This volume is a compilation of seventeen essays that were presented to the Oxford Old Testament seminar between January 2001 and June 2003. As is often the case, the essays themselves have been subsequently revised and often expanded. John Day notes in the preface that the purpose of the volume “is to offer a critique of various aspects of the ‘everything is late’ school of though in Old Testament studies … not from any reactionary standpoint but from a thoroughly reasoned, critical point of view” (vii). An exhaustive reading of the volume reveals that the arguments are thorough, reasoned, and what I would consider vastly comprehensive.

Briefly, this work contains some seventeen chapters that are dedicated to exploring the historical antecedents to preexilic Israel. The scholars come from an international and theologically eclectic background. Ostensibly, the book contains an index of references and authors. I found that no “agenda” was present in the writings with the exception of truly trying to discern what the text and artifacts tell us concerning the nature and extent of preexilic Israel.
The theses of Albright have fallen under challenge since the doyen’s demise.\(^1\) Attack of Albright’s key theses came early in the 1970s from a group of scholars who would become known as “the Copenhagen school.”\(^2\) Writings from Thompson,\(^3\) Van Seters,\(^4\) Lemche,\(^5\) and Davies\(^6\) formed the core challenge to the old Albrightian view of Israel’s history.\(^7\) In essence, the Copenhagen school revisited the perennial questions that had plagued biblical scholars since the 1950s and 1960s. They took to task the older, more accepted works of Israel’s history (e.g., Noth’s *The History of Israel*; Bright’s *A History of Israel*). This shift moved from critically reexamining the methodology of scholars such as Noth and Bright to a type of nihilism concerning anything in Israel’s past.\(^8\) As Nicholson notes in the first article, “Current ‘Revisionism’ and the Literature of the Old Testament,” “More recently, however, there has come a markedly more extreme phase of revision according to which nothing in the Old Testament or at best only some ‘relics’ can be assigned to the pre-exilic period.” (5). The fruits of the Copenhagen school yield a record of the Hebrew Bible composed in the Hellenistic or Persian period at the earliest. Likewise, the records that one finds in the Hebrew Bible are not historical but rather ideological and propagandistic at best. The Copenhagen school ascribes to “pious fiction” at best for the records of the patriarchs, exodus, conquest/settlement, and the united monarchy.

Nicholson takes to task in his article the two chief protagonists of the Copenhagen school: Philip Davies and Thomas L. Thompson. A cursory outline of the main arguments that

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2. For the purpose of this review, the “Copenhagen school” refers to those who would espouse the type of ideology found in the writings of scholars such as Thomas L. Thompson, John van Seters, Niels Peter Lemche, and Philip R. Davies. One will note that not all scholars who feature prominently in the Copenhagen school have to in fact be from Denmark.
7. For the purposes of this review, the term “Israel’s history” covers specific time periods in Syro-Palestinian archaeology roughly from the Middle Bronze Age to the Late Iron Age. The Copenhagen school sought to interact critically with the lack of historical antecedents (in their opinion) to the textual material found in the Hebrew Bible (e.g. patriarchs, exodus, conquest/settlement). Modern scholarship has moved on to tackle the issue of the united monarchy of David and Solomon.
8. See, e.g., Thomas L. Thompson *The Mythic Past: Biblical Archaeology and the Myth of Israel* (New York: Basic, 1999), 98–99. A measure of latitude was given by scholars such as Davies, who lamented the fact that there were three “Israel.” He noted, “I shall be dealing with three Israel: one is literary (the biblical), one is historical (the inhabitants of the northern Palestinian highlands during part of the Iron Age) and the third ‘ancient Israel’, is what scholars have constructed out of an amalgamation of the two others” (Davies, *In Search of ‘Ancient Israel’,* 11).
Davies proposes is given by Nicholson. Namely, Davies argues that the ancient Israel is merely a scholarly invention, an “amalgam of biblical Israel and historical Israel.” Critical reflection, according to Davies, is completely lacking in historical research. The biblical Israel is for Davies an ideological process from the Persian period of Yehud. Authorship of the documents that supported the ideologies was constructed by five “schools which were sponsored and controlled by the ruling elite” (9). Thompson likewise argues that there was little relationship between biblical Israel and historical Israel and Judah. At best, Thompson sees the Israel in the Bible as one that was created for the resettlement process. Propaganda and ideology are responsible for shaping biblical monotheism, the themes of restoration and Jewish self-understanding. Quite likely, according to Thompson, the written text was composed during the Maccabean period in the second century B.C.E. Nicholson appeals to Barr, who has been oft employed by both the Copenhagen school and their detractors. In his book History and Ideology in the Old Testament Barr argues that the above postulates, especially the dubious historical reconstruction of the Jewish people, “is far from being well evidenced historically and is too absurd to be taken seriously.”9

