completely convinced that in no excavated place of so large a region any artifact or any inhumed body from four to five consecutive centuries could be found. How utterly depopulated must have been the area which in the time of the ‘Hittite Empire’ was populated by many nations that carried on intercourse in commerce and in diplomatic relations, had cultural exchange and manufactured goods in abundance.”3

Nevertheless, art historian O. Puchstein in Pseudohethitische Kunst (Berlin 1890) claimed that the motifs of the sculptures and the details of artistic execution required that Hittite art was produced between the tenth to sixth rather than the fourteenth to thirteenth centuries B.C. On page 13, Puchstein wrote:

“‘All those sculptures show clear signs of a much later time of origin; therefore their being the creation of the Egyptian Kheta [Hittites] is excluded.’ [On page 14 he adds:]

“‘In any case, there is neither here [in Asia Minor], nor in northern Syria, evidence that the so-called Hittite sculpture existed already in the tenth century B.C. This fact seems to me incompatible with the views of [A.H.] Sayce. For him, the greatest expansion of power of the Hittite Empire, and with it also the prime of Hittite art, lies almost half a millennium before the time in which the extant

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3 Velikovsky, Ramses II and His Time, op.cit., pp. 157-158
monuments of ancient Commogene and Asia Minor were created.’ [On page 22 he adds:]

“‘Therefore the art which has produced these and similar works does not have to be ascribed to the enigmatic Hittites of the second millennium B.C., but should be regarded as a remarkable sign of the then highly developed culture of the population of Asia Minor and Commogene in the time from 1000 to 600 B.C.’”

James et al. further inform us:

“A further major objection [to the Hittite chronology] concerned the time-scale involved. The Egyptian references to the ‘Kheta’ are dated to the 15th-13th centuries BC. On the other hand, Assyrian records mentioning the ‘land of Hatti’ [Hittites] came largely from the 1st millennium BC: they describe a group of prosperous city-states in south-east Anatolia and northern Syria. . .”

One thing that cannot occur if the established chronology is valid is that after the Hittite Empire fell around 1200 B.C., Assyrian kings could be making war on them after 1100 B.C. Since the Neo-Hittites ruled in Syria after 1200 B.C., it could be argued that the Assyrians were making war on these people. However, Donovan Courville, citing D.D. Luckenbill’s *Ancient Records of Assyria and Babylonia* (1926), par. 96, shows “Tiglathpileser I refers to Carchemish in the land of Hatti, a site certainly not in Syria, but in the [Anatolian] territory known to belong to the Hittites [before they fell].”

Tiglath-Pileser I reigned from about 1115 to about 1077 B.C. in the conventional chronology. Carchemish had long been defeated and the Hittites were supposedly living in Syria. So why was Tiglath-Pileser making war on a defeated empire by attacking a defeated city? Citing Dame K.M. Kenyon’s *Archaeology of the Holy Land* (1960), page 134, Courville further shows: “As late as the mid-9th century [B.C.] Shalmaneser III makes reference to the conquest of fortified cities of the Hatti. This same king refers to a ‘king of Hatti’ as distinguished from the kings of Damascus in Syria, indicating that the land of the Hatti is not the same as the land

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4 *ibid.*, p. 144
5 James *et al.*, *op.cit.*, p. 115
of Syria.” Shalmaneser III ruled from 858-824 B.C. That is over 300 years after the Hittite Empire fell, Shalmaneser III was still making war against it. Speak of beating a dead horse!

But if the Hittites lived in the first and not the second millennium B.C., would not the level of their culture reflect just this condition? Velikovsky points out:

“The royal annals found at Boghazkoi are composed in a manner that reveals a close relation to the Assyrian royal annals of Sennacherib, Esarhaddon, and Assurbanipal of the seventh century [B.C.]. [A. Götze, Das Hethiter-Reich, in Der Alte Orient, XXVII, 2 (Leipzig, 1928), p. 44]

“Other texts of Boghazkoi establish that ‘Babylonian magic and medicine and astronomy were known and cultivated in Asia Minor. Also, a translation of the Gilgamesh epos was found there.’ [ibid., p. 45] The ‘Hittites’ had in common with the Babylonians scholarly works, hymns, writings based on historical traditions, vocabularies, and other literary works. [H. Güterbock, “Die historische Tradition und ihre literarische Gestaltung bei Babyloniern und Hethitern bis 1200,” Zeitschrift für Assyriologie, XLIV (1938), p. 45]

“Assyrian justice, as far as civil laws were concerned, had much in common with the civil laws of the Boghazkoi archives. [L. Aubert, “Le Code Hittite et l’Ancien Testament,” Revue d’histoire et de philosophie religieuses, IV (1924), 352-70]

“The Assyrian Empire is supposed to have begun its ascendancy after the fall of the ‘Hittite Empire.’ But in some ways the ‘Hittites’ were more advanced than the Assyrians, and consequently it is assumed that the Assyrians regressed culturally as compared with the ‘Hittites.’ [G. Contenau, “Ce que nous savons des Hittites,” Revue historique, CLXXXVI (1939), p. 15]

“Scholars wonder about the unknown cause of this retrogression . . . They wondered how it could be that the ‘Hittite’ culture of the

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7 ibid., see also Luckenbill in same par. above.
fifteenth to the thirteenth centuries, in all that concerns science, law, literature, royal annals, traditions, habits, and omens, so closely resembled the culture of the Assyrian Empire of the eighth and seventh centuries and of the Neo-Babylonian Empire of the seventh and sixth centuries."^8

Thus the Hittites employed a whole plethora of cultural concepts that did not come into use for several hundred years after they had been destroyed. This again suggests that they did practice these in the first millennium and not the second. When the science, law, literature, royal annals, etc., have close affinities with the Assyrians and so-called Neo-Babylonians of the first millennium and not with the other cultures supposedly present in the second millennium B.C., it again supports the conclusion that the Hittites were the Lydians of the first millennium.

William Albright, looking at the art of the Hittites, challenged their second millennium placement by such eminent authorities as Henri Frankfort and others who claimed the art of the Hittites was divided by a gap of several centuries:

"... the refusal of an earlier authority to recognize the existence of any monumental art or architecture in the neo-Hittite states between 1200 and 850 B.C. was entirely wrong... the eleventh and tenth centuries were the golden age of Syro-Hittite art and architecture'."^9

While most historians claimed that between 1200 and 800 B.C. the Hittites experienced a "Dark Age", art experts claimed that the art and architecture of that period was that of a "Golden Age." Other authorities who carried out excavations in Anatolia, such as David Hogarth, Leonard Woolley, and T.E. Lawrence, whose story is portrayed in the film "Lawrence of Arabia," held that the monumental art, as for example at Carchemish, clearly resembled first millennium Assyrian art, but under pressure from the historians retreated from this position. Woolley for example assigned certain sculptures to the period before 1200 B.C., but Max Mallowan begged to differ:

"The chronology and sequence dating of the rich series of sculptures discovered at Carchemish remains a problem, even after 60 years of investigation, but it is generally recognised that Leonard

^8 Velikovsky, *Ramses II and His Time*, op.cit., pp. 91-92
^9 James et al., *op.cit.*, p. 123
^10 *ibid.*, pp. 124-127
Woolley exaggerated the antiquity of some of the [items] and it is no longer possible to assign any of them to the second millennium B.C.””

To bridge this gap, the “heirloom” concept was called upon:

“In a tomb securely dated to the 7th century BC, Woolley found a series of small gold figures which bear a striking resemblance to the pantheon on the frieze at Yazilikaya [in Anatolia], conventionally dated to the 13th century BC. Hans Güterbock noted that this discovery ‘links the Late Hittite period with the time of the Empire . . . There is no doubt that both in style and in subjects these figures . . . are Hittite in the sense of the Hittite Empire at Boghazköy.’ Yet he wondered: ‘How did carvings of the thirteenth century get into a tomb of the seventh?’ Woolley himself considered that the jewellery was manufactured during Neo-Hittite times, but in a style which had, somehow, been preserved for 500 years. Güterbock preferred to see them as heirlooms . . . or, alternatively, that they had been carried there by . . . Hittites who had . . . [looted] Late Bronze Age centres . . . [after] they were sacked by barbarian invaders around 1200 BC.””

Just as in Greece, artistic creations of the 12th century are clearly mirrored in those of the 8th century B.C.

If indeed conventional chronology is correct, then the stratigraphy of central Hittite Anatolia should reflect a second millennium B.C. placement. If the conventional chronology is in error, then the stratigraphy of this region should reflect a first millennium B.C. placement. “The archaeology of the Phrygians is . . . reasonably well dated with their first remains placed no earlier than the end of the 9th century B.C.””

Under such conditions, with the Hittites flourishing and disappearing prior to the rise of the Phrygians, we should not expect to find strata containing pottery elements from both nations mixed together. If, on the other hand, the Hittites are the Lydians, living in the first millennium B.C., they could be mixed together. In this respect, James et al. state that at Gordion in Central Anatolia, near the region of Boghazköy:

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11 ibid., pp. 127-128
12 ibid., pp. 128-129
13 ibid., p. 138
“Soundings taken from the underlying strata of earlier phases produced completely unsuspected results. Most Hittite settlements are sealed with a clear destruction level, separating them from any traces of subsequent occupation. No archaeological relationship between the Hittites and Phrygians had therefore been envisaged. At Gordion, however, there was no such break. Instead, the two cultures appear to have coexisted for a considerable time.

“This process was clearly reflected in the successive levels at Gordion. The topmost contained mainly Phrygian pottery, including two painted sherds, but also ‘a large admixture of late Hittite.’ The next ‘produced pottery about half Hittite, half Phrygian,’ the third layer ‘produced again about half Hittite, half Phrygian pottery,’ the Phrygian ware being a mixture of wheel-made and hand-made; the fourth level contained ‘Hittite pottery with a minimal representation of Phrygian, both wheel-made and hand-made’; ‘all the layers below were of Middle and Late Bronze Hittite times.’” [R.S. Young, “The Gordian Campaign of 1965,” American Journal of Archaeology, vol. 7 (1966), p. 276]

Various ad hoc explanations were devised to explain this anomaly – a favored one being that the pottery became mixed together because people kept digging pits; but such a situation would create a jumbled stratigraphy without any significant sequences of style, etc. The fact is that the Phrygian pottery steadily increases in frequency the higher one goes in the four levels, and the technologically more advanced wheel-made pottery of the same people also increases in relation to the proportion of their hand-made ware.

The other solution was to divide the strata into an early and late Dark Age, but this fails to explain how Hittite pottery could have been produced when it no longer existed.

Above and beyond all that, Greek vases were found in Hittite “mound tombs . . . which their discoverers [G. and A. Körte, Gordion (Berlin 1904)] assigned to the seventh and sixth centuries [B.C.].” At Alisar some fifty miles from the Hittite site of Boghazköy were also found “Hittite pictographs together with ceramics painted in [Greek] geometrical designs found also in Gordion [of the

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14 ibid., p. 139-140
15 ibid., p. 140
16 Velikovsky, Ramses II and His Time, op.cit., p. 147
seventh to sixth centuries B.C.]”¹⁷ And further, “The excavators at Boghazkoi assigned the buildings (stratum II) to the period of the ‘Hittite Empire’ in the second millennium [B.C.], but they were compelled to admit that these buildings ‘must have been still occupied in the seventh century [B.C.].” [K. Bittel, H. Güterbock, “Bogazkoy,” Abhandlungen der Preussischen Akademie der Wissenschaften, Philosophische-historische Klasse, 1935 (Berlin 1936)]¹⁸

Therefore, we have both first millennium B.C. artefacts of the Greeks and Phrygians in Hittite sites which discredits the chronology that postulates that the Hittites, because they are tied to Egypt, are of the second millennium B.C., and supports the short chronology which moves Egyptian history from the second to the first millennium B.C.

Allowing the Lydians to be the real Hittites requires no gaps and no Dark Age. If the Lydians were the Hittites, then their history should also parallel that of the Hittites. This is what Emmet Sweeney has attempted to demonstrate in his recent book. For those who have little or no knowledge of history, the various names that will be presented will, perhaps, become quite confusing. Sweeney’s material, which is strictly historical in nature, is aimed at those familiar with these names and the history associated with them. Therefore, those who find the next few pages incomprehensible, are asked to either read them and not worry about confusion, or merely to skip them and pick up the text under the heading “Linguistic Evidence.” With this in mind, here is the text of Sweeney’s work. The following are pages 56 through 58 of Ramessides, Medes and Persians. This material is most vulnerable to historical criticism because documents for the Lydians as well as for the Hittites are not complete and important ones are surely missing.

