Book Review


Reviewed by Vern Crisler, 2015.

1. Introduction

In reading Rohl’s new book on the Exodus, I have to say that it does not inspire much confidence. I won’t say a lot about what Rohl gets right. Rohl is a brilliant thinker, very knowledgeable about Egyptology and archaeology, and a first class writer. I agree for the most part with his critique of the Late Bronze or Iron Age theories of the Exodus and Conquest. This is the primary value of Rohl’s many books, and I have no problem in recommending them to anyone who wants to get his or her feet wet on the subject of chronological revisionism.

However, just because Rohl may be right in his critique of modern theories of the Exodus and Conquest it does not mean his own theory of the Exodus and Conquest is correct. And even then it is not so much that Rohl’s theory is wrong. I believe it is, but I could even grant its truth for the sake of argument. The problem with Rohl, however, is his methodology. It is so bad that any crackpot can use it and come to a different conclusion from Rohl’s. This is not the sign of a good methodology.

2. Camel Races

For instance, whenever Rohl’s theory runs up against biblical data that contradict it, he trashes the biblical data. Even though he calls himself a biblical maximalist vis-à-vis what are called biblical minimalists, he does not accept the relevant truthfulness of the Bible.

He apparently thinks that by being a maximalist it is more acceptable than being a minimalist like Tom Thompson. Nevertheless, Thompson sets a pretty low standard, so it is not really saying much for Rohl’s own position that he thinks it is an improvement over Thompson’s. Well, yes, but just about anything would be an improvement over Thompson’s views.

Rohl accepts the validity of the discredited documentary hypothesis, the notion that the Old Testament is a mere collection of disparate source material and alleged “oral” traditions. What is worse, he appeals to this pastiche explanation when the Bible threatens to contradict some aspect of his chronological theory.

I repeat, this is not a good methodology. Example: Rohl claims that the journey to Marah from his chosen site of the biblical crossing at the Red Sea is about sixty five miles. He further claims that for this to be true, the Israelites had to make twenty two miles per day.\(^1\) This is an absurd figure and Rohl admits that the reality of so many people moving across the desert would have been more along the lines of six to eight miles per day.

He says, however, that twenty two miles could be done with camels. Now, of course, the Israelites did not have camels, at least not for everybody, so how does Rohl solve this? Simple. He says that the Exodus narrative was a mere jumble of divergent sources.

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\(^1\) Rohl, David M; *Exodus: Myth or History?* St. Louis, MN: Thinking Man Media, 2015, p. 195; hereafter, *Exodus*. 

“[T]he evidence…points to several hands at work in the compiling of the Old Testament narratives.”

In this view the Exodus narrative as told in the Bible has “additions” put into the text at a much later time, i.e., during a time when camels were used extensively for travel. By recognizing this one can clear up ostensible exaggerations, falsehoods, or contradictions in the Bible, such as impossibly long distances between two points.

So instead of admitting that his location of the Red Sea crossing, or the Exodus route, might not be correct, Rohl conveniently blames a later “redactor” for using camel distances to describe the Exodus journey.

Emmanuel Anati, the archaeologist who has studied Mount Karkom for many years, has pointed out that if you don’t have the correct location of Mount Sinai, none of the Exodus itinerary is going to make sense:

“Whoever begins an analysis of the topography of the itinerary of Exodus with the preconceived idea that Mount Sinai should be in the region of St Catherine or in any other area in the south of the Sinai Peninsula will find it impossible to give a geographical sense to the sequence of the stations of Exodus.”

3. Ramses 2 versus Merneptah

If disregard for the biblical text wasn’t bad enough, Rohl is equally ham-fisted in his handling of the archaeological data when it contradicts his theory. He trashes both the archaeology and the work of archaeologists who stand in the way of his theory. Here are some examples.

Rohl, who never finished his degree in Egyptology, presumes to correct Egyptologist Frank Yurco’s ascription of some of the reliefs on the walls at Karnak to Merneptah. He says instead they belong to Ramses 2, the Shishak of the Bible in Rohl’s theory. Rohl did the same thing in his earlier book, Pharaohs and Kings:

“[F]or many years the [Karnak] wall reliefs were misattributed to Rameses II as well. However, the latest work on them, by Peter Brand, has confirmed Frank Yurco’s earlier assessment that they were certainly carved for Merneptah…”

This provides an example of why even well-informed revisionists should not take it upon themselves to correct the work of Egyptologists

4. Lost and Found

Rohl also speaks rather quixotically of the “Lost Town of Ai.” In fact Ai is not lost but has been known for a long time as et-Tell. However, since the archaeology of et-Tell contradicts Rohl’s chronological theories, he must look elsewhere for the city of Ai. As Rohl admits, “But unfortunately the

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2 Rohl, Exodus, p. 199.
3 Rohl, Exodus, p. 201. At a later point Rohl says that “biblical sources are unfortunately in conflict with each other.” (p. 237.)
5 Rohl, Exodus, p. 30.
7 Dodson, Aidan; Poisoned Legacy: The Fall of the Nineteenth Egyptian Dynasty, Cairo: American Univ. in Cairo, 2010, p. 18.
Rohl admits that the identification of Khirbet Nisya with Ai “remains problematical.”

So that leaves one more to make up the “several” scholars—Bryant Wood. Wood is an archaeologist who excavated at Khirbet el-Makatir and found a “small settlement” and thinks this is the site of Ai. However, Livingston disagreed with this identification, pointing out that it is not located in the right place. Rohl agrees with Wood, however.

Ziony Zevit and Anson Rainey have criticized attempts to move Ai or Bethel from their currently accepted locations. The main problem is that the attempt to move Ai to some other location means one has also to move the location of Bethel. In contemporary terms it means that if you’re going to move Minneapolis, you’ve also got to move St. Paul.

Zevit and Rainey have pointed out the difficulties of trying to relocate Ai and Bethel, and readers are invited to review their arguments.

5. Whither Bethel?

Having relocated Ai, Rohl argues that Bethel is not Beitin, which is the traditional archaeological site for Bethel. He argues instead for el-Bireh. The church historian Eusebius, in his Onomasticon, said Bethel was at the twelfth Roman milestone from Jerusalem. To this Rohl says, “Yet Beitin lay near to the fourteenth Roman milestone, so it was too far north to be ancient Bethel.”

It is not clear where Rohl got the idea that Beitin was fourteen miles rather than twelve miles, since he doesn’t provide a source. Measurement, however, depends on the starting point. Rohl says his starting point for the measurement of the distance between Jerusalem and el-Bireh was at the main street or “Cardo” in Jerusalem.

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8 Rohl, Exodus, p. 283.

9 Rohl, Exodus, p. 284.


11 Rohl, Exodus, p. 284.
However, when the starting point is at a gate rather than a center point, such as the Cardo, distances make more sense. As Rainey says with respect to an earlier mistaken attempt to measure distances: “Livingston and Bimson have evidently accepted the specious argument that one may measure these distances from the Muristan, a square in the midst of the Old City of Jerusalem. Thus they claim to pull the twelve-mile marker back southward to the town of el-Bireh, where they want to place Bethel.”

Further, “It just so happens that at about twelve Roman miles north of the Damascus Gate (already past the town of el-Bireh) there is a turnoff to the northeast and that road leads one directly to a town called today Beitin.” (My emphasis.)

So like Livingston and Bimson, Rohl eludes the force of Eusebius’s words by changing the starting point of the twelve mile measurement from the gate to somewhere internal to the city. And by changing the starting point of the measurement two miles back from the gate of the city Rohl gets his alleged fourteen miles between Jerusalem and Beitin.

In addition, Rainey points out that it is specious to move the name Bethel from the impressive site at Beitin to the much less impressive village site at el-Bireh. He says, “If Bethel is not Beitin, then there is no Historical Geography of the Bible.”

