



April 2010

Gates, Dates, and Debates: A Review of Megiddo's Monumental Gate and the Debates over Archaeology and Chronology in Iron Age Palestine

Joshua J. Bodine

Follow this and additional works at: <https://scholarsarchive.byu.edu/studiaantiqua>



Part of the [History of Art, Architecture, and Archaeology Commons](#)

BYU ScholarsArchive Citation

Bodine, Joshua J. "Gates, Dates, and Debates: A Review of Megiddo's Monumental Gate and the Debates over Archaeology and Chronology in Iron Age Palestine." *Studia Antiqua* 8, no. 1 (2010). <https://scholarsarchive.byu.edu/studiaantiqua/vol8/iss1/3>

This Article is brought to you for free and open access by the All Journals at BYU ScholarsArchive. It has been accepted for inclusion in *Studia Antiqua* by an authorized editor of BYU ScholarsArchive. For more information, please contact scholarsarchive@byu.edu, ellen_amatangelo@byu.edu.

GATES, DATES, AND DEBATES:
A REVIEW OF MEGIDDO'S MONUMENTAL GATE
AND THE DEBATES OVER ARCHAEOLOGY AND
CHRONOLOGY IN IRON AGE PALESTINE

JOSHUA J. BODINE

Evolving originally from an aim to illuminate and authenticate the biblical text into being part of a wider movement in Near Eastern archaeology, for over a century now “biblical archaeology” (if it can be called such) has been a major activity carried out throughout Palestine.¹ At the same time, archaeology in the land of the Bible has produced endless debates and discussions over archaeological and historical interpretations—evident in recent years as one takes stock of the issues that have arisen over Iron Age Palestine.² As an important period in the study of early Israelite history and the emergence of its territorial state(s), the Iron Age layers at many locales have been focal points of unending archaeological digs and discussions. For the Iron Age II in particular, major differences of interpretation exist between scholars, so much so that establishing an archaeological framework and correlating it with secure historical details has been difficult and extremely divisive and has yielded anything but a scholarly consensus.

In the Iron Age II layers at Megiddo, one of Palestine's most important archaeological sites, are the remains of a monumental city gate that has had an enormous impact on the archaeological debates. Yet aside from its significance and effect on the current controversy over chronological models, its own interpretation is ironically dependent on the ever-evolving arguments of the very same discussions. Megiddo's monumental gate is thus a perfect primer into the wider issues and complexities involved in the archaeology and chronology of Iron Age Palestine. It is an influential architectural remain and is necessarily an important part of any archaeological and historical interpretations that may be offered of the biblical past.

1. See Amihai Mazar, *Archaeology of the Land of the Bible: 10,000–586 B.C.E.* (ABRL rev. ed.; New York: Doubleday, 1992), xv.

2. For a recent and accessible analysis of the issues from the standpoint of two prominent archaeologists who are heavily involved in the discussions, see Israel Finkelstein and Amihai Mazar, *The Quest for the Historical Israel: Debating Archaeology and the History of Early Israel* (ed. Brian B. Schmidt; SBLABS 17; Atlanta, Ga.: Society of Biblical Literature, 2007).

Archaeology, History, and the United Monarchy

It is perhaps obvious, but nonetheless important to point out from the start, that before any historical or chronological questions can be asked of archaeology, a proper archaeological framework must first be established.³ The archaeological framework of a site can be said to be composed of stratigraphy and a relative chronology—in other words, layers of material remains, generally sequential in nature, that place data into a context with which it can be properly interpreted and hopefully dated.⁴

The single greatest problem in establishing such a framework is the fact that the data at any given site can almost always be interpreted in more than one way (e.g., specific remains being assigned to an upper or lower level for different reasons). Adding to the complexity is the reality that the archaeological framework of a given site cannot simply be worked out in total isolation from the surrounding “horizon” of the region. Yet these examples say nothing about the meaningful application of an archaeological framework, only of the difficulties inherent in archaeological work itself. Archaeology is invaluable in what it reveals about the past, but even if developed perfectly in all of its aspects, the archaeological framework of a site or region only offers a relative picture—culturally, historically, or otherwise. Irrespective of the soundness of an archaeological framework, then, unless it can be tied to historical dates, periods, events, and so on, it is in at least some senses meaningless. To truly understand the past, the archaeological and historical data must at some point be combined. However, attempting a synthesis of archaeological evidence with historical information is a significant undertaking that requires a thorough methodology, the cooperation of both archaeologists and historians, and the recognition that the array of data is often problematic in one way or another.⁵

3. See Amihai Mazar, “The Spade and the Text: The Interaction between Archaeology and Israelite History Relating to the Tenth–Ninth Centuries B.C.E.,” in *Understanding the History of Ancient Israel* (ed. H. G. M. Williamson; Proceedings of the British Academy 143; Oxford: Oxford University Press for the British Academy, 2007), 146.

4. On this see David Ussishkin, “Archaeology of the Biblical Period: On Some Questions of Methodology and Chronology of the Iron Age,” in *Understanding the History of Ancient Israel*, 135.

5. Speaking of these issues, David Ussishkin laments the practice of the interpretation of history that is carried out by professional archaeologists whom he believes “should concentrate on the study of the archaeological data, leaving the questions of history and the Bible to be dealt with by the historian and the biblical scholar who are more qualified to do so.” Likewise, “biblical scholars and historians should refrain from analyzing the archaeological data, thinking that they are as well-qualified to do so as professional archaeologists.” In theory, no one would disagree with the essence of this argument; it is sound advice. Even so, the problem is at some point archaeology and history must be combined “in order to provide an overall picture of what happened.” The “proper methodology” for doing this, Ussishkin contends, is “some cooperation between archaeologists, biblical scholars and historians.” To carry this out: “The archaeologist elucidates and organizes the data objectively, explains their meaning and limitations, and summarizes them. In the next stage the historian and biblical scholar study the results of the archeological work and incorporate them into their own research” (see Ussishkin, “Archaeology of the Biblical Period,” 134–35). In reality, however,

To speak of the biblical past, if the several competing chronological models currently used today are any indication, coalescing archaeological and historical interpretations of the biblical text and its land is a seriously problematic endeavor. At the moment no period is more controversial than, roughly speaking, the eleventh to the ninth centuries B.C.E., in which various phases of the Iron Age are dated differently by different archaeologists.⁶ As the very period that represents the formative years of the Israelite state(s), much discussion has centered on the nature and extent of the United Monarchy—in particular the reign of King Solomon.⁷ To be sure, the contention over chronology is not about the existence of David or Solomon as historical figures, but, as Israel Finkelstein puts it, “the extent and splendor of their realm.”⁸

The issues involved in the search for the biblical past of the period of David and Solomon and the surrounding centuries are not simply petty, scholarly arguments; the problems are real and the role that archaeology plays is vital. It has been said that Megiddo is the place where “the archaeology of the 10th–9th centuries was born.”⁹ It is here, then, where we will focus our discussion in order to illuminate the dynamics, trends, and general issues that factor into the archaeological and chronological controversy of Iron Age Palestine.¹⁰

this ideal is hardly ever reached, even by Ussishkin. The tendency for historians to interpret rather than analyze the archaeological data, and the temptation for archaeologists to offer historical interpretations of their archaeological frameworks, is difficult to control. The point is all should attempt to be more careful in their assessments, research, and conclusions, and be willing to hold dialogue as part of creating a synthesis of archaeology and history. The need for dialogue between the fields to create an accurate picture has been called for by others as well; for example, see William G. Dever, *Recent Archaeological Discoveries and Biblical Research* (Seattle: University of Washington Press, 1990): 30–35; and William G. Dever, *What Did the Biblical Writers Know and When Did They Know It?: What Archaeology Can Tell Us about the Reality of Ancient Israel* (Grand Rapids, Mich.: Eerdmans, 2001), 53–95.