“Classical sources inform us that [the Lydian] Alyattes, whom we equate with [the Hittite] Hattusilis, was a mighty king who waged war against many of his neighbours, and who subjugated most of the Aegean coast of Asia Minor. This certainly does not contradict what we know of Hattusilis, whom we have already seen engaging in a war of words with Adad-Nirari III.

“We know that Hattusilis maintained and extended Hittite control over western Asia Minor, and his victories in the Far West are commemorated in various surviving documents. The list of Hittite

¹⁷ ibid., p. 148
¹⁸ ibid.
allies at the battle of Kadesh ‘mentions several peoples who all . . . are hitherto already familiar and recognizable from the Hittite imperial records as being the names of peoples of Western and Central Anatolia.’ The writer of these words, R.D. Barnett, offers the following identifications of these names:

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\begin{align*}
Drdny & = \text{Dardanoi (Homeric name for Trojans)} \\
Ms & = \text{Mysia (a region of Asia Minor)} \\
Pds & = \text{Pitassa (either Pedasa, near Miletus, or Pedasos, in the Troad)} \\
Krks & = \text{Karkisa (Caria)} \\
Lk & = \text{Lukka (Lycia)}
\end{align*}
\]

If these identifications are broadly correct, and virtually no authority denies it, then the Hittites were at that time in control of most of western Asia Minor.

“Herodotus informs us that early in his career Alyattes moved to strengthen Lydian control over Asia Minor, a control established by his immediate predecessors. As part of this policy, we are told, he attacked the Greek port of Miletus, continuing a war initiated by his father [the Lydian] Sadyattes. By our reckoning Sadyattes must of course be the same as Hattusilis’ father [the Hittite] Mursilis, and we must expect this king to be involved in military action on the Aegean coast. Sure enough, Hittite records tell us that Mursilis attacked and conquered a city on the Aegean coast named Millawanda (generally agreed to be Miletus), a settlement which had been the property of the king of Ahhiyawa (generally agreed to be Achaea — i.e. Greece). From the records of Mursilis we find that the king of Ahhiyawa at this time was called Antarawas, a name that has been identified with the Greek Andreus. Twelve years later he names another king of Ahhiyawa, this time Tawalagawas, who is also known as “the Ayawalawas”. This has been interpreted as Eteocles the Aeolian.

“Yet these clear references to Greek settlements in Hittite documents of supposedly the 13th century BC have caused the utmost embarrassment to scholars, since the Ionic and Aeolian colonies are not dated by anyone earlier than the 10th century BC. But if we are actually
in the 6th century BC, there is no problem, and Greek settlements, as well as a Greek city of Miletus, are entirely to be expected.

“During the time of [the Hittite] Mursilis the province of Arzawa, the Lydian heartland, rebelled. Uhha-Zitish, the rebel leader, was, we are told, defeated in a great battle, and pursued to the town of Apasa, identified with Ephesus. Mursilis followed him to Apasa, but Uhha-Zitish had fled “across the sea”, no doubt to Greece.

“Thus it would appear that, during and directly preceding the reign of Hattusilis, the Hittites were busy consolidating their hold over the peoples of the Aegean coast, a situation which agrees precisely with what we know of the Lydian kingdom in the time of Alyattes and his immediate predecessors.

“Herodotus mentions the fact that one of Alyattes’ greater successes was his conquest of Smyrna, and sure enough, a stela of Mursilis, Hattusilis’ father, stands at Karabel, just outside the city.

“In the end, we are told, Alyattes failed to conquer Miletus. That would explain why Hattusilis makes no mention of a successful war against Millawanda. He recalls with pride however his successful fifteen-year war against the Gasga (whom we equate with the Scythians), a fact which recalls Alyattes’ achievement of driving the Cimmerians out of Asia.

“Alyattes, we have seen, was also involved in prolonged warfare on his eastern front against the Medes. In the records of Assyria these wars are recalled in the various campaigns waged by Adad-Nirari III, Shalmaneser IV and Ashur-Dan III against the Great King of Urartu, Argishti. Peace was, however, briefly restored in this region when a major battle was interrupted by an eclipse.

“We know rather a lot about [the Lydian] Croesus because Herodotus describes his ill-starred war against Persia in some detail. Croesus’ kingdom became a byword for power and wealth, yet in the ground archaeologists have found precious little material evidence of this much-vaunted opulence. But of course if the Hittite Empire is actually Croesus’ realm then all is explained.
“According to the reconstruction proposed here, Croesus must be one and the same as the last Great King of the Hittites, known in the cuneiform documents of Boghaz-koi as Tudkhaliash IV. The Hittite Empire was at the very peak of its power when Tudkhaliash IV ascended the throne, yet by the end of his reign it came crashing to destruction. Tudkhaliash IV had inherited from Hattusilis a military machine second to none in the region, and from the archaeological evidence it is clear that he had maintained all the territories added to the Empire by Suppiluliumas and Hattusilis. What then could have caused the sudden and dramatic reversal of Hittite fortunes in the latter years of Tudkhaliash’s life? If we are correct, it was an ill-advised war with Persia.

“Herodotus informs us that in preparation for the war against Persia, Croesus forged alliances with other kingdoms, one of which was Babylon. Sure enough, we find Tudkhaliash in a defensive alliance with Babylon against Assyria. This alliance was concluded, it appears, very shortly before the king of Assyria, Tukulti-Ninurta, commenced hostilities with the Hittites. We shall, of course, identify this second Tukulti-Ninurta with Cyrus the Great.

“It would appear, incidentally, that Tukulti-Ninurta’s later attack on Babylon was a direct result of the Hittite alliance, for we hear the Assyrian king accuse the Babylonian monarch of aggression against Assyrian territory.

“Herodotus informs us that it was the Lydians who started the war. Croesus, he says, crossed the Halys river (close to Boghaz-koi) and,

“‘came to a place called Pteria, in Cappadocia. . . . Here he encamped, and ravaged the lands of the Syrians, and took the city of the Pterians, and enslaved the inhabitants; he also took all the adjacent places, and expelled the inhabitants, who had given him no cause for blame.’

“In like manner, it would appear that it was Tudkhaliash the Hittite who initiated hostilities against the king of Assyria. In a treaty with his vassal Shaushga-muwash of Amurru, the Syrian prince was
called upon to furnish aid in the event of an Assyrian attack, as well as instigate a trade blockade against Assyria:

"‘As the king of Assyria is the enemy of My Sun, so may he also be your enemy. Your merchants shall not go to Assyria, you shall not allow his merchants in your land, and neither shall they pass through your land. If, however, one of them comes into your land then seize him and send him to My Sun. As soon as the king of Assyria begins war, if then My Sun calls up troops and chariots... so do you call up your troops and chariots and despatch them.’

"If we are right, the above letter is an actual command from Croesus to one of his vassals to provoke a conflict with Cyrus. The ensuing war however was a disaster for the Hittites, and we find the king of Assyria boasting of the capture of over 28,800 Hittite prisoners. This indeed was to be the first, but not the last great victory of Tukulti-Ninurta, for his subsequent conquest of Babylon was regarded as possibly even more memorable. It should be noted, of course, that the various campaigns of this Tukulti-Ninurta offer a precise parallel, in terms of the order of events, with the military career of Cyrus.

"The defeat and destruction of the Hittite Empire was long recalled by Tukulti-Ninurta, and in a triumphant summing-up of his early campaigns he boasts that he had conquered everything “as far as the frontier district of Nairi [Armenia/Anatolia] and the frontier district of Makan [Egypt] to the Euphrates.” The account of the Nairi war is sometimes followed by the statement that he made Azalzi and Shepardi his frontiers. Shepardi, apparently a variant of Subartu, is of course identical to Sparda, the normal Persian word for Lydia. Azalzi, it would appear, may well be a form of Sardis. Since vowels are conjectural the word may be read as Zalzi, and with the interchangeability of “l” and “r” this may be written Zarzi or even Sarzai.

"Some scholars claim to have identified two semi-peripheral Great Kings of Hatti after Tudkhaliash IV; yet these men, an Arnuwandash and a Suppilluliumas, belong in an earlier epoch, and the Tudkhaliash they follow was Tudkhaliash I. The truth is that the Hittite Empire comes to a definitive end with Tudkhaliash IV, whose final fate
is said to be unknown. If however he is the same person as Croesus, we know that, being spared execution, he accompanied first Cyrus (Tukulti-Ninurta) and then his son Cambyses (Shalmaneser) in their various campaigns throughout Asia and North Africa.

“It remains for a later chapter to see how Tukulti-Ninurta/Cyrus went on to conquer the city of Babylon, whose king he carried off in chains but later reinstated, before finally making himself king of the city and changing his Assyrian title to Tiglath-Pileser (Tukulti-apil-essharra).]

The validity of this historical sketch can only be proved by it correlating with and being corroborated by scientific and technological evidence which we will turn to shortly.

**Linguistic evidence** also plays a role in identifying the Hittites as Lydians. According to Macqueen:

“As well as cuneiform Hittite [records found in Anatolia], several other languages are used in these texts, and one of them is named as Luwian. The curious thing is that ‘Hieroglyphic Hittite’ [the Hittite language written in a special form of hieroglyphics] is much more closely related to Luwian than it is to cuneiform Hittite [closely related to the hieroglyphic text but not identical; written in cuneiform script], and the two [Hittite hieroglyphic and Luwian] seem in fact to be dialect variants of the same language. But we can go further than this. Luwian is presumably the language of the area known in the cuneiform texts as Luwiya, and Luwiya can be seen to be the earlier name of an area referred to in the Hittite Imperial period as Arzawa. And so we reach the final position that the language originally known as Arzawan [which was dropped by the linguists] is in fact the language of the Hittites, while the language written in ‘Hittite hieroglyphics’ is a dialect of the language of Arzawa.”  

Arzawa has been equated with western Anatolia, either the “Turkish Lake District, or more probably . . . Lydia.” Therefore we have both the Hittites and Lydians occupying almost the same regions of Anatolia. Let us see how closely

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19 Macqueen, *op. cit.*, p. 25
20 *ibid.*
related Hittite is to Luwian. Johannes Friedrich has provided parallel texts of Hittite and Luwian, where the similarities are evident:

Luwian:  
\begin{align*}
\text{d} & \text{Sa-na-ta-aš \ LUGAL-uš} \\
\text{d} & \text{An-na-ru-um-mi-en-zi} \\
\text{d} & \text{Lu-u-la-hi-in-e-za tar} \\
\text{d} & \text{Su-u-la-hi-ia-aš-š a-an} \\
\text{d} & \text{ku-m-zı \ hi-is-hi-ia-an-ti} \\
\text{Hittite:} & \text{MARDUK kat-ti-ma-at-ta} \\
\text{Hittite:} & \text{In-na-ra-u-wa-an-as \ LU}^{\text{ME}} \\
\text{Hittite:} & \text{hu-up-pa-ra-za} \\
\text{Hittite:} & \text{hu-up-ru-uš} \\
\end{align*}

The translation from the Hittite is “Come Marduk (= Luwian ‘King Santa’), but let us with you come the robust (?) (gods), don the bloodstained (clothes), (also) the Lulahi (gods) who (are) swathed in . . .” The languages are close enough to one another to allow the linguists to show that they are parallel texts; however, they are not identical. As Ernst Doblhofer states, the Hittite “language turned out to be closely akin to, although not identical with, Luwian. . .”

In the early part of the 20th century, the Lydian language was thought to be more closely related to Greek than Hittite. But research changed all that.

“Linguistically Lydian too is related to the Hittite-Luwian group, but the curious thing is that unlike most of its contemporaries it seems to be a descendant of the Hittite rather than the Luwian sub-branch.”

Sweeney, who presented this material in his Ramessides, Medes and Persians, page 11, unfortunately got the page reference in Macqueen’s book as 59, instead of 159, and omitted “a descendant of the . . .” These errors, nevertheless, do not detract from his conclusion, which is drawn from page 25 of Macqueen’s book and not page 159 as cited by Sweeney:

\begin{itemize}
  \item [21] Johannes Friedrich, Extinct Languages (NY 1957), p. 79
  \item [22] Ernst Doblhofer, Voices in Stone, transl. Mervyn Savil (NY 1961), p. 196
  \item [23] Macqueen, *op.cit.*, p. 159
\end{itemize}
“And so we reach the final position that the language originally known as Arzawan [which was dropped by the linguists] is in fact the language of the Hittites, while the language written in ‘Hittite hieroglyphics’ is a dialect of the language of Arzawa.”

Thus, the conclusion Sweeney reaches is: “In other words the Lydian language is one and the same as that of the Hittites in their Cappadocian heartland . . . rather than Luwian, a [closely] related tongue employed by many peoples of Asia Minor and Anatolia, such as the Phrygians and Lycians.”

