REVIEW OF WILLIAM G. DEVER,
WHAT DID THE BIBLICAL WRITERS KNOW AND WHEN DID THEY KNOW IT? (2001)
AND
WHO WERE THE EARLY ISRAELITES AND WHERE DID THEY COME FROM? (2003)

Rainer Albertz

In recent years William G. Dever, Professor of Near Eastern Archaeology and Anthropology at the University of Arizona, has written two books bearing conspicuous titles. The first, *What Did the Biblical Writers Know and When Did They Know It?*, was published in 2001 by Eerdmans; the second, *Who Were the Early Israelites and Where Did They Come From?*, was published in 2003 by the same publisher. Both books—the first more than the second—are very polemical rejections of historical minimalism, containing sharp attacks on Thomas Thompson and Philip Davies in particular, as well as the European Seminar in Historical Methodology more generally (2001, 7). Frankly speaking, I am somewhat startled by such personal and emotional polemic in our academic field; I have never encountered anything like this before or since.

1. *Prologue*

Let me start with some personal remarks. I first met Dever in 1993, during a symposium in Bern on "Ein Gott allein, JHWH-Verehrung und biblischer Monotheismus im Kontext der israelitischen und altorientalischen Religionsgeschichte." We seemed to have a good understanding of each other, since we both shared an interest in the actual religious life of Israelite groups that stood apart from official rituals and theological demands. Dever delivered a paper on the topic of "Ancient Israel Religion: How to Reconcile the Different Textual and Artifactual

Portraits?" It seems that my explorations of the differences between family and state religion offered him a possible way of explaining the differences between the two, differences which he had himself noted.

I duly presented Dever with a copy of my book, *Religionsgeschichte Israels*, on which he wrote a short but very kind review (see *BASOR* 298 [1995]: 44–45). When the English translation of my book became available, Dever wrote a much longer review (see *BASOR* 302 [1996]: 83–88). Dever had seemingly become aware that our two positions differ slightly in methodological and material respects. Dever felt obliged to supply data and to correct me where, in his view, I had overlooked or wrongly interpreted an important piece of archaeological evidence.

Personally, I think Dever is right in his repeated pleadings for a critical dialogue between biblical scholars and archaeologists. Indeed, no one can be an expert in both fields, especially not in these days of advanced specialism. On two occasions I attempted to strike up a dialogue with Dever, but I never got a positive response. My work is all but forgotten in the two books to be discussed here. Dever merely mentions my *A History of Israelite Religion* in a summarizing footnote (2001, 174 n. 16). Thus, my encounter with Dever ended somewhat disappointingly for me.

Nevertheless, I cannot help but still feel some sympathy with William Dever. And I see a kind of tragedy that this outstanding expert in Syrian-Palestine Archaeology, this most prolific of writers, has become bogged down with the kind of furious polemic that has featured in his publications.

In the first of the two books to be discussed here, Dever deliberately reveals his personal background and feelings: coming from a pious Irish-American family—his father was a preacher, characterized as an "old fashioned fundamentalist" (2001, IX)—Dever found a more liberal position through his academic studies. He was fascinated by his teacher at Harvard, George Ernest Wright, and abandoned theology, deciding instead to become an archaeologist and historian. Dever studied with Nelson Glueck at the Hebrew Union College. Starting out under the shadow of Albright, he liberated himself from the conservative concept of "Biblical Archaeology," which sought to verify the truth of the Bible by historical and artifactual means. Dever was one of the first to demand the independence of what has come to be known as Syro-Palestinian Archaeology from the Hebrew Bible. According to him, so far as the historical truth is concerned, the archaeological evidence should have priority over the biblical text (pp. 9–10). Yet Dever had to make the tragic discovery that he had been overtaken by others, scholars who were yet more radical than him. His own methodological demands were used not for constructing, but *deconstructing* the history of Ancient Israel. His own aim of writing a "secular history of ancient Israel" (pp. 86, 287) was used to deny any distinguished people bearing this name. In my view, this is the main reason for Dever's obvious anger with the minimalists, a collective of scholars whom he terms, strangely enough, "revisionists." By such a title, Dever seemingly means that that those to whom he assigns the epithet have somehow diverted from the right doctrine, a doctrine which is, of course, defined by him. In his view, the revisionists misuse and distort his archaeological approach. In addition to this, Dever clearly experienced some amount of personal hurt when, during several conferences, he was given only a minority position. He reports:

> At the 1996 national meeting of the Society of Biblical Literature, Thomas Thompson of Copenhagen triumphantly announced to a standing-room-only crowd that not only was there no "ancient Israel," but there was "no Judaism until the 2nd century A.D." His remarks were greeted with applause. Mine was the only voice raised in protest; but I was drowned out, and the chairman closed the session. Afterward, I found many of my colleagues dismayed, but only a few of us had seen the handwriting on the wall (a biblical allusion—Belshazzar's feast—for those who still response to such images). (2001, 7)

As the reference to Belshazzar of Dan 5 shows, the attack on the historicity of ancient Israel has taken on an apocalyptic dimension for Dever. Dever felt especially upset about the doubts cast about his professionalism, the suspicion of his never having left the Albrightian "Biblical Archaeology" position (2001, 33). Finally, he is apparently disturbed by the political use of archaeology in the contemporary struggle between Israelis and Palestinians, a struggle in which the latter have taken up the minimalistic position (2001, 8; 2003, 237–41). Keeping all these experiences in mind, one can perhaps understand Dever's furious polemic a little better.

Having supplied a background context, I wish now to start my review.

2. *Review of* What Did the Biblical Writer Know and When Did They Know It? *(2001)*

Dever wrote his 2001 book in order "not only to counter the 'revisionists'' abuse of archaeology, but to show how modern archaeology brilliantly illuminates a *real* 'Israel' in the Iron Age, and also to help foster the dialogue between archaeology and biblical studies that I had always envisioned" (x). Since Dever and the revisionists have similar methodological demands, he felt obliged to clarify his own approach. In Chapter

3 he gives a useful review of the development of archaeological studies in Palestine from the nineteenth century up to the present. He underlines that the discipline, once it had ceased to be seen as a way of supporting the Bible (i.e. "Biblical Archaeology"), came to be seen as the independent "New Archaeology," an area of study which was oriented towards cultural anthropology and which, for a time, was uninterested in history. But then the research turned to the new direction of "Post-Processual Archaeology", which came closer again to being history-writing. In this connection Dever points out that all Near Eastern archaeologists have always considered themselves "basically historians, not anthropologists" (p. 63).

Drawing upon Ian Hodder in his co-authored (with Scott Hutson) *Reading the Past* (1986), Dever observes that an artifact can be read like a text in the context of his assemblage (2001, 67). On this basis Dever tries to formulate his own approach, one which contrasts to that of the revisionists. But his position is not quite clear: on the one hand, he stresses that the "archaeological data" are the "'primary' sources for history-writing" (p. 89), this being because of their independent witness, their direct approach to reality and their concreteness. Here he praises archaeology over exegesis; only the former, he claims, "can only truly 'revolutionize' biblical studies" (p. 90). On the other hand, he reduces the priority of archaeology and pleads for a double approach to historical investigation: "what I propose here has to do with the independent but parallel investigation of the two sources of data for history-writing, and the subsequent critical dialogue between them that scholars must undertake" (p. 106). Here, Dever is looking for "convergences" between archaeological and biblical data; the model of dialogue between exegetical and archaeological experts was turned into a combination of textual and artifactual data. Since this process remains critical, it should not be discredited as akin to what older "Biblical Archaeology" had done (p. 106). A methodological bridge between textual and artifactual data could be the *Sitz im Leben* of form criticism (pp. 103–4). So, according to Dever's methodological reflection, "we are nevertheless almost totally dependent upon archaeological data for most of what we shall ever know" (p. 105), though the biblical text has supplied some important and useful information.

