1. Homer & Schliemann:

The two incidents that Courville found of most value for a chronological discussion of Greek history are the Fall of Troy and the so-called Dorian invasion. He began his treatment of the chronology of ancient Greece with a discussion of the discovery of the site of Troy by Heinrich Schliemann. Courville pointed out that historians of the 19th century were generally skeptical of ancient Greek writings, and did not believe that the Trojan War had, as related by Homer in the Iliad, any historical basis. (EP, 2:267.) After Heinrich Schliemann’s excavations at Hisarlik, however, the writings of Homer could no longer be thoughtlessly dismissed. Scholars now had to weigh the evidence more carefully.

While Courville himself did not believe in the absolute truthfulness of Homer, he tended to take a more favorable view of Homer’s reliability than some scholars would be willing to countenance. He says,

“It is the present writer’s conviction that while these myths clearly present backgrounds of fanciful details, the characters not only had a real existence, but also the time and genealogical relations as given are factual.” (EP, 2:269.)

Compare this with the evaluation given by classical scholar John Forsdyke:

“The only Greek epics of which we have even a general account are those of the Trojan war. The Iliad, being the earliest of them, ought to stand nearest to historical truth, but we have seen that many of its persons and episodes are fictitious.”

Or Walter Burkert’s evaluation:

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1 John Forsdyke, Greece Before Homer: Ancient Chronology and Mythology, 1964, p. 164.
“Thus the foundations for any ‘historical’ Trojan War of Homeric type, for the attack, siege, and conquest of that citadel at Hissarlik by Mycenaean warriors, have crumbled again. The Trojan War as sung by Homer did not take place.” (In, ed., Jane B. Carter & Sarah P. Morris, “Lydia Between East and West,”

We agree with Courville that Homer should not be dismissed out of hand, and we are prepared to find a great deal more of historical value in Homer than some scholars are willing to allow. In other words, we are far more “unitarian” than we are “separatist” with respect to the Homeric question. The work of archaeologists such as Schliemann, Blegen, and the current Hisarlik excavators has shown that a separatist approach to Homer and Greek tradition cannot have the field by default.

The fall of Troy, like the fall of Rome, was a watershed event in world history, and was remembered for many long years after. It inspired the great poet Homer to write both the Iliad and the delightfully fantastic voyage called the Odyssey. The latter has too much of the wonder tale about it to be regarded as entirely historical. Unfortunately, Homer’s Iliad, a long account of the last year of the Trojan War, was also consigned by some 19th century scholars largely to the realm of wonder tales and myth. That was to change to some degree with the work of Schliemann.

The story goes that Schliemann conceived the idea of excavating Troy when he was a boy. Scholars have doubts about some of Schliemann’s auto-biographical details, and regard some of them as romantic, even untruthful. No doubt Schliemann, like many a boy of his age, had adventurous ideas about Troy, and when the elder archaeologist looked back upon his boyhood, it was only too easy to transform boyhood fancies into programmatic premonitions. “[H]e viewed his life in retrospect as having had a meaningful pattern, and emphasized this at the expense of random or unhappy events, the better to present a moral tale.” Accordingly, Schliemann brought in a childhood sweetheart, who joined him in his youth in the quest to dig up, among other things, the city of Troy:

“[N]ay we could imagine nothing pleasanter than to spend all our lives in digging for relics of the past.”

When he was older, Schliemann worked for a grocer, and we are told, listened to the grocer’s “melodious” reading of Homer:

“From that moment I never ceased to pray to God that by His grace I might yet have the happiness of learning Greek.”

In relating the details of Schliemann’s life, historians have commented that while most children grow out of their childhood fancies, Schliemann did not, for to him “they remained real and permanent.” He was certainly given to exaggeration and “turned his own life into an inextricable tangle of fantasy and truth.” Nevertheless, today’s scholars recognize Schliemann’s importance to archaeology even if they note some of his failings:

“His excavations would occupy him, at intervals, for the rest of his life. His excavations were to wrest some of the secrets from the mound, though its complexities could not be quickly or easily unraveled, and indeed he paid a high price for a too-hasty attack. Nonetheless his is rightly one of the most famous names in archaeology. His finds, both at Troy and at sites on the Greek mainland, were arguably the most dramatic and significant ever made by an individual—in archaeology, as in business, he had a golden touch. He truly claimed to have found a new world and was without doubt one of the discipline’s most remarkable pioneers.”

After becoming rich, Schliemann took up travel writing, and attended classes at the Sorbonne. Eventually, he traveled through Greece, and must have remembered his boyhood fancy, and decided to devote his life to what some might have seen as the rather eccentric task of finding Homer’s world. Because he was so rich, he could very well afford to squander a good deal of money on the project (if it turned out a failure). Luckily, however, the money was not wasted. Others had gone before Schliemann, paving the way for his success, men such as Edward Clarke, Charles MacLaren, Frank Calvert, and Charles Newton.

At least by 1864, Frank Calvert believed that Troy was on the mound of Hisarlik, at the site of New Ilium. Charles Newton, who represented the British Museum, recommended that the Museum support Calvert in excavating the site—but the Museum failed to act. Calvert managed to purchase part of Hisarlik, but he did not have the money for a proper excavation.

Enter romantic Schliemann…and money. “Like many European people in the nineteenth century,”—writes Wood—“he knew Homer and loved his tale, but it was probably only his visit to Greece and Troy in the summer of 1868—and his meeting with Frank Calvert—which gave Schliemann the inspiration to turn to archaeology, and the idea of discovering Homer’s Troy by excavation.”

From the first, Schliemann made many mistakes in his claims about his excavations of the Homeric cities—the most serious one being his belief that Level 2 at Hisarlik was the city of the Trojan War. If Schliemann’s chief moral sin was telling little white lies about his past—“reinventing” himself, as it were—his chief intellectual sin was jumping to historical conclusions. Schliemann was interested in the Troy of history, to be sure, but he was equally interested in the Troy of Homer, and sometimes allowed the romantic to overcome the strictly scientific. Eventually, however, because Schliemann began to adopt a more scientific attitude, and also gained the help of the architect, Wilhelm Dorpfeld, the archaeology of Troy was greatly clarified.