Velikovsky discussed this finding as well: “‘Hittite’ was the language most commonly used during the Empire period. Modern scholarship found that Lydian ‘seems to be Hittite’ [McQueen, loc.cit.] — THE LYDIAN AND THE ‘HITTITE’ KINGDOMS WERE CONTEMPORARY AND USED THE SAME LANGUAGE.” [capitalization added]

Furthermore, of the documents in hieroglyphic Hittite only a few date to the second millennium B.C., but most rather date to the first:

“Comparatively few of the extant inscriptions in this [hieroglyphic Hittite] script have hitherto been found in the territory of the Hittite empire proper, the greater number dating from . . . the tenth to the eighth century B.C. . . . the last examples of it . . . c. 600 B.C.”

Louis De la Haba explains: “These Neo-Hittites [of the first millennium B.C.] . . . used hieroglyphics for the Luwian language exclusively.”

In order to explain this curious anomaly, Macqueen writes:

“One has to assume that in the disturbances following the collapse of the Hittite Empire a central Anatolian group had seized power among the ruins of Arzawa, and a memory of this may be

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24 ibid., p. 25
25 Sweeney, op.cit., p. 11
26 Velikovsky, Ramses II and His Time, op.cit., p. 90
27 David Diringer, Writing (NY 1962), p. 68
preserved in the Herodotean story of a Heraclid dynasty with eastern connections which gained power in Lydia about 1200 B.C.”29

Colin Renfrew shows that one explanation to resolve this anomaly does not stand up to scrutiny:

“It is often assumed that Hattic was earlier [than the Hittite language] in Anatolia, but that is simply the consequence of assuming that the Hittites were themselves an intrusive population. In reality, no good evidence has ever been advanced to document the notion that the Hittites were intrusive to Anatolia.”30

Renfrew maintains that “Hieroglyphic Hittite [spoken during the Empire Period] found on seals and inscriptions, but not on the tablets, records a dialect of Luwian.”31

But accepting that the Lydians spoke the same language as the Hittites of about 1200 B.C. and continued to speak it well into the first millennium creates a tremendous problem. First one must accept that two totally different peoples from two periods of time in the same region spoke the same language. Second, one must also assume that the language spoken in around 1200 B.C. did not change much for 600 years or more.

Living languages do change with time. The theory related to the amount of change that occurs over time has been presented in linguistic analysis as glottochronology. Although the rate of change is highly disputed by linguists there is no doubt that living languages drop and add words and mutate in other ways. Dead languages, such as Latin, do not change; the Latin taught to Shakespeare is the same Latin taught to Catholic students in parochial schools today. Trevor Bryce explains:

“No living language remains static. Over a period of several centuries it will undergo a number of changes in its mode of expression, its grammar, its orthography, and in the characteristics of the script used to write it. . . . We would have little difficulty today in determining whether a piece of English prose was written in Elizabethan, Victorian,

29 Macqueen, *op.cit.*, p. 159
31 *ibid.*, p. 293, note 5
or modern times, on the basis of style, idiom, vocabulary, and even handwriting. Similarly the Hittite language underwent a number of changes during the five centuries of the Hittite kingdom.”

Bryce suggests that in order for the language not to change, a number of individuals within the royal court, to proclaim their genealogy, as well as other kinds of documents, spoke or used the older language. But this does not explain why the Lydians used the same older form of Hittite.

For example, Jared Diamond compares the form of Old English used in 800 to Middle English employed in 1066. In Old English we read:

“Drihten me raet, ne byth me nanes godes wan
“And he me geset on swythe good foreland
“And fedde me by waetera stratum.”

However, the same passage in Middle English reads:

“Our Lord governeth men, and nothing shall defailen to me.
“In the stid of pasture he sett me ther.
“He norissed me upon water of fyllying.”

These passages are from the first three lines of the 23rd Psalm of David:

“The Lord is my shepherd. I shall not want;
“He makes me lie down in green pastures
“He leads me beside still waters.”

Those speaking Old English would be incomprehensible to those who spoke Middle and more modern English, and vice versa. The same problem exists with the proposal that the Lydians spoke the same language as the Hittites several hundred years after the Hittites had disappeared from the pages of history. Over six

33 Jared Diamond, The Third Chimpanzee (NY 1993), p. 275
34 ibid.
centuries a living language (even assuming that the Hittites and their culture did still survive intact in small pockets in northern Syria) would have changed beyond recognition.

From the viewpoint of linguistics we now can relate why the battle Ramses II fought against the Hittites occurred in the first millennium B.C. and not the second.

In Ramses II’s time a “speedy scribe” was using forms of Hebrew that date to the first millennium B.C., not the second. In Ramses III’s time, Greek letters dating from the first millennium, not the second, were used. The Lydians/Hittites, against whom Ramses II fought at the Battle of Kadesh, spoke the Hittite language that supposedly had not changed for several centuries, which is so incredible that it cannot uphold the placement of the Hittites in the second millennium B.C.: they belong in the first. It is impossible to accept that the Hebrew, Greek, and Lydian/Hittite languages of the first millennium B.C. could be known to have existed in, and have survived without change from well inside, the second millennium.

The linguistic evidence thus indicates very emphatically that the Hittites did not live in the second millennium B.C. but were the people known as the Lydians of the first.

Renfrew further points out “The relationship [of Hittite] is clearer in aspects of grammatical structure, for instance in the inflection of the noun; the case endings relate very closely to those of Greek and Latin, and the conjugation of some verbs is very close to that of Greek.”35 This would make excellent sense in terms of Sweeney’s thesis that the Hittites were the Lydians of the first millennium B.C. Their language would be reflected in Greek, and vice versa, because their peoples lived in a somewhat similar time and place and developed close affinities in linguistic structure.

Courville offers the following:

“Consternation reigned in the camp of the archaeologists with the sensational announcement by Forrer that he had found in the Hittite texts references to the Homeric Achaeans of Mycenae in Greece . . . We are in no position to make a pronouncement on these identifications.

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35 Renfrew, op.cit., p. 50
presumed by Forrer. It can only be stated that by . . . [an] altered chronology of the Hittites, the appearance of these names [if valid] does not represent an anachronism.”

Any affinities between Hittite and Greek culture might be explained by their both existing at the same time. For example, Moscati discusses a mythic hero “Upelluri, a giant who holds up the universe.” This “parallels with Greek mythology: the giant who holds up the heaven and the earth is a figure of the Atlas type.”

Sweeney discussed the historical evidence that Hittite Hattusilis, whom he identifies with Lydian Alyattes, expanded the empire over peoples of Western and Central Anatolia, and names the peoples who were conquered. Of course the cities subjugated by the Lydians have been found, but strangely not those enumerated by the Hittites. As Macqueen explains:

“there is one big problem. Survey work in Lycia and Pamphylia has so far shown no sign at all of settled occupation during the Hittite period. It is easy enough to accept that some sites in an area may have been overlooked, or that all settlements there were built of stone which was reused, and so have left little or no trace, or that they were mere collections of wooden shacks on the hillsides or among the trees. But it is difficult to believe that all surface traces of a country as powerful as Arzawa, a country . . . which could challenge the power of the Hittites and at least try to correspond on an equal footing with Egypt, have so far defied all efforts to locate them.”

Kuhrt calls this:

“One of the most rebarbative problems of Hittite history [which] concerns the geography of a number of important places especially in western Anatolia. Innumerable, obvious important cultic and administrative centers and small countries are listed in the Hittite texts, but where they are located on the ground is disputed.”

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36 Courville, op.cit., vol. I, pp. 96-97
37 Moscati, op.cit., p. 185
38 ibid.
39 Macqueen, op.cit., pp. 37-38
40 Kuhrt, op.cit., p. 237
Let us remember how in Chapter 8 we learned that nations as important as the Chaldeans and Medes left no evidence of their existence even in their homelands, while the Persians left none in the occupied territories of Assyria or Babylonia which they occupied. Again, stratigraphy for the Hittites indicates that they were, in fact, the Lydians. Macqueen offers various \textit{ad hoc} explanations for this anomaly but finds none able to satisfactorily solve the problem.

One area of the whole Hittite question yet to be addressed is that relating to technology, more specifically metallurgy and bronze-smelting, as well as tin production. Tin does not appear in Mesopotamia until around 1100 B.C. or after the fall of the Hittite empire. However, the Hittites carried on a thriving trade in bronze for which they imported tin. According to Trevor Bryce, “The texts available to us indicate the importation of some 80 tonnes of tin over a 50-year period, which would have been used in the production of some 800 tonnes of bronze.”\textsuperscript{41} There were donkey caravans consisting of about 200 to 500 animals, each of which carried 65 kilograms \cite{42} (about 143 pounds) of tin to Anatolia.\textsuperscript{42} And there are documents regarding trade and taxes relating to the tin trade in Anatolia.\textsuperscript{43}

The question of course is, Where did all this tin come from? Bryce explains:

“In recent years Turkish archaeologists have claimed to have discovered several sites in Anatolia where tin could have been mined in antiquity. But the evidence is disputed, particularly by the scholar James Muhly, who has long maintained, and continues to maintain, that we have yet to find evidence of workable tin deposits, at least on a significant scale, anywhere in Anatolia.”\textsuperscript{44}

The problem is that the tin was supposedly extracted from granite, as Dr James D. Muhly, a professor of ancient Middle Eastern history at the University of Pennsylvania, stated. “They have identified the geological presence of tin . . . Almost every piece of granite has at least minute concentrations of tin in it.”\textsuperscript{45} In order to cut granite to get to the tin ore one must have a metal that is harder than granite. We have already examined the possibility of using copper to do this, and

\textsuperscript{41} Bryce, \textit{op.cit.}, p. 27
\textsuperscript{42} \textit{ibid.}, p. 30
\textsuperscript{43} \textit{ibid.}, pp. 31-33
\textsuperscript{44} \textit{ibid.}, p. 86
\textsuperscript{45} \textit{The Science Times Book of Archaeology} (NY 1999), p. 48
Garland’s work clearly indicated this is not a viable solution. Bronze requires tin, so that the mine had to be linked with a source of tin to make the bronze tools that worked the granite. Moreover, bronze is not that much harder than copper and could not have cut the granite. So the mine could not have been worked until the development of steel after 1100 B.C. We are, therefore, still without a source of tin until after 1100 B.C.—based on the established chronology. If granite mines were exploited for tin, they could only have been exploited after 1100 B.C., and we are back where we started.

Furthermore, as pointed out earlier, Lloyd Weeks writing in *Antiquity* in 1999 admitted that the “verified tin deposits, and those that are known, show no archaeological evidence of Bronze Age exploitation and are located in areas where the local metallurgy does not incorporate production of Bronze alloys.”

E. Kaptan admitted as late as 1995 that evidence for “tin deposits in Anatolia have as yet not been identified or substantiated.”

It is worthwhile mentioning how desperate the historical community is to resolve this problem. Macqueen for example refers to:

“The vexed question of the source, or sources, of the tin which was widely used in the manufacture of bronze in ancient Anatolia . . . It is clear . . . that in the third millennium BC the percentage of copper-based artifacts containing more than five per cent tin is much higher in northwestern and central Anatolia (and also northwestern Iran) than it is in neighbouring areas such as Mesopotamia . . . The obvious conclusion . . . is that there was a tin-source somewhere in (central?) Anatolia which was available to local metal workers. But herein lies the core of the problem, for despite the most intensive investigation no such tin-source has yet been found. The problem becomes more acute when we move into the second millennium, for not only Anatolia, but neighbouring areas as well, can be seen to have access to supplies of tin for bronze-making, and still there is no clear indication of any source within the area from which it could have come. We have, it seems, to accept the fact that the tin which was used in the Mediterranean basin, Anatolia, western Iran and Mesopotamia, came from somewhere

outside those areas, and that trade in tin played a considerable part in economic life.

“But where did the tin come from?”

With respect to Egypt, Macqueen says, “there is no evidence either for the third-millennium exploitation of this tin or for the second-millennium use of Egyptian tin as a trade-item, and we regrettfully have to look elsewhere.”

Regarding Mesopotamia, “the source of that tin has for long been obscure.”

As for Thailand and Malaysia as sources of tin, “it is very difficult to see the tin used in Anatolia . . . as having its ultimate origin as far away as south-east Asia.” Tin from Afghanistan, discovered in recent years, is proposed, but no evidence exists to suggest it was exploited in ancient times. As we saw in Chapter 5, Lloyd Weeks has admitted that, “Alleged tin deposits in other areas of Western Asia have largely failed to withstand the scrutiny of modern geological research.”