Rainey also points out that the letter “n” replaces the letter “l” in Arabic, so Beitin was originally Beitil, which is equivalent to Bethel. (Other examples: Ishmael to Isma’in, and Jezreel to Zer’in.)

6. What’s in a Name?

Following Martin Noth, Rohl claims that Ai means “ruin.” However, even though the name Ai resembles the Hebrew word for ruin, Zevit pointed out that the name Ai has no etymological link to the Hebrew word for ruin. Based on similar Arab words, Ai was probably a topological reference to the height of the city.

7. Double trouble at Kadesh-barnea

As another glaring example of Rohl’s poor methodology, he disputes the view that the Israelites during their Wandering phase stayed at the traditional archaeological location of Kadesh-barnea. The location is either at the site of Ain el-Kudeirat in the Negev, or at the nearby site of Ain el-Kadeis. Rohl claims instead that the location of Kadesh-barnea is one of the “thorniest issues” in regard to the wilderness wanderings of the Israelites.

As far as I can tell, however, it is only a thorny issue for Rohl and the reason is that there isn’t a shred of pottery indica at Kadesh-barnea that corresponds to Rohl’s MB IIA period for the Exodus, or the MB IIB period for the Conquest. So in the face of archaeological investigations that place Kadesh-barnea in this region, what does Rohl say? Well, he says, “this makes no sense to me….”

One might point out that regardless of whether it satisfies Rohl’s notion of sense, this is where archaeologists place it, and Rohl needs to deal with the facts on the ground. Instead he appeals to a non-archaeologist (and biblical skeptic) named Walter Mattfeld

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to redraw Judah’s “fourth boundary location after Kadesh.” And what this does for Rohl is to “rule out completely the identification of Ain el-Kudeirat with Kadesh…..”\footnote{Rohl, Exodus, p. 246.}

It is pretty obvious that no responsible archaeologists is going to accept the views of a non-archaeologist, so Rohl’s reliance on a non-archaeologist as an authority on archaeology or topology is another example of his shoddy methodology.

Nevertheless, in order to see just how inadequate Rohl’s methodology is, we can explore the reasons that are provided by Rohl for not thinking Kadesh-barnea is where archaeologists place it.

First, he thinks it doesn’t fit with the Genesis 14 campaign of the five Mesopotamian kings, who marched down the King’s Highway along the Dead Sea before “wheeling around” to go through Kadesh, then go back to the Dead Sea to fight the kings of that region. “What would have possessed them to trek for days on end all the way through the Desert of Paran to reach Ain el-Kudeirat…before returning back to the Araba and the Dead Sea? This is plainly nonsense.”\footnote{Rohl, Exodus, p. 243.}

One can speculate all day long about the motivations of ancient people who are no longer alive to contradict one’s speculations. However, it would not have taken an army “days on end” to go to Kadesh-barnea then down to the Gulf of Aqaba.

Second, the fact is, Kadesh-barnea is in a central location in the Negev and the purpose of the Mesopotamian invasion was to send a message. It would be something similar to (say) General Sherman’s march through Atlanta, then to Savannah, then up through South Carolina. The purpose was to destabilize and demoralize the enemy.

So also the Mesopotamian kings had as their goal the “pacification” of the south country. The defeat of Kadesh-barnea in the south country would have furthered this goal. Of course, when there is no evidence one way or another, one can speculate to one’s heart’s content about the motives of ancient kings.

Second, Rohl thinks the biblical mention of four locations between Kadesh-barnea and the Torrent of Egypt (Wadi el-Arish) demonstrates that Ain el-Kudeirat could not be Kadesh-barnea.\footnote{Rohl, Exodus, p. 244.} Rohl says it is only sixteen miles between Kudeirat and the Arish, whereas if Kadesh-barnea is located at Petra, the distance is 75 miles.

However, no argument is provided for any of this. It is likely that the four locations are mentioned between Kadesh-barnea and the el-Arish because they were part of a “map” and the sacred writer is after all giving us the boundaries of Judah’s allotment.\footnote{Book of Joshua, 15:104.}

Third, Rohl wants to re-identify the biblical Ascent of Scorpions. At this point, I’ve grown weary of Rohl’s re-identifications. Suffice it to say that Israeli topologists recognize Nagb es-Sfar (or Zafir) as the site of Scorpion Pass, and there is no good reason to reject this.

Fourth, Rohl makes a big to-do about the fact that the order of the wilderness
wanderings in the *Book of Numbers* contradicts the itinerary given in the *Book of Deuteronomy*. However, it is likely that the *Numbers* list has not been transmitted in its original order in the case of the sites of Bene Jaakan and Moseroth.

The actual order per the *Book of Deuteronomy* was from Hashmonah to Bene Jaakan, then to Moseroth. The *Book of Numbers*, however, has Moseroth first, then Bene Jaakan. This is probably a transmission error in the *Numbers* list (an error made by copyists, easy to do in a list) and there is no need on the basis of this faulty order to invent a “Lesser Kadesh” in contradistinction to a “Greater Kadesh,” nor is there any need to add anything to the text to clear things up as Rohl has done.

Nevertheless, Rohl thinks there were two Kadeshes, one at Ain el-Kadeis or Ain el Kudeirat, and the other at Petra. “So it was near Kadesh/Petra that Miriam died…and where Aaron died….However, later biblical redactors/editors confused the two Kadesh locations…”

Petra, however, was not settled until about the fourth century B.C. In terms of its archaeology, it has indicia from the Upper Paleolithic, Neolithic, Iron Age (Edomite ware), and from Hellenistic and later times. So if two million Israelites, or even thousands in Rohl’s view, stayed at or around Petra for 38 years, why is there no pottery indicia to document their stay?

In fact, the only significant pottery found at traditional Kadesh-barnea was from Albright’s MB1. There is no MB IIB pottery at all at either traditional Kadesh-barnea or Rohl’s Petra so it is difficult to understand how this helps his chronology. It rather seems to provide a counter-instance to it.

This does not mean that there could not be two different locations by the name of Kadesh. After all, there is another famous Kadesh associated with Ramses 2 and his battle with the Hittites. This Kadesh was too far north to be relevant, but it may be the case that that the biblical descriptions of Kadesh in the Wilderness of Zin, and the Kadesh in the Wilderness of Paran are really two ways of distinguishing two sites that were very close together.

The Bible might be saying that the northern site is in the Wilderness of Zin and the southern site is in the Wilderness of Paran. I conjecture that this may have been the Bible’s way of distinguishing the sites of Ain el-Kudeirat and Ain el-Kadeis respectively. The former was located within the southern boundary of the Wilderness of Zin while the latter was located within the northern boundary of the Wilderness of Paran.

Others have argued that the Wilderness of Zin and the Wilderness of Paran overlapped, which is why Kadesh is described in two different ways. Nevertheless, I think it might be more likely that the use of two descriptions

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17 Compare *Numbers* 33:38 and *Deuteronomy* 10:6-7.
to name Kadesh may be an indicator that they were different sites but too close to one another to avoid different descriptions.

8. Mount Hor

As part of his Kadesh = Petra theory, Rohl places Mount Hor at the location of Gebel Harun. If, however, Kadesh is located at the traditional site in the Negev, then in my opinion, Mt. Hor is more likely to be at the site of Bir Rekhme.

Bir Rekhme is situated near the modern town of Yerukham. This location of Mount Hor was also the view of Yohanan Aharoni:

“The fact that Mount Hor is mentioned as being on the borders of Edom accords with its being not too far from Kadesh-barnea, which also is described as a city on the uttermost border of Edom (Num. xx. 17).”

9. Summary Judgment

All of these examples illustrate Rohl’s less than inspiring methodology. There is simply no reason to reclassify the Karnak reliefs, nor to relocate Ai or Bethel, nor biblical Kadesh-barnea from the Ain el-Kudeirat or Ain el-Kadeis area. Tampering with archaeology in this way is not based on independent evidence but only on the needs of Rohl’s chronological theory. Rohl does not adjust his theory to conform to the evidence, but rather forces the evidence to fit his theory. That is the definition of bad methodology.