Nicholson counters the claim that what we have in the Hebrew Bible is merely the product of paid scribal elite. He argues that the trenchant criticisms leveled at the ruling elite by the prophets could not possibly have been done so by paid staff. Likewise, he asks why, if indeed we have accounts in fictitious books (e.g., Amos), these narratives were concerned with contemporary details. Second, he notes Barstad’s work on comparative research into prophetic similarities in the ancient Near East and those of Isaiah, Jeremiah, Ezekiel, and the Twelve. He notes, “As Barstad shows, there are manifestly features in the books bearing the names of Isaiah, Jeremiah, Ezekiel and the minor prophets, and much also in the narratives about the prophets in the historical books of the Old Testament, that are also attested in the growing number of prophetic texts from other ancient Near Eastern nations” (15).

Contra Thompson’s rereading of certain biblical narratives (e.g., Jonah and Lamentations), Nicholson sagely points out that there is “ample evidence of the presence of originally discrete texts within the larger literary complexes of the Hebrew such as the Pentateuch and the Former Prophets, and, indeed, of still earlier stages in the emergence of some of these texts” (18). Nicholson is comfortable finding preliterary narratives behind the stories of Abraham in Genesis, yet he is also comfortable with seeing the Joseph story as an attached Novelle. At issue here is an injunction not to eschew the strides that have been made in understanding the variousGattungen employed by the different books of

the Hebrew Bible. Nicholson closes by stating, “Attempts to limit the creation of such a literature [Hebrew Bible] to a largely scribal activity carried out in the interests of political propaganda or for the purpose of legitimizing a newly founded state of the Persian period or later are in the face of overwhelming evidence to the contrary” (19).

Graham Davies explores the evidence for a historical exodus in his chapter entitled, “Was There an Exodus?” He informs the readers of just how far scholarship has drifted when he notes that only a generation ago this question would have been absurd to ask.10 First, Davies surveys the writing of Lemche,11 who argues for a nonhistorical foundation for the Pentateuch. Lemche goes so far as to claim that there is nothing in the Pentateuch that the historian can use. However, Davies attempts to give the event of the exodus a fair hearing, marshaling both biblical and archaeological evidence to adduce its veracity.

Interestingly, Davies notes that it is fashionable to deny the historical exodus among many differing strands of Hebrew Bible scholarship.12 It seems that when one is investigating the historiography of the Hebrew Bible, it is best to begin with the account in Judges. Davies begins by noting that current scholarship has argued that the Israelites were nothing more than indigenous Canaanites “who made their way up into the hills and built for themselves the Iron Age I village sites that are now so well known” (25). However, he warns that it is troublesome to tie too closely the thesis of Canaanite origin to the Israelites. Second, Davies argues that the historian cannot ignore the textual evidence (both biblical and nonbiblical) that is relevant to this issue.

Biblical evidence is first adduced concerning the exodus event, both Exod 1–15 as well as other references in the Hebrew Bible. After briefly surveying the scriptures, he notes, “If the Exodus tradition is indeed represented in the sacred memories of both kingdoms, then we have good reason to suppose that it goes back at least to the time when they were a united kingdom” (27). Four separate criteria are postulated in order to elucidate the historical foundation of the exodus account.13

10. Davies notes that the majority of scholarship accepted some sort of exodus. The major questions were when it happened, by what route the Israelites escaped, and how many Israelites were on the journey. These elements have now been eclipsed by the Copenhagen school’s contention that the exodus rests on “very limited historical foundations” or that “the quest for the historical Moses is a futile exercise” (23).
12. For instance, he notes that both Lemche and Dever (hardly a minimalist but rather their arch-scourge) share a similar opinion concerning the exodus, namely, that it did not happen.
13. It is not Davies’s intention to try to rebut the claims of those who find no exodus in Hebrew history. Rather, what he proposes to do in this article is to examine a few elements of the exodus tradition that are thought to reflect a common historical core.
It is no surprise that geography plays a great role in both the timing and route of the exodus. Davies takes as his text for investigation Exod 1:11, specifically the two cities Pithom and Rameses. Pi-Ramesse, tentatively identified with Rameses, is located at Qantir/Tell el-Daba. Pithom is identified as either Tell el-Maskhuta or Tell er-Retabeh, with Davies rightly noting, “If we consider the two names Raamses and Pithom together, it has to be said that they are more likely as a pair to belong to a tradition that originated in the Ramesside period than to a later time” (30). Second, Midian is adduced to buttress the claim of proto-Israelites being at least in the south of Canaan at some point in their history. Third, the term “Hebrew” (as opposed to “children of Israel”) is examined for frequency in Exod 1–10. Davies contends that at the least “we could well envisage that this ‘Hebrew’ form of the tradition belonged to an early stage in the history of one of the constituent groups of later Israel, when they were still independent” (31). Fourth, two poems are evaluated for both their antiquity as well as their witness to a catastrophe. The Song of Moses is advanced as the evidence of a deliverance from the Egyptians from the early monarchy period—at the latest. The Song of Miriam is proposed to be ancient due to its connection with the ‘apiru. Davies challenges the consensus by stating, “A connection [between ‘Hebrew’ and ‘apiru] is sometimes thought to be improbable because while ‘apiru is a sociological term for a certain kind of person (‘outsider’), ‘Hebrew’ is an ethnic term…. It seems to me quite possible that there too it (Hebrew) has a social meaning and that it can be dissociated from any particular ethnic identity” (33). Lastly, Davies rightly postulates, along with Knauf, Malamat, and de Moor, that other Egyptian texts may be able to offer confirmation of the historicity of some elements of the exodus tradition. It is concluded, “the tradition [of the exodus] is a priori unlikely to have been invented; the biblical evidence is widespread and can be followed back to a respectable antiquity, within at most two hundred years of the supposed event; some elements of it have a particular claim to authenticity; and in various ways what is said corresponds more closely to the realities of the New Kingdom Egypt than one would expect from a later wholly fictitious account.” (36).