With respect to the prolific tin deposits of Cornwall, Great Britain, “There seems to be a complete lack of tin-bronze in Britain itself before about 2200 B.C., and this makes it totally unlikely that Cornwall was the source of the tin used in Anatolian bronzes in the third millennium.”

As for second millennium British tin, “This is at least a possibility . . . but it must be admitted there is at present little or no evidence for it” which means no evidence for it!

Muhly significantly comments on this problem:

“The source of tin being used in the 3rd [and 2nd] millennium[a] B.C. remains the great enigma of . . . Bronze Age archaeology. The truly surprising fact is that, after many years of effort, we still seem to be no closer to a solution. This has suggested to some that we are looking either for the wrong thing or in the wrong places. Tin existed,

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47 Macqueen, op. cit., pp. 41-42
48 ibid., p. 42
49 ibid.
50 ibid.
51 ibid., p. 43
52 ibid.
and it was even being used . . . but the sources of that tin still escape us.”

The problem remains intractable, not because historians and archaeologists are looking for the wrong thing in the wrong places, but because they are looking for the right thing—tin ore—in the right places, Bohemia, Spain, etc., but in the wrong time frame. By moving ancient Near Eastern chronology much closer to the present—by over 1500 years—the enigma disappears not only for the Hittites but for the Egyptians and the nations of Mesopotamia.

What we have encountered again and again is a monotonous series of unproven assumptions about where the tin might have come from. Other authors go through this litany failing to find the tin for the ancient world before 1100 B.C. in terms of their own chronology. But if the Hittites are the Lydians, their use of tin bronze presents not the slightest difficulty.

There is also the question of iron. Like the Egyptians and Mesopotamians, the Hittites possess and write about iron long before the metal is known. O.R. Gurney discusses Anittas, a Hittite king who lived around 1900-1800 B.C. “The statement that Anittas received among the tribute from Puruskhanda such large iron objects as a scepter and a throne certainly looks like an anachronism.” The anachronism is that iron was unknown, according to the established chronology, until several centuries closer to the present. Therefore, ad hoc explanations, as in so many other instances where iron was found at an anomalous time, had to be invented. Gurney suggested:

“Such elaborate inscriptions as this are not produced out of a literary vacuum. Not a single example of an Anatolian royal inscription of this period has yet come to light. It has been suggested that a whole literature inscribed on some perishable material, such as wood, possibly in Hittite hieroglyphics, has been lost to us; but in view of the very brief and formal nature of the earliest extant hieroglyphic inscriptions, does it seem very likely that such a literature could have existed in these early times, even though ‘scribes of wood’ are frequently mentioned in the later Hittite texts . . . we might then believe our inscription of Anittas is a translation from that language. At present we seem driven to conclude

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53 Muhly, op.cit., p. 32
54 O.R. Gurney, The Hittites (London 1954), p. 20
that the deeds of Anittas became legendary, and were later worked into the form of an apocryphal inscription.” [emphasis added]

Because the inscription is a problem, Gurney claimed “We might then believe our inscription of Anittas is a (later) translation” and that “At present we are driven to conclude . . .” But in order to present these ad hoc explanations we have only the fact that iron did not exist at that time. Yet, not only did Anittas ask for an iron scepter and throne, at a grave a dagger was discovered with the inscription “the palace of Anitta the King.”55 This rather suggests that some writing of this earlier time may not have been on wood.

There are several other mentions of iron in Hittite literature prior to its invention. P.R.S. Moorey writes “As early as the middle of the sixteenth century B.C. in texts of King Hattusilis I ‘iron’ is already used metaphorically in land grants with reference to the word of the king to express permanence and indestructibility.”56 Iron, of course, only has permanence and is indestructible when it is carburized which suggests at least Hattusilis I did not live, as presented, from 1650 to 1520 B.C., but a few hundred years later, based on the established chronology.

Speaking of tombs at Dorak, a northwestern province in Anatolia, dated to around 2300 B.C., Seton Lloyd writes:

“The men were buried with their weapons and women with ornaments and personal belongings . . . The tombs are dated, not only by domestic vessels of gold and silver but, almost miraculously, by a fragment of gold overlay from a wooden throne, bearing an inscription in Egyptian hieroglyphics which includes the name of Sahure, the second king of the Fifth Dynasty of the Old Kingdom. . . . The materials suggest far reaching trade facilities [including] iron.”57

Lloyd discusses “a dagger with a crescent-shaped handle and blade of iron” at Alaça Hüyük, also dated to 2300 B.C. He suggests that iron was apparently known at this very early period and “was more valuable than gold.”58 But iron was not known at this early period, nor was there trade to that area from Egypt. As with Egypt, some have suggested iron was smelted as early as 1600 B.C. or earlier, but

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56 P.R.S. Moorey, Ancient Mesopotamian Materials and Industries, op.cit., p. 288
57 Seton Lloyd, Early Highland Peoples of Anatolia (NY 1967), p. 29-32
58 ibid., p. 30
as with the Egyptians this is offered without direct evidence that the Hittites did so. Conjecture can have little value in this debate.

Again, as with tin bronzes we have anachronistic mention and discussion of iron by the Hittites long before this metal is properly developed or even created. If the Hittites are, as Sweeney suggests, the Lydians of the first millennium, we are not required to invent various *ad hoc* explanations to remove an anomaly. However, when taken together with art, sculpture, language, stratigraphy, etc., none of these explanations are necessary. The scientific and technological facts as well as the various forms of historical evidence all come together to indicate that the Hittites are the Lydians, that this civilization existed in the first millennium B.C. and not the second, that the two cultures are indeed really one.

To then argue that other non-scientific, non-technological facets of historical analysis can be invoked to dismiss these concordances is to deny the validity of scientific and technological evidence. Unfortunately, it seems that this is the final refuge of those who simply refuse to understand the weight of all this physical evidence. Sweeney’s thesis has, it seems, a great deal of such evidence to support it.

There are three additional problems relating to the Hittite question: 1. “Where did they come from?” This is still unknown. 2. The Hittites bring almost nothing of their culture to Anatolia. “The Hittites adopted much of the [earlier] Hattic culture as their own, including religion and myth, technology . . . Scholars continue to search for original Hittite contributions . . .” 3. No one knows where they disappeared to. “One last question remains: Where did the Hittites go? . . . as Dr [Peter] Neve told . . . ‘we have found not a trace of them’.”

The Hittites come from who knows where, have the same culture as the people of Anatolia, including its religion, myths, technology, art, language, etc.; then they disappear into history and go who knows whither. But if they are the Lydians, they have no hidden origin: they come from Anatolia. They do not have to adopt Anatolian culture since it is their own culture. They don’t abandon their religion, mythology, technology, etc. It is their own. And they do not disappear from history or migrate to unknown lands. As Lydians we know full well what happened to them.

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59 Ceram, *op.cit.*, p. 92
60 Louis De la Haba, *op.cit.*, p. 238
61 ibid.
In retrospect, what do we find? Hittite “sculptures show clear signs of a much later time of origin,” according to Puchstein, “the art which has produced these and similar works does not have to be ascribed to the enigmatic Hittites of the second millennium B.C., but should be regarded as a remarkable sign of the then highly developed culture of the population of Asia Minor and the Commogene in the time from 1000 to 600 B.C.” That is, the art is of the time and style of the Lydians, as well as the science, law, literature, royal annals, etc., which also belong to that period.

Records of the Hittites from Egypt are dated to the trunk of Egyptian chronology, to “the 15th-13th centuries BC,” according to James et al., but “on the other hand Assyrian records mentioning the land of the Hatti [Hittites] come largely from the 1st millennium BC,” and “they describe a group of prosperous city-states in south-east Anatolia and northern Syria.” That is, Assyrian records place the Hittites in the time and region as the Lydians who dominated the “prosperous city-states” that existed there. In terms of stratigraphy, the mixture of pottery styles of Phrygians of the first millennium B.C. with those of the Hittites who supposedly flourished in the second, show, according to the excavator of Gordion, that “the two cultures appear to have co-existed for a considerable time.” The Hittites were making pottery in the first millennium B.C. (not the second), i.e. during the era and in the region of the Lydians. Then too, the Greek ceramics of the first millennium lead to the same conclusion, as do Hebrew words.

In historical terms it might be argued that the Lydians could not be the Hittites, as Velikovsky suggests that in the war between the Egyptians and the Hittites carried on by Ramses II, “Jeremiah (46:9) named, next to the Egyptians, the Ethiopians, Libyans, and Lydians as contingents of the Egyptian army . . .” Since Ramses II had mercenaries known as “the Sardan” apparently from the Lydian city of “Sardis,” and the term “Lud” for Lydia is a Hebrew word, one cannot expect the Lydians to be mercenaries against their own country termed the Hittites. Nevertheless Lud, or Ludim (plural) has other possible identifications. In Ezekiel 29:3, Nebuchadrezzar is going to make war against Tyre, and in verse 10 “Persia and Lud and Put were in your [Tyre’s] army as your men of war . . .” According to Insight on the Scriptures, vol. 2 (Brooklyn, NY 1988), p. 281,

“The Ludim who rendered military service for Tyre are more difficult to identify. Their being linked in the text with Put, however, may . . . point to the Hamitic Ludim.

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62 Velikovsky, Ramses II and His Time, op.cit., p. 19
The texts referred to would logically place the Hamitic Ludim in North Africa, but it is not possible to locate them more definitely. Some scholars situate them in the general vicinity of Libya, but they do so on the basis of an arbitrary alteration of the spelling of the name to Lub instead of Lud.”

The argument that Lud is Lydia is disputable but the scientific and technological evidence of tin bronzes, iron, and in part linguistics cannot be dismissed based on such documented words as “Lud” for “Lydia.”

In historical terms there exists much other evidence for identifying the Hittites with the Lydians, some of which is examined in Sweeney’s Ramessides, Medes and Persians. He points out for example that Herodotus speaks of an important Lydian monarch called Myrsilos, but this (Mursilis) is a well-known Hittite name, and is not attested for any other people. In addition, he has recently argued that Hattusilis, whom he has already identified with Alyattes, actually bears the same name as the latter. Hattusilis is composed of two elements, Hattus-ili. But in cuneiform, as well as hieroglyphic Egyptian, the order in which names should be put together is frequently unclear. Thus Hattus-ili can be written as Ili-Hattus; and since vowels are conjectural, it can also be rendered as Ali-hattus. Sounds familiar, doesn’t it! Altogether Sweeney brings forward a large number of very detailed arguments to support these identifications. No doubt historians could dispute some of the points raised by him, but such arguments would be invalidated by the scientific-technological evidence, especially that relating to tin and bronze in Anatolia. Furthermore, the linguistic evidence shows that Lydian is related to Hittite more closely than Hittite to the Luwian spoken in Hittite times, and Lydian “seems to be a descendant of Hittite.” And further, we were told that “the language originally known as Arzawan [spoken in the area where the Hittites and Lydians reigned] is in fact the language of the Hittites.” The many inscriptions in this language date from “the tenth to the eighth century B.C. . . . the last examples of it . . . c. 600 B.C.” That is, the Lydians spoke Hittite and the overwhelming number of the texts belong to the first millennium B.C., to the time of the Lydians and in many respects have a grammatical affinity with Greek.

Lastly, tin bronzes or iron could not have been made in the mid-second millennium B.C. because there were no known sources of tin at that time. Tin is well understood to have come into Anatolia, Mesopotamia, Assyria, etc. in the first millennium B.C. The Lydians had sources of tin in the first millennium based on the established chronology. They also had developed iron at that time.
The historical, artistic, stratigraphic, linguistic, and technological evidence all corroborates, and points inevitably to, the identification of the Hittites with the Lydians of the first millennium B.C. The same evidence flatly contradicts, in a thousand ways, accepted textbook chronology both with regard to the Hittites and their Egyptian contemporaries of the 19th Dynasty. In addition, the fact that the Hittites have to spend 550 years in a period of time that the archaeological and geological research at Tell Munbaqa proves has never existed only reinforces the strength of the foregoing evidence.