10. Conquest and Confusion

Rohl is at his methodological worst in his discussion of the Conquest and its relation to Middle Bronze Age archaeology. I have to admit that Rohl’s discussion of Middle Bronze Age archaeology in general gives me a headache. It is because his discussion seems to be both confused and confusing.

In his earlier book *Pharaohs and Kings* Rohl quoted Piotr Bienkowski to the effect that Jericho was destroyed at the end of the Middle Bronze Age. “Jericho was destroyed,” said Bienkowski, “at the end of the MBA, probably by enemy action and possibly through a failure of the fortification system….The reason for the destruction of Jericho is unknown.”

Rohl then comments

“Bienkowski’s last sentence no longer applies thanks to the work of Dr. John Bimson . . . and the revised archaeological date for the destruction of MBA Jericho provided by [Rohl’s] New Chronology. Both have determined that Middle Bronze Age Jericho was attacked and destroyed by invading Israelites.”

It seems that Rohl believes along with Bimson that the widespread destruction of cities at the end of the Middle Bronze Age should be assigned to the “Israelite entry into Canaan” that took place at the time of Joshua’s conquest.

Now, in standard archaeological terminology, the end of the Middle Bronze Age is correlated with what is called the Middle Bronze IIC pottery horizon. So the end of the Middle Bronze Age is at the end of MB2c in standard terminology. Therefore, logically, Rohl must place the Conquest at the end of MB2c.

23 Yohanan Aharoni, in Rothenberg, *God’s Wilderness*, p. 141.
Here’s the confusing part. Rohl makes the following claim about the Exodus and Conquest:

“The New Chronology postulates that the Exodus took place during the 13th Dynasty (that is at the end of the archaeological phase known as MB II). The Conquest would then have taken place during the MB IIIB.”

So here Rohl places the Exodus at the end of MB2a, and the Conquest occurs 40 years later during the MB2b phase. What is going on here? The Conquest is either at the end of the Middle Bronze Age (end of MB2c), or it is 40 years into MB2b. How are these two views to be reconciled? The Conquest was either at the end of the Middle Bronze Age or it wasn’t.

11. A Single MB 2?

It might be thought that Rohl was using the term MB2b to cover both MB2b and MB2c. However, this does not clear up the problem because (a) Rohl claims that the Exodus is at the end of MB2a, which means the Conquest cannot be far from the beginning of MB2b, i.e., not at its end; and (b) Rohl says that the Conquest is “well before the foundation of the Greater Hyksos 15th Dynasty.”

The Hyksos dynasty is assigned to MB2c by archaeologists. So Rohl is here saying that the Conquest took place before MB2c (the Hyksos strata). So, if the Conquest took place before the time of the Hyksos, i.e., before the MB2c strata, how could the Conquest have been at the end of the Middle Bronze Age?

In order to be at the end of the Middle Bronze Age, the Conquest would have to be at the end of MB2c. If so, then the Conquest would be after the time of the Hyksos, not “well before” the 15th Dynasty Hyksos.

If Rohl was using MB2b to refer to both MB2b and MB2c, and he placed the Conquest before the Hyksos 15th dynasty, then he would have to bring the Conquest to somewhere near the middle of MB2b, not at its end. This is because a single MB2b terminology would still refer to the same physical reality captured by the b and c terminology.

Hence, when an archaeologist uses MB2b to refer to both MB2b and MB2c, he wants it to refer to earlier and later, so that MB2b (earlier) refers to the old MB2b, and MB2b (later) refers to the old MB2c. So there is no change in the physical reality. There was a gradual evolution of pottery styles throughout the Middle Bronze Age. It is that gradualism that has led some to want to speak only of one period divided into an earlier and later.

In addition, the lack of any catastrophic pottery style changes means that the culture of the Middle Bronze Age never changed. Even the destructions at the end of the Middle Bronze Age, presumably brought about by the Egyptian kings of the Late Bronze Age, did not significantly change the culture of the Middle Bronze Age.

This is a major problem for those who would place the Exodus and Conquest in the Middle Bronze Age. It is the same problem facing the Late Bronze Age Exodus and Conquest theory.

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25 Rohl, P&K, p. 309.
26 Rohl, P&K, p. 309.
The problem is that even with evidence of destruction of towns during various parts of the Middle Bronze Age or Late Bronze Age, the succeeding culture grew out of the preceding culture. Of course, this is the complete opposite of what happened with regard to the Exodus and Conquest as described in the Bible.

Rohl himself criticizes the Late Bronze and Iron Age Exodus theory for this very reason, i.e., for ignoring the indigenous development of culture during this time, a development at odds with the biblical account of the Exodus and Conquest. So this criticism would apply equally to Rohl’s Middle Bronze Age theory of the Exodus and Conquest.

Returning to Rohl’s claims about the Hyksos dynasty, if Rohl placed the Conquest before the Hyksos 15th dynasty, then it must be at least in the middle of Rohl’s MB2b, not at its end, for even with a single MB2b terminology, the Hyksos dynasty must be part of MB2b-later.

And yet, if the Conquest was not at the end of Rohl’s MB2b, then it would not be at the end of the Middle Bronze Age. That would mean Rohl’s reliance on Bienkowski or Bimson is irrelevant since they correlated the destructions of cities to the end of the Middle Bronze Age. Let us consider the following chart:

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<tr>
<th>Chronology, Location</th>
<th>Archaeological Phase</th>
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<tbody>
<tr>
<td>Rohl Terminology</td>
<td>MB IIB (earlier)</td>
<td>MB IIB (later)</td>
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<tr>
<td>Albright Terminology</td>
<td>MB2b</td>
<td>MB2c</td>
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<td>Egyptian Dynasty</td>
<td>13th</td>
<td>15th, Hyksos</td>
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<td>Bible History</td>
<td>Conquest? (Rohl)</td>
<td>Conquest? (Bimson)</td>
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<td>City</td>
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<td>City</td>
<td>Jericho City 4</td>
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<td>City</td>
<td>Hazor 16</td>
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<td>City</td>
<td>Shechem 15</td>
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As you can see Rohl’s single MB IIB terminology, with its earlier and later periods, would correspond with Albright’s terminology of MB2b and MB2c respectively. The 15th Dynasty is the Greater Hyksos Dynasty and it is archaeologically placed in MB2c. In Rohl’s terminology, the Hyksos 15th Dynasty would have to be his MB IIB (later) phase.

So if the Conquest is very early on in Rohl’s MB IIB (earlier), before the Hyksos 15th dynasty, it cannot have been at the end of the Middle Bronze Age. And yet that is what we were led to believe by Rohl when he made his initial claim that the destruction of Jericho at the end of the Middle Bronze Age was the same Jericho destroyed by Joshua.

12. Clearing up the Muddle?

Has Rohl cleared up this problem in his new book? No, he has not in the slightest. In fact it is worse than it was before. Let us examine his discussion to see what I mean. He still has the “invasion” of the Israelites in MB IIB. He then points to the destruction of Jericho City 4 as the city that was sacked by the Israelites.

Kenyon said of this level: “This final stage of the Jericho Middle Bronze Age town was destroyed by a violent fire….Since, as will be seen, the [burnt] contents seem to go down to the end of

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28 Rohl, Exodus, pp. 265ff.

29 Rohl, Exodus, pp. 274, 276.
the Middle Bronze Age and not beyond, it is probable that this destruction is connected with the disturbances caused by the expulsion of the Hyksos from Egypt.”

Jericho 4 is assigned to the final stage of Jericho, so the destruction of this city was at the end of the Middle Bronze Age. In addition, the expulsion of the Hyksos was at the end of the Middle Bronze Age. So once again, since Rohl has the Conquest prior to the Hyksos period, the Conquest could not correlate with the city destructions at the end of the Middle Bronze Age, including the destruction of Jericho 4.