6. A glance at the terminology of archaeological periods used by different scholars testifies to the existence of diverse archaeological schemes for the Iron Age. On this see the brief discussion by Lester L. Grabbe, *Ancient Israel: What Do We Know and How Do We Know It?* (New York: T&T Clark, 2007), 10–11.

7. See, for example, the discussion in Gary N. Knoppers, “The Vanishing Solomon: The Disappearance of the United Monarchy from Recent Histories of Ancient Israel,” *JBL* 116, no. 1 (1997): 19–44; or part one of the volume edited by Andrew G. Vaughn and Ann E. Killebrew, *Jerusalem in the Bible and Archaeology: The First Temple Period* (SBLSymS 18; Atlanta, Ga.: Society of Biblical Literature, 2003), 13–182.

8. Israel Finkelstein and Neil A. Silberman, *The Bible Unearthed: Archaeology’s New Vision of Ancient Israel and the Origin of Its Sacred Texts* (New York: Free Press, 2001; repr., New York: Simon & Schuster, 2002), 142.

9. Israel Finkelstein, “State Formation in Israel and Judah: A Contrast in Context, a Contrast in Trajectory,” *NEA* 62, no. 1 (1999): 35–36.

10. Of course, this is also to recognize the fact that the archaeological and historical data is much more nuanced, plentiful, and wide-ranging than Megiddo; in this regard, a review of Megiddo is in no way exhaustive and cannot pretend to be comprehensive enough for a complete understanding of the issues.

Megiddo, Its Monumental Gate, and the Debate

Home to one of the largest archaeological undertakings at an Iron Age site, the ancient, northern Palestinian site of Megiddo (modern-day Tell el-Mutesellim) is widely regarded as an important—indeed key—archaeological site.¹¹ Even before the Iron Age, extending back into the Late Bronze, Megiddo was a prominent city with monumental buildings and remained an important administrative center well into the Iron Age.¹²

The Stratigraphy of Megiddo

Beginning in 1903, several major and minor excavations¹³ were carried out at Megiddo, resulting in a complex archaeological framework which continues to be challenged and developed today.¹⁴ The convoluted nature of the archaeological framework owes itself to, among other things, the mismanagement and serious methodological errors of early digs, the ambiguity of several important archeological layers, and the differences of opinion by assessors of the data. Yet, despite these types of issues—or perhaps because of them—Megiddo became a centerpiece of the reconsiderations of the traditional chronological framework.

The Megiddo strata at the heart of the current controversy, and those that directly affect interpretations of the Israelite state(s) in the eleventh to the ninth centuries B.C.E., are those strata labeled V and IV, along with their sub-phases. It is in these layers that we find the remains of a monumental city gate whose impact and importance on this period cannot be understated: the so-called “Solomonic” gate. Unfortunately, in the words of André Lemaire, this is precisely the period where the “stratigraphy of Megiddo . . . is not at all clear.”¹⁵

Megiddo and its “Solomonic” Monuments

Dating back to the 1920s and 1930s, in the expedition of Megiddo by the University of Chicago’s Oriental Institute, many Iron Age remains were first associated with King Solomon by the excavators. Among these were the famous “Solomonic” stables and gate—both originally assigned to Stratum IV—that appeared to be prime evidence confirming the accuracy of the biblical

11. For a general review and introduction to Megiddo, see G. I. Davies, *Megiddo* (Cambridge: Lutterworth, 1986).

12. See Israel Finkelstein, David Ussishkin, and Baruch Halpern, eds., *Megiddo III: The 1992–1996 Seasons* (2 vols.; Tel Aviv University Institute of Archaeology Monograph Series 18; Tel Aviv: Institute of Archaeology, 2000), 593–97.

13. For a brief review of Megiddo and an overview of the history of its archaeological excavations, see David Ussishkin, “Megiddo,” *ABD* 4:666–79.

14. One recent argument is that of Norma Franklin, a Megiddo excavator who proposes that all the sub-phases of Megiddo Stratum V and IV be done away with and classified as they originally were. On this see Norma Franklin, “Revealing Stratum V at Megiddo,” *BASOR* 342 (2006): 95–111.

15. André Lemaire, “The United Monarchy: Saul, David and Solomon,” in *Ancient Israel: From Abraham to the Roman Destruction of the Temple* (ed. Hershel Shanks; rev. and enl. ed.; Washington, D.C.: Biblical Archaeology Society, 1999), 118.

record. The following biblical passages were particularly important for this interpretation: (1) in 1 Kgs 9:15, in addition to Solomon's palace and temple at Jerusalem, Megiddo is listed as one of three cities, along with Hazor and Gezer, which Solomon rebuilt or fortified; (2) 1 Kgs 7:12 describes Solomon's buildings as being composed of "hewn stones" (i.e. ashlar); (3) 1 Kgs 9:19 mentions cities for housing chariots and cavalry. So, when the Oriental Institute's excavators uncovered a large gate structure (gate 2156) built of high-quality ashlar masonry, as well as two large buildings composed of rows of individual chambers separated by low walls with troughs on the opposite side, it seemed only reasonable, based on the biblical evidence, that these finds be dated to the period of Solomon in the tenth century (ca. 970–930 B.C.E.).

Two decades later the importance of these finds was greatly enhanced by the work of Yigael Yadin. Yadin, excavating at Hazor in 1957, uncovered a casemate wall surrounding the city which was connected to a large six-chambered gate. When Yadin noticed that the size and layout of the gate at Hazor was remarkably similar to the one unearthed at Megiddo twenty years earlier, Yadin was led to assert that Megiddo and Hazor were not only built by Solomon, but that both gates were "built by the same royal architect."¹⁶ Not long after, Yadin took to the task of examining the archaeological reports of the third city mentioned in 1 Kgs 9:15, Gezer, and discovered, yet again, a similar type of gate structure to those at Megiddo and Hazor.¹⁷ In Yadin's opinion, the controversy over the dating of the gate at Megiddo ended.¹⁸

One of the problems with Yadin's interpretation was that the gate at Megiddo was not found connected to a casemate wall like those at Hazor and Gezer. So, in the early 1960s Yadin set out to Megiddo to uncover a casemate wall that in his view surely must have been missed by earlier excavations. In the process, Yadin found the remains of (among other buildings) two palaces (1723 and 6000) built of ashlar stones. One of those palaces (6000) was connected to a row of structures interpreted by Yadin as a casemate wall, which in turn appeared to be connected to the city gate. This palace, according to a later article by David Ussishkin, was clearly similar to the biblical description of Solomon's royal palace in Jerusalem, thus allowing us, as Ussishkin put it, "but a glimpse of the magnificent buildings of this glorious king."¹⁹

16. Yigael Yadin, "Excavations at Hazor, 1957, Preliminary Communiqué," *IEJ* 8 (1958): 3.

17. See Yigael Yadin, "Solomon's City Wall and Gate at Gezer," *IEJ* 8 (1958): 85–86.

18. Yadin mentioned his opinion that the controversy was ended in his report of Hazor even before examining Gezer (see note 16); Gezer only cemented this. It should also perhaps be pointed out that the recent controversy is not an entirely new development, demonstrated by the fact that in 1958 Yadin had to call an "end" to the controversy over dating the gate to a period later than Solomon, signifying that such a debate had already arisen. In fact, in 1940, John Crowfoot had already argued for a lowering of the date of Megiddo Stratum IV to the ninth century based on his excavations at Samaria, and the similarities between it and Megiddo. See John Crowfoot, "Megiddo—A Review," *PEQ* (1940): 132–47.