In order to demonstrate his distance to the old "Biblical Archaeology" Dever classifies many books of the Hebrew Bible as unhistoric. Among these he lists the Pentateuch, the book of Joshua, prophetic legends such as "the Elisha–Elijah cycle"(!), prophetic books, the Psalms and wisdom literature (pp. 101–2), although some of these do provide some information about daily life. Only the books of Judges, Samuel and especially Kings are of some historical value. In his overall historical judgment, Dever presupposes the classical source theory for the Pentateuch, which he still thinks valid (p. 102), and the hypothesis of a Deuteronomistic history in the form advocated by Frank Cross.

At the same time, in order to counter the scepticism of the revisionists, Dever tries to demonstrate the convergences between biblical and archaeological data by means of two different case studies. In the first case study he deals with the pre-state period. Summarizing the archaeological investigations—surveys and excavations—of around 300 Iron I settlements in the central hill country, Dever points out that his "symbiosis-model" conceptualizing an indigenous origin of Israel contradicts the view found in the Pentateuch and the book of Joshua, but converges with several details found in the book of Judges (p. 122, chart 125). Dever goes so far as to identify the hill settlers on archaeological grounds as "Proto-Israelites"; for him, the whole assemblage of farm-house compounds, terraces, plastered cisterns, stone-lined silos, unwalled villages and relative dearth of pork bones refers to an agrarian society with distinguished cultural features (large multigenerational families, no central authority, no large city temples). That this society, wherever its members may have come from, understood itself or were identified by others as "Israel" can be verified by the Merneptah inscription (1210 B.C.E.). Thus Finkelstein's, Thompson's and Edelman's scepticism toward ethnicity is unfounded.

The second case study has to do with the period of the United Monarchy. For Dever, the most significant criterion for defining "statehood" is the centralization of power (p. 126). According to him, many archaeological finds from the early tenth to the early ninth century point to a "large-scale process of organization and centralization" (p. 137; most prominent, for Dever, are the city gates of Gezer, Megiddo and Hazor). In accordance with the Solomonic lists of districts in 1 Kgs 4, Tirzah and Bet-Shemesh can be verified archaeologically as administrative centres for Northern Ephraim and Benjamin (pp. 142–43). Every detail of the biblical description of Solomonic Temple can be paralleled by Bronze and Iron Age temples of the region, the closest examples being in Northern Syria (pp. 155–56). Thus, according to Dever, there is no wonder that "today nearly all archaeologists recognize a small-scale but authentic 'state' in central Palestine in the mid-late 10th century, or the beginning of Iron II, on archaeological grounds alone" (p. 128). The biblical view, that this state was founded by David, is corroborated by the Dan inscription. A specific Judaean material culture cannot be distinguished before the ninth and eighth centuries (p. 130).

A nice little piece of evidence for the United Monarchy is mentioned later (pp. 213, 223): shekel-weights with hieratic symbols for numbers are spread out over all Judah and Israel, so they must have been introduced before the Divided Monarchy and presuppose a common economical and administrative unit (cf. Na'aman).

For the time of the Divided Monarchy Dever focuses more on the archaeological evidence for daily life. So, in a lengthy treatment he deals with religion and cult of this period. Here Dever presents a contrast: while the Bible is to be seen as "an elitist document," "archaeology at its best provides a graphic illustration of the everyday masses, the vast majority of ordinary folk, their brief lives forgotten by the biblical writers in their obsession with eternity" (p. 173). This reminds of the difference between "official" and "popular religion" or "family religion"—Dever uses the terms equivocally, but strangely enough distributes it to two different kinds of sources. I think that is a misleading overstatement. For sure, on the one hand, the Hebrew Bible also speaks of "family religion" and mentions dozens of deviating cults, as I have shown elsewhere in detail. On the other hand, archaeology can also tell us a lot about the official cult, and indeed would tell us even more if the main temples could be excavated (Jerusalem, Bethel etc.). Based on this unclear differentiation Dever deals with the very different kind of cult places without any systematic order (Bull Site, gate-shrine in Tirzah, Megiddo, Locus 2081, probably a family shrine, the cult places in Taanach, Beersheba, and Kuntillet 'Ajrud, the temple in Arad and the graves in Ketef Hinnom and Khirbet el-Qom). For Dever, "popular religion" is defined purely negatively: "Popular religion is an alternate, nonorthodox, nonconformist mode of religious expression... [I]t appeals especially to minorities and to the disenfranchised (in the case of Israel, most women)" (p. 196). Admittedly, "popular religion" has a lot to do with the needs of women, such as childbirth, but is it therefore an "almost exclusive province of women"? (p. 196). Dever critiqued Susan Ackermann and Karel van der Toorn for being unable to offer a working definition of "popular religion." Yet, in my opinion, it is a pity that Dever did not clarify his own historical and sociological categories relating to religion.