The third chapter, “Back to Basics: A Holistic Approach to the Problem of the Emergence of Ancient Israel,” is an effort to once again seriously reconsider the issue of the emergence of ancient Israel into Canaan.14 Anthony J. Ferndo rightly argues that it is “time that we go back to basics, and seriously reconsider the issue of the emergence of ancient Israel in Canaan in light of all the available evidence without unjustly preferring one type of evidence to another” (41, emphasis added). Ferndo begins by laying down certain methodological presuppositions. First, he argues in favor of what Newman called

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14. Ferndo argues that, although many entertain the notion that Israel’s entry into Canaan is in essence fictional, such thesis is sustained by repetition rather than by reasoned proof (41).
the “preponderance of probability.” Simply stated, this postulate argues that Israel (until proven contradictory) must have somehow come from outside Canaan. Obviously, we have a multiplicity of biblical texts that purport to tell the story of how Israel was ensconced in the local environs. Ferndo, following Danell, suggests that the group that contained Joshua and his house were not originally Hebrews but upon entering Canaan took on this identity. Indeed, he points out that the covenant made in Josh 24 reflects another earlier group of immigrants who were already in Canaan.15

Differences are noted in the separate conquest accounts, specifically the Blitzkrieg type of activity in Josh 1–12; Num 21; 32 and the peaceful settlement suggested in Josh 13 and Judg 1. While Ferndo interacts with both Finkelstein and Silberman, he argues for a “Joshua like figure” who inspired an earlier historical tradition. He notes, “Given such a complicated history of the ‘conquest’ traditions, it is no wonder that contradictions are found in the narrative we have and that we cannot find a solution to each of them” (46). The author convincingly argues that what is presented in the biblical text of Joshua and Judges is hyperbole alongside more accurate historical information. A call for a fresh examination of the archaeology surrounding the conquest is given by Ferndo, especially the Late Bronze Age and Early Iron Ages in Palestine. A cursory review is given by the author of certain sites in Palestine, and some history is detailed. He argues that Syro-Palestinian archaeologists have now begun to study settlement patterns. However, this emphasis began years ago with Finkelstein’s publication of his survey of settlement patterns.16

Linguistically, the author argues that two Egyptian texts make mention of Yahweh in relation to the southern Transjordan. He argues that columns at the temple of Amnu has an inscription that makes mention of “the land of Shasu of yhw’” (50). Likewise, in a list linked with Ramesses II we find the phrase “land of Shasu (of) yhw’” (50). Clearly both Egyptian references are adduced to argue for the broad knowledge of Yahweh throughout Egypt and the Levant. More convincingly, Ferndo briefly explores the Merneptah Stela and its role in dating the nation of Israel. The author postulates that “Israel” in the stela is either the “House of Joseph” or people stemming from Leah (e.g., Reuben, Simeon, Levi, Judah, Issachar, and Zebulun). Ethnographically, Israel has been subjected to a variety of interpretative models. Ferndo cites the studies involving the lack of pig bones in some of the early settlements. The author admits that the presence/absence of pig bones cannot be directly related to a particular social identity. However, the presence/absence of pig bones can be related to ethnicity (e.g., the study by Hesse and Wapnish). An upsurge of

15. Ferndo suggests that this earlier group of people worshipped El Elohe Israel instead of Yahweh Elohe Israel.
population growth in Palestine in the Early Iron Age is also cursorily explored by the author. This citation is not new, having been bantered about by Dever for some time now. Briefly, it is argued that during the Early Iron age the situation in the central hill country of Palestine had a huge upsurge of people (ca. 40,000). Dever, Ferndo, and others have argued that this increased number of people was surely none other than Israel.