Also, Rohl says Jericho 4 was “apparently” destroyed by an earthquake but he did not provide a source for this claim. I did not see anything in Kenyon’s discussion that ascribes the destruction to an earthquake, although I may have missed it.

Let us look further. In P&K Rohl reported the destruction of Hazor at the end of stratum 16: “This stratum XVI marks the end of the MBA city and, in the New Chronology, would be the city destroyed by Joshua.” He sums up: “The city destroyed by Joshua’s army was MB IIB Hazor, burnt at the end of stratum XVI.”

There is a problem with this. According to Yiguel Yadin, the excavator of Hazor, Stratum 16 is the last stratum of the MB2c pottery horizon. As can be seen in the chart above, this would be at the end of the Middle Bronze Age, or Rohl’s MB2b-later.

And yet if Stratum 16 was destroyed by Joshua, then the Conquest could not have been before the Hyksos 15th dynasty but only after it. This is because the destruction of Stratum 16 is also the destruction at the end of MB2c, and the end of MB2c correlates with the end of the Hyksos era. So if Rohl believes Hazor 16 was destroyed by Joshua, he’ll need to place the Conquest after the Greater Hyksos dynasty, not before it.

13. The Temple of Doom

The situation is muddled further with the problem of Migdol Shechem. In P&K Rohl identified this fortress-temple with the time of Abimelech. It is the “Temple of Baal-Berith which is now recognized as the MB IIB temple.” I agree that this is the Temple destroyed by Abimelech. However, the destruction of this temple was assigned by the excavators of Shechem to the end of the MB2c (stratum 15). In fact the whole city was destroyed at this time. So the destruction would have to be MB IIB-later in Rohl’s terminology.

In his new book, Rohl no longer discusses the archaeology of the Migdol Temple. He mentions that the Temple was burned during the time of Abimelech but doesn’t say anything

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31 Rohl, P&K, pp. 315, 316, 317.

32 Rohl, P&K, p. 323.

about what archaeological strata saw its destruction.\textsuperscript{34}

If you look at the chart above, you can see the problem. On Rohl’s original view, the destruction of Shechem by Abimelech occurs at the end of the Middle Bronze Age. As we’ve seen, however, Rohl in following Bimson believed this was the phase that saw the invasion of the Israelites. So Rohl has the destruction of Abimelech’s Migdol Temple and Joshua’s Conquest occurring in the same archaeological stratum!

It was in fact this faulty view of Shechem that led me to criticize Rohl on the New Chronology list, a cyber hangout where Rohl’s acolytes get together to discuss his views. I did not receive a response from Rohl nor much of a response from anyone else. Nevertheless, a few months later, John Bimson raised the same issues, though much more thoroughly than I did.\textsuperscript{35}

\textsuperscript{34} Rohl, \textit{Exodus}, p. 368.

\textsuperscript{35} My criticism ran from October 30, 2002, message 16087 through November 11, 2002 message 16258. Bimson brought up the subject on May 18, 2003 message 17200 and Rohl responded on July 23, 2003 message 17459.

Apparently, Rohl finally realized that something was wrong and he now includes a chapter in his new Exodus book entitled “Conquest – IIB or not IIB.”

\textbf{14. A Rose by Any Other Name}

I ask again, has Rohl cleared up the problem with his new chapter? He says that “many scholars” do not hold to the idea of a division between MB2b and c, but want to use just the MB2b terminology.

“There is no reason to divide the latest pottery development into two phases (Albright’s MB IIB and IIC or MB II and III) since we observe only a gradual evolution of the same basic pottery styles. Kathleen Kenyon herself did not recognize a division and treated it as one period (her MB II). As a result, this alternative view proposes that we accept an MB IIA, followed by MB IIB (early) and MB IIB (late) with no distinct boundary in MB IIB.”\textsuperscript{36}

As far as I can tell the “many scholars” Rohl mentioned are mainly Aharon Kempinski and Piotr Bienkowski. In fact, archaeologists to a great extent still use either Kenyon or Albright’s terminology. Kenyon’s terminology seems simpler, MB I, MB II, and MB III, although my preference would be to use non-Latin numbering: MB 1, MB 2, and MB 3.

However, Albright’s MB2a, MB2b, and MB2c is in widespread use. It should also be noted that the current Italian excavators use Sultan subdivision (after the site name Tell es-Sultan). Nevertheless, they still correlate these subdivisions to both Kenyon & Albright’s terminology.\textsuperscript{37}

For what it is worth, Wikipedia uses Albright’s MB2a, MB2b, and MB2c terminology under its entries for “Bronze Age” and “Biblical Archaeology.”\textsuperscript{38}

In any case, the problem is not terminological. Even Kempinski and Bienkowski still recognize a physical subdivision of the strata, i.e., earlier and later pottery indicia, and Rohl

\textsuperscript{36} Rohl, \textit{Exodus}, p. 293.


\textsuperscript{38} Accessed, 11/22/2015.
seems to accept this. Indeed, if there was a “gradual evolution” of pottery styles it means there was a change from an earlier style to a later style. It is this change that is picked up in the terminology of MB2b and MB2c.

I have no quarrel with anyone who wants to use the single MB2b terminology, in which the pottery indicia is divided into earlier and later. There certainly is pottery continuity but it is not a static continuity, so the use of earlier and later, or the use of b and c, are each capable of representing the facts on the ground.

15. Mazar’s View

Amihai Mazar wrote the standard text on the archaeology of the Holy Land and he uses Albright’s terminology of MB2a, b, & c. He relates that Kenyon studied the MB2b-c tombs at Jericho and divided them into five succeeding groups. The pottery assemblages showed “gradual changes,” were “slow,” and there was “much overlapping.”

He points out that Groups 1-2 are different from Groups 4-5, so that “there is evidence for the existence of earlier and later assemblages in this period.” Mazar concludes: “It appears to me that a general division of the entire MB II period into three phases (A, B, C) is well documented on the basis of stratigraphy, pottery typology, and development of other artifacts.”

Summing up then, we can say that yes, MB2b and MB2c show gradual development and it is difficult to tell when one ends and the other begins, but there is still a pattern of earlier and later that can be discerned by trained archaeologists.

16. More Entanglement

Now let us return to our previous question. I ask a third time, has Rohl untangled the confusions of his earlier discussion in Pharaohs and Kings? Has he responded to his critics? He says:

“[W]hen books refer to ‘the end of the Middle Bronze Age’ this tends to be based on the old idea that MB IIB came to an end with the destruction of Jericho, which Albright and Kenyon erroneously attributed to the Egyptian army of Ahmose . . . after the expulsion of the Hyksos….What we have here, then, is a dangerous hangover from the Albright and Kenyon era, when an arbitrary link between the start of the 18th Dynasty and the end of the Middle Bronze Age was made, without any historical foundation.”

We point out again that it is presumptuous for even a well-informed amateur to take it upon himself to re-assign the destruction level of a city such as Jericho. Archaeologists study the facts on the ground and report them, and it is not the place of a chronological revisionist to “correct” the work of archaeologists.

Rohl further says, “The destruction of Jericho City IV (and other cities of the Conquest, including his [Wood’s] Ai) did not take place towards the end of LB I but sometime in the MB IIB.”

So, again, Rohl thinks he has the professional authority to correct archaeologists, not only with respect to the timing of the destruction of Jericho, but also with respect to the “other cities” that were destroyed at the same time.

39 Mazar, pp. 193, 195.

40 Rohl, Exodus, p. 294.
41 Rohl, Exodus, p. 298; emphasis added.
Bienkowski, in agreement with prior excavators, dated the destruction of Jericho 4 to the end of the Middle Bronze Age. On Albright’s terminology, this would be at the end of MB2c. In Bienkowski’s terminology, it would MB2b-later.

Rohl, however, disagrees: “I would go even further and argue that Jericho City IV was destroyed not close to the end of MB IIB but long before the end of the Middle Bronze Age.”