As far as religion and cult is concerned, Dever noted a contradiction between the Bible and the results of archaeology. In this case, he agrees with the revisionists that the biblical view must be deconstructed, even more so than he had himself done before. And he concludes: "It is by reading many of the biblical texts 'against the grain,' or despite their idealistic pretensions, that we may best get at the truth about ancient Israelite religions. This may not be the religious 'truth' that the biblical writers had in mind, but it is the historical truth, and that is our proper goal as archaeologists and historians" (p. 198). In my view, the recognition that not only the Bible, but all written sources must often be read "against the grain" in order to achieve a realistic historical reconstruction, is a banality. Yet, in contrast to Dever, I must insist that the concepts of the biblical authors, although they might sometimes be idealistic, remain likewise an important part of the religion of ancient Israel.

Dever sees more convergence with regard to many *realia* of daily life, among which he mentions such things as the benches sited at city-gates, seals, ostraca, inscribed decanters for libation, tombs, weights, scales, pottery, ivories, and secondary residences (Jesreel, Ramat-Rahel). In contrast to D. Jamieson-Drake (1991), Dever argues, on the grounds of schoolboy practice texts of the eleventh and tenth centuries (letters of the alphabet in Izbet Sartah, Gezer calendar), on the existence of an early "functional literacy" (2001, 203ff.). The first inscribed seals come from the ninth century, the small archives in Samaria, Arad, Lachish, contained texts from the eighth to sixth century. Dever reminds us that most of the texts written on papyri were lost due to the damp winter climate in Palestine (p. 209). For me, this is a striking example of how the same archaeological evidence can be interpreted in opposing directions.

In his 2001 book, Dever tries to equate the different types of pottery with Hebrew terms transmitted in the Bible. He admits that this equation is "still speculative and preliminary" (pp. 232–33), but nevertheless states that apart from the frying-pan, which is mentioned only by P and is archaeologically unattested before the Hellenistic period, all other kinds of pottery belong to the E II level. So, Dever concludes, the "biblical texts that mention these vessels—mostly the J, E, and D sources—were largely composed and edited in penultimate fashion precisely in that period, i.e., in the late Monarchy" (p. 234).

So, at the end of his book, has Dever answered the question posed in title: What did the biblical writer know and when did they know it? For Dever, the answer is that "They knew a lot; and they knew it early" (p. 273; cf. 295). For Dever, had the Bible been written in the Hellenistic period, it would have looked quite different—more like the book of Daniel.

3. *Review of* Who Were the Israelites and Where Did They Come From? *(2003)*

The second of Dever's books to be reviewed, *Who Were the Israelites and Where Did They Come From?*, was published in 2003. This work is less polemical in tone than the 2001 book, although it owes its origins to a public controversy surrounding the historicity of the exodus that arose in Los Angeles during the spring and summer of 2001, a debate in which Dever was involved (2003, 205). Here Dever presents a readily comprehensible and well-documented argument, dealing with such issues as why, on the one hand, Israel's exodus from Egypt must be judged as a historical myth when there is almost no external textual or archaeological confirmation, while, on the other hand, Israel's indigenous emergence in Canaan can be historically verified by ample archaeological evidence. The amassing and brief assessment of all the scattered archaeological results, along the synthesis of the methodological and scholarly discussions, are very helpful for those readers who do not possess archaeological expertise.