A review of the “basic evidence” leads the author to posit an early Israel made up of various groups of hill-country villagers originally indigenous to Canaan. It is further postulated that a “small group of Hebrews” joined these indigenous Canaanites, bringing the Yahwistic faith with them from Egypt. The nature of the conflict was peaceful at first, but inevitable small scale conflict took place intermittently. Ferndo sagely comments, “When the biblical, archaeological, and extra-biblical literary evidence are taken into account, it appears that no single model can do justice to the complex phenomenon of the emergence of ancient Israel in Canaan around 1200 B.C.E.” (61).

William Dever, long held to be the arch-antagonist of the minimalists, presents his case for a united monarchy in the fourth chapter, “Histories and Non-histories of Ancient Israel: The Question of the United Monarchy.” Dever is comfortable in labeling the Copenhagen school as “minimalists” but rather prefers “nihilists.” He argues that the methodology of the Copenhagen School is one of “historiographical crisis,” which has spread to both biblical and archaeological circles in both Israel and America. (65). The nuance of Dever’s article is such that he seeks to apply his understanding of the Copenhagen school’s methodology to a current problem in biblical historiography. He chooses for his model the geographical/political scope of the united monarchy. Quickly he notes that both Lemche and Thompson have rejected any notion of recovering a historical David or a historical united monarchy.

In seeking to rebut the methodology of the Copenhagen school, Dever outlines eight major arguments adduced by the minimalists. First, it is alleged that the Copenhagen school understands the Hebrew Bible as “not about history at all” (66). Rather, the material in the Hebrew Bible is a fictional story or “a phantasmagoria of confused Jews seeking their identity in a second-century Hellenized Palestine” (66). Dever rests his rebuttal of this charge by noting that the language of the Deuteronomist is much like that of the material we have from Iron Age inscriptions. He also argues that we have not a single Greek loanword, institution, or Hellenism in all the Tetratuech or Deuteronomistic corpus. Second, it has been alleged that the population of Israel in the tenth century B.C.E. was too small (ca. 2,000 inhabitants) to have supported a military/political state as appears in the Hebrew Bible. Dever challenges this assumption by noting that the source employed by the Copenhagen school (Finkelstein) was misinterpreted. Instead of the whole of Palestine containing only 2,000 people, it is argued the number is more likely
100,000 inhabitants. Third, Dever points out that the Copenhagen school asserts that Israel did not become a state until the ninth century B.C.E. when so recognized by certain Neo-Assyrian texts. The only rebuttal given by Dever at this point is that the Copenhagen school refuses to define what a “state” actually means in their methodology. He further notes that later in the article he will present archaeological correlates of the state formation. Fourth, it is asserted that there was no united monarchy with Jerusalem as its capital because there is no archaeological evidence for Jerusalem as a political capital in the tenth century B.C.E. Dever rightly argues that this argument is from silence. He cites Na’aman, who has pointed out that as early as the fourteenth century B.C.E. Amarna letters Jerusalem is known as a capital of a larger city-state system. The fifth argument of the Copenhagen school involves a lack of archaeological understanding. Dever correctly notes that city walls, gates, and palaces at Hazor, Megiddo, and Gezer are dismissed by the revisionists. Dever states, “Initially, being innocent of any archaeological expertise, they simply asserted a ninth century BCE date; but nowadays they are emboldened by the ‘low chronology’ of Finkelstein, their new authority” (68). The sixth point—the lack of Israel in extrabiblical Assyrian sources until the ninth century B.C.E.—is cogently refuted by Dever. He points out that due to the lack of Israel being mentioned as Israel but rather House of Omri, Thompson refuses to speak of Israel by name. In support of an ancient state by the name Israel, Dever notes that the Neo-Assyrian texts could not have known Israel by name before the battle of Qarqar (ca. 853 B.C.E.). Likewise, in the Assyrian materials Ahab is said to have fielded some 2,000 chariots barely two generations after the death of Solomon. Reflecting, Dever notes, “The ascendancy of Omri and Ahab by the early to mid-ninth century BCE requires us to postulate predecessors who had founded a kingdom somewhat earlier—if not the biblical Solomon, then Solomon by another name” (69). Dever presents one of his more potent arguments against the Copenhagen school by noting the Tel Dan inscription. It is rightly noted that the attempts of Thompson, Davies, and Lemche to somehow “misread” dwd border on despair. Lastly, it is argued that the “revisionist” arguments are ideological in nature. This is especially true, claims Dever, for the biblicalists and to a limited extent archaeologists.