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6 Fitton, pp. 53-54.
7 Wood, p. 48.
8 For detailed and balanced accounts of Schliemann’s work, see Michael Wood, In Search of the Trojan War; J. Lesley Fitton, The Discovery of the Greek Bronze Age, and Herve Duchene, Golden Treasures of Troy: The Dream of Heinrich Schliemann, 1996.
2. The City of Priam

After Schliemann’s death, Dorpfeld excavated the site of Hisarlik again and determined that the last phase of the sixth city of Troy (now called Troy 6h) was the city defeated by the Greeks. This conclusion was later disputed by the archaeologist Carl Blegen, who chose Troy 7a as the city defeated by the Greeks. It is not easy for us to depart from the views of an expert archaeologist such as Blegen, but we believe Dorpfeld was right that the last city of Troy 6 was the city of the Homeric saga, and that Blegen was mistaken in his choice of Troy 7a. We are in good company, however, since many archaeologists today reject Blegen’s view, and adopt Dorpfeld’s view:

“There can be little doubt that Schliemann would have been delighted with the emergence of Troy VI and the growing feeling that this, if any, is the city most likely to lie behind the story of the Trojan War. The excavations since his time, up to and including the modern campaign [since 1988 under Manfred Korfmann], could all—with his eye of faith—be seen as making more likely the equation between Troy VI and Homer’s ‘broad city’ with ‘fine towers’ and ‘lofty gates’. ”

“It was Dorpfeld who, between 1893 and 1894, unearthed Troy VI….German and Turkish archaeologists believe that this wealthy and well-fortified settlement was the Troy of Priam.”

“Troy VIIa, which Blegen urged as a candidate, is now widely rejected.”

There primary reason for the change of view among scholars is stratigraphic. It is very important that the reader understand why scholars now reject Blegen’s identification of Troy 7a as the city of Homer. A. Furumark had noted in 1963 that Troy 7a contained Late Helladic 3c sherds. What does this mean? Crucially it means that the purported city of Priam, Troy 7a (advocated by Blegen), was not destroyed until the LH3c block of time. In contradistinction, Troy 6h does not have pottery beyond LH3b, so Troy 6h came to an end before the time of LH3c. On the basis of these stratigraphic facts, Michael Wood pinpointed the problem with Blegen’s correlation of Troy 7a with Priam’s city:

“Blegen asserted that ‘not a single piece’ of LH III C pottery was found in Troy VIIa...(now placed at 1190-1185 or later). However, it is now clear that several pieces of LH III C were found in Troy VIIa and that it was probably destroyed around 1180 BC. This is confirmed by the appearance of another kind of pottery, the so-called ‘Granary Class’, in the next phase of Troy, VIIb: this phase can hardly have begun until the ‘Granary Class’ was widespread in Greece, that is, in 1170-1160. Troy VIIa, then, which Blegen thought Homer’s Troy, is far too late for the Trojan War.”

Why too late? Simple enough—the Mycenaean civilization collapsed at the end of LH3b, meaning that there was no large empire in existence at the time of the destruction of Blegen’s Troy 7a—correlated to LH3c. Wood says,

9 Fitton, p. 181.
13 Wood, p. 224.
“Troy VIIa fell in around 1180, after destructions on the mainland which in some cases ruined the great palaces forever. It would appear that Troy VIIa cannot be Homer’s Troy; Troy VIh could be.”

To put it another way, Blegen’s theory that Troy 7a was Priam’s Troy could only work if no LH3c pottery were associated with Troy 7a. The reason is that during the LH3c pottery phase, there was no one left from the Greek mainland to conquer Troy. The great Mycenaean civilization—presumably the civilization of Agamemnon—had already passed away. This is probably why Blegen was so insistent that “not a single piece” of LH3c pottery was found in Troy 7a. He knew very well that if such were found, his designated city could not work.

With regard to Troy 6h as the city of Priam there is one possible problem that stands in the way of complete acceptance of the city, and that is that Troy 6h was destroyed by an earthquake. This is not entirely consistent with the story of the Trojan horse. In response, we point out that the German excavators found arrowheads and spearheads in the destruction levels of 6h, showing that Troy 6h also saw its share of battle. The conjunction of a natural disaster with human warfare is unusual but not unheard of. In 525 B.C., while attempting to invade Egypt, Cambyses 2 reportedly lost 50,000 of his Persian soldiers in a catastrophic sandstorm. Many warships over the years have been lost in sudden storms. Russia has also survived invasions due to its treacherous climatic conditions which have become rather proverbial (“never invade Russia”). In the twentieth century, Hitler’s armies found this out when they were defeated by the Russians under the leadership of the famed military commander, General Winter.

To be sure, the concept of Troy 6h falling to the combined forces of an earthquake, a resulting conflagration, and an opportunistic invasion, is not a perfect fit with Homer—and yet it is inherently more plausible than alternatives. It has been suggested (by Pliny, Servius, Pausanias) that the story of the wooden horse might have originally been an attempt to explain the destructive effects of a battering ram. Another suggestion (by Wood) is that the story of the wooden horse might be connected to the legendary “Earthshaker”—i.e., the god Poseidon. This would allow for a legendary cause of a rather mundane, though terrible, seismic event.

The notion of a connection between Poseidon and the earthquake that contributed to the destruction of Troy 6h seems closer to the truth than the battering ram theory. It at least agrees with the Homeric presupposition that the Greeks could not really get the job done. Some means other than arms or prowess in battle had to be devised. Thus both the story of the Trojan Horse and our present day knowledge of the earthquake that brought down Troy 6h presuppose that the Greeks could not prevail militarily, but won due to external circumstances. The theory of a battering ram fails to convey this common theme, and indeed holds the opposite view, that the Greeks were able to overcome Troy by their own efforts.

However, neither suggestion—the Poseidon connection nor the battering ram theory—can rise above speculation at this time. The only thing we can be sure of is that Troy 6h was

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14 Wood, p. 225.
damaged by an earthquake, thus possibly allowing an army to sack the city. In our view, despite the uncertainties, this view has much to recommend it:

“First weakened by earthquake, there is every reason to believe that Troy VIh was then attacked and sacked by Mycenaean marauders, who removed and processed the survivors—executing some and en sla ving others—before putting it to the torch. The era of peace and prosperity, and of the successful rule of a strong citadel, its ships, horses and chariots, was over.”

We should also mention in this connection that the German excavations have brought new evidence to light regarding the size of Homer’s city. If you recall, Schliemann had laid claim to Hisarlik as the site of Troy but his excavations did not reveal a very large city. This must have been something of a disappointment to the romantic excavator. However, the new excavations have found the Lower City (a three-terraced enclosure surrounded by towers and a defensive wall), and this shows that Troy 6h was every bit as large as Homer had claimed, and every bit as large as Schliemann had hoped for but could not establish.

Note: One writer, Sinclair Hood, rejected both Dorpfeld’s Troy 6h and Blegen’s Troy 7a in favor of Troy 7b as Priam’s city. He invoked a situation-historical argument and spoke of a “picture beginning to emerge” from Hittite records, and claimed that the “scenario in Anatolia” makes it difficult to fit an Achaian sack of Troy into the scene. This is a debatable point, however. It is unlikely that the Hittites would have benefited from interfering in such a war and Hood provided no real evidence that the geopolitical situation would have prevented a Greek attack upon the city of Troy. He also did not think Homer’s poems reflected the world of the “literate bureaucracies” of Mycenaean Greece. It is hardly likely, however, that an epic poet would regale his audiences with heroic stories about accounting, so there is little reason to reject the presumption that the world depicted in Homer’s epics was largely a Mycenaean world.