In spite of the dating in 1 Kgs 6:1, the exodus could not have happened in the year 1446 B.C.E., because, according to Dever, "the major break in the archaeological sequence in Palestine that would have to be correlated with a shift from 'Canaanite' to 'Israelite' culture occurred at the end of the Bronze Age, ca. 1250–1150 B.C." (2004, 8). Thus the Pharaoh of the exodus cannot be Tutmoses III, but only Ramses II. Dever notes that there is no archaeological evidence to support the existence of Pithom or Ramses, the places mentioned in Exod 1:11, during the thirteenth century B.C.E. Tel el-Maskuta, the first candidate for identification as Pithom, was settled in the Middle Kingdom and after that not before the seventh century B.C.E.; Tell el-Retabeh, the second candidate, was not resettled before the late Rameside period in the twelfth century. Tel el-Dab'a, which was previously equated with Ramses, was destroyed in 1530 and rebuilt by Ramses II, yet no slave camps were found there. Nonetheless, Dever will not exclude that here and somewhere else "Asiatic slaves—among them possibly the ancestors of the Israelites—may indeed have been employed in making mudbricks (Exod 5:5–21) for Ramses II's construction projects there and elswhere in the Delta" (2003, 15). Some other evidence contradicts the reliability of the exodus report strictly: the Egyptian fortress Migdol, mentioned in Exod 14:2, was only settled in the Saïtic period (seventh–sixth century); in Kadesh Barnea (Tell el-Qudeirat), where the Israelites are said to have camped for many years (Num 13; 14; 20), there existed only a small fortress from the tenth–seventh century. Dever summarizes: "Thus after a hundred years of exploration and excavation in the Sinai desert, archaeologist can say little about the 'route of the Exodus'" (2003, 20).

In the next step Dever convincingly demonstrates that also the Israelite conquest of Canaan, reported in the Pentateuch and the book of Joshua, could not have happened. He briefly investigates 23 different settlements in the Negev, Transjordan and Cisjordan, all which could be identified with alleged conquered cities in the Hebrew Bible. The results are almost exclusively negative: "There is no Late Bronze Age Canaanite occupation of the 13th century B.C.E. at Tel Masos (Horma), nor anywhere in the northern Negeb" (2003, 27). In Transjordan only one of the excavated sites was conquered during the period in question, Tell el-'Umeiri south of Amman—but that is not mentioned in the Bible. In Cisjordan only the destruction of Hazor in the mid- or late thirteenth century, which was confirmed by a new excavation, can probably be brought into connection with the Proto-Israelites (pp. 66–68). Dever summarizes: "Of the more than forty sites that the biblical texts claim were conquered, no more than two or three of those that have been archaeologically investigated are even potential candidates for such an Israelite destruction in the entire period from ca. 1250–1150 B.C." (p. 71). Thus, in Dever's view, the "Conquest Model" created by W. F. Albright, his pupils, and older Israeli archaeologist is definitively refuted.

Dever does not have much sympathy for the alternative model of a "Peaceful Infiltration" created by A. Alt and M. Noth; he mentions it only briefly (pp. 50–52). For him, such a process is highly improbable, since ethnographic studies have shown that nomads usually do not settle of their own initiative. In a detailed discussion of I. Finkelstein's theories, Dever argues that the number of nomads from the hill country in the Late Bronze Age was simply too small to explain the "demographic explosion" in the new settlements. Presupposing that some 10 to 15% of an estimated total hill-country population of 12,000 people were nomads in the Late Bronze Age results in a total nomadic population of just 1,200 to 1,500 nomads—far to few when compared with the estimated population of ca. 50,000 in the 350 Iron I villages. Moreover, nomads would not have had the experience to establish a flourishing agrarian economy in a difficult rocky environment. Dever questions Finkelstein's archaeological results: his claim that most of the first Iron I sites in Ephraim "are located in the desert fringe, and that this proves that the first 'wave' of settlement reflects pastoral nomads settling down, rests on four sites and four identifiable sherds" (p. 161). His own excavation of Izbet Sartah refutes the thesis of a general direction of colonization from the east to the west. Likewise, Dever rejects the argument of Finkelstein and Fritz that the oval or circular plan of some Iron I settlements (Izbet-Sartah, Tell Masos, Beersheba) and the ground plan of the four-room houses reflect the position and division of nomadic tents. Although Dever will not deny that some nomadic elements (Shasu) belonged to the Proto-Israelites (pp. 180–82), he insists that the emergence of early Israel cannot be explained as a sedentarization of nomads.