19. David Ussishkin, "King Solomon's Palaces," *BA* 36, no. 3 (1973): 105. See also David Ussishkin, "King Solomon's Palaces and Building 1723 in Megiddo," *IEJ* 16 (1966): 174–86.

In an unexpected turn, however, palace 6000 was found to be underneath the building previously identified as the “Solomonic” stables of Stratum IV.²⁰ Yadin’s conclusion: the two palaces, gate, and “casemate” wall were part of a large city comprising stratum VA–IVB, while the stables belonged to a later city (Stratum IVA), built most probably by King Ahab of the northern kingdom of Israel in the early ninth century B.C.E.²¹ Even with the loss of the magnificent stables to a city later than Solomon’s, there were still enough monumental remains left assigned to the stratum above to lend evidence to the greatness of the “Solomonic” city of Megiddo VA–IVB. That is, until another decidedly “Solomonic” structure, the city gate, began to encounter its own stratigraphical challenges.

Connecting the Gate: A Casemate Wall, a Solid Wall, Both, or None at All?

The difficulties with the stratigraphical interpretation of the gate revolve around the lower courses of its structure. Yadin, after finding the rooms east of palace 6000, declared that he had found the missing casemate wall he had been looking for that corroborated his theory about the Megiddo gate and its similarities to those at Hazor and Gezer. In Yadin’s opinion, the gate was connected to this casemate wall and belonged to the same stratum as the monumental palatial edifices of Megiddo VA–IVB—for Yadin, and others, all this was clear evidence for the great royal building projects of King Solomon.²² In Yadin’s view, then, contrary to the assessment of its first excavators, the lower portion of the gate connected to the casemate wall was thus not the gate’s foundation, but its original superstructure.²³ To be sure, the advantage of this interpretation, as Amihai Mazar notes, was that the ashlar in the lower courses of the gate would be visible – as one would expect such beautiful stonework to be – and “not intentionally buried in foundation courses where it could not be seen”; the disadvantage was that the massive gate was left without a foundation.²⁴

It was Yohanan Aharoni, in his own analysis of the stratification of Megiddo, who gave the first major blow to Yadin’s interpretation of the gate. Aharoni disagreed with Yadin in the following important aspects:

20. By this time, Megiddo Stratum V and IV had already been divided into sub-phases within which Yadin worked to present his own interpretation of the archaeological data.

21. See Yigael Yadin, “New Light on Solomon’s Megiddo,” *BA* 23, no. 2 (1960): 62–68 and Yigael Yadin, “Megiddo of the Kings of Israel,” *BA* 33, no. 3 (1970): 66–96. Yadin didn’t necessarily rule out the existence of stables belonging to Solomon, only that the ones on top of the palace did not date to his time. Graham Davies has attempted to show the possibility of Solomonic stables at Megiddo in a similar building that dates earlier than the one Yadin re-dated. See Graham I. Davies, “Solomonic Stables at Megiddo After All?” *PEQ* 120 (1988): 130–41.

22. For example, prominent archaeologist William Dever holds to this view unflinchingly; see Dever, *What Did the Biblical Writers Know*, 131–38.

23. See Yigael Yadin, *Hazor: The Head of All Those Kingdoms* (The Schweich Lectures of the British Academy, 1970; London: Oxford University Press, 1972), 147–64.

24. See Mazar, *Archaeology of the Land of the Bible*, 400, note 15.

(1) that Yadin's interpretation of the row of rooms east of palace 6000 as a casemate wall was weak and untenable; (2) the wall that the city gate was stratigraphically connected to was actually a solid wall (city wall 325) that ran *over* the top of one of the monumental palaces (1723); and (3) as the gate and the palaces were clearly stratigraphically separated, Stratum VA–IVB should be separated as well. Still, Aharoni agreed with Yadin in another respect: coupled with the passage in 1 Kgs 9:15, and with the evidence at Hazor and Gezer, the Solomonic provenance of the city gate was not in question. Aharoni thus saw the solid city wall (325) and the gate as belonging to Stratum IVB, while the palaces that ran underneath the solid wall he assigned to Stratum VA, which he interpreted as a “Davidic” city.²⁵

Then, years later, building upon the argument of Aharoni (as well as Ze'ev Herzog), David Ussishkin maintained the city gate's connection to the solid wall running over the top of the palaces, but also highlighted the problem of its missing substructure. For Ussishkin, it was necessary to distinguish between the gate's foundations and its structure. He contended that the lower courses of the gate found in Stratum VA–IVB were not its superstructure, but its subterranean foundation. Moreover, the reason that the foundation was composed of ashlar, was that it was a “constructional fill” built up to ground level by the builders of Stratum IVA, utilizing ashlar from the destroyed buildings (e.g. palace 6000) of Stratum VA–IVB—a type of foundation and structure that is attested at other sites. For Ussishkin, then, the gate should be a part of Stratum IVA along with the stables, and not assigned to the combined (Yadin) or separated (Aharoni) VA–IVB layer(s) along with the palaces.²⁶ To be without a city wall, without a monumental gate,²⁷ and without great stable (or storage²⁸) compounds—all part of a later monumental city with strong fortifications—meant that the “emphasis of the Solomonic constructions at Megiddo mentioned in 1 Kgs 9:15 was clearly on monumental palace-compounds.”²⁹ In some respects, then, the monumental “Solomonic” city was slowly being stripped of its monumental architecture.

25. See Yohanan Aharoni, “The Stratification of Israelite Megiddo,” *JNES* 31, no. 4 (1972): 302–11.

26. David Ussishkin, “Was the ‘Solomonic’ City Gate at Megiddo Built by King Solomon?” *BASOR* 239 (1980): 1–18. Ussishkin admitted in footnote 2 that he had originally accepted Yadin's date in his earlier studies on King Solomon's palaces, and though his view had changed in that specific, he held to his analysis of the palaces themselves.

27. Ussishkin did not see Stratum VA–IVB as totally devoid of a gate; he argued that the six-chambered gate in question was actually preceded by a small two-chambered type, consistent with “the nature of the fortifications at that level.” Ussishkin, “‘Solomonic’ City Gate,” 17.

28. If they are indeed to be interpreted as stables, which, incidentally, is also a point of debate.

29. Ussishkin, “‘Solomonic’ City Gate,” 17. In this view, Stratum VA–IVB is still Solomon's city. However, the “Solomonic city—characterized by monumental palaces, residential quarters, weak defenses, and a small city gate (see note 27)—had been replaced by a city protected by massive city walls and a massive city gate, large stable compounds . . . and a water system.” See David Ussishkin, “Notes on Megiddo, Gezer, Ashdod, and Tel Batash in the Tenth to Ninth Centuries B.C.,” *BASOR* 277/278 (1990): 73.