Unfortunately for Hood’s theory, Troy 7b is too poor to be Priam’s Troy. The dominant pottery was “Knobbed Ware” and “Coarse Ware”—indicating the low material level of the population. Hood to the contrary, this is not consistent with Homer’s description of the city—“the topless towers of Ilium”—and few, if any, scholars would accept Troy 7b as the proper city of Greek epic.

3. Herodotus and Greek Chronography

Greek history begins with Herodotus. That is to say, it is to him that scholars first turn in their attempts to understand early Greek history. “The fundamental text remains Herodotus” said Walter Burkert in his essay on the dating of the Trojan War.

15 Fields, p. 59.
16 Fields, passim.
18 Hood, p. 28.
Accordingly, the first question that needs answering is how Herodotus came up with his date for the Trojan War.

First, it’s important to note that Herodotus often says that he is not vouching for the truth of the narratives he is recording, but merely reporting what he had heard. There is then no need to adopt a view which would regard all of Herodotus’s narratives as pristine truth: He says, “I am obliged to record the things I am told, but I am certainly not required to believe them—this remark may be taken to apply to the whole of my account.” Also, “Anyone who finds such things credible can make of these Egyptian stories what he wishes. My job, throughout this account, is simply to record whatever I am told by each of my sources.”

We can also apply this to the issue of chronology. Herodotus is not necessarily giving us accurate time lines, but is merely reporting what others have told him, or what others gave him as historical sources, including king lists, annals, genealogies, etc. With regard to chronology, Walter Burkert says, “The basic facts are simple. Herodotus is making use of king lists of Near Eastern type.” Moreover: “This includes the Twenty-sixth Dynasty of Egypt since the time of Psammetichus, the Lydian lists from Gyges to Croesus, the Persians from Cyrus to Xerxes, and also the Medes from Deioces to Astyages.” Herodotus also has human sources, e.g., Persian logioi (experts), priests of Ptah at Memphis, priests of Amon-Re at Thebes, and priests at Heliopolis.

Nevertheless, with respect to Egyptian history, it is fairly obvious that Herodotus (or his sources) cannot be trusted in giving a correct chronology of Egypt. For instance, Herodotus has the pyramid builders of the fourth dynasty wildly out of place, and also confused the 12th dynasty kings Sesosiris 1 and Sesosiris 3 with the 19th dynasty king Ramses 2. A second example is the history of the Mermnadae in Lydia, from Gyges to Croesus. The chronology of these kings as given by Herodotus places Gyges too early. We know this because we know that Gyges lived during the time of Assurbanipal (668-627). As Burkert says, “One usually accepts Herodotus’ date for Croesus’ reign and shortens those of his predecessors.”

Most scholars, however, accept his reporting on the chronology of the 26th dynasty, despite some mistakes, e.g., Psammetichus’s reign began 664, rather than Herodotus’s 670 B.C.; and Apries’ reign length is actually 6 years shorter than given by Herodotus.

Herodotus dates the fall of Troy to 1250 B.C. Unfortunately, this date is one among several dates for the Trojan War given by, or inferred from, ancient voices:

21 Herodotus, Histories, 2:123.
23 Herodotus, The Histories, notes, pp. 597; 615.)
24 For a scholarly review, see The Histories, Waterfield edition, editor’s notes to Book 2, para. 102-235.
26 Burkert in Carter & Morris, p. 141.
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<td>Hecataeus</td>
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<tr>
<td>Athenian Philaids</td>
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Obviously, these dates diverge by as much as 368 years. Forsdyke says, “The methods by which these authorities obtained their results are seldom reported by the transmitters.”\(^{28}\) In addition, most of the dates go back to a single source, the Lacedaemonian (or Spartan) king list:

“Of the dates for the Fall of Troy that were proposed by reputable historians, most, if not all, were based upon the genealogies of the royal Lacedaemonian families, since the Return of the Heracleids was the event of primary interest for their authors. They were the most stable pedigrees that the Greeks possessed, though they were partly fictitious.”\(^{29}\)

Herodotus was aware of these lists, though he does not give the reign lengths of each king.\(^{30}\) At one point, he claimed that there were three generations in a hundred years, about 33 years to a generation, and from this Herodotus was able to calculate the length of time from the first king of Egypt to “Sethos,” and he came up with 11,340 years. This shows that the use of Egyptian genealogies to develop a chronology is a very ancient practice, and certainly got no nearer to the truth than does the conventional chronology of Egypt. Conventional chronology would shave off about 8000 years from the chronology provided by Herodotus.

Nevertheless, besides the 33-year generation length, Herodotus also used a 40-year generation length. Eduard Meyer, in his German article “Herodots Chronologie der griechischen Sagengeschichte,” [1892], demonstrated that the Spartan king list assumed a

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\(^{27}\) The above table is based on Burkert’s discussion in Carter & Morris, *The Ages of Homer*, pp. 139-148; and Forsdyke’s discussion in *Greece Before Homer*, p.62.

\(^{28}\) Forsdyke, p. 63.

\(^{29}\) Forsdyke, p. 67.

generation length of 40 years, and that this was how Herodotus arrived at the figure for Heracles as “about 900 years ago” and the figure for the immediate period following the Trojan War as “about eight hundred years before my time.”\textsuperscript{31} Calculating back from the Spartan king Leonidas (\textit{born} 530 B.C.), who was the 20\textsuperscript{th} descendant of Heracles, and using 40 years for a generation, we arrive at 1330 B.C. for Heracles. This is indeed almost 900 years before the time of Herodotus (444 B.C.).

Burkert objected to Meyer’s theory on the grounds that Meyer was assuming that Herodotus began the period with the birth date of Heracles, and that he should have used the “akme” of Heracles—which must mean the period when Heracles made his mark on the world. Nevertheless, if we start the time with Heracles at his maturity, we should be consistent and make the end of the period start with the “akme” of Leonidas, \textit{i.e.}, when he became king, c. 490 B.C. That would merely shift the dates of reckoning forward. It would not change the length of each generation, calculated at 40 years.

Moreover, Burkert argued that forty years to a generation was empirical nonsense, and thus the Greeks would not have used it. However, while we may not accept a 40-year generation count, it is not relevant to Meyer’s argument, which is that the Greeks certainly accepted it. They may have been skeptical in their day, those Greeks, but they were somewhat less restricted in regards to their ontology than we are. Thus, a 40-year generation count would not have seemed out of the ordinary for the Greeks, and thus Eratosthenes and Apollodorus could use such a count without any consciousness that they were moving beyond the limits of reason and experience.

