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The Future of Biblical Archaeology: Reassessing Methodologies and Assumptions


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“Minimalism” and the Context of Scripture: Reassessing Methods and Assumptions—A Review and Reply

Israel’s Ancestors, Reprise” (193–232); Richard S. Hess, “Multiple-Month Ritual Calendars in the West Semitic World: Emar 446 and Leviticus 23” (233–53); K. Lawson Younger, “The Repopulation of Samaria: 2 Kings 17:24, 27–31 in Light of Recent Study” (254–80); Cynthia L. Miller, “Methodological Issues in Reconstructing Language Systems from Epigraphic Fragments” (281–305); John M. Monson, “The Role of Context and the Promise of Archaeology in Biblical Interpretation from Early Judaism to Post Modernity” (309–27); Richard E. Averbeck, “Ancient Near Eastern Mythography as it Relates to Historiography in the Hebrew Bible: Genesis 3 and the Cosmic Battle” (328–57); David B. Weisberg, “‘Splendid Truths’ or ‘Prodigious Commotion’? Ancient Near Eastern Texts and the Study of the Bible” (357–67); Andrew G. Vaughn, “Can We Write a History of Israel Today?” (368–85); no indices or bibliography.

This volume publishes the proceedings of a symposium, held on August 12–14, 2001, at Trinity International University, whose expressed purpose was to address a crisis in “biblical archaeology.” In the preface by James Hoffmeier and Alan Millard, the onset of this crisis is attributed to two developments in the fields of archaeology and biblical studies: (1) Palestinian archaeology’s increasing separation from biblical studies and (2) an “attack” on the principles of biblical archaeology from “biblical scholars, often known as historical minimalists, who have been inspired by post-modern literary approaches and who tend to trivialize, ignore or misuse archaeological data” (xi). Together these developments have created a “climate of growing scepticism towards biblical archaeology and the historical worth of the Bible’s narratives” (xi). The interrelationship of these two developments and a brief sketch of the principal stages of these scholarly debates reiterate the perspective of William Dever, especially as expressed in his What Did the Biblical Writers Know? of 2001. Speakers were invited to this symposium on the condition that they held a “positive attitude” to the Hebrew Bible and would examine it “in the light of archaeological data from the ancient Near East” (xii). The first condition of participation restricts a speaker’s intention, perspective, and understanding. One must wonder about those implicitly excluded from the symposium: those with “negative” attitudes. Is a critical conclusion regarding the historicity of a biblical narrative the same as having a negative attitude toward the literature? As we will see, few of the papers reflect much knowledge of recent debates on historical method (see the series edited by L. L. Grabbe, European Seminar in Historical Methodology, 1997–), and few appear to have actually read those biblical scholars “known as historical minimalists,” in opposition to whom participation in the symposium is directed by its organizers. The present writer is one such scholar, and I write this more as a response than a review (pacem, Hagelia).

It has been now more than forty years that the intellectual restrictions of my childhood’s Catholicism has influenced my work in biblical studies, and I have since grown deeply committed to the European traditions of academic freedom. I must, therefore, protest the
restrictive and directive conditions imposed on the participants of this symposium. That the symposium’s organizers, moreover, use caricature and anonymity in speaking of those whom the congress is explicitly assembled to oppose raises related questions of scholarly ethics: How narrow and theologically restrictive was the conference? How directed were its conclusions? How uncritical is its plea for a return to a biblically oriented archaeology? The symposium’s unabashed opposition to openness makes me question whether we have in this volume a product of ideologically directed evangelical apologetics or a contribution to the wider world of scholarship. Neither alternative, as we will see, can be given an unequivocal answer.

The opening section of five papers presents perspectives on the “debates” surrounding the two issues of biblical archaeology and historicity and proposes changes for the future of biblical archaeology. The discussion of a more secular “Syro-Palestinian” archaeology refers primarily to William Dever’s well-known critique from the mid-1970s of “biblical archaeology” as an academic discipline. Ziony Zevit opens with an American-oriented discussion of the debate that followed Dever’s original proposals in 1973–1974. Zevit discusses both semantic and theologically oriented political problems within the American Schools of Oriental Research. Unfortunately, he does not deal with other American perspectives nor with parallel developments in the more secular environments of Israeli, Jordanian, or European archaeology, where a similar but far less tendentious transition has occurred, with less debate over the term “biblical archaeology” itself.