Note: The objection may be raised that certain artefacts of Lydian culture are not met with in Hittite culture. But it must be remembered that the “Lydians were famous as . . . traders.” As such, different ports or cities of the empire would be carrying on trade with different parts of the ancient world. Therefore, various items of trade would influence the arts of these cities in different ways and at different times. This consideration must be taken into account regarding the Lydian-Hittite equation. For a general overview of the various elements that influenced Lydian/Hittite art and culture, see C.W. Ceram, *The Secret of the Hittites* (NY 1955), pp. 207-215. Statements such as “The Kingdom consisted not of a single Hittite nation, but of numerous peoples of diverse psychology. . . .” (p. 208); “The nation was not united by a single language; in Boghazköy alone traces of eight . . . have been found. Of these four were widespread. The Hittite nation also had no unified script.” (p. 209) “The Hittite Empire was not spiritually united by a single religion.” (p. 209) “The plastic art of the Hittites [shows] no tendencies towards form. The sculptures were frivolously experimental . . . old and new work were jumbled together” (p. 210). “Normally Hittite art has no style” (p. 210), etc. Thus, the arts of the Lydians/Hittites might vary from region to region.

Moscati describes what are usually termed Neo-Hittite states, or those coming after the fall of the empire, which in Sweeney’s chronology all belong to the first millennium, as follows: “As these are closer to the centres of foreign influence, it is understandable that the external elements grow stronger and the autochthonous elements are reduced to the point of creating an art which has hybridism as its distinctive feature.” As regards Hittite architecture, he points out that Hittite palaces are “fundamentally the same . . . as [its] temples” containing “an entrance hall with roof supported on columns . . . their bases have . . . pairs of sphinxes or lions carved in stone . . . [and] beyond the main hall, a parallelogram set lengthwise with smaller rooms arranged around it. Palaces of this type are found in many

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64 Moscati, *op. cit.*, p. 193
neighbouring centers from Assyria to Syria, and they are a difficult problem of origin." Assyrians had just such palaces and this suggests that not only were the Hittites at war with the Assyrians after they fell, but they used similar forms of architectural designs as the Assyrians whom they evidently knew after 1100 B.C.

To add to this, Kaj Birket-Smith succinctly points out that “Assyrian King Ashur-nasirpal II ([who reigned] 883-859 [B.C.]) received as tribute two hundred and fifty talents of iron from the Hittite king in Carchemish . . .”. But the Hittites in Carchemish had disappeared from the pages of history c. 1200 B.C. or over 300 years before the tribute was sent. How could an Assyrian king of the first millennium B.C. receive iron from a second millennium Hittite monarch, based in a Hittite city, part of a Hittite civilization that could not have existed based on the established chronology? If the Hittites are the Lydians of the first millennium, this fits perfectly with Sweeney’s shortened chronology.

Further, as the Hittites are the Lydians, the invention of iron surely belongs to the first millennium B.C., and the iron artefacts in Tutankhamen’s tomb attributed as gifts of the Hittites and all the other terrestrial iron relics of the Near East also belong to the first millennium. Since the Hittites/Lydians did not live in the second millennium B.C., their invention of iron cannot be assigned to that millennium.

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65 ibid.
66 Kaj Birket-Smith, Paths of Culture, (Madison, Milwaukee, WI 1965), p. 85)
CHAPTER 17
CORROBORATION, CONVERGENCE, ANALYSIS

“Of ‘historical truth’ [Henri] Frankfort (1951) [in The Birth of Civilization in the Near East] wrote [that] a concept whereby ‘many seemingly unrelated facts are seen to acquire meaning and coherence is likely to represent a historical reality’.”

Barbara Bell

“How do you decide whether someone or something is reliable? Consistency is clearly vital; if a person makes ten claims all of which turn out to be corroborated by other evidence [especially scientific and technological evidence] this is a high level of consistency.”

Ludmilla Jordanova
History in Practice (London 2000), pp. 95-96

“What he [the historian] cannot do is to make the initial assumption, to go on thinking and to arrive at a conclusion different from that which is scientifically correct.”

R.G. Collingwood
The Idea of History (NY 1994)

Historical truth can be determined more accurately when “many seemingly unrelated facts” point consistently to a chronology of the ancient Near East where the phenomena all corroborate and converge. This is especially so when many of the phenomena are of a scientific and technological nature and are not
derived from a chronology that is based on historical documents and contexts. As this author has stressed repeatedly, a historical interpretation of documents cannot be valid if it is contradicted by scientific and technological facts. Only when historical documentation is corroborated by scientific and technological facts can these have any validity.

As Handlin explains, cited by Fischer:

“Concordance. Is this testimony in accord with the other evidence? The agreement of several [historical] witnesses is not enough, for they may have spoken in collusion or, more likely, may have had the same distorted, limited view of the incident [or chronology]. But confirmation from different types of evidence is more persuasive.” [Oscar Handlin, *Truth in History* (Cambridge, MA 1979), p. 129]

Fischer in his concluding remarks stresses the importance of utilizing an unbiased or neutral device for understanding history and chronology:

“Within an appropriate time scheme, events must be located with accuracy and precision. A historian must preserve an uncompromising respect for the temporal integrity of his story and of its various components . . . he must guard against anachronisms in every form.

“[Therefore] . . . a historian must develop interpretive devices [such as science and technology] which neutralize bias. . . . A device has been created by an imaginative historian of science, Thomas S. Kuhn, from whom historians in every field have much to learn. . . .

“Kuhn’s conceptualization of change and continuity in the history of science is organized around a series of ‘shared paradigms’, sets of scientific methods and understandings which were consistent with what was known when they established it, and which persisted until anomalies generated a crisis. The old paradigm was blurred and . . . then a new paradigm was generated.
“Kuhn’s method may be marred . . . but it is a closer approximation to the requirements of the subject than anything we have had before. And it is relevant to all fields of historical inquiry.”

We have gone through an extensive number of scientific and technological facts that are not only anomalies for the traditional chronology of the ancient Near East, but are, in fact, in direct contradiction to it. These many contradictions in terms of that lengthy chronology have still not been resolved nor can they. They have persisted and indicate individually and in concert via their corroboration and convergence a greatly shortened chronology. This is ultimately what Fischer terms as:

“The chronic fallacy [which] is a kind of misplaced temporal literalism in which the historian forces his story into an over-rigid chronological sequence and tells everything in the precise order of its occurrence, with results that are dysfunctional to his explanatory purpose.”

The dysfunctional aspects of the accepted long chronology are all the scientific and technological facts that cannot be ordered with it.

In this respect let us examine these dysfunctional facts as they corroborate one another, repeatedly converge to reinforce the short chronology and contradict the long one.

Geological evidence of weathering on the Sphinx, Valley Temple, and Mortuary Temple at Giza contradicts the established chronology. If these three monuments were constructed around the same time as those others dated earlier, contemporary with them and hundreds of years after they were built, these other monuments of similar materials, subject to the same erosive forces, would all exhibit similar evidence of weathering. That they do not points to the fact that in Egypt these other monuments were built long after the Sphinx, Valley, and Mortuary Temples. It is clear that these others were constructed at a time long after the Sphinx was built, and that could not have been, as the historians suggest, in the third millennium B.C. Rather, the evidence of erosion points to a period well into the second or even the first millennium B.C.

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1 Fischer, *op.cit.*, pp. 161-162
2 *ibid.*
Astronomy, the Queen of the Sciences, supports unambiguously the historical fact that the 12th Dynasty of Egypt existed in the first and not the second millennium B.C. The astronomical data related to Sirius, the Moon and the Earth’s orbit correlate, corroborate, and converge on that fact.

Radiocarbon dating, which has been called upon to buttress the established chronology, nevertheless consistently points to the fact that all dates taken from wood, charcoal, and short-lived plants will always render dates that are older or considerably older than the sites from which they were taken to be measured. This, too, strongly indicates that the length of the chronology of Egypt, Mesopotamia, Syria, Anatolia, Greece, and other areas of the Near East cannot be as old as the published dates have led the historians to believe. Instead, they consistently point to a significantly shortened chronology which, as with the erosion on the Sphinx, etc., and astronomical dating, correlate with and corroborate a considerably shorter chronology.

Pottery dating based on the great trunk of Egyptian history with which all other ancient nations were forced into agreement, is based on a monumental failure. The pottery of Old Kingdom Abydos that Petrie placed in the fourth, and which present-day historians place in the third millennium B.C., is undeniably Mycenaean, Aegean, and Syrian pottery of the first millennium B.C. Not only did Petrie freely admit this identification, but Dayton also fully proved that these pots belong in the first millennium. Not only does the evidence require that the Old Kingdom be placed in the first and not the third millennium, but, on the basis of cross-reference to Egypt, all the nations of the Near East with the same pottery must also be placed in the first millennium.

Bronzes with significant amounts of tin also contradict the established chronology. Since tin was employed in Egypt, Mesopotamia, and Anatolia only after about 1100 B.C., all these ancient nations with tin bronzes cannot be placed in the third or early to middle second, but to the end of the second and to the first millennium B.C.

Carving hard rock, especially diorite, with copper, bronze, wrought or meteoric iron implements in Egypt, Mesopotamia, or other regions of the Near East could never have been carried out in the third millennium because carburized iron, that is, steel, does not come to be employed until around the first millennium B.C. Since the historians attribute the development of iron to the Hittites, and several forms of evidence, outlined above, clearly show that they were actually the Lydians
of the first millennium, iron comes into being and use at that time. Egypt was not a backward nation in this respect because it enters the Iron Age around the time iron was first created. But clearly, the early Egyptian and Mesopotamian civilizations were able to carve diorite, indicating that they did so in the first and not the second or third millennium B.C.

Stratigraphy at Tell Munbaqa in Syria has been examined by a geologist. The geology incontestably shows that where the established historians placed a 700- to 800-year settlement gap between the Old Akkadians and Mitanni, there is none. The 700 to 800 years of Mesopotamian history for this time is a fiction. All the correlations that were derived from the historical assumption that that settlement gap was real, destroy the very foundation of established chronology. At the very least, it requires that Mesopotamian history be shortened by 700 to 800 years, bringing the bulk of that history into the late second and first millennium.

However, when we remove the additional 500- to 600-year Dark Ages of the Greeks and the Hittites, that requires that Mesopotamian history must be placed predominantly in the first millennium B.C., at least, insofar as it correlates with Hittite, really Lydian, history. And see how closely Tell Munbaqa and the 500- to 600-year Dark Age match the 1477-year lowering of Egyptian history from Astronomy proved by Rose.

This being the case, one can well understand that the Royal Tombs at Ur are those of Scythian princes and that the Sumerians of the third millennium were the first Chaldeans of the late second to the first millennium.

The consistent convergence of various types of evidence that correlate and corroborate one another all point to an extremely shortened chronology and flatly contradict the long chronology. Over and over, the scientific and technological facts speak clearly that the chronology of the ancient Near East requires a total overhaul and must be greatly shortened.

Stiebing has argued: “... the Velikovsky Affair emphasizes a point that is not often recognized by Velikovsky’s scientific critics, namely that Velikovsky’s thesis was essentially historical, not scientific.” But now, in part, Velikovsky’s thesis becomes scientific as do Heinsohn’s, Rose’s, and Sweeney’s, and historical only insofar as the history supports the science. Of course, as with so much of

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Stiebing’s criticism of Velikovsky and Heinsohn, he can continue to evade, misrepresent (as he has been shown above to do so often) or ignore the science and technology as these clearly correlate, corroborate, and converge to uphold a short chronology.

Erosion on the Sphinx, astronomical Sothic and radiocarbon dating converge with, and are corroborated by, pottery dating, the production of tin bronzes, as well as iron development to carve diorite etc., further converge with stratigraphy at Abydos in Egypt and Tell Munbaqa in Syria, and all the other evidence outlined above; there is clearly a consistency of evidence that all broadly correlates and gives corroboration to Heinsohn’s, Rose’s, Sweeney’s, and, to a great extent, Velikovsky’s shortened chronology. It is this over-arching, consistent correlation and corroboration of scientific and technological as well as all the other historical factors that point to the general validity of their conclusion.

David Talbott, Dwardu Cardona, and Ev Cochrane, who have been either unsupportive or deeply critical of these greatly lowered chronologies, have raised the issue of convergence and cross-reference of evidence from numerous sources with regard to Venus having been observed as a “great comet” in ancient times. Ev Cochrane asks the pertinent question: “rather than confront the issue of recurring anomalies [critics] descend into a swamp of marginal details. . .” As Cochrane himself has argued:

“The mark of a sound theory is how many anomalies and unexplained problems it can handle without introducing a host of new problems requiring ad hoc solutions. While I do think it is possible that Heinsohn’s historical reconstruction answers a few anomalies, it seems clear that his theory raises more problems than it solves . . . Heinsohn’s reconstruction cannot be made to square with the historical record.”

But that is precisely the point. The historical record in terms of the established chronology cannot be made to square with the scientific and technological facts. These are indeed the real facts! Heinsohn’s theory exhibits all the marks of a sound theory because of the many scientific and technological

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6 Cochrane, “Heinsohn’s Ancient ‘History,’ op.cit., last page
anomalies and unexplained problems it does handle without introducing a host of ad hoc solutions. In reality, it is the established chronology which raises more of these problems than it solves. Some of these anomalous problems in fact have stood for over a century while others have come to light only in the past several decades without a hint of their being resolved.