Nevertheless, in place of the 40-year generation count, Burkert claimed that the Greeks were using a preconceived date for the Trojan War, and made the generations fit back to it:

“If the interval of forty years ever entered Greek chronography—and there is no doubt that Eratosthenes and Apolodorus adopted it—this must have been due to the conflict between different traditions: generations had to be stretched artificially in order to meet some other date. In other words, there was an authoritative early dating of Herakles and/or Troika which forced chronographers to extend genealogies to an average of forty years.”\textsuperscript{32}

Burkert provided no evidence for these fixed dates, nor cited any sources for them. His claim that the Greeks stretched their king lists or pedigrees to accommodate such fixed dates (derived from some unknown source) is purely gratuitous. The fact that absolute dates given by Herodotus make sense on a 40-year generation count, and that Eratosthenes and Apollodorus used a 40-year generation count, argues in favor of the idea that these historians were estimating backwards as best they could, not starting from a preconceived date for Heracles or the Trojan War.

Burkert offered another reason for rejecting Meyer’s claim. He thinks Hecataeus, who could trace, as Herodotus says, “his family history back to a divine ancestor in the sixteenth generation” was wrong. No “self-respecting Greek family,” said Burkert, “ever

\textsuperscript{31} Cited by Burkert in Carter & Morris, \textit{The Ages of Homer}, p. 143.

\textsuperscript{32} Burkert, p. 144.
claimed a divine ancestor more recent than the Trojan War,” and that because of this “Hecataeus could place the Trojan War at a maximum distance of fourteen generations.” Moreover,

“This means that in Hecataeus’ view the Trojan War must have happened in ‘966’ at the earliest, or somewhat later.”

Burkert did not believe that the Trojan War happened this late, but he thought he was refuting the view that the Greeks used a 40-year generation count. If indeed, the Trojan War happened in 966 B.C., he would be quite right—the Greeks could not have been using a 40-year generation count. In confirmation of this, the genealogy of the Athenian Philaids, as given by Pherekydes, is cited:

“[T]hey count thirteen generations from Ajax to Hippokleides, archon of the first Great Panathenaia in 566. Assuming, with Herodotus, three generations per century, the interval of twelve generations would locate Ajax, and the Trojan War, 400 years earlier, i.e., ‘966.’”

It is argued that this would conflict with other dates in Herodotus if it is assumed he is using a 40 year generation count. Burkert concluded:

“Contrary to Eduard Meyer’s thesis, then, the absolute dates given by Herodotus for Herakles and Pan, i.e., Troika, do not go back to Hecataeus and have nothing to do with Greek estimates of generations, either Spartan or Athenian. All Greek genealogies would lead to much lower dates.”

As suggested, the fallacy in Burkert’s argument is that he is confusing what the Greeks would think likely and what we would think likely. However, from an alternative chronology point of view, it is quite correct to lower the length of time between Heracles and Troy and the Classical period, based on a more realistic generation length of 30 years, or even less. However, we cannot assume that the Greeks thought this way—that they were as concerned with the empirically possible as much as we are. In fact, Herodotus accepted many aspects of Egyptian chronology that cannot be sustained, so why should it be thought strange that he would accept a 40-year generation length when he accepted a start of Egypt more than 11,000 years before his time? John Forsdyke, representing the traditional view, says,

“The date [for the Trojan War] quoted by Herodotus, which is one of the highest in the series (about 1250 B.C.) was evidently derived from the old 40-year generation. The lowest recorded estimate, that of Ephoros (1135 B.C.) is demonstrably based upon the 33-year generation without regard to fictitious regnal years.”

Burkert may be right that the Greeks “knew nothing about the dates for either Herakles or Troika” but not for the reasons he cites. We point out once again that Burkert has no evidence at all that the Greeks were using a preconceived date and stretching all their

33 Burkert, p. 143.
34 Burkert, p. 144.
35 Burkert, p. 144.
36 Forsdyke, Greece Before Homer, p. 68.
genealogies to fit with it. In fact, we think the opposite is the case: the basic problem was precisely that the Greeks were using a 33- or 40-year generation count.

“Particularly influential was the date of 1183 BC given by Eratosthenes, librarian at Alexandria in the third century BC and a respected authority, though his estimate—like that of Herodotus—was worked out on the basis of the Spartan king-lists, the disagreement arising only from their calculating the length of a generation differently.”

If they had interpreted their genealogies as king lists, and used an average reign length (ARL) instead of an average generation length (AGL), they would have come to a more accurate date for the Trojan War. As A.M. Snodgrass says,

“[W]hether or not one believes in the possibility of an independently-reached date for the Fall of Troy, the pedigree of forty-year generations and, a fortiori, the events ascribed to a generation or reign on such a basis, must remain chronologically valueless.”

We would agree with this only if a generation count is used when an average reign length would be more appropriate. A generation count would seem to be appropriate for counting the descendants in a family—i.e., non-kings—like those found in the line of the Heraclids. A generation count would be less trustworthy when applied to a line of kings since kings did not rule an average of 40 years or more.

4. The Spartan King List

All chronological roads point to the Spartan king list as the key to solving the puzzle over the date of the Trojan War. To quote Forsdyke again, “Of the dates for the Fall of Troy that were proposed by reputable historians, most, if not all, were based upon the genealogies of the royal Lacedaemonian families, since the Return of the Heracleids was the event of primary interest for their authors.” We construct the following table of the Spartan kings, based on Herodotus, Pausanias, and others:

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37 Fitton, Discovery of the Greek Bronze Age, p. 23.
39 The spelling for the list is that given in W. G. Forrest’s, A History of Sparta, 1968, pp. 21-22; and the format is partly based on Forsdyke in his Greece Before Homer, p. 31.
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<td>Eukleidas 227</td>
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</table>

Eratosthenes’ date of 1184 B.C. for the Trojan War was based on this same Lacedaimonian (or Spartan) king list, and was generally accepted in the ancient world. Eratosthenes, however, was using (from our perspective) an implausible generation length of 40 years to arrive at his date. As we’ve noted, it just seems wrong to use a generation count to determine the length of a dynastic period. Indeed, a 20-year average reign length is more likely to give the approximately correct length of time for the whole line of kings, much more so than any count using generation estimates. Thus, for the above chart, we have used a 20-year average reign length.
This principle of average reign length was first recognized by the redoubtable Isaac Newton. Peter James pointed out:

“…[A]s Sir Isaac Newton pointed out long ago, continuous father-to-son successions for the two Spartan royal lines over twenty-one generations is highly improbable on biological grounds. He suggested that the genealogies recorded by Herodotus and others must in fact have been a king list. From his own analysis of recorded dynasties he arrived at an average of eighteen to twenty years for a reign-length [ftn: F. Manuel, *Isaac Newton: Historian*, 1963]; Burn, taking the English and French monarchies as a basis, produced a figure of 23.5 years.”

In *The Histories* 7:205, it is recounted how Leonidas became king, and how the kingship always passed from father to eldest living son or to his heir. In the case of Leonidas, Herodotus says that the kingship passed to him because his two older brothers died without leaving any heirs. In our opinion, this is enough to prove that the genealogical line given by Herodotus was really a line of hereditary kings, and that the Spartan genealogies are actually king lists. We therefore follow Newton with regard to king lists, that the use of average reign lengths gives a more accurate length of time for the whole dynastic period.