Yadin, in a rejoinder to Ussishkin, responded that Ussishkin had failed to account for a building method Yadin referred to as “no foundations proper,” where the composition of the underlying ground is believed to be “sufficient to support the structure.”³⁰ Having recognized the gate’s connection to the city wall of Stratum IVA, Yadin concluded that the gate could have had two distinct building phases: “the original one” belonging to Stratum VA–IVB, and a “later stage [tentatively labeled IVA1] . . . in which the whole area was raised and a new floor was built,” and was only then connected to the massive city wall of Stratum IVA.³¹ Since the existence of a true casemate wall that preceded the solid wall was no longer tenable, this would mean that the original phase of the gate in Stratum VA–IVB would not have been connected to any wall at all. One plausible explanation is that “the gate could have formed the entrance to a city which lacked a city wall and in which the outer walls of the outer belt of buildings [i.e. Yadin’s row of rooms] created a defense line.”³² This would allow the high level of a floor that joins the city wall of Stratum IVA to represent a second phase in which a lower, original floor belonging to Stratum VA–IVB butted against the five courses of ashlar and wooden beams in the lowest portions of the structure, and was thus above the ground and visible as would be expected. There are, admittedly, some other stratigraphical problems with this view; however, it is nonetheless an acceptable interpretation.³³

To put it succinctly, depending on one’s analysis, the gate can belong either to both Stratum VA–IVB and IVA in two phases, or to IVA alone. Stratigraphically speaking, then, as expressed by Mazar, “we can progress no further with this matter.”³⁴

Dates and More Debates

Dating the Megiddo gate, however, is a completely separate issue. Once again, arguments vary and views are divided. Any problems of interpreting the archaeological remains are exacerbated by the fact that there are no historical anchors within immediate sight of this period that can be confidently used to ascertain absolute dates. The closest secure anchors are Egypt’s hegemony over Canaan until ca. 1140–1130 B.C.E., marking the upper limit, and the Assyrian campaigns of 732–701 B.C.E., designating the lower.

We can narrow the range between the two somewhat, but not with the same confidence. The lower boundary can plausibly be moved up to ca. 840–830 B.C.E., based on pottery assemblages found at the one-period site of Jezreel—the second royal residence of the Omride dynasty.³⁵ The ceramics, similar to those found in Megiddo Stratum VA–IVB, were found in the

30. Yigael Yadin, “A Rejoinder,” *BASOR* 239 (1980): 19.

31. Yadin, “A Rejoinder,” 20.

32. Mazar, *Archaeology of the Land of the Bible*, 400, note 15. As Mazar notes, such “town planning” is attested elsewhere.

33. See Mazar, “The Spade and the Text,” 156.

34. Mazar, “The Spade and the Text,” 157.

35. See David Ussishkin and John Woodhead, “Excavations at Tel Jezreel 1994–1996: Third Preliminary Report,” *TA* 24, no. 1 (1997): 6–72.

destruction level of the royal compound at Jezreel, and also below it.³⁶ While this chronologically-restricted site can offer a credible lower boundary for Iron IIA type pottery, the fact that similar pottery was also found below the royal residence (and thus from an earlier period) means the similar pottery of Megiddo VA–IVB could be associated with either the period of the royal enclosure in the mid-ninth century B.C.E., or with the earlier pottery found beneath it.

Regarding an upper boundary for dating the Megiddo gate, an important historical datum is the invasion of Palestine ca. 925 B.C.E. by Sheshonq I (biblical Shishak). Though important in certain respects, this event is not without its problems. Based on the assumption that Sheshonq ravaged the Palestinian countryside, destruction levels at many sites have naturally been attributed to him. However, no longer do all scholars accept the interpretation of widespread destruction. In fact, according to Gabriel Barkay, it “has not been proven that any sites were destroyed by Shishak in 925 B.C.E., and the attribution of destruction layers to the end of the tenth century at many sites is mere conjecture.”³⁷ Hence, a growing number of scholars would agree with Ussishkin when he says that “one way or another Shishak’s list [of cities he conquered] is useless as a secure archaeological and chronological anchor.”³⁸

In any case, although the use of Sheshonq’s invasion and its association with destruction layers has difficulties, a royal stele he erected at Megiddo may provide for a possible chronological interpretation. To be sure, the stele was not found within a stratigraphical context, so it cannot be used as a datable anchor, but, as notes Ussishkin, the very act of erecting a stele at Megiddo indicated Sheshonq’s intention to create a “foothold” in Canaan and use Megiddo as a base.³⁹ In other words, Sheshonq would not have erected a stele in a desolate city, nor would he have destroyed the city if he intended on holding it. Thus, at least at Megiddo, attempts to associate a total destruction layer with Sheshonq (like Megiddo Stratum VIA⁴⁰) are probably misguided. Yet, it would be naïve to assume that an Egyptian takeover did not result in at least some sort of trauma to Megiddo. Indeed, this is a scenario we see in the last phase

36. See Orna Zimhoni, *Studies in the Iron Age Pottery of Israel: Typological, Archaeological and Chronological Aspects* (Tel Aviv Occasional Publication Series 2; Tel Aviv: Institute of Archaeology of Tel Aviv University, 1997), 13–56.

37. Gabriel Barkay, “The Iron Age II–III,” in *The Archaeology of Ancient Israel* (ed. Amnon Ben-Tor; trans. R. Greenberg; New Haven, Conn.: Yale University Press, 1992), 306–7. See also G. J. Wightman, “Megiddo VIA–III: Associated Structures and Chronology,” *Levant* 17 (1985): 125, who argues that “there is no clear evidence for a Shishak destruction level at Megiddo.”

38. Ussishkin, “Archaeology of the Biblical Period,” 137.

39. See David Ussishkin, “Notes on Megiddo, Gezer, Ashdod, and Tel Batash,” 74.

40. Despite arguments otherwise, Megiddo Stratum VIA should probably be seen as having to do with King David and his military exploits, as argued by Timothy P. Harrison, “The Battleground: Who Destroyed Megiddo? Was it David or Shishak?” *BAR* 29, no. 6 (November/December 2003), 28–35, 60–64. For a detailed analysis of Megiddo Stratum VI see Timothy P. Harrison, *Megiddo 3: Final Report on the Stratum VI Excavations* (OIP 127; Chicago: The Oriental Institute, 2004).

of Stratum VA–IVB (i.e. a partial destruction of palace 6000 and a few other buildings). Given the circumstances, it is possible that this was the result of Sheshonq’s intended occupation of Megiddo, and it is thus feasible to date the end phase of Stratum VA–IVB to around 925 B.C.E.

In response to the question of whether or not the city gate at Megiddo can be dated with certainty, then, the answer must be no. The monumental six-chambered gate clearly belongs *at least* to Stratum IVA, but the question is whether this was a second phase of the gate, or whether it existed originally in the preceding stratum (VA–IVB). If belonging to both, then a date around 925 B.C.E. (in accordance with the interpretation of Stratum VA–IVB above) is as close as we can get, since we can only conjecture its possible existence at the time and not its date of construction. If to Stratum IVA only, then determining the construction date is more problematic, but its range cannot extend beyond our lower boundary ca. 840–830 B.C.E. (for reasons cited above and others).⁴¹ As of now, inseparably tied as it is to the larger debate, any date assigned to the gate at Megiddo is necessarily tentative. In light of this, and with the recognition that the spectrum of archaeological and historical data is much more complex and plentiful than the brief summary offered here, a synthesis of the available data (at Megiddo and elsewhere) can suggest narrowing the date to somewhere in between ca. 925–840 B.C.E.—a range that allows for the gate at Megiddo to fall within several different chronological models. Is it, then, Solomonic? Yes, no, maybe.