Thomas Davis follows with a paper about a second “debate” and introduces “minimalism” to the audience. His paper is less clear than Zevit’s and even less aware of the Atlantic divide. He suggests that both the symposium and his own paper present a “reasoned inter-faith response to the minimalist approach to biblical archaeology” (20), yet he makes no effort to explain what this “approach” might be. Rather, he contends that the methods of Albright, Wright, Glueck, de Vaux, and Dever stand together on one side of the “debate” and opposed to the approach of Finkelstein, who is given to represent a “minimalist” perspective (26; see, however, Dever, “Histories and Non-histories of Ancient Israel,” and Finkelstein, “From Canaanites to Israelites,” pp. 40–42 and 11–13, respectively in M. Liverani, ed., Recenti Tendenze nella Ricostruzione della Storia Antica D’Israele, 2005). Unlike Dever and Finkelstein themselves, Davis concludes that we all “share the same theoretical and methodological approach to the archaeological record.” Although he argues that differing biblical interpretations and not archaeology underlie the debate, his surprising conclusion that archaeological methods must then change hardly follows (26–28)!

David Merling offers a summarizing discussion that centers on the rejection of the historicity of Joshua by Dever (2001) and J. Maxwell Miller (Israelite and Judean
History, 1977). Merling stresses the obvious difference between judgments that stories are not verifiable or are unhistorical and judgments that they are not true or are in error (32). He also asserts but somewhat overstates the argument that a lack of evidence cannot be used to support any conclusion (33). His logic is simple but well taken, yet his virtual debate on Ai hardly carries the historical discussion further than it was in 1977. Significantly, he shows no interest in Joshua’s narrative about the conquest of Ai. Uninterpreted and undefined, it stands as his alternative to an archaeologically oriented question. With Davis, Merling prefers biblical story to history and rejects any archaeology—“biblical” or otherwise—that does not support his preference.

The paper of Randall Younker is an advertisement for the Andrews University Hesban project. It is promoted as a model for integrating faith, the Bible, and archaeology, but no explanation is given as to how it does this. A clear and convincing case is made that this excellent project offers a respectable form of archaeology. On the basis of what is described, the relationship of the project to faith and the Bible was irrelevant to the project’s success.

James Hoffmeier’s paper, closing this section of the symposium, offers a similar advertisement for the North Sinai project, which organized the symposium. We are told that it is a model for “biblical archaeology,” but we are not told how this is so. The promotional work includes a very brief introduction (53–54) on “the current state of biblical archaeology” in which the audience gets its first recognizable glimpse of “minimalists” as “sceptics, disproportionately dominating the discourse of biblical archaeology.” Named are Thomas L. Thompson, John Van Seters, Niels Peter Lemche, Giovanni Garbini, Philip Davies, and Gösta Ahlström. Having already described “minimalists” in his preface (xi) as “post-modernists who trivialize, ignore or misuse archaeological data,” Hoffmeier places the beginning of this development of biblical studies in my dissertation (The Historicity of the Patriarchal Narratives, Tübingen, 1971, publ. 1974) and Van Seters’s monograph (Abraham in History and Tradition, 1975). Although he does not state that these are the very works he refers to as trivializing, ignoring, or misusing archaeological data, the absence of other references suggests that either these or other of the contributions of “minimalists” to the field of biblical archaeology are trivial, ignorant, and abusive postmodern works. Does Hoffmeier shy from citing, for example, Garbini’s I Filistei (1980), Lemche’s Early Israel (1985), Ahlström’s A History of Palestine (1993), or Davies’s The Complete World of the Dead Sea Scrolls (2002)? If I have guessed wrongly about the identity of the offending works, why is it that Hoffmeier and other contributors to the symposium who present “responses” to this pernicious stream of scholarship do not give clear references that their readers might separate such chaff from the good grain?
The misrepresentation of alternatives to a biblically centered archaeology—both tacit and explicit—in this opening section of the symposium hardly improves in the following four papers dealing with archaeological approaches and their applications. “Minimalism” continues in its role of global opponent, while arguments actually written by any of the scholars referred to by this epithet find neither description nor response. Edwin Yamauchi opens this section by citing Dever’s claim that “minimalists” neglect Homeric studies and an alleged historicity of the “tales of the Trojan Wars.” Dever, in contrast, links such “tales” to history’s “Sea Peoples” and the Bible’s “Philistines.” Yamauchi views Dever’s assertion of a long oral tradition, preceding the writing of an approximately eighth-century Homeric epic, as an important contribution to the refutation of “minimalism.” Yamauchi himself is satisfied with modestly updating discussions about historicity in Homeric studies from the 1960s and early 1970s to draw the unexceptional conclusion that archaeology neither disproves nor proves the *Iliad*'s historicity. His understanding of “minimalists” is entirely dependent on Dever. Lemche, however, was a student of Eduard Nielsen (*Oral Tradition*, 1964), and Van Seters traced the origins of the Pentateuch’s Yahwist to oral tradition (*Abraham in History and Tradition*; see also Thompson, in *Israelite and Judean History*, 1977, 149–212). Van Seters and I also debated biblical roots in oral tradition through the late 1970s and 1980s (most recently in Van Seters, *A Law Book for the Diaspora*, 2003, 29–46; Thompson, *The Messiah Myth*, 2005, 21–26). Yamauchi himself attributes to oral tradition no more than Jens Kofoed had in his book (*Text and History*, 2005, 58–68), namely, that a scenario of combined oral and written transmission from projected event to extant text has not been shown to be impossible in Homeric or biblical studies (89). This is a conclusion with which most critical scholarship has agreed since the 1960s! It is well recognized that the biblical stories resemble Homer in many ways—as they do the Gilgamesh Epic and many other narratives, some of which have roots in oral tradition. It is not oral tradition’s link with written traditions that is lacking, but with alleged historical events. In spite of many delightful parallels in the *Iliad* to the tales of David, Goliath, Samson and Saul—parallels to which Cyrus Gordon directed our attention a half-century ago (C. H. Gordon, *HUCA* 26 [1955]: 43–108)—these are all parallels between written texts. There is no argument, however, that has seriously linked both the history of Palestine and biblical “Philistines” with Homer’s story world, nor does Yamauchi—in spite of his use of Dever to give legitimacy to his paper—refer to any.