According to Forsdyke, the average reign length for the historical Spartan kings was 23 years. Mention could also be made of the kings of Israel, who had an estimated ARL of about 23 years, and the kings of Judea, an ARL of about 15 years. These, however, still overstate the length of the total kings since it doesn’t take co-regencies and parallel reigns into account (resulting in approximately 21 years for Israel; 13 years for Judea). We are probably safe if we accept Newton’s suggested ARL of 20 years—as in the above table. Interestingly, Forrest says:

“Spartans traced the ancestry of their kings back through the eponymous founders of the two royal houses, Agis and Eurypon, to their fathers, Eurysthenes and Prokles, to Aristodemos and ultimately to Herakles. It was assumed that kings reigned for forty years and so, accidentally, roughly the right date [sic] was reached for the invaders, for the collapse of the Mycenaean kingdom. But a more realistic calculation gives not 1200 but 960-930 for Eurysthenes and Prokles, and 930-900 or thereabouts for Agis and Eurypon who as eponyms must be the real founders of their dynasties.”

Forrest’s chronology uses a 30-year generation length, and this would certainly be more realistic than using a 40-year generation length. However, it is our contention that a chronology based on a 20-year reign length would be even more realistic, and would place Agis and Eurypon within the early 9th century B.C.

5. *The Date of the Trojan War*

According to legend, Eurystheus—the king who assigned the famous labors to Heracles or Hercules—expelled the sons of Hercules from Greece in fear that they would

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41 Forsdyke, p. 34.
42 Forrest, *A History of Sparta*, p. 27.
eventually depose him. If we accept at least the real existence of a king named Hercules, we of course cannot accept all the stories about him—his descent from a god, his superhuman strength, his mythical labors under Eurystheus, etc.—because in our day we have a few more restrictions on our ontology than the Greeks had. Nevertheless, we can perhaps recognize that behind the legendary material is the view that Hercules was a popular king, who became a source for later legends, and that his “labors” appear to suggest a memory that he was a vassal (or subordinate) king under the suzerainty of king Eurystheus.

The sons and descendants of Hercules were known as the Heraclids. The Athenians gave refuge to the expelled Heraclids, who, according to the Delphic Oracle, would return to the Peloponnese in the third generation (“wait for the third crop” it said). This means two generations had to pass before the return could be accomplished. The later Greek historian, Thucydides, believed this “third crop” meant 80 years after the Trojan War (two 40-year generations). On the other hand, Ephorus dated it 66 years after the Trojan War (two 33-year generations), and Strabo dated it 60 years later (two 30-year generations). Obviously, if the Return took place about 900 B.C.—as we have suggested on our theory—this means the Trojan War took place somewhere between 980 and 960 B.C.

The New Courville position is that the Trojan War began during the last days of David, or the beginning years of Solomon, circa 977 or slightly later. This would place it after the time of the war between Ramses 2 and the Hittites (using NC dates for Ramses 2). On the basis of 966 B.C. as the approximate date of Troy’s fall, we would place the Dorian migration (Return of the Heraclids) around 906 to 900 BC, followed by the Ionian migration in the middle part of the 9th century.

We are not the first to correlate the date of the Trojan War with the time of Ramses 2. This was suggested by W. E. Gladstone. Gladstone compared the three wars against Egypt under Ramses 2, Merneptah, and Ramses 3:

“The characteristic names of the three Expeditions, which supply the links with Greek history, are respectively Dardanians, Achaians, and Danaans. The first expedition was certainly, and the second probably, before the War of Troy; the third must in all likelihood have been later than the War.”

The BC dates that Gladstone assigns to the time of Ramses 2 are conventional, sometime in the 13th century B.C., whereas New Courville would assign the time of Ramses 2 to the 10th century B.C. Also, we agree that the first attack against Egypt came before the War, but we believe the war against Merneptah was after the war against Troy. While it may not be conclusive evidence, it is noteworthy that the Derden are only heard from in the time of Ramses 2, but did not participate in the wars against Merneptah or Ramses 3.

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The Derden are the Dardanians, *i.e.*, Trojans, descendants of Dardanus.\(^{46}\) The following list provides the line of descent:

1. Dardanus  
2. Erichthonios  
3. Tros  
4. Ilus, Assaracus, Ganymede  
5. Laomedon, son of Ilus; Capys, son of Assaracus  
6. Priam, et al., sons of Laomedon; Anchises, son of Capys  
7. Hector, son of Priam; Aineias, son of Anchises

In a discussion of the likelihood regarding whether the Trojan War took place, Wood pointed out that,

“Hittite and Egyptian evidence suggests that Homer was correct in the names he called the peoples: Achaians and Danaans, in the case of the Greeks, and Dardanians in the case of the Trojans.”\(^ {47}\)

This means that Troy possibly ruled over a strong nation-state known by the Egyptians as the Derden when they fought Ramses 2. Sometime after, however, the Derden or Trojans were defeated by the combined forces of the Greeks under Agamemnon. Their defeat during the Trojan War may have been the reason for the absence of Derden in the next two Aegean attacks upon Egypt (in the time of Merneptah and of Ramses 3). It is in fact their absence from these two later attacks that leads to our disagreement with Gladstone. It is unlikely that the Trojans would have been able to participate in any wars after their loss to the Greeks. If we are right in our view that the Trojan War took place around 966 B.C., using New Courville dates, then this provides a plausible explanation of why the Trojans (or Dardanoi) were part of the war against Ramses 2, but were not present for the wars against Merneptah and Ramses 3.

The date of c. 966 B.C. for the Fall of Troy is plausible also in terms of average reign length. Legend has it that Heracles lived a generation before the Trojan War and that the Dorians attacked two generations after the Trojan War, so we have placed the Heraclid line between these two points. For the Heraclid family, this means we must use a 30 to 40 year average generation length (since they were not kings)—but of course we are only estimating in this area. According to tradition, Heracles and Eurystheus were contemporaries, and the son of Heracles, Hyllus, killed Eurystheus. After this, Hyllus, a contemporary of Atreus, was soon killed in battle, and did not live a full generation.

With regard to the Perseid and Pelopid dynasties, we are using an average reign length, since these were lines of kings:\(^ {48}\)

\(^ {47} \) Wood, p. 138.  
\(^ {48} \) With the exception of the dates, this table is based in part on George E. Mylonas, *Mycenae and the Mycenaean Age*, 1966, p. 6.
<table>
<thead>
<tr>
<th>Perseid, est. 20 year ARL</th>
<th>Pelopid, est. 20 year ARL</th>
<th>Heraclid, est. 30-40 AGL</th>
<th>New Courville B.C. Dates</th>
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<td>Tisamenos, Dorian</td>
<td>Aristodemos</td>
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<td>926-906</td>
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On the basis of the presence and absence of the Derden, and the use of average reign length, we believe we have come to a plausible time frame for the Trojan War. Of course, we may be accused of guessing too much on this subject, but that would not single us out as unique since everyone else who approaches this subject must also engage in similar guesswork. However, we think that our guesses are educated guesses since they are presupposing a Newton-derived concept of 20-year average reign length, which places the time of Tisamenos around 906 B.C. It’s only a matter of counting back two generations with a plausible generation length of 30 years, and arriving at circa 966 B.C. This date also agrees with some of the older Greek genealogies, of Hecataeus, and of the Athenian Philaids, who were apparently using a 33-year generation count and this would result in a date of c. 966 B.C. as their date for the Trojan War.