A Crisis of Chronologies

What began with a reconsideration of archaeology and chronology that centered on Megiddo has become a crisis of chronologies, as it were, of Iron Age Palestine—in its current state, what Lester Grabbe describes as an “only-partially controlled chaos.”⁴² Two of these chronologies will be mentioned here.

The most notable—and controversial—assault to the conventional chronology came in the mid-1990s with the development of Israel Finkelstein’s so-called *Low Chronology* (LC), in which he argues for a down-dating of 75–100 years for the entire Iron Age chronology in Palestine.⁴³ It was, in fact,

41. An interesting example used by one archaeologist to corroborate our lower boundary is the remains of a four-chambered gate found in Bethsaida Stratum V (ca. 850–732 B.C.E.), and its implications for the four-chambered city gate at Megiddo that succeeded the six-chambered one. As Megiddo’s four-chambered gate is sometimes assigned to Stratum III and sometimes to Stratum IV, if it is to be related to the similar gate type at Bethsaida Stratum V, the implications for our six-chambered gate are clear: since Bethsaida Stratum V was constructed shortly after 850 B.C.E., the six-chambered gate must have been destroyed around 850 B.C.E. and replaced by the four-chambered type (there are no layers in between). However, this is still only a possible correlation with its *destruction* and not its *construction*. Bethsaida’s director, Rami Arav, is of the opinion that if the four-chambered gate at Bethsaida is parallel in time to the four-chambered gate at Megiddo that sits atop the six-chambered one, then dating the six-chambered gate to the ninth century is “improbable.” Rami Arav, email message to author, 25 October 2008.

42. Grabbe, *Ancient Israel*, 11.

43. See his original remarks on this in Israel Finkelstein, “The Date of the Settlement

his renewed excavations at Megiddo that led him to this conclusion.⁴⁴ In short, Finkelstein's redating scheme means that most of the monuments associated with Solomon are moved from the tenth into the ninth century B.C.E., under the northern Israelite kings of the Omride dynasty (Omri and Ahab; ca. 884–842 B.C.E.). In this view, the United Monarchy is nothing more than a small chiefdom that “could have been an expanding ‘early state’ rather than a full-blown, mature state,” with the real development occurring in the ninth century B.C.E. with the northern kingdom of Israel.⁴⁵ The LC, then, can be said to be a paradigm shift that changes the entire understanding of state formation in the biblical world.

As expected, the LC was met with much criticism that continues today.⁴⁶

of the Philistines in Canaan,” *TA* 22 (1995): 213–39, and Israel Finkelstein, “The Archaeology of the United Monarchy: An Alternative View,” *Levant* 28 (1996): 177–87. His views were developed in later articles and in a full synthesis in his book on the subject (see Finkelstein and Silberman, *The Bible Unearthed*). A similar lowering of dates was already argued by G. J. Wightman in his “Megiddo VIA–III: Associated Structures and Chronology,” *Levant* 17 (1985): 117–29, and “The Myth of Solomon,” *BASOR* 277/278 (1990): 5–22. The main arguments Finkelstein uses to support his view (summarized in his book; see 123–45, 169–95, 342–44) are: (1) pottery assemblages found in the one-period site of Jezreel that “safely” date to the ninth century were “almost identical” to pottery found in the palaces at Megiddo; therefore, not only the gate and the stables, but the palaces too, all date to the ninth century; and (2) similarities in construction and layout between the palaces at Megiddo and those at Samaria dating to the ninth century. Of course, additional reasons beyond these were also involved, such as his criticism of the heavy dependence on a single biblical verse by adherents of the traditional chronology, circular reasoning employed as a result of the use of the biblical text to offer an interpretation of the archaeological data which then authenticates the text, similar gate types not limited to the tenth century B.C.E. but that appear to be a popular form throughout Palestine up until the seventh century B.C.E., the problem of a Jerusalem-centered royal monarchy, the existence of a strange gap (or “dark age”) in the archaeological record that leaves not much in the way of monumental architecture for the ninth to the eighth centuries B.C.E. for which there are clues, and more.

44. See Finkelstein, “State Formation in Israel and Judah,” 35–36.

45. Finkelstein, “State Formation in Israel and Judah,” 42. In this Finkelstein built upon an argument by David W. Jamieson-Drake in his *Scribes and Schools in Monarchic Judah: A Socio-Archaeological Approach* (JSOTSup 109; Sheffield: Almond Press, 1991), in which he had argued that the real rise of Judah as a full-fledged state came in the eighth century B.C.E.

46. See Amihai Mazar, “Iron Age Chronology: A Reply to Israel Finkelstein,” *Levant* 29 (1997): 157–67; Amihai Mazar, “The Debate Over the Chronology of the Iron Age in the Southern Levant,” in *The Bible and Radiocarbon Dating: Archaeology, Text and Science* (ed. Thomas E. Levy and Thomas Higham; London: Equinox, 2005), 15–30; William G. Dever, “Archaeology and the ‘Age of Solomon’: A Case Study in Archaeology and Historiography,” in *The Age of Solomon: Scholarship at the Turn of the Millennium* (ed. Lowell K. Handy; SHCANÉ 11; Leiden: Brill, 1997), 217–51; Dever, *What Did the Biblical Writers Know*, 131–38; William G. Dever, “Visiting the Real Gezer: A Reply to Israel Finkelstein,” *TA* 30 (2003): 259–82; Amnon Ben-Tor, “Hazor and the Chronology of Northern Israel: A Reply to Israel Finkelstein,” *BASOR* 317 (2000): 9–15; Lawrence E. Stager, “The Patrimonial Kingdom of Solomon,” in *Symbiosis, Symbolism and the Power of the Past: Canaan, Ancient Israel and their Neighbors from the Late Bronze Age through Roman Palestine* (ed. William G. Dever and Seymour Gitin; Winona Lake, Ind.: Eisenbrauns, 2003), 63–74; David Ben-Shlomo, Itzhaq Shai, and Aren M. Maeir, “Late Philistine Decorated Ware (‘Ashdod Ware’): Typology,

The individual arguments are varied and many. Leading the way in this regard is Amihai Mazar, an outspoken opponent of the LC, who has been active in his efforts to demonstrate the issues with Finkelstein's chronology. Yet at the same time, the arguments raised by the LC have not been entirely dismissed by Mazar either;⁴⁷ he recognizes that there are some issues with the traditional chronology as well.⁴⁸ In fact, it was the "results of the archaeological work of the 1990s and renewed analysis of various sites" that led to an additional chronological development—Mazar's *Modified Conventional Chronology* (MCC).⁴⁹

As its name indicates, the MCC is not a radical altering but rather a minor adjustment and modification to the traditional chronology. It posits a longer duration for the Iron IIA period, covering most of the tenth and ninth centuries B.C.E.⁵⁰ While the MCC fits well enough with most of the archaeological data for this period, it is perhaps not saying much due to the ambiguity it implies—of course it fits the data better if we do not have to be specific as to what century a particular strata dates to. (In the context of Megiddo, then, with the MCC "the door is left open to date Megiddo Stratum IVB–VA to either the tenth or the ninth centuries."⁵¹) But Mazar is not oblivious to this uncertainty as he admits that the specific assignment of remains to either the tenth or the ninth century B.C.E. is obscured in the MCC.⁵² Then again, such flexibility might just be the point, and the reason why the MCC is becoming so appealing to more and more archaeologists (although many still adhere to the traditional chronology). As Mazar points out, the situation is "far from being 'crystal clear,'" and in a sense requires such openness—one reason why he criticizes the LC for its unbending assignment of all Iron IIA data to the ninth century B.C.E., and its emptying of the tenth of any notable remains.⁵³ For Mazar, the option of dating Iron IIA remains to either century is open, making the MCC "the most reasonable and acceptable chronology."⁵⁴

Chronology, and Production Centers," *BASOR* 335 (2004): 1–35; and Raz Kletter, "Low Chronology and United Monarchy: A Methodological Review," *ZDPV* 120 (2004): 13–54.