To paraphrase Talbott, and to some extent Cochrane and Cardona: In all this there is a fundamental issue of logic. How does one properly weigh the lines of evidence, the repeated convergence of science in the forms of astronomy and geology, technology, stratigraphy, radiocarbon and pottery dating, as well as tin faience and tin bronze evidence supported by iron to cut diorite etc., pointing to an extremely short chronology for ancient Near Eastern civilizations? Having had many opportunities to muse over the way the experts skirt these issues, I am convinced the real question never enters their minds. Until one asks the question, How does one explain all these various forms of evidence that contradict the conventional chronology? even the most obvious evidence will be seen as something else; it will be seen through numerous ad hoc hypotheses or reckless dismissal of facts that contradict that chronology and therefore are not taken seriously.

Once the critics of the chronologies of Heinsohn, Rose, Sweeney, and Velikovsky resort to unbridled, unproven, improvable, or disproven explanations, they are left with nothing in the way of scientific or technological evidence to account for these convergences.

In truth, virtually all of the respected authorities and critics look for ad hoc explanations and references, because no one could seriously believe that such dramatic scientific, technical, stratigraphical, etc. facts could dominate as evidence for the chronology of the ancient world with such a strong link to history.

As Livio C. Stecchini wrote in Kronos:

“It is in the light of Newton’s preoccupation with the role of comets that one must understand his concern with historical studies; these included a complete theory of the origin of civilization, a theory about how mythology should be interpreted, and a radical revision of accepted ancient chronology... Nicolas Fréret... the first permanent secretary of the Académie des Inscriptions, in a series of monumental studies published in the acts of this academy... foresaw the immense advances that could be made in the study of ancient history
by combining linguistics, mythology, chronology, geography, astronomy [and technology] and the history of science in general . . . concerning the civilizations of Mesopotamia, Persia, India, and China.”^7 [emphasis added]

This was in very great measure the approach of Immanuel Velikovsky. This author has emphasized the scientific and technological data as a method to analyze ancient chronologies. Only John Dayton previously emphasized this method as valid. His work has been largely disregarded by the establishment.

How, then does one break through the vicious circle? Go back to the most frequently employed method of pottery dating; many of the forms of the body of 1st Dynasty ware at Abydos are identical with that of the later Aegean or Mycenaean pottery; the patterns are common, and indeed no patterned Egyptian pottery of this kind is known until the XVIIIth Dynasty. Not only the general motifs pointed out by Dayton, but virtually all of the variations are attested for pottery of the first millennium B.C. Is sheer coincidence possible as an explanation in such an extreme case as this? The same applies to tin bronze, stratigraphy, iron, glass, astronomy, radiocarbon, etc.

For starters it needs to be understood that we are not dealing with a “multiple choice” situation with respect to possible interpretations: if one is permitted to take the evidence at face value, then first millennium B.C. pottery found in the Old Kingdom points to a first millennium placement of the Old Kingdom. In spite of the conventional chronology, tin bronze found in the Old Kingdom points to a first or second millennium placement of the Old Kingdom. In spite of the conventional chronology, the stratigraphy of the Old Kingdom Abydos relics together with those of the New and Middle Kingdom points to a first millennium placement of the Old Kingdom. In spite of the conventional chronology, pyramids with hard rock on them and inside of them point to the use of hardened iron, which only comes to be used in the first millennium and this requires a first millennium placement of the Old Kingdom. In spite of the conventional chronology, astronomical evidence places the 12th Dynasty of the Middle Kingdom in the first millennium. And even in terms of radiocarbon dating, there is plenty of evidence to significantly shorten conventional chronology. When we use ancient Egypt as the trunk for the rest of Near Eastern chronology, in terms of conventional chronology, it requires that nearly all the other nations surrounding Egypt be placed in the first

millennium B.C. as well. As has been shown above, many of these same problems exist for Mesopotamia and Anatolia.

Given the nature of all these converging forms of evidence, the sheer scale of it is stunning; and one wonders how the Egyptologists could have dealt with these convergences without a crash course in science, technology, etc. Historians and archaeologists, rather than accepting all these forms of evidence that so clearly corroborate that ancient Near Eastern chronology demands a great shortening, have steadfastly clung to Manethon’s erroneous long outline. As Cardona, citing Talbott, asks: At what point, then, do so many forms of converging evidence from science and technology become worthy of pursuing? Is it really possible that so much converging evidence of a physical nature, which all points to a much shortened chronology, is in error? Better yet, can historically interpreted data override scientific and technological facts?

The problem is that science and technology have been given short shrift and have played a secondary role in the interpretation of ancient chronology, when in reality, as the arbiters of truth, scientific and technological data should weigh as the most important and final determinants in this area of research. But as we have seen, this has not occurred. Radiocarbon dating is a scandal with respect to its use and abuse in determining chronological truth. Indeed, the only evidence acceptable to historians and archaeologists wedded to the conventional chronology is that which supports that paradigm. Is it any wonder that the scientific and technological evidence has been dealt with by these advocates of conventional chronology by nothing but unproven, and improvable, invented hypotheses as outlined repeatedly in this book?

Until scientists and technologists such as Schoch, Dayton, and Rose, stand up to these promulgators of ad hoc inventions and demand that historians accept the real facts which their fields have produced, history will remain outside the realm of reality. As Oswald Spengler, who so well understood the nature of historical research, said in no uncertain terms, “Historical writing is fiction.” The Dutch historian Johan Huizinga expressed a similar view with great profundity:

“History is the intellectual form in which culture decides for itself the meaning of the past.”

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8 quoted in Ceram, The Hittites, op. cit., p. 120
History will remain fiction as long as historians fail to accept that they must be guided by the facts of science and technology. So long as they fail to demand that these facts are the alpha and omega of chronology, so long as they fail to fully take these methodologies into their professional considerations, they will never be able to show what actually happened in the ancient past. There is no history without chronology. We will never understand the truth about the past until history is buttressed by, and not contradicted by, science.

John Dayton cited Benjamin Disraeli, “To be conscious that you are ignorant of the facts is a great step to knowledge.”9 In his “Epilogue” Dayton concludes:

“One of the most astonishing things in the writing of this book has been to find, time and time again, that statements of archaeological fact had absolutely no basis in reality. A classic example was the ‘fixed’ Sothic date for Sesostris III, the anchor point of chronology. Yet for three-quarters of a century, myths have been perpetuated, while the foundations have hardly ever been questioned. We have moved from the position of Bishop Ussher, who stated that the world was created in 4004 B.C., and the rabid opponents of Darwin into yet another ossified situation where no one has had the courage to point out ‘that the king has no clothes on’ . . . How can ‘scholars’ postulate a Bronze Age without the slightest investigation into the occurrence of tin and other minerals? . . . Today we do not burn Savoranola and Huss at the stake, we consign them to oblivion . . . The universities today are not places of mental adventure, but dull workhouses of conformity.”10

Dayton sums up the condition of present-day historical research:

“In conclusion, the writer would suggest that the time has come to shake off the illusory shackles of Manetho, and that, with the elimination of Petrie’s 1st Dynasty error and the Abydos-Amuq G-Jemdet Nasr equation, archaeology can start again and a clean start be made with the scientific and objective facts that are available. Instead of relying on quotations of ‘old masters’, every fact must be verified anew, every anachronistic object treated with suspicion rather than with joy, as in the past. Perhaps, dealing as they do with axes, archaeologists

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9 Dayton, op. cit., p. 468  
10 ibid.
have been too ready to sharpen them on a pet theme in the past. The great [historians] of the last hundred years did a great job . . . We now have new tools . . . which they would have been the first to welcome. It is time for the scientist to come out of his cupboard at last, but also to realize that, in archaeology, one is not dealing with parts per million but rough rule-of-thumb beginnings, where a 9 per cent tin bronze has equal significance with one of 11 per cent . . .

“The so-called anachronistic objects are then found to fit very well if Manetho and Petrie are ignored. . . . If we add to [science and technology] the principles of ‘reasonableness’, as in a jury trial, it will be seen that technology shows such [reasonableness] . . .

“Perhaps astronomy [as Rose has clearly shown] will . . . tell us the absolute dates that will enable us to fit our ‘reasonable’ structure into a firm table of events.”

On the basis of the scientific and technological evidence, the established chronology has “no basis in reality”, meaning it is fiction. As fiction it has undoubtedly been a great success, as history a monumental failure. To paraphrase Voltaire’s “The Holy Roman Empire was neither holy, nor Roman, nor an empire,” it is clear that the long historical chronology of the ancient Near East is neither long, nor historical, nor a chronology, although it may “last for a thousand years.” This is a jibe at Hitler’s Third Reich which he claimed would last for a millennium. If the long chronology does so without countenancing and solving these fundamental negations to its validity, it, too, will be but fiction and historians who contribute to its chapters and acts are merely novelists and playwrights.

As far back as 1951, George Hanfmann made a comment that warned us that the history of the Bronze Age had not really been properly structured. And this now applies to all ancient history:

“. . . it is well to remind ourselves from time to time that the two great pillars of . . . chronology . . ., the Egyptian and Mesopotamian, are not two stout towers resting on immovable foundations. . . . Both chronologies include problems which cannot as yet be solved except by reasonable guesses—the specific years to which observed astronomic

\[11 \textit{ibid.}, p. 419\]
data should be assigned, the estimates for the lengths of obscure periods, and the evaluation of possible gaps, duplications, and exaggerations in Royal lists and building inscriptions.”

What makes the short chronology superior to that of the long-established one is what makes a good scientific theory superior to another. It is the concept of “Occam’s Razor” sometimes known as the “simplicity postulate.” When two hypotheses are in conflict with one another, scientists provisionally choose that which they judge to be simpler, based on the supposition that the simpler theory which explains more evidence with less ad hoc or associative theories is most likely to explain the facts. The famous dictum of Occam’s Razor is entia non sunt multiplicanda praeter necessitatem or “entities [or pluralities] are not to be multiplied without necessity.”

The theory that explains more with less is the short chronology. It explains why the Sphinx has rainfall damage yet the other monuments around it show no such damage because they were built at different times. We do not require the ad hoc theories of Gauri et al., Harrell, etc., which were shown to be without scientific merit. It explains why the 12th Dynasty belongs in the first millennium not the second on the basis of astronomical dating of Sirius and the Moon, as well as the 12th Dynasty possessing iron. Those who oppose this have only ad hoc theories to explain these anomalies away. It explains why radiocarbon dating becomes more and more out of date with the dynasties as we go back in time. This simply follows if the dynasties all ruled over a much shorter period of time and co-reigned in different districts of Egypt, and then were aligned behind each other and pushed back in time. The ad hoc hypotheses to explain away the many anomalous dates are necessary for the long chronology. Pottery dates for Old Kingdom ceramics are not an anomaly in the shortened chronology, as Dayton so deftly illustrated. The anomalous tin bronzes need to ad hoc explanations. As first millennium relics they should exist because tin trade existed then. The stratigraphy of Egypt exhibits the same basic fit without the ad hoc theories of restorations and revivals of ancient forms. No ad hoc hypotheses are necessary to explain how hard rock was cut, shaped, and incised. In the first millennium iron and gemstones are known and will do the work then. Lost and unknown civilizations simply disappear and known civilizations that have lacked stratigraphical evidence of their existence take the place of those that were invented. One does not have to turn to the ad hoc supposition that erosion selectively eroded away only those civilizations that were

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known. Stratigraphy at Tell Munbaqa is fully explained by the short chronology as well. I believe if the archaeologists have the courage to test all the other sites where Heinsohn claims there are no settlement gaps, these digs will also confirm Heinsohn’s hypothesis. We do not have to hold to the ad hoc hypothesis that irrigation could have continued for millennia in southern Mesopotamia. Three hundred to four hundred years fit the short chronology quite well. The Dark Age of Greece is an unnecessary ad hoc hypothesis. We do not have to invent a whole series of ad hoc explanations for why writing, pottery making, architecture, and several other forms of development cease to exist and then begin again almost half a millennium later in largely the same form. They are largely the same form because there was no Dark Age separating their development. The Hittites become the Lydians and speak the Lydian language because of this identity without ad hoc explanations to explain how a living language failed to evolve as do all others. Glassmaking in several parts of the ancient Near East does not undergo dark periods either. We need no ad hoc hypotheses to show that there was a general steady development of the craft.