6. Courville and Greek Pedigrees

We do not have Courville’s confidence in Alexander the Great’s genealogy. (EP, 2:273.) In our view such a genealogy is at least in part fictitious, as were most late Classical attempts to trace pedigrees back to the Homeric heroes. These have the appearance of being “snob pedigrees”—meant to show that the family in question had noble blood, and was above the common, run-of-the-mill sort of Greek. To be sure, Alexander’s motivations may not have been snobbery, but it’s more than a little likely that politics was at the center of his desire to genealogize himself back to the Greek heroes. Courville related the fact that Alexander was able to trace his genealogy back to the Trojan heroes “to the complete satisfaction of those making the demand.” (EP, 2:273.) We have no doubt that Alexander met the historical standards of his times with great brilliance, but those of us whose chronographic faith is not large enough must suspend judgment.

49 Cf., Forsdyke, pp. 87ff., who also dismisses them as fictitious.
7. Courville’s Date for the Fall of Troy

We certainly think the date of 966 B.C. for the Fall of Troy is more plausible than the date suggested by conventional chronology. We might also add that we think it’s also more plausible than the date suggested by Courville. Courville is aware of the Spartan king list (noted above), and has this to say about it:

“Greek chronology is not solidly fixed until one reaches the era of the mid-6\textsuperscript{th} century B.C. However, from this point, the chronology can be traced back in time by means of the Spartan and Macedonian kings…to a date that cannot be far removed from the mid-8\textsuperscript{th} century.” (EP, 2:270.)

In terms of conventional chronology for this period, King Theopompos of the Spartan list can be correlated to the Messenian War (735-715 B.C.) indirectly based on Olympic records.\textsuperscript{50} Another Olympic-based date is Sparta’s defeat by Argos (669 B.C.), and this can be correlated to the Spartan king, Polydorus. So we have fairly firm dates for two Spartan kings, Theopompos and Polydorus, and can space the other kings around them. Once we’ve done this, we can see that certain anachronisms arise from Courville’s dating:

\begin{itemize}
\item \textit{First Anachronism}-- Courville’s date for the Fall of Troy is 788 B.C. and his date for the Dorian invasion is 750 B.C. (EP, 2:274; 276.) Unfortunately, his allowance of only a 38-year period between the Trojan War and the Dorian invasion would put a real squeeze on those Spartan kings who came before 750 B.C. Where does Courville place Eurysthenes to Archelaos? Or Procles to Charillos? On his date for the Dorian invasion, there is hardly any room left for them to have reigned. Does he dismiss them as fiction? On what basis? There is no question that Courville correlates the earliest kings of Sparta with the Dorian invasion:

“Since the earliest kings of the Spartan lines belong to the era of the Dorian invasion….” (EP, 2:271.)

Yet if the Dorian invasion occurred at 750 B.C., in Courville’s reckoning, it follows that all the kings of the Eurypontid line, from Procles to Charillos (760 B.C.), including Nikandros (740 B.C.), must be fitted within the putative 30 years from the Dorian invasion (750 B.C.) to Theopompos (720 B.C.). A similar situation would prevail for the Agiad line. If Courville is willing to grant that the genealogical relations of the characters in Greek myths are based on fact, \textit{a fortiori}, he should grant that the genealogical relations of \textit{historical} kings are based on fact. (EP, 2:269.) Courville either had to dismiss the earlier Spartan kings completely or squeeze them all into a 30-year period. Neither solution seems plausible.

\item \textit{Second Anachronism}: Not only does Courville have a problem fitting the early Spartan kings into his chronology, he also has little room for the entire Geometric pottery period. Given Courville’s general respect for archaeology—not shared by Velikovsky or some other revisionists—it is amazing that Courville did not recognize how decisively
\end{itemize}

\textsuperscript{50} Cf., W. Forrest, p. 20.
this problem undermined both his and Velikovsky’s chronology of the ancient Greeks. Courville certainly recognized some kind of threat, for he says:

“How long did this period of use of geometric pottery last, and does it necessarily represent an era independent of the use of Mykenaean or Archaic art?” (EP, 2:279.)

What Courville is suggesting is that the Geometric pottery period was contained within the Mycenaean period, i.e., time within time. Since Courville placed the Trojan War around 788 B.C., he also had to place the Mycenaean pottery period here as well, since both candidates for the doomed city—Troy 6h or 7a—contain Mycenaean pottery. But 788 B.C. is the date usually ascribed to the Middle Geometric 2 period. In fact, the whole Geometric period for most scholars begins at 900 B.C. and runs down to 700 B.C. This is then followed by the Attic sequences at 700 B.C., and the Black Figure sequences at 625 B.C. 51

We agree with the CoD group that the whole Geometric period needs to be brought forward by about 50 years, placing the end of the Late Geometric at about 675 B.C. 52 We also agree with J. M. Davison that the Early Geometric should start at 850 B.C. 53 Thus, New Courville regards the whole of the Geometric period as running from about 850 B.C. to 675 B.C., the Attic and Black Figure starting respectively at 675 B.C. and 625 B.C.

Despite this, however, New Courville still respects the need for these pottery sequences to be spread out over a reasonable period of time. Our disagreement with conventional views is over minor shifts in starting or ending dates for the Geometric period. On conventional views the Late Geometric period begins at 760 B.C., and we see no realistic possibility that the Early and Middle Geometric periods can be shrunk to fit within the 28 years between the Late Mycenaean (788 B.C.) of Courville, and the Late Geometric (760 B.C.) of the archaeologists. While the Submycenaean and Protogeometric periods are eminently shrinkable--at least in our opinion--these are two further pottery styles that would have to be fitted within Courville’s 28-year period.

Parenthetically, we must reject the CoD suggestion that the early Iron Age Submycenaean and Protogeometric could span the period from 900 B.C. down to 775 B.C. 54 This would be in conflict with the archaeology of Samaria (early 9th century BC), which starts at least with Iron Age 2a, i.e., too late for the Submycenaean and Protogeometric pottery styles.

Courville’s only solution to the lack of room for the Geometric period on his chronology is to suggest that the Geometric period was not independent of Mycenaean and Archaic periods. He sought to bolster this view by reference to Demargne, and says,

51 Peter James, et al., Centuries of Darkness, Table 5:2, p. 101, citing Coldstream, Cook, Boardman, Amyx. Cf., also A.M. Snodgrass, The Dark Age of Greece, p. 122; R. M. Cook, Greek Painted Pottery, pp. 18-20. 52 James, p. 111.
54 James, p. 111.
“From the review of Demargne on this point, we can only conclude that the available evidence does not provide clear answers.” (EP, 2:279.)