So not content to correct archaeologists in general, Rohl takes it upon himself to correct Bienkowski in particular. This is certainly what the British would call cheek.

17. Rohl’s Solution?

Rohl believes Jericho 4 was destroyed by the Israelites at the time of the Conquest, and he believes the Conquest took place in an earlier part of MB IIB, not in the last part. It was “long before” the end of the Middle Bronze Age. In addition, “Jericho and the other cities of the Conquest did not fall at the end of the Middle Bronze Age but some considerable time earlier.”

If we use standard archaeological terminology, Rohl is wanting to place the Conquest at the end of Albright’s MB2b. However, since Rohl places the Exodus near the end of MB2a, I think Rohl would actually have to place the Conquest closer to the beginning of Albright’s MB2b, i.e., 40 years after the Exodus, or 40 years after the end of MB2a.

Rohl sums up his theory and in the process takes a swipe at the work of two of the most accomplished archaeologists of the twentieth century:

“In effect, the Middle Bronze Age IIB continued long after the fall of Jericho and Jericho’s demise did not mark the end of this archaeological period as has so often been claimed. Kenyon and Albright’s now discredited view [sic] that Jericho fell at the hands of Ahmose’s Egyptian army did much to incorrectly establish the end of MB IIB at the fall of Jericho, along with the contemporary destruction wave across the whole region.”

Since Kenyon and Albright have “now” only been discredited by Rohl’s speculations, I think that for the time being their reputations are safe.

Unfortunately for Rohl’s re-dating, there is no archaeological evidence of widespread destruction of cities at the transition between MB2b and MB2c (or his MB IIB, earlier and later). So now Rohl is attempting to re-assign all of the destructions at the end of the Middle Bronze Age to an earlier time.

The defective nature of Rohl’s methodology is seen clearly at this juncture. Rohl does not have an advanced degree in archaeology, but is nevertheless now informing the professional archaeology community that they got it wrong. They have mistakenly correlated the widespread city destructions in the Holy Land to the end of the Middle Bronze Age. Instead, they should have placed these destructions in an earlier part of the Middle Bronze Age.

Rohl seems not to have realized that by re-assigning the destructions at the end of the Middle Bronze Age to an earlier point in the Middle Bronze Age, he has also re-assigned the destruction of the Migdol Temple at Shechem which he

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42 Rohl, Exodus, p. 299.
43 Rohl, Exodus, p. 306.
44 Rohl, Exodus, p. 312.
previously thought was destroyed during the days of Abimelech. With Rohl’s revised archaeology, there is now no longer a Temple and City that can be associated with the destruction wrought by Abimelech.

The only way Rohl can preserve his views on Shechem is to keep the destruction of the Migdol Temple at the end of the Middle Bronze Age while placing the destruction of the other cities during the middle of the Middle Bronze Age. Such a procedure would, of course, be entirely arbitrary, but it would be interesting to see what criterion Rohl would use to separate some MB2c destructions from others.

The answer to our question at long last is that Rohl may have cleared up some of the confusions of his earlier discussion in Pharaohs and Kings, but he has done so only at the cost of discrediting his theory entirely.

18. Chaos in the Middle Bronze Age

So what gives Rohl the chutzpah to go contra mundum against the whole of the archaeological profession, including Bienkowski? What is the basis for such cheek?

The short answer is he re-assigns the destruction of Jericho 4, and therefore all the other cities, on the basis of a single scarab. I will return to this in a moment.

The long answer is that Rohl rearranges the Hyksos dynasties to his liking. This is another area in which Rohl’s discussion gives me a headache. Why is he doing this? Why choose a different arrangement of the Hyksos dynasties from the generally accepted arrangement? Doesn’t it just add more confusion to the whole period in question?

The reason is first, that Rohl wants to get the Hyksos pharaoh Sheshi away from the 15th dynasty, and as far away from the end of the Middle Bronze Age as possible. Second, Rohl wants to identify Sheshi with biblical Sheshai, one of the three Anakim brothers who ruled at Hebron, and who were defeated at the time of the Conquest.

How does Rohl account for the fact that biblical Sheshai was a ruler of Hebron while Sheshi presumably ruled at Avaris in northern Egypt?

He claims that biblical Sheshai fled after the Conquest and took up residence in the Philistine lowlands, then later went to Sharuhen, then to Avaris in Egypt. He bases this on the Book of Joshua 11:21-22. However, all this verse says is that the Israelites destroyed the giants, or Anakim, in the mountainous areas, and the only ones who remained were in the cities of the Philistines. It doesn’t say anything at all about Sheshai.

One can only conclude that Rohl’s just-so story regarding Sheshai’s supposed flight and subsequent reign as a Hyksos king is merely historical gossamer.

19. Sharuhen and the Giants

Rohl follows Aharon Kempinksi in correlating Tell el-Ajjul with biblical Sharuhen. The latter city is where the Hyksos fled in order to escape Ahmose, the 18th Dynasty king. Rohl calls in Claire Epstein for support of the view that scarabs of Sheshi were found in City 3 of Ajjul. Kempinski, however,

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45 Rohl, Exodus, p. 299.
46 Rohl, Exodus, p. 308.
47 Rohl, Exodus, p. 309.
said the Sheshi scarabs were found in building deposits for City II, not in City III. Epstein was writing in the late 1950s whereas Kempinski was writing in the 1990s and his work would normally have priority over an earlier work.

In order to shore up Epstein’s early views, Rohl relied on Bob Porter, a Rohl-supporter. While Porter is well-informed on archaeological issues and carbon dating, Rohl should not have relied on him vis-à-vis Kempinksi. In addition, Porter himself based his views on Petrie’s reports, which is not a good idea since Petrie was writing too early in the history of archaeology to be authoritative on how strata are to be interpreted.

It is entirely possible that Rohl and Porter are correct in their placement of the Sheshi scarabs. What I am objecting to here rather is Rohl’s poor methodology in preferring an older archaeological report to a more recent one, or by relying on one of his own supporters to prove a point.

We should also note that Tell el-Ajjul might not be Sharuhen but could be ancient Gaza, one of the cities of the Conquest. This city was given to the tribe of Judah, but later turned into a Philistine city. Current researchers have located the changing sites of Gaza during its existence in the Early, Middle, and Late Bronze Ages:

“Tell Es-Sakan in the Early Bronze Age, Tell Al-Ajjul in the Mid-Bronze Age and finally the city of Gaza, which was built in the Late Bronze Age and still stands. Tell Es-Sakan, five kilometres [3 miles] south of present-day Gaza City, began as an Egyptian fortress built in Canaanite territory during the Early Bronze Age I.”

The original excavator, Petrie, also adopted the view that Gaza was located at Tell Al-Ajjul.

As far as the location of Sharuhen is concerned, Anson Rainey believes it is Tel Heror (or Haror), although in our opinion Tel Haror might instead be biblical Gerar. Yael Yisraeli identified Sharuhen with Tell El-Far’ah South. Perhaps the city has not yet been found or has simply not been recognized.

The bottom line is that there is no one definitive viewpoint on the location of Sharuhen, nor what city is represented by Tell Al-Ajjul. In other words, the archaeologists are in disagreement with one another on the site location. So it is not part of a sound methodology to base any far-reaching chronological conclusions on such an ambiguously identified site.

20. The Short Form

Now, back to the “short” answer—Rohl’s single scarab. (At least this was a “short” answer when I first started.) Most archaeologists would caution that using scarabs for dating purposes is not free from danger. The reason is that they were popular items, used as heirlooms. This means they were kept as keepsakes over long periods of time and hence show up in widely dispersed archaeological periods.


In addition, the wide dispersion of scarabs cannot automatically be
evidence for widespread rule. Gardiner says: “[T]he wide distribution of such
easily portable and marketable objects as scarabs is worthless as evidence [of]
the type of rule involved].”