Benjamin Scolnic’s paper presents a classic effort to identify Migdol of Exod 14:2. Identifying himself as a “maximalist,” he offers an amusing parody of an antiminimalist: one who ignores and dismisses nothing (91)! While I have nothing whatever against the four principles governing his methods of site identification (and have used similar considerations in my own work [*Toponomie Palestinienne*, 1988]), I am not convinced
that Scolnic’s methodological interests extend much beyond a coincidental concurrence of geography and nomenclature, with a very flexible assumption of historical plausibility based on geographical realism and the acceptance of an implied chronology, but not on the historicity of the narratives in which a site is mentioned (93). The biblical narratives, however, are not discussed in regard to their form or function, let alone their realism. Neither is the chronology supposedly implied by narratives questioned or discussed. One is encouraged to accept that the biblical site is to be identified with a known geographical location because a projected scenario that Scolnic imagines contains a high frequency of coincidences and allows him to historicize the biblical story within one of the periods in which Archbishop Ussher’s translation of the Masoretic chronology suggests.

Steven Ortiz’s article dealing with Iron Age chronology and the biblical trope of the united monarchy opens with the charge that Niels Peter Lemche, Philip Davies, and I have ignored or misused archaeological data in our discussions about “the nature of the Israelite monarchy.” Although I do not think any of the works he refers to discuss the nature of Israelite monarchy—although they do discuss the historicity of the biblical narrative—I appreciate his effort to refer to things we actually have written. However, beyond this opening citation, he does not deal with them. He rather uses us much as 1 Kings used Jeroboam. “These trends,” he surmises, “have now been adopted by Finkelstein” (121), who not only is—with Herzog and others in Tel Aviv—interested in the nature of kingship but also has questions about the pottery chronology that is so dear to Ortiz’s heart. Ignoring Ussishkin, Herzog, and others and having blackened Finkelstein for having walked in the path of the “minimalists,” Ortiz worries, in a rhetorical flourish, whether Finkelstein “could be right and everybody else wrong.” He does not mention that there is anyone with him in his “everybody” but enters directly into an interesting discussion of pottery chronology in support of three conclusions: (1) that there are regional variations in the material culture during Iron Age I; (2) that there was considerable ethnic and regional diversity during this period; and (3) that the nature of David’s kingdom cannot be defined as a tribal chiefdom. Unwilling as he is to walk in the path of a “minimalist,” he is certainly unaware that the first two of these conclusions were drawn more than ten years ago (Thompson, The Early History of the Israelite People, 1992, 221–300; 301–38, respectively). His third conclusion, however, does not follow from his arguments. It is not a historical but an exegetical question and remains wholly unaffected by his debate with Finkelstein over the dating of Iron I pottery! David of the Davidic kingdom is a complex literary figure who takes on significantly different roles, for example, in 1–2 Samuel, 1 Chronicles, the Psalter, and Isaiah. In none of these biblical texts, however, is he given the role of a tribal chieftain. The plot line of 1 Sam 16 presents David not as clan leader but as the youngest son among seven brothers. If this David of the world of stories is not a tribal chieftain, such a chieftain in a historical world...
must be someone else (Thompson, *Early History*, 108–11; idem, “History and Tradition,” *JSOT* 15 [1980]: 57–61)!