Dever shows much more sympathy with the "Revolt Model" created by G. Mendenhall and N. K. Gottwald, a model which he praises as "one of the most highly original contributions to American biblical scholarship in the 20th century" (p. 52). It is to be noted that Dever has been one of Gottwald's principal archaeological informants (p. 53). In his present book, Dever looks more critically at this model; nevertheless, but accepts that, "stripped of its Marxist baggage, the peasant revolt model can still be useful" (p. 74). It has, for Dever, the important advantage that "it draws attention for the first time to the largely indigenous origins of the early Israelite people" (p. 74). Dever emphasizes: "Gottwald was right: the early Israelites were mostly 'displaced Canaanites'—displaced both geographically and ideologically" (p. 54).

On the basis of this general insight, Dever presents in detail the results of the excavations and surveys of the Iron I settlements in the hill country (Chapters 5–7 and Chapter 10). The excavations in Raddana, Tel Masos, Giloh, Izbet Sartah, Shilo, Beersheba, and Khirbet ed-Dawara (the only fortified settlement) verify, for him, a family-based agrarian society in the Iron I villages; according to the surveys in Galilee and the West Bank, 93 percent of the 350 villages were newly founded. Two insights are fundamental to Dever: the first is "the population explosion in the 12th century" (p. 98) which increased the population in the hill country from an estimated 12,000 people during the Late Bronze Age to estimated 50,000 in Iron Age I. Dever (p. 99) quotes with approval the words of L. E. Stager: "There must have been a major influx of people into the highlands in the twelfth and eleventh centuries B.C.E." The second is the evidence "that the inhabitants were farmers and stock-breeders who had long previous experience with the problems of local agriculture in Canaan" (p. 107; cf. the silos and the large quantity of cattle bones). Thus Dever concludes that most of the inhabitants must have come from the Canaanite lowlands.

Dever reconstructs the reasons for this "mass migration" within Canaan along the lines proposed by N. K. Gottwald, mentioning the decline of the Late Bronze culture, the political instability of the Canaanite city states, the social gap between extremes of wealth and poverty, and the collapse of international trade (pp. 168ff.). Accordingly, Dever takes up Gottwald's and Mendenhall's term, "withdrawal." Yet, in contrast to these writers, Dever states: "It was not flight from intolerable conditions or necessarily a revolutionary Yahwistic fervor that propelled people toward the frontier, but rather simply a quest for a new society and a new lifestyle. They wanted to start over. And in the end, that *was* revolutionary" (p. 178).

Dever calls this revised theory an "agrarian frontier reform model," because according to him the "land reform must have been the driving force behind, the ultimate goal of the early Israelite movement" (p. 188). And he compares the Proto-Israelites with radical U.S. settler groups like the Amish in Pennsylvania. Thus, the second question of the book's title is answered: "Where did they come from?" They came from different parts of Canaan, but mostly from the lowlands.

Finally, Dever deals with the question of the identity of early Israelites (Chapter 11). Dever had already pointed out that in his view the Proto-Israelites were "a motley crew" (pp. 181–82). They consisted of urban dropouts, 'Apiru and other "social bandits," refugees, displaced villagers, impoverished farmers, local pastoral nomads "including some from the eastern steppes or Transjordan (Shasu), and even perhaps an 'Exodus group' that had been in Egypt among Asiatic slaves in the Delta" (p. 182). Nevertheless, Dever is convinced that these different groups quickly constructed a new ethnic identity after having become "agrarian reformers with a new social vision" (p. 191). Accepting to the definition of ethnicity espoused by Fredrik Barth (1969), Dever points out that the country pioneers partly already stood in a common continuity with the Canaanite culture (pottery, art, language, and religion), but that they went some way to creating a new agrarian lifestyle, a new economy, a new type of farm houses, and a deviant social structure and political organization with specific common values. These are, for Dever, markers significant enough to distinguish a new ethnicity.