In Dever’s opinion, the view of the Copenhagen school is worthless—in large measure because they refuse to integrate modern archaeology with their ideology. This opinion forces Dever to deal with Israel Finkelstein, whom the Copenhagen school has relied on increasingly for their interpretation of “archaeological facts.” In assessing Finkelstein and his now infamous “low chronology,” Dever gives a type of excursus on elements of ceramic data and carbon dating. The data presented in the excursus is presented so as to refute Finkelstein’s redating effort. It should be noted that Finkelstein has not aligned

17. Dever argues that Finkelstein’s demographic estimate of 2,000 people was for the few villages surrounding Jerusalem alone rather than the whole nation.
himself publicly with either Dever or the Copenhagen school. His results have been cited by both sides of this controversy, but as of yet it seems Finkelstein has remained silent. Helpfully, Dever next summarizes the material that he contends to be relevant to “state formation processes” (76). He defines “states” archaeologically and includes a helpful table on sites in Palestine that would qualify as states. Likewise, he narrates the formation of elements such as architecture and administrative centers. In seeking to rebut Finkelstein, he briefly examines the architecture of Hazor, Megiddo, and Gezer. It should be noted that while Hazor has recently been excavated by Ben-Tor, Megiddo is being excavated by Finkelstein and Gezer was excavated by Dever. Ancillary elements such as script, language, and literacy are given their due in Dever’s refutation of the Copenhagen school’s claims. Dever charges, “Upon closer examination, it seems to me that the ‘revisionists’ wholesale rejection of the concept of ethnicity (and its archaeological correlates) simply reveals their mistaken equation of ethnicity with racism—another of postmodernism’s conceits” (86).

John Barton delivers a quite helpful and nonacrimonious article entitled “Dating the ‘Succession Narrative.’ ” Barton’s scholarship on the David and the Succession Narrative does not need to be belabored here. This short chapter is devoted to correcting dubious assumptions concerning the dating and historicity of the Succession Narrative (2 Sam 9–20 and 1 Kgs 1–2). Scholars from the Copenhagen school such as Van Seters have argued that the Succession Narrative is the youngest part of Samuel. Accordingly, he discounts many, if not all, of the elements associated with David (e.g., his moral behavior and the Bathsheba incident). Thus members of the Copenhagen school understand the Succession Narrative as a type of prose fiction. Barton points out that this type of doubt is not new, as a cursory reading of Perry and Sternberg will illustrate. Briefly, Barton gives an overview of the elements that contribute to the narrative art of the Succession Narrative. Most interesting is Barton’s presentation of Friedman’s thesis that the Succession Narrative is a larger part of J. While Friedman’s thesis was anticipated as far back as Hölscher, it has some obstacles to overcome in order to be accepted fully by the majority of the guild. Dating the Succession Narrative for Barton is a relatively easy task. He decides on a pre-Deuteronomistic (most likely preexilic) date for the text. One can obviously make a close parallel with the narrative style of the Elijah and Elisha stories. As a whole, this type of literature is usually dated to around the eighth or seventh century B.C.E.

In the sixth chapter J. A. Emerton delves into the perennial question of “The Date of the Yahwist.” Helpfully Emerton outlines five key presuppositions with which he defines the

18. Finkelstein has engaged Dever from time to time, but this appears to be as a result of exchanges of a personal rather than professional nature.
parameters of the article. First, this essay actually assumes the existence of J and E sources. Second, while Emerton is quite aware of the multiplicity of sources even within J (e.g., J1 and J2), he notes that “none of these sub-divisions of J has been established with sufficient probability” (108). Third, the focus of this article is to discuss the “classical” date of J as it occurs (e.g., JEDP) in the sources. Fourth, Emerton kindly points out that he believes in two sources for the material in Kings. Following Nicholson and Cross, he posits a preexilic source during the time of Josiah and an exilic source sometime before the return from exile (ca. 562 B.C.E.). Fifth, the primary aim for the essay will revolve around answering the question of when J is to be dated: preexilic or exilic.