Alan Millard opens his paper by drawing a truly delightful fictive scenario paralleling the Amorites of Mesopotamia at the end of Ur III with the stories of Israelite settlement in the narratives of Joshua and Judges (148–49). He makes the point that the Mari texts can be used as an analogy for a process of sedentarization, much as I once argued in my discussion of these texts (*Historicity*, 87–88). The weakness of this as an analogy lies in its dependence on Alt and Noth’s understanding of the amphictyony as politically uniting early Israelite tribes, understood either as immigrating nomads or transhumant pastoralists. That it can serve as a historical example of what the stories of Joshua and Judges reflect, however, is not argued by Millard. His fictive scenario departs considerably from both biblical narrative and the current understanding we have today of the settlement of the highlands during the Iron I-II periods (N. P. Lemche, *JSOT* 4 [1977]: 48–59; I. Finkelstein, *The Archaeology of the Israelite Settlement*, 1988; Thompson, *Early History*, 1992, 215–300). Mari provides an analogy—not to Joshua and Judges—but to the Late Bronze/Iron I settlement and movements of peoples that scholars had imagined a half century ago to have been reflected in the biblical stories. This is very far from any historical reconstruction based on evidence (*Early History*, 34–47, esp. 44). Certainly Millard is right that one people can adopt the material culture of another without leaving much physical evidence of their origins (158), but we cannot include such unknown peoples in our histories. I also heartily agree with Millard that the “interpretation of texts has to be made within their own parameters” (160; as in *Historicity*, 1974, 328). It hardly follows, however, that “the reports ancient documents give should be credited” (160) as historically reliable accounts. Rather, such “reports” need to be understood within their own parameters (*Historicity*, 328)! Millard closes his paper and the second part of the congress with a religious claim that one who holds the Bible to be divinely inspired will understand “its record of past events to be true if correctly interpreted” (160). He adds the admission that his own faith affects the direction of his research. Without evidence, he also charges that the scholarship of more critical scholars is comparably distorted—a most serious and, to my knowledge, untrue allegation.

The symposium’s third section on the use of texts in biblical archaeology is appropriately opened with a paper by William Hallo on Sumer and the Bible. It is an *apologia* for Hallo’s departure from Landsberger’s insistence on the Eigenbegriﬄichkeit (conceptual autonomy) of ancient Mesopotamian culture. This illustrated and convincing defense of the translator offers convincing examples of motifs and genres that “often endure over millennia … across linguistic boundaries” (167): proverbs, riddles, letter-prayers, hymns, fables, disputations, debates, diatribes and others. While I think it is important to
maintain that Landsberger’s use of the term “canon” to refer to an implicit body of literature known to scribes (168–69) can be misleading and is far from the concept of biblical canon, Hallo’s more flexible understanding of the Sumerian canon as forming “the core of the curriculum of scribal schools, wherever Sumerian was taught” (169) is an extremely useful explanatory concept for literary transmission. His discussion of “casuistic” legislation, with its close parallels between Exodus and Deuteronomy in the Bible and Hammurabi and Eshnunna in cuneiform texts, casts serious doubt on efforts to restrict such transference to the sixth or any other given century. The principle supporting such legislation, “that the strong not oppress the weak,” Hallo finds, is a principle that is found across the entire spectrum of Mesopotamian literature. This principle, analogous to related tropes forming a cluster of ethical principles, which I have addressed with the term “the poor-man’s song,” is also significant in Egyptian literature and dominant in both the Psalter and Isaiah (Thompson, The Messiah Myth, 2005, 107–35; 323–35). I find Hallo entirely convincing in his insistence that the “five linguistic cultures” included in his Context of Scripture were inextricably linked with each other and with biblical literature (173).

The second paper in this section brings a touch of dissonance to Hallo’s congenial conclusions. Harry Hoffner, comparing what he refers to as “Israelite heritage” with Hittite texts, begins with a rather odd and insubstantial complaint about what he sees as the considerable neglect of comparative literature by biblical scholars. He suggests that this neglect is rooted in the influential “impression” that little of the biblical narrative is historical (176). However, he also charges biblical scholarship generally with striking neglect in this regard because, as he points out, the intricate associations that can be traced between the Bible and the literature of the ancient Near East are not in fact seriously affected by issues of either historicity or the dating of biblical texts. In a quite informative survey, Hoffner nevertheless draws the reader’s attention to a considerable and ongoing interest in Hittite texts, in spite of the fact that references to Hittites in Genesis cannot be linked to what we know of Hittites historically. Interest involving treaties, customs, rituals, and literary works persists among biblical scholars, and Hoffner makes several suggestions as to how the channel of Hittite cultural influence on the Bible can be exploited further. The concluding plea to make a concrete historical link, however, by finding a late second-millennium context for such cultural influence is weakened by the placement of Hoffner’s paper immediately after Hallo’s.