So the problem of “posthumous veneration” means that one must be
cautious in using scarabs for dating purposes, as well as for determining the
nature and extent of a king’s rule.

Isolated scarabs can provide a terminus post quem date, a cannot-be-earlier-
than date. For instance, the presence of a scarab in a tomb tells us the burial
could not have been earlier than the time of the king depicted on the scarab.
It does not always show how much later the burial was after the time of the
king depicted on the scarab.

To use a modern example, if someone were buried with a campaign poster
from an FDR election campaign, it means he could not have been buried
during the presidency of Grover Cleveland. But it is possible that the
man was buried much later than the FDR administration, say during the
days of Richard Nixon or Jimmy Carter or even later.

Rohl, however, thinks the scarab found at Jericho 4 is absolved of any of these
problems. It is true that scholars have been able to develop a scarab typology
for chronological purposes, but such typologies are not always reliable. In
some cases, scarabs were produced after the time of the king whose name
is on the scarab.

For example, the destruction deposit at Gezer, dated to the end of the Middle
Bronze Age, “contained a seal ring bearing the royal name Khyan
(Khyran), the third king of the Fifteenth [Hyksos] Dynasty….The presence of
this gold-inlaid ring in the destruction layer shows that it remained in use until
the days of Apophis or possibly later, until the reign of Khamudy….“

Daphna Ben-Tor points out that a reliable scarab typology has to be
developed based on large groups of scarabs in clear archaeological
contexts, but even so such a typology is not without controversy. Fortunately,
in the case of the “Hyksos” scarabs, the foreign kings who ruled in Egypt were
regarded as illegitimate, so there was not as much posthumous veneration.
Hence, their dating is more chronologically useful.

The scarab discussed by Rohl had the name of the king Sheshi, so it shows at
least that Jericho stratum 4 could not have been earlier than the time of
Sheshi. Nevertheless, how much later is Jericho 4 after the time of Sheshi?

One problem for Rohl is that Egyptologists are divided as to when
Sheshi ruled. Some identify him with 15th Dynasty Salitis, others that he
ruled after 15th Dynasty Khyan, while others think he was a 14th Dynasty
ruler. And scholars are not exactly sure how these dynasties relate to one
another. There is a lot of overlapping, for instance. In addition, Sheshi has

53 Kempinski, Aharon, “The Middle Bronze Age,” Ben-Tor, Amnon, ed., The Archaeology
of Ancient Israel, Open Univ of Israel, 1992, p. 189.

54 Daphna Ben-Tor, Scarabs, Chronology, and Interconnections: Egypt and Palestine in the
scarabs all over the place, whether under the name Sheshi or under the name Maaibre. There are hundreds of these scarabs.\textsuperscript{55}

Rohl, however, follows Ryholt in thinking Sheshi was an early king who reigned long before the main Hyksos line. He lived “well before the Hyksos age,” says Ryholt. In fact, Ryholt believes, based on a seal of Sheshi found in the destruction layer of the Mergissa Fortress, that Sheshi reigned during the time of the early 13\textsuperscript{th} dynasty, and was a contemporary of Neferhotep I.\textsuperscript{56} This would be Tell el-Dab’a stratum F, which reflects the MB2 a & b transition.

So if Rohl is going to follow Ryholt’s placement of Sheshi during stratum F at Dab’a, he should also date Sheshi to the time near the beginning of MB2b. That is because, as noted, stratum F at Dab’a correlates to the MB2 a & b transition.

However, Rohl does not do this. He merely states that Sheshi was far earlier than Dab’a stratum D/2, which is the last stratum, and represents the end of the Middle Bronze Age.

Rohl concludes on the basis of the fact that Sheshi’s reign “does not fall anywhere near the end of the Middle Bronze Age” that “Jericho was also destroyed long before the end of MB IIB [Albright’s MB2c], since Sheshi’s scarab was found in one of the last burials at the site before its destruction by the Israelites.”\textsuperscript{57}

\textbf{21. Sheshi and Jericho}

So Rohl’s position is that Jericho 4 was destroyed during the middle of the Middle Bronze Age rather than at its end. In response to Rohl, we can say first of all, not all archaeologists agree with Ryholt’s placement of Sheshi. As against Ryholt, Ben-Tor says of the scarab typography that comes after Sheshi’s scarabs:

“The important point, which argues in favour of the sequence proposed by Ward, is that, when occurring in the Palestinian series, these designs are not attested earlier than Jericho Group IV. The latter corresponds with stratum E/1 at Tell el-Daba, which is dated by the excavators to the advanced Fifteenth Dynasty….This is supported by two stratified examples from Tell el-Daba, which come from strata D/3 and D/2 of the late Fifteenth Dynasty [MB2c].”\textsuperscript{58}

Ben-Tor’s point is that the immediate post-Sheshi designs begin at the same time as Dab’a E/1, which is during the main Hyksos dynasty. So the Sheshi scarabs may not be as early as Ryholt thinks. In addition, Ben-Tor says:

“It is also important to note that design 7C, which dominates the scarabs of Yaqubhar and Sheshi, is in Palestine attested only in the late series. The early Palestinian series displays only imitations of the late Middle Kingdom paired scroll border design 7B. The absence of design 7C in the early series supports the chronological range proposed by Ward and argues against dating the scarabs of Yaqubhar and Sheshi to the Fourteenth Dynasty.”\textsuperscript{59}

The bottom line for our purposes is that Rohl is basing a far reaching chronological conclusion using data that is in dispute among archaeologists.

\textsuperscript{56} Kim Ryholt, \textit{The Political Situation in Egypt During the Second Intermediate Period}, University of Copenhagen: Museum Tusculanum Press, 1997, p. 42. For an alternative view of the Mergissa dating, see Ben-Tor, “Sequences…” pp. 94, 95.
\textsuperscript{58} Ben Tor., D. “Sequences and chronology of Second Intermediate Period royal-name scarabs,” in Maree, Marcel, ed., \textit{The Second Intermediate Period}, Leuven, Paris, Walpole, MA, 2010, pp. 102-03. Note also that Ben-Tor uses Albright’s terminology of MB2 a, b, & c.
\textsuperscript{59} Ben-Tor, p. 103.
Once again, this reflects a poor methodology.

**22. Contemporary Kings**

Another problem with Rohl’s conclusion that Jericho was not destroyed at the end of the Middle Bronze Age is that recent research casts doubt on the traditional placement of the main Hyksos line in relation to the 13th Dynasty.

Egyptologists have long recognized that the 14th, 15th, 16th, and to a great extent, the 17th dynasties were more or less contemporary with one another. It seems that divided rule was the chief characteristic of Egyptian political life in the 13th and succeeding dynasties.60

The *Cambridge Ancient History* says of the time of Sobekhotep 4: “We know, in any event, that within a very few years after the accession of this king the ancient town of Avaris, twelve miles south of Tanis, was in the hands of the Hyksos, and we must suppose that even during his reign Egyptian authority in the Delta was being gradually overshadowed by that of the Asiatic intruders. The existence of a king of the Fourteenth Dynasty at Xois, and perhaps also of a Hyksos prince at Avaris, lends colour to the statement of Artapanus (first century b.c.) that King ‘Chenefres’ (=Khaneferre?) was ‘ruler of the regions above Memphis, for there were at that time several kings in Egypt’.” 61

Indeed, new archaeological work by Moeller, Marouard, and Ayers, has shown that Sobekhotep 4 of the 13th dynasty was a contemporary of Hyksos king Khyan of the 15th dynasty. As the authors point out:

> “During the most recent fieldwork seasons in 2010 and 2011, excavations continued northwards from the columned hall and led to the discovery of another large adjoining hall. Sealings showing the cartouche of the Hyksos ruler Khyan have been found here within a large sealing corpus together with sealings naming Sobekhotep IV. These finds come from a secure and sealed archaeological context and open up new questions about the cultural and chronological evolution of the late Middle Kingdom and early Second Intermediate Period.” 62

In addition: “The other issue that needs to be addressed in view of the discovery at Tell Edfu is the internal chronology of the Second Intermediate Period and the overlap of various dynasties. The presence of the nine Sobekhotep IV sealings in the same closed context as the Khayan ones suggests that these two rulers reigned probably not too far apart from each other or were even contemporary.”