As stated above, Occam’s Razor is used by objective scientists and should be used by historians as well when antithetical theories are involved. In this regard it has been shown that this principle of science indicates that the short chronology, even now at its fairly early inception, fits well with this concept.

Another concept related to scientific concepts is that of Imre Lakatos which was touched upon above, or that of ad hoc hypotheses. Grayson summed up this concept up thus:

“Central to Lakatos’ analysis is his distinction between ‘progressive’ and ‘degenerating’ research programs. Each step of a progressive program increases its imperial content, predicting new facts as it moves along; and seeing those predictions corroborated as time passes. In contrast, degenerating programs are marked by the accretion of ad hoc hypotheses designed to protect the heart of the program [theory] from important inconsistencies, while failing to predict new and unexpected phenomena.”13

In this respect it is glaringly obvious that the long-established chronology requires a host of *ad hoc* hypotheses to protect it while the short chronology does not.

Then there is the concept presented by Karl Popper related to falsification of a hypothesis. Astronomer Tom Van Flandern explains it thus:

“The logical deductions from a theory or model are its ‘predictions.’ They must be compared with reality at every opportunity. It is of paramount importance for any hypothesis which purports to be of value that it be ‘falsifiable’; that is, that it be able to make predictions which if false invalidate the theory . . . . For a hypothesis to gain acceptance, it must predict things of seemingly low probability that will invalidate the theory if the predictions are wrong. The convincing power of the predictions will be proportional to how much is at risk if the theory is placed by them. This is usually a sign of a bad theory if it must be ‘patched’ as a result of a bad prediction [by *ad hoc* hypotheses], or even a prediction it simply failed to make at all.”

Let us review only a few predictions of both the short and long chronologies. The long chronology predicts that Sothic dating will be corroborated by lunar data to fit a second millennium placement of the 12th Dynasty. The short chronology requires that Sothic dating will be corroborated by lunar data to fit a first millennium placement. Prediction based on astronomy supports the short chronology and falsifies the long chronology.

Pottery dating predicts Old Kingdom pots in design, form, and in terms of tin content must be of the third millennium in the long chronology. Pottery dating predicts Old Kingdom pots in design, form, and in terms of tin content must be of the late second to early first millennium in the short chronology. Prediction based on pottery supports the short chronology and falsifies the long chronology.

Stratigraphy in terms of the long chronology predicts a settlement gap at Tell Munbaqa of around 700 to 800 years. Stratigraphy in terms of the short chronology predicts that there is no settlement gap at Tell Munbaqa. Prediction based on stratigraphy supports the short chronology and falsifies the long chronology.

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chronology. Much more could be put forth on this point but the reader can find these other predictions that should flow from both theories with a little diligence in the foregoing text.

Then there is the modern concept held by many scientists that “Extraordinary hypotheses require extraordinary proofs.” Therefore, the short chronology which some will label an extraordinary hypothesis should be supported by extraordinary proofs. Extraordinary proofs in this case are scientific and technological factual ones. The long chronology is supported mainly by interlinking historical evidence but the short chronology is supported by interlinking scientific and technological evidence. To suggest that a historical chronology contrary to scientific and technological facts, as is the long chronology, can be valid, flies in the face of logic and reason. To suggest such a concept would be itself extraordinary. The short chronology, buttressed by scientific and technological facts, is valid because it is supported by this extraordinary real evidence.

On all points it appears that the established long chronology fails at every level on which it is examined. On all points it appears that the revisionist short chronology succeeds at every level on which it is examined. In a sense this book is arguing for the logical positivist model of Auguste Comte, who wrote:

“From the study of the development of human intelligence, in all directions, and through all times, the discovery arises of a great fundamental law, to which it is necessarily subject, and which has a solid foundation of proof, both in the facts of our organization and our historical experience. The law is this:—that each of our leading conceptions,—each branch of our knowledge—passes successively through three different theoretical conditions [or stages]: the Theological or fictitious; the Metaphysical, or abstract; and the Scientific or positive. In other words, the human mind by its nature employs in its progress three methods of philosophising, the character of which is essentially different and even radically opposed: viz., the theological method, the metaphysical, and the positive. Hence arise three philosophies, or general systems of conceptions on the aggregate of phenomena, each of which excludes the other. The first is the necessary point of departure of human understanding; and the third is its fixed and definitive state. The second is merely a state of transition.
“In the theological state, the human mind, seeking the essential nature of beings, the first and final causes (the origin and purpose) of all effects,—in short, Absolute knowledge,—supposes all phenomena to be produced by the immediate action of supernatural beings.

“In the metaphysical state, which is only a modification of the first, the mind supposes, instead of supernatural beings, abstract forces, veritable entities (that is, personified abstractions) inherent in all beings, and capable of producing all phenomena. What is called the explanation of phenomena is, in this stage, a mere reference of each to its proper entity.

“In the final, the positive state has given over the vain search after absolute notions, the origin and destination of the universe, and the causes of phenomena, and applies itself to the study of their laws,—that is, their invariable relations of succession and resemblance. Reasoning and observation, duly combined, are the means of this knowledge. What is now understood when we speak of an explanation of facts is simply the establishment of a connection between single phenomena and some general facts...”\(^{15}\)

What this book contends with regard to ancient Near Eastern historical chronology is that the history of that period was established not as Comte suggests on abstract forces, but from abstract ideas, that is documents, as was pointed out in the first chapter. Collingwood has likened the modern development of research to that of science, calling it “scientific history” wherein the documents are examined from many viewpoints to extract the real facts. But documents are the creation of people with bias, egos, etc. As I asked in Chapter 1 about the battle fought by Ramses II with the so-called Hittites, How could we know the truth if, say, one of the accounts of the battle was lost and never found? The problem with the present approach to ancient historical chronology is that there are too few documents upon which to make the judgments. Therefore, no matter how hard the historian questions them he is really doing scissors-and-paste research.

Collingwood tries to employ the analytical approach of an investigator of a crime. But up until quite recently crimes were investigated almost solely on what people reported, and observations and the interpretations of these. Today the

forensic anthropologist and forensic scientist who analyze DNA, fibers, and many
other areas, as well carrying out as medical examination of the victim determine
most clearly the facts of the crime. This has been evidenced by research that in
certain states in the United States about half the convicted inmates, awaiting death
for their crimes, where shown to be innocent because of DNA evidence.

Teasing the guts out of documents can never be as conclusive as
scientific evidence. As criminologists have come to rely ever more strongly on
scientific evidence, those in the justices system, the district attorneys and judges,
have tended to fight these innovations, claiming the convicted inmate received a fair
trial. But as the evidence presented by the forensic anthropologists and other
forensic scientists has shown, the trial was not fair. The real evidence, the scientific
one, was not permitted to speak. The overturning of so many criminal cases is a
great embarrassment to those district attorneys, judges, and others involved in
having placed innocent persons in prison and even killing them.

In this respect the modern historian is in a similar situation as those
district attorneys, judges, juries, and even journalists who never questioned the
validity of these cases. While their historical analysis presents a long chronology,
the science contradicts and disproves their case repeatedly.

Lawyers and district attorneys often point to the preponderance of
evidence. It is not just one piece of evidence, they often maintain, that allows for a
proper judgment or verdict, but the preponderance of all the evidence that must be
taken into account before rendering a verdict. In this regard there can be very little
doubt that the preponderance of the scientific and technological evidence is rather
overwhelmingly in contradiction to the established long chronology but
overwhelmingly gives support to the short chronology. If Collingwood and his
colleagues in the philosophy of historical research as well as historians and
archaeologists of ancient Near Eastern history and chronology believe in their
concept of “scientific historical” analysis, then the evidence presented above renders
a verdict. That verdict is that the long chronology cannot be sustained and that the
short chronology of Heinsohn, Rose, Sweeney and, in part, Velikovsky, must be
sustained. But they will not be able to do that as will be explained below.

Lastly, we come to the influential work of Thomas Kuhn, Structure of
Scientific Revolutions (Chicago 1962). Kuhn suggests that the concept presented by
Robert K. Merton, that the scientific endeavor is an open, democratic marketplace
of competing theories, is nothing of the kind. This, he maintains, is an idealized
account. Kuhn’s work in great measure shows that most scientists are bound by a commitment to the reigning paradigms in each field. Like the district attorneys who won their cases, that precedent is the only one that is acceptable to, and defended and guarded by, the scientific elite. These elites license, supervise and franchise the initiates into their organizations through their education. The newcomers’ activity trained in their traditions are set to work, solving problems or puzzles that are in conformity with that paradigm and solved within its parameters. Revolutions in this environment are extraordinarily rare and are seen as disruptive, to which the guardians of the reigning paradigm respond with a vengeance, getting rid of dissidents, attacking or ignoring their work.

Therefore the shift from one paradigm is not a rational procedure, but like political revolutions an irrational one. Since the paradigms are incommensurable they cannot be understood by the proponents of the other. The textbooks that are written as well as the other literature presented by the establishment, according to Kuhn, are not part of a free market of ideas, rather the literature is engaged to suppress revolutionary or revisionist concepts and to uphold those entrenched in the corridors of the institutions of science which are its seats of power. Truth is determined by experts in power who have reached agreement on that truth. The great popularity of Kuhn’s ideas among the scientists and academics over the past four decades since they were first presented is indicative that in their heart of hearts many scientists and academics recognize that the marketplace is largely closed to revolutionary paradigms.

Closely related to the causes of scientific revolutions, according to Kuhn, is the question of evidence. If all the evidence correlated with and corroborated the reigning paradigm, then there would be no evidential basis upon which a revolution could begin. But this is not the case, rather there are exceptions to the paradigm that do not fit it. Kuhn has presented the concept that revolutions in science occur when a gathering number of anomalies become so significant that even some members of a discipline may come to the realization that the paradigm they have worked with all their lives is untenable. He maintains that the shift from one paradigm to another is not a matter of polite, peaceful debate. In fact, he submits that across a paradigm threshold communication between proponents of the established and revolutionary paradigm cannot occur because the foundations on which these proponents work are foreign to each other and have different methods, values, etc. Is it any wonder, then, that those of us involved in exploration of the short chronological paradigm for the ancient Near East have made very little, or no, inroads against those proponents of the long chronological paradigm?
Some revolutions in science have occurred over generations before the new paradigm ultimately found its place. Great intellectual revolutions may take centuries before those wedded to their well-established paradigm die off and new students with unfettered minds can emerge familiar with the new theory and not hostile to it, who begin the task of filling in and fulfilling its paradigmatic promise. In the final analysis whether or not the short chronology becomes the new model which historians, archaeologists, and, most importantly, scientists accept as the basis upon which their research will be conducted cannot be known. This book, among others that this author has presented, is yet another attempt to analyze the cogent evidence that is so vital for the research path that Velikovsky first blazed half a century ago. As an amateur in this field I am honored to have been able to raise my voice in this important undertaking. As Bertrand Russell said, “Even when the experts [of history] all agree, they may well be mistaken.”

When we examine the forms and character of the critics of the short chronology, many of them exhibited behavior that fully conforms with Kuhn’s description of the way the establishment deals with innovation of a revolutionary nature. Anthony Spalinger repeatedly presented the falsehood that Rose equated Sothis with Venus, but it was shown in three places that Rose in his book properly equated Sothis with the star Sirius. Spalinger accused Rose of a lack of condescension with regard to Parker and his work, which has nothing to do with evidence but much to do with assaulting Rose’s integrity. This is clearly a blatant attempt to castigate Rose in the eyes of his colleagues, not an attempt to present a balanced account of the evidence. So, too, with Ronald Wells in his criticism. He falsely accused Rose of failing to report the names of the months in the Greek and Egyptian calendars while, as a matter of fact, Rose gave the names of these months in both the Egyptian and Greek calendars. He claimed Rose did not give analysis or criticisms to the relevant bibliography in the endnotes or footnotes. We were able to demonstrate that Rose did not employ endnotes or footnotes, but instead discussed, analyzed, and criticized the relevant bibliography directly in the text. And Wells’ argument, based on misplaced precision, proved untenable requiring miracles of probability to discredit Rose. Presenting falsehoods in his criticism clearly demonstrates Wells’ animus and bias against Rose’s thesis. This is hardly an attempt to give a balanced account of the evidence. But most significantly, neither Spalinger nor Wells attempted to prove Rose made a single mistake in his work. Rather than do that, they evaded that material because, as this author believes, they could not find such errors, though there may be some. Their behavior fully conforms to Kuhn’s description of how establishment scientists deal with revolutionary theory.

\[16\] quoted in Rosten, *op.cit.*, p. 438
and evidence. With regard to Rose’s evidence, these establishment scholars misrepresented it and, in their failure to squarely face the solid evidence Rose presented, attempted to stifle and destroy it by deeply unscholarly behavior.