The title of Daniel Flemming’s paper echoes John Van Seters’s Abraham in History and Tradition. Accordingly, the paper opens with a charming apology that Flemming’s teaching and research interests are a bit out of date (193). Dissatisfied with conclusions that—like Millard—see the Mari material as analogously useful to biblical interpretation, he proposes a direct use of these documents for the evaluation of the Genesis traditions of
“Israel’s tribal ancestry.” He proposes four “points of contact,” namely, the name Benjamin, the city Harran as ancestral homeland, the biblical use of “Hebrew,” and the story of Lot and Abraham’s division of pastoral land (193–94). Flemming does not argue for the historicity of the biblical narratives but rather understands them as an interpretation of the past. After a brief but well thought-out survey of the debates over the historicity of the patriarchs since Albright (195–99), Flemming addresses Van Seters’s and my studies from the 1970s (199–202). He gives a concise outline of the issues raised by these two works and concludes that the two studies were effective as they pushed the “burden of proof” onto those who would see the second millennium as the historical context of the biblical narratives. Flemming’s neglect of the second half of van Seters’s book—where positive arguments for dating the narratives to the mid-first millennium—and of the later expansion of these arguments in In Search of History (1983), is unfortunate, as it has misled him and reduced his discussion to one of historicity alone. Given his careful survey, I find it surprising that he judges us to have been wrong in drawing a conclusion that Israel’s ancestors cannot fit the Bronze Age. We had not, in fact, drawn that conclusion and challenged such a possibility. As I understand Van Seters, his argument had been that the stories fit the later Iron Age better than the Bronze Age. I, on the other hand, disagreed with Van Seters’s relative dating of the traditions (JAOS 98 [1978]: 76–84) and insisted that there was a lack of evidence for dating them to either the Bronze Age or to the sixth century. Flemming takes up the issue regarding the nomadism of the shepherds at Mari. He departs from Jack Sasson’s judgment that Mari’s nomadism should be used analogously in biblical studies (RA 92 [1998]: 98–99). Instead, Flemming argues in a summary and unfortunately random and coincidental way, which places all too much importance on the belief that understanding of the Bible’s relationship to Mari has changed since the 1970s. It is far from certain that he can do more than build the analogies suggested then. The lack of evidence for his scenario is patent. Although he cautiously introduces Van Seters’s claim for the oral roots of the patriarchal narratives (see, however, Thompson, “A New Attempt to Date the Patriarchs,” JAOS 98 [1978]: 76–84), he asserts without argument that a Hosea must have had access to oral traditions in the eighth century! He takes this as a further warrant to conclude the obvious on the basis of a rhetorical chain of arguments without substance, namely, that oral traditions must also have been possible at an earlier time. Armed with the infinity of this possibility, he relates Genesis, portraying patriarchs in pastoral roles, with scenes implicit in Mari’s historical texts. Concluding—but not arguing—that the biblical narratives have an Israelite character and, therefore, seem to have been written in the Iron Age, he ignores the possibility of yet later dates and points to elements that he believes “can be explained only by an origin in the Bronze Age and not later” (214). This argument is not new. Although he points out correctly that my contention that the Mari designation of the Binu Yamina could not be understood as a proper name was mistaken, his conclusion that there
were ancient *Binu Yamina* in Israelite ancestry exceeds the evidence. I do not see that he has more than a translator’s analogous argument for this. Certainly he does not establish a historical connection. Rather, he points out—with Dossin already in the 1930s—that the patriarchal ties with Syria are in the region of Harran, where the ancient *Binu Yamina* had been during the Mari period, identifies the use of Hebrew with the term ‘*ibrum*’ at Mari—the dominant social category for the mobile pastoralist community (214–21)—and asserts that Gen 13’s division of land between Abram and Lot, to “the left” and to “the right,” reflects Mari roots (see Thompson, *Historicity*, 58–66). The remaining similarities are largely coincidental. I have always found the comparisons fascinating—although hardly convincing. Flemming has not distinguished similarities of social structures from linguistic parallels or chronological developments. He does not discuss the specific nuances of actual Mari texts or the literary nuances of biblical texts, let alone the issue of the historical relationship of these stories with an ancient Israel’s actual origins. In my dissertation I argued that the parallels were sufficient to build quite useful analogies to what scholars then thought concerning Israel’s origins as reflected in the stories of Joshua and Judges but that such analogies were insufficient to draw a historical relationship. The actual historical origins of the patriarchal stories have not been demonstrated. Flemming does not trace an unspecified oral tradition about Jacob from the eighth century back to the eighteenth. He is dealing with a very specific written narrative that has not yet found an appropriate context in the Iron Age, let alone in the Middle Bronze Age. Moreover, the relationship of such an analogy to the question of Israel’s origins is no longer as it had been. Nothing has been related—directly or analogously—with the historical origins of any of Palestine’s peoples, but only about characters in stories that scholars fifty years ago thought looked like sedentarizing nomads.