The primary scholars with whom Emerton interacts are Hermann Vorländer and, to a lesser extent, Van Seters and Schmid. Emerton chose Vorländer because of his work on dating J and E as well as JE. Accordingly, Emerton notes, “He regards JE as exilic or post-exilic, with a terminus ad quem around 400, but he appears to favor a dating of J and E in the sixth century” (109). An in-depth reading of this article reveals that Emerton takes Vorländer to task on six salient points. First, clearly Vorländer has argued that the document JE was unlikely to be preexilic due to the lack of its mention in early literature. Emerton rightly points out that Vorländer’s ideas are based on Vorländer rather than arguments. Likewise, his claim that belief in Yahweh as creator is not an exilic idea is also mistaken. Second, although Vorländer argues that the JE document was written in a time of peace (and therefore for him the exilic or postexilic times), certain elements that one would expect to find are missing (e.g., the fall of Jerusalem or exile). Third, it is argued that the cultural and historical arguments that Vorländer puts forth are not persuasive. For instance, Emerton notes, “The presence of Mesopotamian ideas in JE need not presuppose the Babylonian exile, but may owe its origin to mediation through the Canaanites at an earlier date.” Fourth, Vorländer’s hypothesis that the Table of Nations in Genesis is dependent on South Arabian texts is problematic at best. Likewise, Emerton helpfully points out that this contention depends largely on the questionable hypothesis that the entire chapter is Yahwistic. Fifth, Emerton rightly states, “Contrary to Vorländer’s claim, the Elephantine papyri do not testify to an exilic date for J” (128). Finally, and most important, Emerton points out that Vorländer’s hypothesis fails to provide a satisfactory account of parts of the document. Emerton concludes, “If my hypothesis that the matrix of Israelite history writing was the literary tradition seen in North-West Semitic inscriptions of the ninth-seventh centuries is correct, then the composition of J may plausibly be associated with it” (128).

Walter Houston researches a very perplexing question in the seventh chapter, entitled, “Was There as Social Crisis in the Eighth Century?” Indeed, what is being asked here is, Against what social/historical background should the books of Amos, Micah, and Isaiah be read? Three postulates are given by Houston at the outset. First, he questions the
provenance and reliability of the evidence (e.g., are we really dealing with eighth-century texts?). Second, he questions the hermeneutical grid with which the texts are interpreted (e.g., are we interpreting the texts correctly?). Third, he brings to light evidence from the eighth century that disproves Albertz’s understanding of these texts. Houston marshals the evidence in such a way as to posit two very interesting conclusions. First, he argues that conditions in the eighth century (in Israel and Judah) were such to cause the development of unjust economic pressure. Second, he admits that such conditions occurred on a regular basis and were even perhaps more severe in the fifth century. Therefore he concludes, “we cannot date any specific text in these books to the eighth century simply on the grounds of its subject matter” (147). His research provides a useful caveat for those interested in the social climate of Israel and Judah.

Gary Knoppers provides his usual methodical analysis of the postexilic situation after the fall of the northern kingdom. He entitles his article, “In Search of Post-exilic Israel: Samaria after the Fall of the Northern Kingdom.” Among scholars Knoppers has the ability succinctly to state a complex case with profound ease, making all points salient and sensible to most scholars. This article seeks to examine the fall of Samaria from both the maximalist and minimalists positions, then come to some type of understanding of the evidence. I will not belabor each position, for they are already well represented in contemporary scholarship.

Knoppers does a superb job of surveying the sources in his article. He concludes that the “evidence” archaeological and otherwise presents a picture that is at variance with both schools of thought. Knoppers notes, “In the regions of Galilee and the northern Transjordan, the Assyrian invasions cause widespread devastation” (170). This finding would generally corroborate many of the maximalist findings (e.g., histories of Israel much like Bright’s). Yet Knoppers also notes, “Analysis of the material remains from the hill country of Ephraim and Manasseh suggests a mixed picture…. A few locations, showing no traces of destruction, evince continuity in occupation” (170). Thus Knoppers concludes that such evidence points to a “limited” Assyrian forced deportation. Exile did occur, however, as the evidence points out that some survived and remained in the land. Israelites who remained in the land account for three major features of the material remains. First, these people account for why there is a strong continuity in material culture in both Samaria and the surrounding highlands. Second, Knoppers argues that the remaining Israelites help clarify features of the foreign presence that appear in the material record. Lastly, the Assyrian selectivity (e.g., limited deportation at some sites and minimal damage at other sites) with the House of Omri “explains why the region of Samaria made a swifter recovery after the Assyrian campaigns than Judah did after the Babylonian invasions in the early sixth century BCE” (171). Knoppers also briefly examines the situation under the Babylonians, specifically Nebuchadnezzar. He embraces
the research carried out by Lipschits, which understands a major Babylonian undermining effort to Jerusalem’s fortifications. This outlook of the Babylonians toward Judah may be contrasted with their relatively benign campaign into Samaria. Knoppers concludes his article by a cursory examination of the role of the Yahwistic Samarian community in the Persian period.

In like spirit, H. G. M. Williamson searches for the “preexilic Isaiah” in the ninth chapter. Williamson outlines five methods that support a preexilic date for “some” of the materials in First Isaiah. Regarding the first method, he notes, “The one which I instinctively find most compelling is that where the account of a purportedly early event for which there is no other direct evidence in the biblical text is corroborated by some contemporary external source which could not, in all probability, have been known to a later biblical writer” (184). In this assessment, I concur with Williamson, and this argument is the strongest of the five. The author rightly argues that his third (quotations and allusions by later authors) and fourth (historical literary analysis) methods are applicable to a wider range of texts. Williamson notes, “from them [his methods] something of a portrayal of the prophet may be developed” (200).