47. Amihai Mazar, "Does Amihai Mazar Agree with Finkelstein's 'Low Chronology'?" *BAR* 29, no. 2 (March/April 2003).

48. Based particularly on the pottery finds at the site of Jezreel (the same key site figuring prominently in Finkelstein's LC), he suggests a modification that extends the accepted Iron IIA chronology to the destruction of Jezreel ca. 840–830 B.C.E. See Mazar, "Iron Age Chronology," 157–67.

49. Mazar, "Debate Over the Chronology," 21. To be sure, like the LC, Mazar's chronology is not an entirely new development either, but is a modification of some earlier proposals (see Mazar, "Debate Over the Chronology" 16, Table 2.1).

50. Mazar, "Debate Over the Chronology," 21–26.

51. Mazar, "The Spade and the Text," 149.

52. Mazar, "Debate Over the Chronology," 26.

53. Mazar, "Debate Over the Chronology," 26.

54. Mazar, "Debate Over the Chronology," 26.

Concerns, Clarifications, and Observations

With the expectation that the details of Megiddo—along with those of the two chronological developments adumbrated above—offer at least a flavor to the archaeological issues and how they are engaged by scholars, it is perhaps appropriate at this point to offer a few critical comments that relate to the chronological controversy, as well as a reflection on some implications of the data. In the interest of brevity, a short summary of some substantial, general concerns will be touched upon (with the footnotes providing further detail and examples), followed by some observations.

To be sure, both the LC and the MCC are viable paradigms for different archaeologists, and both are important contributors to the current chronological debates. Yet serious concerns remain with all chronologies, and resolving the issues is far from a simple matter. The following key considerations (one could say “problems”) highlight the inconclusive nature of any interpretation.⁵⁵

The Problem of Dating

Beyond the fact that there are virtually no *solid* historical anchors in the tenth and ninth centuries B.C.E. with which to date any archaeological remain with any degree of accuracy, most dating alternatives also pose their own problems.⁵⁶ Pottery, for example, has its limitations in that, for a specific region, the data is obfuscated by questions such as how quickly a new type may have spread to other areas (or if they did at all in the case of poor backwaters), whether old types survived simultaneously and for how long, how to account for the tension between local fashions and regional tendencies, and so on. All make it difficult to compare the remains between many sites and determine an appropriate pottery sequence for the entire region.⁵⁷ With respect to architectural remains—such as city gates or royal palaces—things fare no better. Architectural fashions at one site are often dated relatively by their comparisons to those at other sites. However, such parallels are insufficient in particular respects and there are good reasons why each must be dated on

55. Some of these issues (and others) can be found expressed similarly in Grabbe, *Ancient Israel*, 12–15.

56. For a brief summary of the archaeological dating methods and their problems, see B. S. J. Isserlin, *The Israelites* (New York: Thames and Hudson; repr. Minneapolis, Minn.: Fortress Press, 2001), 17–19.

57. Since similarities in pottery assemblages between one-period Jezreel (ninth-century B.C.E.), and those found in the palaces at Megiddo, are a key factor in Finkelstein’s dating of Megiddo VA–IVB to the ninth-century B.C.E., this is not an insignificant matter. In fact, it may well be, as suggested by Ussishkin, that the monumental palaces of Megiddo VA–IVB were originally built in the tenth century B.C.E., and continued in use into the ninth century (see Finkelstein, Ussishkin, and Halpern, *Megiddo III*, 600). This could explain the existence of similar pottery in the royal compound at Jezreel and the palaces at Megiddo, and not necessarily require Megiddo VA–IVB to date to the same period as Jezreel. Thus, one can still date the Megiddo gate to the ninth century (if part of Stratum IVA) without necessarily adhering to LC.

their own merits.⁵⁸ Floor remains and building construction too are just as inconclusive.⁵⁹ Even radiocarbon dating, while promising, is not without its limitations and requires further developing.⁶⁰

58. To put it frankly, similar gate types—whether six-, four-, or two-chambered—are not enough to draw an unequivocal association between any two, based on similar construction alone (at least not with the preciseness needed in many cases). True, the six-chambered gates at Megiddo, Hazor, and Gezer, are all similar and may all be associated with one another. But what about the six-chambered gate at Lachish that likely comes from a later period and has a peculiar similarity to the gate at Megiddo, or the same type at the Philistine city of Ashdod which was clearly not built by Israelites? (On this see Ussishkin, “Solomonic City Gate,” 17.) Furthermore, if we are to declare connections and similar dating between gates because of architectural similarities, then what are we to do with the four-chambered gate at Megiddo that follows the six-chambered one? Arav would have us use the mid-ninth century, four-chambered gate at Bethsaida as a paradigm for the four-chambered gate at Megiddo to assert that it should be dated to the ninth century and thus that the six-chambered gate should be pushed back to the tenth (see note 41). If so, then what about the recently excavated four-chambered gate at Khirbet Qeiyafa, which seems to be from the tenth century? Should we then attempt to associate the four-chambered gate at Megiddo to the tenth century, pushing the six-chambered “Solomonic” gate that preceded it even earlier? As the association of the Megiddo, Hazor, and Gezer gates was the original linchpin of traditional tenth-century chronology, these types of concerns should not be taken lightly and should, at the very least, not be used as a solid method for constructing a chronology. (Similar to this discussion, for a relevant study which calls into question the similarities of these three gates, see David Milson, “The Design of the Royal Gates at Megiddo, Hazor, and Gezer,” *ZDPV* 102 [1986], 87–92.) Moreover, comparisons between the gates should not “presuppose a centralized building program, and by extension some sort of centralized building government” (J. Maxwell Miller and John H. Hayes, *A History of Ancient Israel and Judah* [2d ed.; Louisville, Ky.: Westminster John Know Press, 2006], 203). To speak briefly of another important architectural fashion, the same goes for the monumental palaces. Finkelstein wishes to down-date Megiddo VA–IVB to the ninth century B.C.E. because of similar architectural features and building plans—including similar mason marks—between its ashlar palaces and the ninth-century palace at Samaria. Yet as Mazar points out, “this resemblance can be explained if we assume that both kings—Solomon and Ahab—used Phoenician masons” (Mazar, “The Debate Over the Chronology of the Iron Age,” 21). The point is that while it may be instructive to draw correlations—and some of those may be entirely correct—each gate, palace, or whatever must be evaluated on its own stratigraphical and other terms, and any similarities must not be used as *decisive* chronological markers—unless, of course, the architecture is accompanied by an inscription!

59. Provided certain finds can even be dated historically, interpretational problems with the find’s relation to a building’s floor obscures obtaining a precise construction date for the building that it may be associated with. In other words, should the construction date for a particular building be determined by the finds *below* or *in* the floor, or by those deposits found *on* the floor? “A compromise between these positions,” notes Isserlin, “may be the best solution. Finds below or in floors may be much earlier than the date when the buildings concerned were erected; on the other hand, since floor deposits often represent the last rather than the initial phase of use, they may be rather later. Together, deposits below, in, and on floors may thus offer a time span of variable length; a more exact construction date within these limits may sometimes be suggested by historical or other considerations, but all too often it remains indeterminate” (Isserlin, *The Israelites*, 18–19).