Richard Hess’s paper deals with cultic calendars and their relative chronology. He draws on thirteenth-century texts from Emar to suggest that the legal material of the “Holiness Code” (Lev 23) “preserve early material of great antiquity” (234). He eschews an effort to date Lev 23 early but seeks only to alter the description of the origins of the text and the development of its elements (235). Although I doubt our ability to date the origin of elements of texts on the basis of comparable texts (see my *Messiah Myth*, ch. 3), Hess’s argument that comparative analysis provides insight into the meaning and significance of the form and structure of texts (241) is a sound principle. His negative statement that variations in formula do not give evidence of later redaction (244) and that the presentation of apparently duplicate or similar texts side by side has precedence in the West Semitic traditions in both Emar and Ugarit (246) are similarly convincing. I also agree with his important conclusion that the analogy with Emar demonstrates that Lev 23 is not necessarily late, relative to other biblical cultic calendars, as well as that the common speculations about redaction are unwarranted (252). While Hess’s caution about
claiming historical relationships between elements of Lev 23 and earlier West Semitic texts is commendable, his conclusion that Emar gives evidence for dating Lev 23 (and possible rituals implied) to an earlier preexilic context is unwarranted. This spoils a fine paper. Again, evidence is not offered, but analogy. This is the most that the comparative method can provide.

K. Lawson Younger deals with the Judean story of Samaria’s repopulation in 2 Kgs 17, by asking two (in fact, three) quite limited questions: whether one or two Assyrian kings were involved and the identity of the peoples settled in Samaria and of the gods associated with them (254). Pointing out the archaeological continuity of the region, Younger turns to texts to suggest a more “comprehensive” view (than the Bible or archaeology offers) of new ethnic elements in Samaria. That is, he adds detail to what may be implied in the distinct texts we know. He suggests that there had been a two-stage process to Samaria’s resettlement: (1) the restoration of Samaria and (2) the building of secondary centers, some of which were previously uninhabited. By comparing the biblical texts with records for Sargon II’s and other deportations, Younger makes several interesting suggestions regarding the identification of the deities mentioned in 2 Kgs 17 (esp. the epithet Banit or Banitu, Sakkut, and Nabu) and gives some very useful information regarding the geographical location of place-names. The essay is quite carefully executed. He concludes that the biblical texts provide only incomplete information and that actual historical deportations implicit in 2 Kgs 17 may in fact have covered the entire period from Shalmaneser V to Assurbanipal. As Younger has restricted his interests to questions of name identifications during the Assyrian period, his never-argued assumption that 2 Kgs 17’s purpose is to describe such events is not obviously distorting. Yet the article seems truncated, and one misses an engagement with biblical scholarship on this interesting text, as well as an exegetical discussion of the narrative functions of 2 Kgs 17, especially about its markedly anti-Samaritan polemic (see I. Hjelm, Jerusalem’s Rise to Sovereignty, T&T Clark 2004, 75–78 and—including a discussion of Josephus—idem, The Samaritans and Early Judaism, Sheffield Academic Press, 2000, esp.192–97).

The final paper in this section, by Cynthia Miller, dealing with the possibilities of reconstructing linguistic systems on the basis of epigraphic fragments, emphasizes the usefulness of language typology. Apart from the brief discussion of Ullendorff’s question of whether biblical Hebrew is a language (286–87), this paper seems out of place in this symposium.