“Following the traditionally proposed order of reigns, Sobekhotep IV and Khayan are separated by about 100 years, but this cannot be correct in view of the stratigraphic evidence from Tell Edfu.”

They rule out the possibility of later usage: “The possibility of a much later re-use of a Sobekhotep IV scarab for these seal-impressions has to be excluded according to the ceramic evidence as well as the 14C date…."

And finally: “Thus the conclusion is that Khayan must date earlier than

60 Gardiner, p. 150.
61 *Cambridge Ancient History*, II:1, pp. 50-51.
previously thought and there might have been an overlap between the late 13th Dynasty and the early 15th Dynasty.”

So Rohl’s discussion of the Hyksos kings is not in keeping with the latest research on this period. Sheshi may be closer to the end of the Middle Bronze Age than Rohl thought, and this means Sheshi may be much closer to Jericho 4 than Rohl would like him to be for purposes of theory.

23. It Is All Relative

The final problem with Rohl’s re-dating of Jericho comes in two parts. One, it is a non-sequitur. Even if Sheshi’s reign were much earlier than, or far distance from, the end of the Middle Bronze Age, this does not in itself have any bearing on the time of Jericho’s destruction. There is of course, the problem of dating a stratum on the basis of one scarab. This is never a good idea, especially if it has such far-reaching effects. Such re-dating should be left up to professional archaeologists.

Two, Rohl is guilty of a subtle confusion of relative dating with absolute dating. The destructions at the end of the Middle Bronze Age were correlated to archaeological indicia, such as pottery that existed at the end of the Middle Bronze Age. Mazar says:

“W. F. Albright and G.E. Wright divided the period into two major subphases, denoting them ‘MB IIB’ and ‘MB IIC’, the latter comprising the last hundred years of the period. This subdivision is mainly based on refined ceramic typology and the relative sequence of phases, particularly at Shechem and at Gezer.”

Subphases are distinguished because of “refined ceramic typologies” which archaeologists are trained to recognize. On the basis of this ceramic inventory, they are able to correlate similar strata over time.

Relative dating also involves fortifications, city gates, temples, tombs, weapons, art, jewelry, and writing, etc. These can all be dated in terms of standard Middle Bronze Age terminology. They are relatively dated based on comparing them with similar indicia in other cities.

By cross-comparing enormous amounts of archaeological data such as the above, archaeologists can more or less tell when urban structures existed and when they ceased to exist. These cross-comparisons have led archaeologists to divided the Middle Bronze Age into three main periods, MB2a, MB2b, and MB2c (or MB I, MB II, MB III, or MB2a, MB2b-early, MB2b-late).

It is ultimately this comparative approach that provides a relative date for the destruction of a city—and Jericho 4 had characteristic pottery of the MB2c horizon when it was destroyed. In other words, as Kenyon pointed out, it was destroyed at the end of the Middle Bronze Age.

However, as opposed to relative dating, absolute dating cannot depend on pottery typology but has to be correlated to a reconstructed Egyptian timeline in order to arrive at a fixed calendar date:

“The absolute chronology of the beginning and the end of MB IIB-C depends on correlations to Egyptian and Mesopotamian chronologies.”

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63 Mazar, p. 193; emphasis added.

64 Mazar, p. 193.
This means the relative chronology stays the same, no matter what the absolute chronology is. It would not matter if the absolute date for the end of the Middle Bronze Age were dated to the time of Socrates. It would still be the end of the archaeological stratum known as MB2c (or Rohl’s MB IIB-late).

So on a purely archaeological basis Rohl cannot simply roll back the destruction of Jericho 4 to the middle of the Middle Bronze Age, without also pulling the pottery with it. And that is not feasible if the science of archaeological stratigraphy has any meaning.

Or to put it another way, if Jericho 4 is correlated to the MB2c horizon based on characteristic pottery indicia, then Rohl cannot re-date the destruction of Jericho 4 to an earlier pottery horizon, such as MB2b. If he did that then the one hundred or so years that are allotted to the 15th Dynasty (1663 BC to 1555 BC on conventional dating) will have existed without a pottery horizon. By such re-arrangement of archaeological strata, Rohl will have created a ghost century for the Hyksos, an impossible situation.

To sum up then, there are three main problems with Rohl’s claim that city destructions at the end of the Middle Bronze Age should be re-dated to the middle of the Middle Bronze Age. First, archaeologists are not in agreement as to when Sheshi ruled. Second, Khayan and Sobekhotep 4 may be contemporary or close in time, thus shortening the distance between Sheshi and Jericho 4. Third, the dating of cities to the last stratum of the Middle Bronze Age is based on pottery indicia, and it is not permissible for Rohl to separate a city from its pottery.

24. Other Problems

I won’t spend a lot of time with Rohl’s speculations as to the identity of Joseph. The main problem is that there is no evidence of a famine during the days of Amenemhet 3. In Rohl’s chronology, this is the pharaoh whom Joseph served under, so this would seem to represent a refutation of Rohl’s placement of Joseph in Egyptian history.

In reality, the reason there was no famine during Amenemhet’s reign is due to the high Niles that occurred. How does Rohl deal with this? Simple: he merely claims that high Niles can result in famine. Indeed, he speaks of the high Niles under Amenemhet 3 as being a “terrifying threat to the survival of the Egyptian Middle Kingdom.”

He goes on to claim that the “Nile floodwaters swept over the valley like a tsunami, bursting levees, drowning livestock, washing away the mudbrick villages built on the low-lying, sandy mounds along the valley floor. Temples were flooded, tombs filled with detritus, the whole land came to a grinding halt as people struggled with the consequences of the abnormally gigantic flood….Famine threatened Egypt on an unprecedented scale.”

Rohl does not provide the slightest bit of evidence from any reliable sources for this claim. He merely states it. Once again my head is starting to pound. The fact is, in Egypt famines are associated with low Niles not with

65 Rohl, Exodus, p. 95.
66 Rohl, Exodus, pp. 95-96.
high Niles. As Kathryn Bard points out:

“[T]here were relatively low floods after 2900 BC [sic], with a brief minimum ca. 2200 BC, and exceptionally high floods ca 2150-1900 BC. Low Nile floods would have meant less land under cultivation—and lower crop yields….With such a large population in the later Old Kingdom and problems in agricultural yields, famine for some may have been the result. Possibly the state could have responded to environmental problems of low Nile floods with technological intervention, such as sponsoring irrigation works, but this did not happen.”67

In addition, the time of Amenemhet 3 was a time of prosperity, with no hint of any major economic problems. Peter Clayton says of Amenemhet that:

“His reign was the apogee of economic growth in the Middle Kingdom. Interest in the agricultural potential of the Faiyum increased….Above all, Amenemhet III exploited the quarries of Egypt and the turquoise mines in Sinai.”68

This is another example of Rohl’s poor methodology. When the actual data on the ground contradicts his historical reconstruction, he tries to explain it away. And what is worse, this explanation is entirely gratuitous in that nowhere does Rohl provide any documentation to support it. This is not good scholarship.

Rohl also makes a mistake in claiming that the Ipuwer Papyrus is describing the time of travail at the close of the 13th dynasty.69 He references Van Seeters for this claim, but most Egyptologists refer the content of Ipuwer to the Old Kingdom, even if they might be guarded about how truthful it is in its descriptions. Redford points out:

“The extant copies of Ipuwer all date to the New Kingdom, but a passage was excerpted as early as the 12th dynasty for inclusion in the Instruction of Amenemhet; and the historical milieu of the piece clearly points to the period between Pepy II and the rise to power of the 11th Dynasty for its formulation.”70

I’ve always felt that the Admonitions of Ipuwer is very similar in tone to the curses listed in the biblical Book of Deuteronomy. The difference is that Ipuwer is describing an already existing state of affairs, the topsy-turvy nature of Egyptian society, whereas in Deuteronomy, this topsy-turvy situation would only come about if the Israelites were disobedient to the law of God. Compare these lines from Ipuwer who describes the ironic reversals of Egyptian society:

“Indeed, poor men have become owners of wealth, and he who could not make sandals for himself is now a possessor of riches.”