Let us examine the criticisms of Egyptologist William E. Stiebing, Jr. He claimed that at specific digs in and around Greece there were no problems discovered by the archaeologists who performed the research there related to the Dark Age history of Greece. However, when Eddie Schorr examined the literature of these various researchers he reported, with citations from their reports, that they found numerous problems. Why would Stiebing have misrepresented this material evidence unless he was ignorant, irresponsible or, worse, determined to disinform his colleagues about the revisionist research? Then with respect to king lists he claimed there were Assyrian King Lists when there was only one. He discussed kings’ names that, he said, were in the list but, as a matter of fact, the list never mentions these kings’ names and does not even cover the period under debate. He also made other such claims that all proved to be false. This is hardly the scholarly approach of a historian interested in the truth. It is more of what Kuhn claimed to be the response of a power structure intent on destroying a rival theory.

What, then, of the injury meted out to Eddie Schorr when he presented his research to his fellow graduate students? How can destroying the promising career of a student who merely presented evidence—which his mentor did not disprove as wrong or even attempt to disprove—not be regarded as material proof that Kuhn’s thesis is valid? Such strong-armed behavior cannot be seen other than as evidence that when a revolutionary theory is presented, its foes in the establishment attempt to kill it and its proponent by any means within their power.

What, too, of Cardona’s failure to report evidence in the same books he used to attack Heinsohn? Surely spreading disinformation is yet another indicator that Kuhn’s thesis is correct. While Cardona saw clear pictures of Scythians without beards in various places in the reference he cited, he never breathed a word that these contradictory facts were there.

Then there is the behavior of the researchers at the dig at Tell Munbaqa. When they discovered that the archaeological and geological evidence fully confirmed Heinsohn’s thesis and prediction, they then refused to discuss it, as Pape pointed out, and disregarded its obvious implications. Can this be taken as other than their joint agreement to stifle and remove this overwhelming negation of their chronology from their colleagues’ knowledge? And, again, isn’t this indisputably
the way Kuhn claimed the establishment would behave towards the revolutionary paradigm and its proponents?

In all these cases the underlying issues are not being discussed in a rational manner but in an emotional manner. The appeals are not to evidence and reason but rather to irrational factors such as group relatedness (how Parker was elevated over Rose’s evidence), or mob behavior (how Schorr’s career was destroyed without any protest by the historians). Though there is no bloodshed, the tools utilized by the establishment are based on force, propaganda, and suppression. Kuhn is right, and he deserves immense praise for finally explaining to the scientists and academics that they are human and emotional, especially in their professional pursuits. But even knowing this, and having read this far, they will remain intractable, as the following discussion will show.

Oscar Handlin warned historians that they must not dismiss criticism:

“Criticism is no less vital to the [historical] community, which in its absence either will sink into dreary veneration of a few respected old books or will, generation after generation, repeat the errors of its predecessors, making each day the fresh start of the return to the treadmill and mistaking energy expanded for distance advanced. The historian and the audience alike require a reliable process of evaluation to access the merits of books and articles and preserve the vitality of the discipline.”17

This author believes that the evidence of this book will never be seen in the light of chronological revision by those members of the historical community who ought to read it, because they are too deeply imbued with the established chronological paradigm. James F. Wells explains why this is so:

“A group is defined as ‘Individuals who share a common set of norms, beliefs and values’ (i.e. a schema [such as the established chronology of the ancient Near East]). The behavior of any member is usually of consequence to all the other members and for most people [for example, historians], the social support of the group is vital in that it defines existence [within the field of research]. A sense of belonging is one of the most compelling factors in the human experience and the

17 Handlin, op.cit., p. 111
feeling of isolation a tempering sensation unpleasant to most. The vast majority or people [including historians] do almost all their learning in the immediate presence of others who serve as teachers or role models. Their socialization [into the group] proceeds as initiates learn appropriate behavior and correlated linguistic values which make group members out of an assembly of individuals.

“Norms [of the group] function in the formation of the schema by providing social reinforcement (positive and negative) to the development of both the linguistic value system and the behavioral control system. It is group norms which define group values by shaping the language, attitudes, sentiments, aspirations and goals of the members. These give the in-group a sense of identity and a degree of solidarity proportional to the hostility which may be directed toward conflicting out-groups [such as the historical revisionists].

“Norms function to induce conformity wherever social organization is found. They provide the means group members use to exert subtle and indirect pressure on each other to think and behave appropriately [that is, to conform]. They are the customs, traditions, standards, rules, fashions and other unofficial criteria of conduct which organize the interactions of individuals into codified behavior of group members. In fact the initiate becomes a member [of the historical establishment] to the degree that he focuses on the norms . . . and guides his actions according to them. Identification is complete when the norms become internalized and function as subconscious reward systems. They serve as the criteria that sustain the attitudes and objectives [like upholding the established chronology] of the group members’ judgements and interpretations [which must] tend toward conformity. The result is similarity if not uniformity of thought and action—a condition which can be regarded as normal [for the group] or intellectually depressing.”

Therefore, those historians imbued with the group indoctrination of the establishment chronology will, if they have read to this point, have attempted to dismiss all this evidence or have invented excuses to do the same. They probably are returning to historical aspects of that chronology which they will still maintain contradict the shortened chronology, such as architecture, forms of writing, etc. But

these must now be recast so that they agree with the scientific and technological evidence. To a great extent these scientific and technical forms of evidence will not register with them because they cannot conceive that what they have learned and studied and taught for their entire professional lives could be in error.

Thus, the insider historians must cleave to that which they have known and have been secure and comfortable with in their professions even if they sense it may, in fact, be invalid. They cannot surrender a major element of their citadel for fear that the rest of the edifice will also crumble. As A. Cairns-Smith explains:

“One of the troubles with starting well is that you may look onto a point of view and then stay with it long after it has failed to maintain its initial promise. There are many examples from the history of science. The phlogiston theory was one. This was the idea that an inflammable material contained a ‘principle of fire’—phlogiston—which was given off when the material was burnt. This explains the obvious facts of combustion so well that it was quite natural to accept it and to interpret new data in terms of it. Suppose that as a respectable eighteenth century phlogistonist, you had been asked to explain why a candle goes out when a jar is put over it. Would you have taken this as a puzzle or an anomaly? Neither, you would have taken it as a confirmation of the phlogiston idea because it was so easily explained; the air in the jar has become saturated with phlogiston and cannot take up any more. What about the gain in weight when a metal converted to a calx [by joining together with oxygen atoms] (i.e. oxide)? Here you would put on a more patient expression. This is not so much a puzzle, you would explain, as a misunderstanding: phlogiston is the principle of fire, you see, not an insoluble substance: metals all contain this principle (which is why incidentally they have so many properties in common). When the corpuscles of metals are imbued with phlogiston the effect of gravity is lessened . . . You would I dare say, have been able to ward off all comers with such explanations. Nothing would force you to change sides. (Nothing ever forced Priestly.)”

The point of significance is that the historians of the ancient Near East may be constitutionally incapable of looking at the chronology from any other viewpoint than the one that presently reigns. Until they do so they will filter out all the scientific and technological evidence—this large number of anomalies—from

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their thought processes. As I believe Hempel put it, “man is a rational being indeed he can give a reason for anything he does.” This is a group failure to see these many scientific and technological anomalies and understand what they mean for their chronological construct. Until they remove that filter they will see only what they believe. An interesting case in point is presented by Ruben Able, the curator of the Metropolitan Museum of Art, regarding a famous ancient Greek statue of a horse:

“For some years every day or two I would walk through the Greek bronze gallery. I’d look at the horse from a different angle every day. One day, in July 1961, I did a double-take. For the first time I paid attention to a line—it can be seen in all the photos in all the books—that runs from the top of the mane down to the tip of the nose. I examined it through the glass showcase... I knew as sure as I was standing there that the piece was a fraud and the line had not registered. This time the line did.”

So, too, with the evidence presented in this book. The edifice of ancient Near Eastern chronology will only stand so long as the historians fail to see it from other than the viewpoint that has for so long directed their vision. That evidence can be seen in numerous books and journals but has hardly been given the attention it deserves. Until the historians examine it with the dispassion it deserves they will continue to fail to realize that that piece of ancient historical chronology is a fraud and that the line of evidence has not registered. As with that long-standing fraudulent Greek statue, accepted for so long as genuine, so too with the mighty edifice of ancient Near Eastern chronology which has stood even longer.

What must be pointed out as being of greatest significance is that new evidence played no role with the authorities and great thinkers of Galileo’s time in changing their minds. As Lynn Rose points out:

“It is well to remind ourselves that the revolution we are studying turned almost not at all on new evidence. New facts, when they came, did not change any of the leading minds of that period. Tycho Brahe’s facts did not make him a Copernican. Galileo was a Copernican before as well as after his telescopic discoveries. Kepler was a Copernican even before, and Bellarmino remained anti-Copernican even after, the observations of Tycho and Galileo became available. (For a discussion of Francis Bacon’s [anti-Copernican] attitude see [Antoinette Mann]"

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20 Able, op.cit., p. 40
Paterson [“An approach to the history of Early Astronomy,” *Studies in History and Philosophy of Science*, 1970, 1:(93-133)] pp. 38-40 and 159) . . . Finally, those who opposed Copernicanism on theological grounds were swayed neither by Tycho’s facts nor by Galileo’s facts nor by anyone else’s facts for several centuries.”

It is therefore the conclusion of this author that none of the facts in this book will play the slightest role in changing the minds of the historians, archaeologists, and any of the other academics. As Sir Edward Bailey said, “to find a thing you have to believe it to be possible.” The same applies to the historians, archaeologists, and academics who read this book. Because they cannot believe that the paradigm to which they have devoted their energies, studies, writings, researches—their professional lives—might be wrong, they will not be capable of changing their minds. The revisionist revolution may take decades or even centuries, if ever, before the mighty edifice of ancient Near Eastern chronology collapses. But in a certain sense, it has already fallen. In the final analysis their only response to all this is psychological denial. By being silent about this book they will ensure it will all be forgotten by themselves and their colleagues.

This author sees the edifice of ancient historical chronology as a great Ozymandian monument to the dogmatism of the academic world. To paraphrase Shelley’s poem *Ozymandias*:

“I met a historian of the ancient lands who said: Two vast and trunkless, Egyptian and Mesopotamian legs solid as stone stand in the desert. Near them on the sand, half sunk, a visage lies, whose frown, and wrinkled lip, and sneer of cold command, tell of its dogmatic structure. . . . which yet survives and has given support to these lifeless things, that authority mocked revision and that heart was dead to evidence. This historian is Ozymandias, who proclaims: ‘Look on my thousands of works in libraries, and despair!’ Nothing besides remains. Round the decay of that colossal wreck, boundless and bare, the lone and level sands stretch far away.”

Having examined the foundations of ancient Near Eastern chronology, this author, in large measure, concurs with Dayton’s Epilogue, but will go much

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further: There never was a Dark Age in Greece, nor in Anatolia and several other areas. The only Dark Age that exists regarding the chronology of the ancient Near East is the one which exists in the minds of historians and the one we live in today! Their great Ozymandian monument is made of diorite which was never shaped and incised with copper and quartz sand but by their soft ephemeral thoughts and dull, and pointless opinions.

As W.E.B. Du Bois wrote in “The Propaganda of History,”

“We shall never have a science of history until we have in our colleges and universities men [and women] who regard the truth as more important than defense [of what they were taught, what they teach and were indoctrinated to accept], and who will not deliberately encourage students to gather thesis material in order to support [their] prejudice[s] or buttress a lie.”

This author asks historians to consider the appeal of Oliver Cromwell: “I beseech you, brethren, think it possible that ye may be mistaken.” Arthur Koestler fully understood the psychological and sociological nature of those who will fight with all their might against a major overhaul of a cherished paradigm. And thus I close this work with this fundamental insight into the minds of those who must dismiss the evidence in this book:

“The inertia of the human mind and its resistance to innovation are most clearly demonstrated not, as one might expect, by the ignorant mass—which is easily swayed once its imagination is caught—but by the professionals with a vested interest in the monopoly of learning. Innovation is a twofold threat to academic mediocrities: it endangers their oracular authority, and it evokes the deeper fear that their whole laboriously constructed edifice might collapse. The academic backwoodsmen have been the curse of genius from Aristarchus to Darwin and Freud; they stretch, a solid and hostile phalanx of pedantic mediocrities, across the centuries.”

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23 W.E.B. Du Bois, in Gordon-Reed, op.cit., p. 105
24 R.G. Collingwood, op.cit., p. 494
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