Part 4 of the symposium, entitled “Hermeneutics and Theology,” barely touches on either. The first paper, by John Monson on the role of “context” in biblical archaeology, attempts to adapt Hallo’s “contextual approach” to biblical archaeology (309–10). Centering the “approach” on the question of judging the appropriateness of a given
context, Monson offers his audience a brief sweep of the history of Christian exegesis that he might contrast the “past century and a half” of using context to “illuminate” scripture to a more recent reversion to allegorical methods! This reversion he identifies with scholars such as Van Seters, Thompson, and Davies, who are associated with a “subjective humanism devoid of historical contextual restraints” (314–15). He limits his citation here to pages 200–28 of my The Mythic Past, where I have discussed some well-known ambiguities of historical contexts where evidence is abundant. However, he does not refer to pages 229–397, where biblical context is the explicit topic and fills nearly half the book. In charging neglect of a “contextual approach” in favor of an allegorical exegesis, Monson seems unaware of what I have written on the Bible’s context and archaeology (Historicity, 1974; Early History of the Israelite People, 1992) as well as of Davies’s significant contributions to this field (In Search of Ancient Israel, 1992; Scribes and Schools, 1998). Van Seters has written two of the most important works of his generation in “contextual studies” (Abraham in History and Tradition, 1975; In Search of History, 1983)! Not one of Monson’s “allegoricists” has ever doubted the importance of the role of archaeology in reconstructing the Bible’s historical context. Monson’s paper reflects little knowledge of those he criticizes and seems based on what he has read in BAR or the recent book of Dever (What Did the Biblical Writers Know?, 2001). Those he criticizes also accept the importance of social history for establishing biblical contexts (Niels Peter Lemche, Early Israel, 1985; Davies, Second Temple Studies 1: The Persian Period, 1991). It would be far more appropriate to attribute “social history’s” early development with Max Weber (Das Antike Judentum, 1921), Albrecht Alt (Die Landnahme der Israeliten, 1925), and Martin Noth (Das System der zwölf Stämme Israels, 1930), or, in the United States, with George Mendenhall (BA 25, 1962) and Norman Gottwald (The Tribes of Yahweh, 1979), than with a 1985 BASOR article by Larry Stager. When Monson pleads that “We must learn to enter the biblical world and harmonize it with the text without the burden of ‘proving’ historical reliability,” he confuses two categories of discussion: that of the historian with a real need for evidence and reliability, on the one hand, and that of the reader of literature, with very different needs, including perhaps even Monson’s faith-oriented fantasy, capable of re-creating myths of the past in which the voice of the text can find resonance and confirmation. The former belongs to the issues under debate within the critical disciplines of archaeology, history, and biblical studies. The latter belongs to the continuing world of the Bible’s reception—right and wrong. Finally, Monson’s closing discussion of the geographical aspects of context establishment is surprisingly romantic (a “matrix” giving “birth to the biblical text” [325]) and is hardly integrated with his argument for a contextual approach or his understanding of “minimalism” (cf. my The Bronze Age Settlement of Sinai and the Negev, 1975; The Bronze Age Settlement of Palestine, 1979; Toponomie Palestinienne, 1988).
Richard Averbeck discusses the difference between Van Seters’s understanding of biblical narrative as “historiography” and my understanding of it as an account of Judaism’s “mythic past” (329). He correctly understands Van Seters’s relative lack of interest in historicity and his use of Huizinga’s definition of historiography. On the other hand, in contrasting my insistence on an element of historicity in historiography, Averbeck misunderstands this as “imposing modern standards for history writing (contra my “Historiography: Israelite” in ABD, 1992). This misunderstanding supports a caricature of my “radical historical scepticism” as if Genesis–2 Kings were claimed to be “misrepresentations of historical fact,” “political propaganda,” or (influenced by) “theological tendenz” (330). Averbeck’s confusion is fundamental, as he wishes to distinguish myth from fiction, whereas I often identify them and—thus—separate them from historiography (see my “Historiography in the Pentateuch,” SJOT 13 [1999]: 258–83; “Kingship and the Wrath of God,” RB 109 [2002]: 161–97). His distinction between three kinds of stories in the ancient world—myth, fiction, and history (331)—I think is untenable, although his failure to clearly distinguish his understanding from mine creates difficulties. I find the term “historical myth” (332) absurd, and his claim that ancient myth was “not necessarily fictional to the ancient writers” I find meaningless. Efforts to speak of “pure imagination” and “just fiction” hardly help (333). The pedagogical function of ancient Near Eastern story is analogous and parabolic, much as is both heroic tale and myth. I do not oppose the limited “Conclusion” of his paper (354–56).