60. As the calibration process can only yield approximate dates, it is perhaps enough to say here that the recent attempts to use ¹⁴C dating have not been conclusive or perfect enough to assign a precise date so as to clarify the debates over dating. Ironically, the imperfect nature of the results is admitted by those who would use the data, yet they nonetheless forge

The Problem of Jerusalem

In its essence, the problem is a simple one: if Jerusalem was the center of a full-fledged Israelite state with a strong centralized government marked by the United Monarchy of David and Solomon, why is this not borne out in the archaeological record? Of course, such phrasing of the issue hides the complexities of the historical situation of Jerusalem during the tenth–ninth centuries B.C.E., which is not always conveyed well amidst the polemic that occurs on both sides of the debate.⁶¹ As well, it runs the risk of amounting to an argument from silence,⁶² and further archaeological discoveries may offer vindication for this important city.⁶³ Still, when due consideration is given

ahead and declare that it supports their particular chronology (e.g., Finkelstein, Mazar, and others). See, for example, the various contributions to the following volume: Thomas E. Levy and Thomas Higham, eds., *The Bible and Radiocarbon Dating: Archaeology, Text, and Science* (London: Equinox, 2005).

61. In reality, Jerusalem does offer some archaeological evidence for this period, but the definitions and terminology used to describe the nature of the Jerusalem's politics (e.g., chiefdom, flourishing political center, royal capital, and so on) can mean different things to different scholars. It may not have been the great, flourishing political center described in the biblical text, but it was certainly no mere "cow town." While Jerusalem might be best referred to as a "chiefdom" during the tenth–ninth centuries B.C.E., which only gradually developed into a fully-mature state in the eighth century B.C.E., for ancient rulers and peoples, it was the "governing center of the tenth-century B.C.E. kingdom." On this see Nadav Na'aman, "Cow Town or Royal Capital? Evidence for Iron Age Jerusalem," *BAR* 23, no. 4 (July/August 1997): 43–47. What is important is that however the phrase "kingdom" might be defined today, tenth-century Jerusalem was likely not the glorious kingdom depicted by later biblical authors.

62. This is the main criticism of Jane Cahill, who admits that even though "no archaeological remains in Jerusalem can be identified confidently with any of the structures named in the Bible," the negative evidence should not be preferred over "positive evidence" (see Jane M. Cahill, "Jerusalem at the Time of the United Monarchy: The Archaeological Evidence," in *Jerusalem in Bible and Archaeology: The First Temple Period* [ed. Andrew G. Vaughn and Ann E. Killebrew; SBLSymS 18; Atlanta, Ga. Society of Biblical Literature, 2003], 54, 73).

63. For instance, despite the controversy that surrounds it, the building being excavated by Eilat Mazar could provide promising results (see Eilat Mazar, "Did I Find King David's Palace?" *BAR* 32, no. 1 [January/February 2006]). As also could some of the recent research on metallurgical activity being pursued by Thomas Levy in southern Jordan, where there is evidence of a tenth–ninth century B.C.E. industrial-scale copper industry at Khirbat en-Nahas and nearby Rujm Hamra Ifdan. Regarding this, however, additional research is required to determine what, if anything, it had to do with a tenth-century Judean kingdom under Solomon (i.e., the tradition of "King Solomon's mines") or if it was a production center controlled by ancient Edom (see Thomas E. Levy et al., "High-Precision Radiocarbon Dating and Historical Biblical Archaeology in Southern Jordan," *Proceedings of the National Academy of Sciences* 105, no. 43 [October 2008]: 16,460–16,465). A dramatic example is the archaeological excavations now underway twenty miles southwest of Jerusalem at Khirbet Qeiyafa, an early Iron IIA massively fortified city that appears to date to the tenth century B.C.E. and may be part of an early Judean kingdom. (Interestingly, it has a four-chambered gate connected to a casemate wall that surrounds the city, offering some additional evidence for the problems of gate types and architectural comparisons as a dating method [see note 58]). In any case, its current excavators have already asserted that the "traditional view [that] points to a single powerful centralized authority in Jerusalem

to the evidence currently available, it must be admitted that at this point, Jerusalem remains an enigma.⁶⁴

The Problem of the Biblical Text

If the scholarship of the last century or so has shown us anything, it is that the biblical text in its present form is not history in the modern sense, though this is a far cry from declaring the text completely worthless. Indeed, this can be said to be *the* problem of the biblical text. Yet, without the biblical text we would know almost nothing about the emergence of the Israelites, or even where to look for them exactly; rhetoric aside, there is good evidence that the biblical text has traces of history embedded within it that, *properly used*, can provide a useful framework for evaluating archeological or historical data. As a late and biased text, however, its proper use is a matter of debate.⁶⁵

that controlled the entire country” is now vindicated by the evidence at Khirbet Qeiyafa, “proclaim[ing] the power and authority of a centralized political organization, namely a state” (Yosef Garfinkel and Saar Ganor, “Khirbet Qeiyafa: Sha’Arayim,” *Journal of Hebrew Scriptures* 8, no. 22 [2008]: 5). Additionally, they have claimed that the “low chronology is now officially dead and buried” (presentation of Garfinkel and Ganor at the 2008 ASOR conference found here: Yosef Garfinkel and Saar Ganor, “Khirbet Qeiyafa: An Early Iron IIA Fortified City in Judah,” http://qeiyafa.huji.ac.il/qdb/ASOR_2parts.pdf). However, not all scholars are prepared to accept such certainty at such an early stage. Though it is a site of potentially great importance, the entity responsible for its fortifications, who the resident population was, and how it all may relate to an early Judean kingdom, remains to be seen. In fact, Nadav Na’aman questions the declaration that it was a “Judahite stronghold on the border of Philistia” and, based on several reasons, entertains the notion that it may have been “connected to the neighboring lowland kingdom of Gath” (Nadav Na’aman, “In Search of the Ancient Name of Khirbet Qeiyafa,” *Journal of Hebrew Scriptures* 8, no. 21 [2008]: 2–3). See also Na’aman’s rebuttal of Garfinkel and Ganor’s article in his “Shaaraim—The Gateway to the Kingdom of Judah,” *Journal of Hebrew Scriptures* 8, no. 24 (2008): 2–5.

64. The problem of Jerusalem is summarized by Grabbe: “When one considers the *longue durée*, it would have been extraordinary for the Judaeen highlands to dominate the north in Iron I or IIA. A number of archaeologists argue that the archaeology does not support the text which depicts a Judaeen-highland-centered united monarchy. Those who do argue for archaeological support for the united monarchy generally do so by explicit—or implicit—appeal to the biblical text as the guide for interpreting the archaeology. Jerusalem remains an area of considerable controversy, but those who maintain that Jerusalem did not develop into a substantial city until Iron IIB have current archaeology on their side . . . Those who maintain an earlier development must argue on the basis of what is presumed to have disappeared or what might be found in the future. This is why a substantial argument is now made that the northern kingdom (in the form of the Omride dynasty) was the prior development to a state in the ninth century, with Judah coming along more slowly, reaching its height only in the eighth century. But the debate continues” (Grabbe, *Ancient Israel*, 76–77).