In order to determine this new ethnic identity as "Proto-Israelite," Dever deals not only with the Merneptah stele (pp. 201–8), but also refers to the strong continuity between the Iron I and Iron II culture, the latter of which can be clearly determined as Israelite. There is a continuity of settlement (Dan, Hazor, Beth-Shean, Tirzah, Beth-Shemesh, Lachish, Beersheba a.o.), a continuous demographic, technological and political development, and a continuation of the typical house-type and several Canaanite rites and beliefs (pp 195–200). In the end, however, Dever limits his ethnic definition "Proto-Israelites" to the settlers on the Samarian hill country, since the settlements in upper Galilee, on the

Judaean hills and in the Negev Valley show some differences in the material culture and social organization (pp. 208–21). This late limitation is somewhat confusing, since Dever has previously used the data from all the hill country areas in order to describe the new Iron I society. In any case, the first question posed in the book's title—"Who were the early Israelites?"—can be answered: the early Israelites were the Iron I settlers of the Samarian hill country, who came from different areas of Canaan, but mostly from the urban and agrarian lowlands.

In the final chapter of the book, "Salvaging the Biblical Tradition: History or Myth" (pp. 223–41), Dever feels obliged to mediate between his partly negative results and the biblical tradition. Having denied the historicity of the Exodus at the beginning of his book, he now asks now why the Exodus–Sinai tradition should become so dominant in the Bible. For Dever, there exist some hints in the Joseph story that minor elements of the tribes Ephraim and Manasseh "probably had come out of Egypt to Canaan, and in a way that upon reflection seemed miraculous to them. Later they assumed (or dictated?) that other of the heterogeneous groups that had made up early Israel had had the same experience" (p. 231). Likewise, he believes that "some of these 'Shasu of Yhw' were among the tribal peoples who became early Israel, and that they may indeed had been guided through the desert by a charismatic, sheikh-like leader with the Egyptian name of 'Moses'" (p. 237). These assumptions are reminiscent of M. Noth and indeed my own religious-historical reconstruction (Albertz 2004). They are, however, rather surprising given that Dever can elsewhere emphasize that "there is no longer a place or a need for the Exodus as a historical explanation for the origins of Israel." It "is best regarded as a myth" (p. 232). Dever makes a similar statement with regard to Moses: "Current theories of 'indigenous origins' for early Israel have no place for Moses, nor any *need* of him" (p. 235). Did Dever not just develop such a theory himself?

So, at the end of his book, Dever's position, which he sees as "aligned with the middle-of-the road option" (p. 226), seems to be not so clear. On the one hand, he claims "that the newer and sometimes revolutionary archaeological evidence must become our primary source for writing (or rewriting) any history of early Israel" (p. 223), while on the other hand he seems to be startled by the negative results of his own methodology and ready to make some concessions to the biblical text and to his pious audience. Perhaps Dever reveals the hidden reason for this inconsistency when he emphasizes at the end of his book that the controversy about the origins of ancient Israel is not simply an antiquarian pursuit: for him, "It is very much a question of our own self-identity, for in some ways we see ourselves as the New Israel" (p. 237). Thus the American foundation myth stood in the background of this historical investigation.

3. *Epilogue*

The two books by William Dever discussed in this review clearly reveal the ambivalence of current researchers working on the archaeology of Palestine towards using the biblical text as a source for reconstructing the history of ancient Israel. As far as developments of *longue* or *moyenne durée* are concerned, for such topics as Iron I settlement of the hill country or family-based agrarian production, archaeology is able to supply impressive and illuminating results. On the hand, as far as the more concrete political, cultural, and religious aspects of the history of Israel are concerned, archaeological evidence seemingly becomes more ambiguous. To give just a few examples: whether the Iron I settlers in the hill country can really be identified with the early Israelites or not remains questionable, since clear inscriptions from Iron I are lacking. On the basis of the same archaeological evidence, one can claim an early (so Dever) or a late (so Jamieson-Drake) literacy in Israel and Judah. Also, on the basis of the cultic artifacts from Iron I, nobody would suppose that these Israelites venerated the god YHWH at all—indeed, the evidence offered by the bronze bull figurine from the Samarian hill country would probably suggest the worship of the god El. On this last example Dever makes a remarkable comment:

> Curiously enough, religion and cult—which Mendenhall, Gottwald, and many other biblicists have taken as a crucial factor in the "social revolution" that produced early Israel—is virtually unattested archaeologically. (p. 126)
>
> The deity Yahweh is attested as early as the 13th century B.C. in Egyptian texts that place him among the Shasu-bedouin of southern Transjordan... But archaeologically Yahweh is invisible in Iron I villages... The apparent silence of the archaeological record may be misleading, however, because we lack any written texts, and these would be necessary to characterize early Israelite ideology in any depth. (p. 128)

That means that, on the basis of archaeology alone, one could conclude that there existed no YHWH worship among the Iron I settlers on the hill country. This conclusion is made by the minimalists, of course; and there is no strong archaeological argument that could hinder them. Negative archaeological evidence is taken by them as proof of non-existence. Yet Dever tries to escape this conclusion by admitting that the given archaeological evidence may be misleading, noting that because we "lack any written texts" we would need to make a reasonable decision. Thus he admits the lack of an important type of archaeological source material, material which would be necessary for a proper reconstruction of an important detail in the early history of Israel.

Dever agrees with the revisionist in claiming time and again the priority of archaeological sources over the biblical text (p. 71). And I think nobody would oppose this methodological statement had we the normal diversity of archaeological findings in Palestine—not only nice architectural remains and many artifacts, but also some interesting inscriptions and literary texts, as is usual in many other regions of the Ancient Near Eastern (Egypt, Asia Minor, Northern Syria, Mesopotamia). Yet among the findings unearthed by archaeological excavations in Palestine, inscriptions are relatively sparse and literary texts are lacking completely. All that has been found are a few inscriptions, some letters, many notes, a lot of seals and seal impressions, and some interesting graffiti. In spite of every effort, archaeologists working in Palestine have yet to find any major palace or temple archive, and they not yet found even a single royal inscription of an Israelite or Judahite monarch. Dever mentions the fact that all of the more important texts were written on papyri, yet almost none of these has survived—most likely because of the wet winter conditions experienced in Palestine (2001, 209). In my opinion, however, he did not seriously consider the far-reaching consequences of this fact.

Taking the lack of textual findings into account, I would like to question whether one can still claim, as Dever does, that the "archaeological evidence must now become our primary source for writing (or rewriting) any history of early Israel" (2003, 223), or writing history more generally (2001, 89). In my opinion, this methodological rule would overtax the archaeological evidence gathered from Palestine thus far. No matter what one thinks of the historical value of the Hebrew Bible, by any account it offers some detailed textual information. It is clear that, mainly because of the lack of usable texts found so far, the archaeological results available at present are not sufficient for writing a history of ancient Israel in any detail. This statement can easily be verified by the fact that the minimalist historians are able to deny the existence of an ancient Israel before the ninth, the fifth or even the second century B.C.E. on the basis of the same set of archaeological data. Thus I plead for the giving up of ambitious claims that archaeology should have methodological priority, so long as no further inscriptions and literary sources are found. Facing the given sparseness of our historical data concerning ancient Israel, we should place equal weight on the archaeological and biblical data. Whether we can reconcile converging biblical and archaeological data, and deciding which element of data should take precedence cannot be decided generally, but can only be handled on a case-by-case basis. In order to make this decision, in my view, a much higher degree of exegetical sophistication is required.

The last methodological demand is not too far away from what Dever has argued in his two books. Nevertheless, I think that he should have given a more rational assessment of the limitations of the present state of the archaeology of Palestine, pointing out what it realistically can contribute to the writing of Israelite history and what not. Despite Dever's claim that, "As an archaeologist, I could easily write a 1000-page, richly documented history of an 'ancient Israel' in the Iron Age and the early Persian period" (2001, 296), I have my serious doubts about what kind of history that would be.