David Weisberg’s paper also engages an exaggerated polemical tone against biblical scholars, with whom he associates “postmodern” approaches of “deconstructionists” and whom—following Zipora Talshir—he accuses of defying the “major achievements of centuries of specialized research” and, thereby, forfeiting “scientific methods” by dealing with overarching theories (358–59)! Given this ringing charge, it is rather surprising that he skips over specific arguments and then misrepresents a disagreement between Avi Hurvitz and Philip Davies on diachronic changes in biblical Hebrew. Citing Hurvitz’s opinion that there is great uniformity underlying both “preexilic” (= before 587?) inscriptions and classical biblical Hebrew, he asserts that Hurvitz’s conclusion renders it “unimaginable” that biblical Hebrew be understood as a “scholarly construct.” This brief exchange between Davies and Hurvitz is a very minor and secondary quibble of a larger debate. Weisberg’s lack of ability to imagine biblical Hebrew as a construct should have been addressed to Axel Knauf (“War ‘Biblisch-Hebräisch’ eine Sprache?” ZAH 3 [1990]: 11–23) and Edward Ullendorff (Is Biblical Hebrew a Language?, 1977) rather than either Davies or Hurvitz. Weisberg goes on to wonder aloud about what “minimalists” like Davies might make of a brief and congenial quote from Leo Oppenheim, but he does not deal with what Davies himself has thought or written on the issue. He seems to think that Davies understands the Bible’s “composition, editing, and canonization as a one-time act.
of a later period,” that is, later than what Weisberg calls the “biblical period” (see, however, P. R. Davies, Scribes and Schools, 1998). Given his misrepresentation of Davies’s actual theories on biblical composition, Weissberg’s presentation of Hallo and Younger’s Context of Scripture as “an answer to the minimalists” reflects a lack of awareness of both past and present work in comparative studies by those he scorns so deeply (J. Van Seters, The Hyksos, 1966; idem, Abraham in History and Tradition, 1975; idem, In Search of History, 1983; Th. L. Thompson, Historicity, 1974; idem, The Messiah Myth: The Ancient Near Eastern Roots of Jesus and David, 2005). Closing with what he calls an “informed theological approach” (362–67), he continues his campaign of disinformation, claiming that “minimalists” downplay the “greatness of the prophets” (see, however, R. Carroll, Jeremiah, 1986; I. Hjelm, Jerusalem’s Rise to Sovereignty, 2004). The most amusing aspect of this final polemic is Weissberg’s stinging rebuke of James Barr as “minimalist” (364–65)!

The final paper by Andrew Vaughn takes up the question: “Can We Write a History of Israel Today?” a title that he attributes—without reference—to titles of essays by Lemche (probably: “Is It Still Possible to Write a History of Ancient Israel?” SJOT 8 [1994]: 163–88) and me (probably: “Can a History of Ancient Jerusalem and Palestine be Written?” in Th. L. Thompson, ed., Jerusalem in Ancient History and Tradition, 2003, 1–15; or perhaps, more likely: L. L. Grabbe, ed., Can a History of Israel be Written?, 1997). It is uncertain that he has read our essays, as they have nothing to do with the issues he engages. He rather assumes that the “Copenhagen school” attempts to impose a unified structure on biblical narrative and denies that materials within the Pentateuch and “Deuteronomistic History” might derive from earlier periods (see, however, my Early History, 1992, 383–99; and esp. The Messiah Myth, passim). His dismissal of such “arguments” with what appears to be a brief paraphrase of nineteenth-century literary criticism’s source critique is both ironic and patently ingenuous. In fact, Vaughn does not seem interested in either his own title or those who have written articles with similar titles. Rather, he engages another discussion altogether: Walter Brueggemann’s Theology of the Old Testament (1997), which he cites with appropriate bibliographical reference. This discussion leads him to propose his own “positive biblical history,” by which he understands a rather traditional and harmless form of Bible illustration, largely compatible with what I have proposed for use in biblical exegesis (The Mythic Past, 1999, 228–33). However, he neglects the literary and intellectual pasts ever implicit to biblical texts (The Mythic Past, 267–92, 375–97, respectively), which need to be engaged theologically, much as Vaughn has engaged Schleiermacher. I am particularly concerned about the mythical function of the historical imagination he proposes, as it reifies what I see as an essentially intellectual, literary, and aesthetic meeting with a text. The past Vaughn creates is markedly different from the past the biblical author projected and
created. Vaughn’s past faces its nemesis within an old-fashioned historical critique—the more negative the better.

In concluding, I would like to return to the questions I posed in the opening of this response. The Future of Biblical Archaeology, for the most part, presents itself as a theologically motivated apologetic. Many of the individual papers are as closed to any who do not share their biases and theology as the congress has been. I am deeply troubled by the rhetoric of many essays, particularly its distrust of critical scholarship. On the other hand, several central articles reflect both sound archaeology and critical comparative methods. The striking differences among the critical capacities of contributors encourage me to read this volume as a significant witness to challenges that currently face evangelical scholarship—not unlike those that radically changed Catholic biblical scholarship a half century ago. One might suggest that the challenge presented by the symposium regarding the future of biblical archaeology is not that explicitly addressed by the symposium regarding either the secularization of Palestinian archaeology or the influence of “minimalism.” Evangelical scholarship faces rather a choice between dogmatic presuppositions that exclude critical thought from their scholarship, as, for example, have been expressed by Alan Millard (160) and others, and an openness toward the literary forms and cultural contexts of the Bible, which are foreign to our own, as is preferred by William Hallo and others (165; Thompson, Historicity, 328).