David Reimer investigates the “Jeremiah before the Exile” in the tenth chapter. A brief introduction is followed by the “external evidence.” In this regard, Reimer understands external evidence to comprise Babylonian records, Lachish ostraca, and selected bullae. Perhaps the most interesting is the contention that, while not “conclusive” proof, we nonetheless see an indication that there is some type of demonstrable continuity between Jeremiah’s stories and history. The author also note the “internal evidence” for a preexilic Jeremiah. As would be expected, traditional elements such as historical setting are adduced for the reader. Reimer closes by affirming the role for historical/literary criticism in reconstructing preexilic Jeremiah.

A striking question is posed by John Day in his essay entitled, “How Many Pre-exilic Psalms Are There?” The evidence that Day first presents concerns “royal psalms,” which were long thought to be mostly postexilic. A quick survey easily demonstrates how Day interacts with past and contemporary scholarship on this subject. The second category of preexilic psalms that are examined are the “inviolability of Zion psalms.” Psalms such as 46, 48, and 76 are shown not to be eschatological but having a definite root in the pre–586 B.C.E. experience. Additional elements such as references to the ark of the covenant, enthronement psalms, and communal laments (implying Israel had an army) are examined for their support of a preexilic core in the Psalter. Day then brilliantly balances his article by briefly surveying the Psalter for exilic evidence. As one would expect, references to “return from exile” make the list quite early. However, other elements such as references to the House of Aaron, emphasis on the law, dependence on the Priestly
source, and the concept of life after death all play a large role in determining the exilic influence. Linguistic elements such as late Hebrew forms, Aramaisms, and acrostics are also given ample consideration for their help in proving a psalm late. Day notes, “it is not possible to know precisely how many pre-exilic psalms there are in the Psalter” (244). However, he has cogently summed up indicators that help scholars answer the “all is late” school in Psalter studies.

Katharine Dell proposes a three-tiered (literary-historical, theological, and comparative) approach to studying “Wisdom Literature and Its Roots in the Pre-exilic Period.” The author is cognizant of the Herculean task of having to sort out a “date” for wisdom literature. She notes, “We are essentially looking at material that has a timeless and universal quality and attempting to date it early or late” (266).

The thirteenth chapter is a masterpiece of writing by Bernard Levinson entitled, “Is the Covenant Code an Exilic Composition? A Response to John Van Seters.” Largely, this article interacts with Van Seters’s *A Law Book for the Diaspora*. It should come as no surprise that Van Seters dates the Covenant Code to the exilic period nor that Levinson takes Van Seters’s argument apart quite nicely in this article, which is one of the most technical in the book with copious tables, illustrations, and much Hebrew exegesis. Particular points of criticism include a lack of perspective specifically in terms of a mutual exclusivity between author and redactor. Levinson states that Van Seters’s “approach does not take cuneiform literature into account. In particular, it overlooks the evidence for the importance of redaction to the composition of the Laws of Hammurabi, the very text that allegedly served as the Covenant Code’s literary exemplar” (316). Second, Levinson argues that the Sinai pericope my indeed have preexilic literary precursors that Van Seters ignores. Lastly, Levinson argues that the Neo-Assyrian period provides the most logical setting for a Judean author to pattern the Covenant Code after the Laws of Hammurabi (317). I believe Levinson makes a strong case that the altar law of the Covenant Code is at least pre-Deuteronomic. The author argues in terms of syntax and meaning that the Covenant Code assumes a plurality of cultic sites, not a single site.

B. A. Mastin’s essay, “Yahweh’s Asherah, Inclusive Monotheism and the Question of Dating,” takes on one of the main criticism of the Copenhagen school. Namely, they argue that little extrabiblical evidence exists that sheds light on relevant religious ideas and practices in Palestine during the Iron Age. Mastin reexamines the significance of the inscriptions from Kuntillet ‘Ajrud and Khirbet el-Qom. A cursory overview of the material presents the reader with strong evidence that both inscriptions are evidence for “a type of cult which was of more than marginal importance in both the north and south of Palestine in the eighth century BCE” (345). Extensive examination reveals that most likely “his [Yahweh’s] Asherah” is incorrect. Rather, the inscriptions probably refer to
the goddess’s wooden symbol. In this fashion, Mastin agrees with Keel and Uehlinger, who argue that the goddess had “become a gender-neutral symbol of numinous power which was associated with Yahweh to mediate his blessing” (345). Mastin prefers to classify this development as “inclusive monotheism” that was prevalent from the eighth century B.C.E. onward, a movement that the Copenhagen school puts much later. Mastin concludes, “There is not extrabiblical evidence to show when it [this Asherah movement] began, but in the eighth century BCE its influence can be seen in both the north and the south of Palestine, long before the Persian or the Hellenistic period, where the revisionists place it” (346).