Courville was referring to P. Demargne’s *The Birth of Greek Art*, pp. 270-71. Unfortunately, no quotations are supplied from the cited reference work but Courville feels entitled to conclude:

“Howver, certain scholars have entertained the concept that the Geometric Period was not a period outside the limits of the Mycenaean and Archaic period.” (EP, 2:279.)

By this Courville meant that the Geometric period fell within the scope of the Mycenaean and Archaic periods, so that it did not really take up any independent bloc of time. Contrary to Courville, however, Demargne says explicitly that archaeology has confirmed the view that there was a sequence between the Mycenaean period and the Geometric period, not a parallel:

“The position of the Geometric style between the Mycenaean and the Orientalizing periods, was perhaps the most hotly contested issue. Wilhelm Dorpfeld was alone in holding that the Orientalizing style grew directly out of Mycenaean art; he regarded Geometric as a development running parallel to these two styles, not as an intermediate stage between them. Other scholars maintained (and the excavations confirmed this view) that Geometric was an intervening, not a parallel development.”55

Here there is no question that the Geometric style is placed between Mycenaean and the later period. Demargne believed that there was indeed a clear answer—that excavations showed that Geometric was an intervening strata between Mycenaean and later Orientalizing styles—that “the excavations confirmed this view.”

Dorpfeld, Schliemann’s assistant and excavator at Troy, was cited as the principle advocate of the view that the Geometric styles were not intermediate between the Mycenaean and Archaic styles. We find Courville’s reliance on Dorpfeld to be unfortunate, in that Dorpfeld was working too early in the science of archaeology to be called in as an expert for the proper dating of Greek ceramic styles. Courville’s view is quite radical, but he believed that “further observations” would prove:

“that the so-called Geometric art had its origin in a mixture of influences, including that of the Mycenaean period (which was not 300 years in the past, but only a matter of a decade or two….” (EP, 2:280.)

Dorpfeld did not bring the Mycenaean pottery forward to agree with the date of the Geometric pottery, as Courville did. Rather, he pushed the Geometric period backward in time to meet up with the conventional date of the Mycenaean pottery, and even to surpass it in age:

“This geometrical style is very old; it existed before and next to the Mycenaean art, nor was it replaced by it.”56

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A. Furtwangler argued against Dorpfeld that a centuries long gap existed between the end of the Mycenaean pottery and the beginning of the Geometric pottery. (Most scholars would try to fill this gap in by stretching the Submycenaean forward and the Protogeometric backward.)

Velikovsky, like Courville, championed Dorpfeld’s view. He cited cases where fragments of Geometric vases have been found on some sites together with Late Mycenaean pottery. However, at most this only proves there was an overlap at the end of one period and the beginning of another. It does not prove any larger contemporaneity. Today, all archaeologists reject the view that the two pottery styles ran concurrently—which is what Courville would need in order to make his theory plausible. Velikovsky had no room at all for the Geometric style.

We will see that this radical restructuring of the Mycenaean and Geometric periods left Courville with little choice but to greatly truncate the New Kingdom dynasties of Egypt. I don’t think I would be going out on a limb in stating that no present day archaeologist—even ones favorably disposed toward alternative views—would entertain such a radical reworking of the Egyptian dynasties, nor of the Mycenaean and Geometric pottery periods, nor of the Spartan king list, as Courville has suggested it.

8. The Geometric Period.

On conventional chronology the Geometric (including proto-Geometric) period runs from about 1025 B.C. to about 700 B.C. This is roughly parallel to the latter part of Iron Age 1b to the early part of Iron Age 2c. The CoD group discussed the Geometric period with special reference to Samaria and Megiddo. They noted that virtually none of the relevant pottery material from these cities were found in stratigraphically safe contexts.

Recently, however, Nicolas Coldstream and Amihai Mazar clarified the relationship between Greek pottery and Holy Land stratigraphy. The following table is based in part on their discussion:

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58 James, *Centuries*, pp. 106-07.
### Table of Greek Pottery and Holy Land Archaeology

<table>
<thead>
<tr>
<th>Greek Pottery</th>
<th>Tel Rehov</th>
<th>Megiddo</th>
<th>Tel Abu Hawam</th>
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<td></td>
<td></td>
<td></td>
<td>4-6</td>
<td>5a, 5b, 6, 7, 8</td>
<td>Iron 2b</td>
<td>9th-8th to 732 B.C.</td>
<td>675 B.C.</td>
</tr>
<tr>
<td>MG</td>
<td>3</td>
<td>4a</td>
<td>3</td>
<td>6</td>
<td>Iron 2a</td>
<td>10th-9th century</td>
<td>c. 825</td>
<td></td>
</tr>
<tr>
<td>SPG; PG</td>
<td>5</td>
<td>5a/4b</td>
<td>3</td>
<td>2</td>
<td>9a, 9b</td>
<td>end of IA2a</td>
<td>840-830 BC</td>
<td>Uzziah’s Earthquake</td>
</tr>
<tr>
<td>PG</td>
<td>6</td>
<td>5b</td>
<td>1</td>
<td>10a, 10b</td>
<td>Iron 2a, Iron 1</td>
<td>10th century</td>
<td>879 B.C.</td>
<td></td>
</tr>
</tbody>
</table>

In the chart above we follow G. Wright in correlating Hazor 9 with Samaria 2, and under New Courville, Samaria 1 & 2 are correlated to the beginning of the Iron 2a period at the time of Omri and Ahab. Coldstream and Mazar do not specify the time of Early Geometric (900 to 850 B.C.?) but it apparently runs the length of the Protogeometric, and of course comes just before Middle Geometric. More specific correlations between the Geometric pottery and the Iron Age are educated guesses at this point, but the above general correspondences appear to be based on solid grounds.

Since writing his book on the archaeology of the land of the Bible, Mazar has changed his views on the extent of Iron Age 2a, and now believes it covers not only the 10th century but also the 9th century. Since Iron 2a has been extended downward by a century, if follows that Iron Age 2b on Mazar’s chronology would start at around 800 B.C., nearly the same time as New Courville’s start date for Iron 2b, c. 800-780 B.C.

Mazar’s “extension” is mainly a reaction to the Low Chronology. Whatever one might think of this extension, it is at least remarkable to see that a pottery stratum can be so easily extended by a hundred years by an orthodox archaeologists, and no one bat an eye. It would appear that the strata are not really all that fixed in stone, nor pegged too strictly.