65. It is no secret that the archaeology of Palestine started out as the archaeology of the Bible (and in some ways still is). Moreover, in many ways it was this “biblical archaeology” that profoundly influenced the development of the traditional chronology. For example, there is no doubt that the biblical text was a dominant factor in the interpretation of the gates at Megiddo, Hazor, and Gezer (e.g., 1 Kgs 9:15). And, indeed, it is such influence that is a main complaint of Finkelstein, and the reason why he views his own work as “part of a quest to emancipate Iron Age archaeology from Bible archaeology” (Israel Finkelstein, “Bible Archaeology or Archaeology of Palestine in the Iron Age? A Rejoinder,” *Levant* 30 [1998]:

Observations

Reflecting on the brief considerations above, perhaps the first observation that can be made is that no chronology is immune to criticism; nor is any one of them free from the myriad problems inherent in developing an archaeological framework and a sound historical interpretation. The problems this poses for reconstructing the biblical past of the tenth–ninth centuries B.C.E.—our immediate concern—almost need not be mentioned. The current gaps in our knowledge, and the complications encountered when utilizing archaeology to reconstruct an historical past, prevents the perfecting of a chronology of Iron Age Palestine that accounts for all the data, and is precise enough to make confident historical judgments.

What this all means for our discussion of state formation in the biblical world and the nature and extent of a united kingdom of David and Solomon is clear: the present situation precludes us from creating an unassailable picture of the emergence of the Israelite state(s). So, as Mazar would claim, “the archaeology of the United Monarchy’ remains a legitimate possibility, though not mandatory.”⁶⁶ In contrast, Finkelstein would counter that it was certainly not a united monarchy at the insignificant site of Jerusalem in the tenth century B.C.E., but rather northern Israel in the form of the “forgotten first kingdom” under Omri and Ahab in the ninth century B.C.E., that “had the necessary organization to undertake monumental building projects, to establish a professional army and bureaucracy, and to develop a complex settlement hierarchy of cities, towns, and villages—which made it the first full-fledged Israelite kingdom.”⁶⁷ Either historical situation is plausible in the absence of any further defining elements.

This leads us to a final and vital observation, suitably summarized by two historians of Israel:

In the final analysis, one’s interpretation of the archaeological evidence depends heavily upon the degree of confidence that one places in the biblical profile of Solomon. If one begins with the biblical vision of

167). Yet, he is also justly criticized for his own use of the biblical text to validate his theories and supply his dating scheme (e.g., his use of the text in evaluating and dating the palace at Jezreel to Ahab and the palace at Samaria to Omri [1 Kgs 21:1]), which leads us to the reality that, like it or not, its use simply cannot be avoided. It can and must, however, be used cautiously. While it may or may not justify his use of specific biblical verses, Finkelstein’s reply is nonetheless a responsible handling of the problem of the biblical text: “The biblical material cannot be treated as a monolithic block. It does not require a take-all-or-leave-all attitude. Two centuries of modern biblical scholarship have shown us that the biblical material must be evaluated chapter by chapter and sometimes verse by verse. The Bible includes historical, nonhistorical, and quasi-historical materials . . . So, yes, one may doubt the historicity of one verse and accept the validity of another” (Finkelstein and Silberman, *The Bible Unearthed*, 343–44). The “lateness” of the biblical text, and whether the books of Kings can be considered as reliable evidence of Iron Age Palestine, is an issue dealt with by Jens Bruun Kofoed in a chapter from his *Text and History: Historiography and the Study of the Biblical Text* (Winona Lake, Ind.: Eisenbrauns, 2005), 33–112.

66. Mazar, “Debate Over the Chronology,” 21.

67. Finkelstein and Silberman, *The Bible Unearthed*, 169–70.

Solomon as a powerful ruler and great builder, then it makes sense to credit him with the fortifications at Megiddo, Hazor, and Gezer, along with roughly contemporary architectural remains at other Palestinian sites, and to see these cities, towns, and villages as belonging to a Jerusalem-centered government. . . . On the other hand, if one is not convinced in advance by the biblical profile, then there is nothing in the archaeological evidence itself to suggest that much of consequence was going on in Palestine during the tenth century B.C.E. and certainly nothing to suggest that Jerusalem was a great political and cultural center.⁶⁸

Admittedly, the total deconstruction of at least some sort of historical kingdom under David and Solomon may be taking things too far,⁶⁹ it must be said that the biblical text in its present form is certainly an idealized depiction of a glorious past from a southern Judean point of view—the reality likely being that David and Solomon’s kingdom was a small, developing entity with a short-lived power that may have *temporarily* extended well beyond the borders of its center at Jerusalem.⁷⁰ By no means, however, does the evidence indicate a fully mature state the likes of which developed under the Omrides in the ninth century B.C.E.

Conclusion

By now, after a focused look at an important Iron IIA site, coupled with a general review of the wider discussion it is a part of, the complexities involved in the debates over the archaeology and chronology of Iron Age Palestine as well the implications that can be drawn therefrom, should be glaringly apparent. With its particular importance and impact on Iron IIA chronology, the site of Megiddo and the controversy surrounding its monumental gate offer a context within which to explore larger issues. Yet the review of Megiddo was only an inroad into the expansive subject matter; it by no means exhausts the array of issues and details involved, hopefully evident from the brief discussion of the current chronological crisis and the problems facing it.

In truth, from the lack of indisputable historical anchors, the ambiguity of stratigraphical layers, the problems of comparing archaeological remains, and the difficulties of using the biblical text as a source of positivistic history, to the differences of interpretation and the ever-evolving views of archaeological and historical interpretations of the data, the current crisis of Iron Age chronologies seems to forestall a resolution on the immediate horizon, as Knoppers stated:

68. Miller and Hayes, *A History of Ancient Israel and Judah*, 203–4. It should be noted that Miller and Hayes nevertheless “resist” the LC but also “recognize . . . that under the Omrides ancient Israel reached a level of economic and political strength unprecedented in its history” (311–12).

69. At the very least the Tel Dan inscription is solid evidence in itself of the existence of an historical figure named David who was credited with being the founder of a dynasty of Judean kings. For this find, see Avraham Biran and Joseph Naveh, “An Aramaic Stele Fragment from Tel Dan,” *IEJ* 43 (1993): 81–98; and Avraham Biran and Joseph Naveh, “The Tel Dan Inscription: A New Fragment,” *IEJ* 45 (1995): 1–18.

70. Na’aman argues in this direction in his “Cow Town or Royal Capital?” 43–47.

“The only present certainty is that the age of consensus is past.”⁷¹ A synthesis of archaeological evidence and historical information is what must be done to provide an accurate depiction of the past, yet such cannot be done without a solid archaeological framework that accounts for all of the disparate data. At the moment such does not exist and contributes to the presence of what seems to be truly irresolvable differences. However unfortunate it may be, reality tells us that “as long as no new additional data are available it would be impossible to solve the chronological differences being debated at present.”⁷² Perhaps this is why—despite the ambiguities it allows in its flexibility of dating remains to either the tenth or ninth centuries B.C.E.—the MCC is, in my mind, the most appealing chronological development. Without new data, such uncertainty may be as close as we can get to secure historical interpretations for this period of the Iron Age in the southern Levant.

71. Knoppers, “The Vanishing Solomon,” 44. The cogency of such a statement is perhaps more acute today than when stated a little over a decade ago.

72. Ussishkin, “Archaeology of the Biblical Period,” 139.