“[T]he well-born man…through lack of recognition, and the child of his lady has become the son of his maidservant.”

“All these years are strife, and a man is murdered on his housetop even though he was vigilant in his gate lodge.”

And now just one of many curse passages in Deuteronomy:

“You shall betroth a wife, but another man shall lie with her; you shall build a house, but you shall not dwell in it; you shall plant a vineyard, but shall not gather its grapes.”71

68 Clayton, Peter, Chronicle of the Pharaohs, UK: Thames and Hudson, 1994, p. 87.
69 Rohl, Exodus, p. 150.
70 Redford, Donald B. Egypt, Canaan, and Israel in Ancient Times, New Jersey: Princeton Univ. Press, 1992, p. 66; also p. 63. See also, Gardner, Alan, Egypt of the Pharaohs, 1961, p. 110; also Dodson, Aidan, Monarchs of the Nile, Cairo: American University, 2000, pp. 43, 44.
In order to put *Ipuwer’s* lamentations into a curse formula, one would merely need to make them promissory, e.g.: “Poor men will become owners of wealth, and a shoemaker will become a rich man”; or “The children of the well-born will become the sons of maidservants”; or “A man who is vigilant at the gates of his house will be murdered on the rooftop.”

In any case, regardless of whether *Ipuwer* is describing the First Intermediate Period or Second Intermediate Period, it is not sound methodology to rest any great historical conclusions on it since its content is in dispute among some Egyptologists.

Rohl also makes the claim that the increase in tombs at the end of Jericho 4 is best explained by a plague that struck down so many Israelites just before the Conquest. However, there was a similar increase in tombs during the earlier MB1 period where north of the town there was a “tremendous expansion of tombs during the E.B.-M.B. [MB1] period.”

So the increase in tombs really proves nothing about which level at Jericho saw the presence of the Israelites. The tomb expansion would be a dependent variable, not an independent verification of the presence of the Israelites at the time of the Conquest. One would have to determine that actual Israelite level before one could use the tomb data as a possible verification.

### 25. Conclusion

It is my opinion that chronological revisionists, especially those who don’t have advanced degrees in relevant subjects such as Holy Land archaeology or Egyptology, should be as scholarly as possible when discussing revisionism.

I am not saying that professional scholars or archaeologists are infallible when it comes to describing the strata and pottery of Egypt or of the Holy Land. And they are certainly not always correct in their identification of various sites in the Holy Land with biblical cities. Nevertheless, the revisionist should not ordinarily be disagreeing with archaeologists when it comes to describing the facts on the ground in these regions, or with city ascriptions.

In addition, if a revisionist hopes to demonstrate his views to the satisfaction of the professional scholarly and archaeological community, he will attempt to show first, that his theory accounts for the archaeological and biblical data, and second that it doesn’t contradict these sources.

As noted above, Rohl’s methodology leads him to do both, to contradict both the Bible and archaeology. For this reason, his Middle Bronze IIB Conquest theory looks to me like a dead end. I don’t think Christians should appeal to it in support of biblical history nor should revisionists adopt Rohl’s methodology.

I have been studying chronology and archaeology for many years now. It is my opinion that a sound, scientific methodology has the best chance of discovering the true chronology of ancient history—if one is even possible given the fragmentary nature of the data.

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72 *Cambridge Ancient History*, II:1, 93.
Rohl’s chronology strikes me as impossible in the same way Velikovsky’s chronology struck many of his followers as impossible. They went on to develop their own “Glasgow” chronology. Rohl has taken the lead in developing a chronology of the Israelite Sojourn, Exodus, and Conquest, while others have been content to deal only with Israel’s later history.

I think it is high time for Rohl and his followers to abandon any chronology that places the Exodus and Conquest in the Middle Bronze Age, just as they have argued it is time for modern historians to abandon the idea of a Late Bronze Age or Iron Age Exodus and Conquest.

If Rohl had to adopt such an inadequate methodology—as we have documented above—in order to defend a Middle Bronze Age Exodus and Conquest, then perhaps it is time to start thinking outside the box and to reconsider other possibilities that have not perhaps been given a fair hearing in the debates over chronological revisionism.

Finis

Addendum:

I published a much shorter “Reader’s Digest” version of this review at the Amazon website for Rohl’s book Exodus. Rohl responded and the reader can follow up on Rohl’s response and my counter-response. One thing Rohl said that needs criticism is his claim that he never said the Conquest was at the end of the Middle Bronze Age. According to Rohl:

“I never said that the Conquest took place at the end of the Middle Bronze Age. Are you unable to read? I frequently state ‘towards the end of the Middle Bronze Age’, i.e. around the middle of the MB II B (after MB I, MB IIA and the first part of MB IIB). Perhaps the reason you are more than a little confused is that you simply don’t understand very much about archaeology or ceramics?’’

Despite Rohl’s assurances, however, he clearly placed the Conquest in the stratum the signals the end of the Middle Bronze Age. In his book The Lost Testament Rohl said:

“In the New Chronology, however, the Conquest took place during the last phase of the Middle Bronze Age (MB II B c. 1440-1353 BC). . . . The archaeological evidence is unambiguous. The momentous events of the Israelites’ Conquest of Canaan occurred during the last phase of the Middle Bronze Age.”

What does Rohl mean by the “last phase of the Middle Bronze Age”? To make it plain as day, Rohl says in a footnote on the same page:

“The last phase of the Middle Bronze Age (otherwise known as MB II-C or MB III) extends into and therefore overlaps with LB I-A (which starts within the Greater Hyksos Dynasty in Egypt).”

So clearly Rohl believed that the last phase of the Middle Bronze Age was identical to the standard archaeological terminology of Albright’s MB IIC or Kenyon’s MB III. It was the Hyksos 15th Dynasty, which is correlated to MB2c in standard terminology, i.e., the last phase of the Middle Bronze Age.

This means logically for Rohl the Conquest must have happened during MB2c. To be consistent, Rohl’s new designation “towards the end of the Middle Bronze Age” would therefore have to mean towards the end of MB2c.

74 Rohl, D., Lost Testament, p. 240; my emphasis.
If it occurred at an earlier point, it would no longer be the “last phase” of the Middle Bronze Age. It would be an “earlier phase” of the Middle Bronze Age.

Here again, the headache re-surfaces. Rohl’s current placement of the Conquest in the middle of the Middle Bronze Age is not what would normally be interpreted as towards the end of the Middle Bronze Age. There does seem to be a satisfying explanation for this, however, even if it doesn’t work as well as aspirin.

I think Bimson had it right when he criticized Rohl for relying on an early 1983 work of Kempinski, who placed the fall of Jericho 4 to a time considerably before the end of the Middle Bronze Age.75 Bimson says of Kempinski’s work:

“[T]his was written before volumes of the Jericho excavation report containing pottery from the tell (as distinct from tombs) were published. Later, and perhaps in the light of those, he seems to have changed his mind….But the bottom line is that the context in which Kempinski dated the fall of Jericho significantly earlier than the end of MBIIB [i.e., end of Middle Bronze Age] is a book that predated the full publication of Kenyon’s excavations, specifically those volumes containing the pottery from the tell.76

So if the early Kempinski was confused on the subject, and if Rohl was relying on him, it is no wonder that Rohl was likewise confused. With his new book I say once again that Rohl has actually made things worse if that were possible. His earlier books had only been confused but his new book adds impossibility into the mix.