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to an absolute chronology, if they can be so readily displaced. Mazar also picks and chooses which radiocarbon dating he will use to fix the strata to the B.C. time scale. It may be thought that this is an exaggeration, but sadly it is not. In responding to an article by Gilboa and Sharon, who argued for “extremely low” dates based on twenty $^{14}$C dates, Mazar rejects the results of two radiocarbon methods in favor of the results of one radiocarbon method. Why? Because the two rejected methods showed “consistently lower” dates while the one Mazar accepts shows higher dates. And why should higher dates be preferred? Here is Mazar’s answer:

“[Y]et the Groningen cross-dating of the same horizons or even of the same loci fits the conventional chronology for the Iron Age I and the ‘extended conventional chronology’ of the Iron Age IIA.”

In other words, if it supports conventional chronology or Mazar’s preferred chronology—as the Groningen method does—it is acceptable. If it does not—even if two independent methods support lower dates—it must be rejected. This is the kind of petitio principii that Low Chronologists and chronological revisionists have objected to for many years.

A Middle Geometric 2 fragment was found at the site of Tyre Stratum 3, dated by the excavator, Patricia Bikai to c. 750-725 B.C. As can be seen from the New Courville dates above, the 750 date correlates with the Middle Geometric period. Moreover, Coldstream’s correlation of an MG2 sherd with the early 8th century (Samaria 5) is also close to New Courville dates for Samaria 5 and the Middle Geometric period.

Late Geometric, Attic, and Black Figure follow Middle Geometric, and the date of the start of Late Geometric should probably be sometime near the latter part of the 8th century, say 740 B.C. The estimated New Courville dates for Attic and Black Figure are respectively c. 675 B.C. and 625 B.C.

As noted, since certain sherds are correlated with specific stratigraphic levels, and these can be roughly placed around the archaeology of Samaria and later Greek history, the notion that the entire Geometric period can be encompassed within a relatively brief period of time, as Courville argued, cannot be seriously entertained as a plausible interpretation of the archaeological evidence.

As also noted, the CoD group’s date for the length of the Protogeometric period is difficult to reconcile with the archaeology of Samaria. They say,

“As an experiment, if one placed the close of the Late Geometric period about 675 BC [New Courville agrees—VC] and adopted Davison’s figure of 100 years for the whole Geometric style, the end of the Protogeometric would fall around 775 BC. Following Popham’s estimate of a century for the Protogeometric would put the end of Submycenaean at c. 875 BC. Allowing one generation for the Submycenaean brings us to a date for the end of LHIIIC around 900 BC, nearly two centuries later than its conventional placement.”

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62 Mazar, *IEJ*, 54:1, 2004; p. 34.
64 James, p. 106.
65 James, p. 111.
On this view, the end of the Protogeometric would correlate with the time of Jeroboam 2, and with Samaria level 2, in accordance with the CoD group’s revision of the dates of Samaria’s stratigraphy. However, Protogeometric is correlated with the Iron 1 period and part of Iron 2a, and since Samaria begins with Iron 2a this puts a terminus on how late one can date the Protogeometric pottery. It cannot be much later than c. 879 B.C., when Samaria was first built. On conventional views, Samaria was built in a later archaeological strata, in Iron 2b. This is too late for Protogeometric, as can be seen in the above chart, which has Protogeometric ending during the Iron 2a period, and which also has Middle Geometric beginning at about the time the CoD group wants to date the end of Protogeometric. There seems little doubt that the archaeological numbers just don’t add up for the CoD group’s proposed redating of the Protogeometric strata.

9. The Dorian Invasion

Courville endorsed the view that southern Greece was invaded by a “northern people” called the Dorians (EP, 2:272). Greek legend had it that the return of the Heraclids took place during the reign of Tisamenos, about two generations after the Trojan War, and this return was often correlated to the “Dorian invasion.” Presumably, the descendants of Hercules—the two brothers, Kresphontes and Temenos, and a third brother represented by his sons, Eurythymes and Procles—divided the Greek Peloponnesian among themselves. Some scholars believe this “Dorian invasion” caused the collapse of Mycenaean civilization at the end of Late Helladic 3b. Nevertheless, difficulties have arisen in finding archaeological evidence for such an invasion:

“Until a few years ago it was conventional to associate this [the destruction of Mycenaean civilization] with what the ancient writers called the invasion of the Dorians, which Greek tradition held to have been the arrival of Greeks. This, however, seems to have no archaeological basis and it is now believed that the Dorians were already within Greece, and that they were Greek-speaking people...who succeeded their masters after the fall of the palaces.”

The lack of archaeological evidence for a Dorian invasion is well-known and well-documented. Even those who accept the Dorian invasion as a fact concede that the evidence is thin on the ground:

“It is now time to probe the possible causes of the catastrophic period which led to the decimation of many of the central regions and to many attendant losses....The various known factors, the destructions—especially of certain important citadels—the wholesale destruction of great tracks of mainland Greece, the consequent flight by land to remote and isolated districts or sites on the mainland, and by sea both westwards and eastwards (above all to Cyprus), all these suggest that there was an invasion, or a series of

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66 James, p. 187.
67 Mazar, Archaeology..., p. 296.
68 Wood, In Search of..., p. 214
invasions...and that these brought about a situation so desperate that there was a mass exodus of the survivors...If, however, there was an invasion, one would naturally expect the invaders to take over the country they had overrun. Of this there is no sign whatever.\textsuperscript{70}

“Almost more striking is the fact that the invaders have left no mark of themselves. Scattered around the Spartan plain there was a sufficient population to maintain one centre, at Amyklai; a few other men left their pots around on sites near the southern coast, but these were survivors of the old regime, not the victorious carriers of a new culture or even of a new barbarism.”\textsuperscript{71}

“There is no archaeological evidence for the identity of the people who destroyed the Mycenaean culture, and no positive signs of an influx of new people.”\textsuperscript{72}

Desborough thinks the invaders “withdrew,” but still posed a “continuing potential and actual menace.” Howatson & Chilvers attempt to mitigate this lack of evidence by suggesting that the Dorians were of the same culture as the Mycenaeans, and thus would not show up in the archaeological record. However, these negative arguments do not provide evidence for the supposed invasion, but rather presuppose the lack of evidence. It is also not likely that a massive invasion would go unnoticed in the archaeological record, even if carried out by the same culture, or by one that (implausibly) hovered around the outskirts. The main arguments for such an invasion are usually of a linguistic sort, \textit{i.e.}, differences in dialect among the later Greeks; and also the legendary material regarding the Return of the Heraclids.

While we might not go as far as Wood in denying any sort of invasive movement of peoples from northern Greece into southern Greece, we have to conclude that given the available evidence, it is just as easy to speak of a Dorian \textit{migration} rather than an invasion. In the former case, the Dorians simply moved into territory that had already been abandoned. Anything beyond that appears to be more of a theoretical preference than a deliverance of fact.

\textsuperscript{70} V. Desborough, \textit{The Greek Dark Ages}, pp. 21, 23.
\textsuperscript{71} W. Forrest, \textit{A History of Sparta}, p. 251.
\textsuperscript{72} Howatson & Chilvers, \textit{Oxford Companion to Classical Literature}, p. 188.