W. G. Lambert gives an excellent overview of Mesopotamian sources in “Mesopotamian Sources and Pre-exilic Israel.” The first half of this essay is given over to a careful noting of historical reconstructions of both Assyrian and Israelite kingdoms. Lambert, a scholar of renowned reputation, does an outstanding job of presenting the historical material. He covers all the principal players from not only the book of Kings but also Assyrian inscriptions. Israelite kings who make an appearance in Assyrian literature are Ahab, Jehu, Joash, Menahem, and Jehoahaz. Assyrian rulers such as Shalmaneser III, Tiglath-pileser III, and Sennacherib are mentioned alongside other foreign kings (e.g., Ben-hadad). Once the historical material is collated and presented for the reader, Lambert gives a short excursus on the historical methodology of the Copenhagen school. He states, “Now for P. R. Davies and N. P. Lemche … their ideology is redolent of postmodernism with a whiff of nihilism. They seem to be saying that the historicity of an ancient text has to be proved 100 per cent by irrefutable evidence, and that the historical books of the Old Testament are not history but ‘literary constructs’ with a bit of history in some of them” (362). Lambert is of the opinion that evidence contained in the Neo-Assyrian royal inscriptions supports the sequence of kings in both the Israel and Judahite kingdoms. Historical figures in the book of Kings can be traced back to none other than Solomon.

No stranger to epigraphists, André Lemaire, presents a very helpful catalogue of inscriptions in “Hebrew and West Semitic Inscriptions and Pre-exilic Israel.” In this article Lemaire limits his coverage to materials from the ninth century B.C.E. onward, a time during which Northwest Semitic inscriptions flourished. Lemaire contends that inscriptions during this period “can sometimes indirectly shed light on the earlier period” (from the beginnings of the Iron Age until about 830 B.C.E.). Lemaire first surveys the following inscriptions: Tell Fekheryeh, Moabite Stone, and Tell Dan. The reason for dealing with these three major inscriptions first no doubt stems from their importance in establishing the Bet-David. In addition to inscriptions such as the Zakkur Stela, Deir ‘Alla, Çineköy, the Aramaic stela from Bukân, Kuntillet ‘Ajrud, and Khirbet el-Qom, other elements such as seals, bullae, and Philistine inscriptions are examined for their
usefulness in illuminating the preexilic period. Lastly, a number of ostraca are briefly touched upon, as are the silver amulets from Ketef Hinnom. Lemaire concludes, “One thing, however, is clear: even if virtually all the papyri and all the parchments of the monarchical period no longer exist and have perhaps disappeared for ever, writing was well known in the kingdoms of Israel and Judah before 587 BCE” (378). Refreshingly, Lemaire puts to rest the ridiculous idea that the Siloam Tunnel inscription is a product of the Hellenistic era. He notes that this idea “is not serious from the point of view of epigraphy and archaeology” (378).

In the last essay, “Hebrew Poetic Structure as a Basis for Dating,” Terry Fenton seeks to demonstrate the antiquity of Hebrew poetry. Copious examples are adduced for the reader in both Hebrew as well as ancillary languages. Fenton first examines the concept of “climactic” or “staircase” tricola in Hebrew poetry. He subsequently presents his case by appealing to the phenomena of “forked parallelism.” In each of these two distinct rubrics Fenton seeks to flesh out the comparison between early Canaanite and Hebrew poetic structures. Fenton notes, “Details of content show that the time span of the biblical Hebrew literary tradition runs from at least the eleventh century BCE to the Persian period” (408).

I will not say that this work has sounded the “death knell” for the Copenhagen school. Scholarship has too often dismissed this school of thought out of hand or “finished” in a terminal sense only to see the adherents rise once again to the scholastic challenge. However, the essays presented in this volume all share a common ideology that grants that preexilic Israel (or parts thereof) can and should be reconstructed via historical inquiry. Far from being polemical in nature, each of the authors precisely stakes out a thesis or theme to which the Copenhagen school has objected or denied that it existed. Each scholar meticulously brings to bear the full weight of his or her collective acumen to demonstrate that one can shed light on this formative period of Israelite historiography. This book must be read by those interested in the “other” side of the “everything-is-late” debate raging in the academic guild. John Day must be congratulated on the fine job of editing and shepherding the papers to full fruition for readers to enjoy. Most helpful are the copious bibliographies appended at the end of each article. Upper-level graduate students cannot afford to be without this magnificent work.