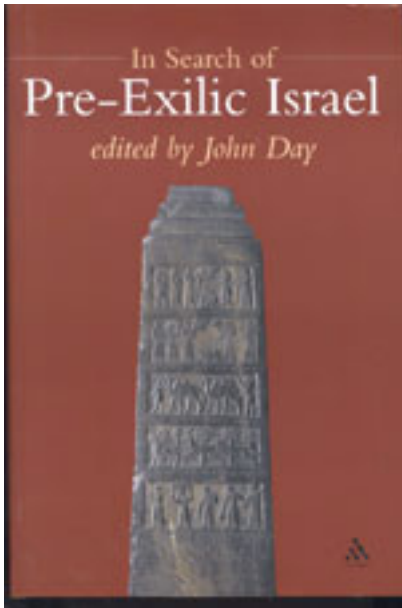


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**Day, John, ed.**

***In Search of Pre-Exilic Israel: Proceedings of the Oxford Old Testament Seminar***

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In 2001–2003 the Oxford Seminar held a series of lectures on preexilic Israel, now collected in this book. The purpose of the volume is stated to be to offer a critique of various aspects of the “everything is late” school of thought, “not from any reactionary standpoint but from a thoroughly reasoned, critical point of view” (vii). Seventeen essays cover a wide variety of topics, including some of the main issues that one would expect to be addressed in such an endeavor. A number of themes can be distinguished, cutting through this collection.

Several of the contributions are straightforward investigations of specific passages or writings, asking what might be preexilic. John Barton (“Dating the ‘Succession Narrative’”) gives a solid survey of past scholarship and the present situation. He concludes that it is later than G. von Rad (1944) had assumed but thinks it could be the seventh or even the eighth century B.C.E. J. A. Emerton (“The Date of the Yahwist”) gives mainly a response to H. Vorlaender (1978), concluding that J is preexilic. Hugh Williamson (“In Search of the Pre-exilic Isaiah”) considers how one can separate early from later traditions in Isaiah. He advances five methods, for each of which he gives examples and illustrations, with a sixth tacked on. They are all common-sense approaches and yield reasonable results, even if no method is foolproof. David Reimer (“Jeremiah

before the Exile?") takes up the guidelines of Philip Davies to look for preexilic remnants in Jeremiah, and he finds some.

John Day's question, "How Many Pre-exilic Psalms Are There?" comes up with a number of categories of psalms that are likely to be preexilic (e.g., the royal psalms and the inviolability of Zion psalms), a few likely to be exilic, and some likely to be postexilic. The arguments are mostly reasonable, though at times he simply appeals to current consensus. A table of the psalms dated to particular periods would have been helpful. Katharine Dell ("How Much Wisdom Literature Has Its Roots in the Pre-exilic Period") also argues too often from current opinion, but she challenges a number of views (rightly, in my opinion) about the wisdom literature, especially that it is all late. But the earlier material is found almost entirely in Proverbs, as one might expect (though I personally would argue that the poem of Job is much older than often recognized).

Bernard Levinson's title tells what he wants to do: "Is the Covenant Code an Exilic Composition? A Response to John Van Seters." He expresses his appreciation for Van Seters's contributions to the debate (Van Seters 2003), including his often well-aimed critique of previous positions. Nevertheless, a first-millennium borrowing of Mesopotamian law into the Covenant Code (with which Levinson agrees) does not have to translate into "Neo-Babylonian"; the Neo-Assyrian period is more compelling, Levinson argues. W. G. Lambert gives a quick survey of "Mesopotamian Sources and Pre-exilic Israel," with a critique of P. R. Davies (1992) and N. P. Lemche (1998). Although the subject is far too large for a brief essay, he makes some valid points (especially on the nature of Akkadian inscriptions), but he is not up to date with current scholarship on some of the biblical passages he treats. André Lemaire also gives a very helpful survey of "Hebrew and West Semitic Inscriptions and Pre-exilic Israel," but it must be said that some epigraphers have serious doubts about some of the unprovenanced inscriptions that he cites (e.g., the "Berekiah son of Neriah" seal).

A major theme is a specific evaluation of the "revisionists." This is begun by Ernest Nicholson ("Current 'Revisionism' and the Literature of the Old Testament"), who discusses the term extensively but mainly follows James Barr (2000) in his critique. Anthony Frendo has some good points in his rather pompously titled article ("Back to Basics: A Holistic Approach to the Problem of the Emergence of Ancient Israel"), such as his discussion on the Merneptah Stela, but one has the impression that he does not entirely control the mass of material he cites. In any case, he mainly picks and chooses among scholarly opinions rather than developing a proper argument. In his "Histories and Non-Histories of Ancient Israel: The Question of the United Monarchy," William G. Dever says a lot of what he has been saying in recent articles over the past ten years, and with much the same tone. When he says that with those he is critiquing, "Reasoned, well-

documented dialogue has given way to escalating rhetoric and near-hysterical personal invective,” I cannot help feeling that this often applies to Dever. He has himself been the object of personal invective, of course, but I do not think his own personalizing of the critique helps his case to a wider readership, however much his immediate audience might have liked it. Dever is not a bumbling, no-nothing expounder of prejudices with poor logic and no evidence—but then neither are those whom he calls “revisionists.” If they are so ignorant and incompetent as the essay alleges, why does anyone find them persuasive? A number of Dever’s criticisms are well taken, if only he would use a more reasoned vehicle that recognizes there are valid arguments on the other side as well.

In other articles, Graham Davies asks, “Was There an Exodus?” He surveys nearly all the evidence available, but little of it is very compelling. Indeed, he finds the attempts to equate the exodus tradition with certain figures known from Egyptian sources as “not very convincing.” Thus, his conclusion that the exodus “tradition is a priori unlikely to have been invented” appears tacked on rather than arising from his data. Walter Houston asks, “Was There a Social Crisis in the Eighth Century?” Although he notes that some of the arguments are problematic (e.g., the reign of Jeroboam II was not particularly prosperous), there was a social crisis, with the lowest stratum of society having a difficult time. Gary Knoppers (“In Search of Post-exilic Israel: Samaria after the Fall of the Northern Kingdom”) states both the maximalist and the minimalist case for the destruction of the northern kingdom, then finds a *via media* through it. B. A. Mastin on the whole presents a solid treatment of “Yahweh’s Asherah, Inclusive Monotheism and the Question of Dating”; however, his slap at the “revisionists” for arguing for polytheism in early Israel appears unjustified. Quite a few scholars now accept this view (including Dever!), and it is Mastin’s attempt to refute this conclusion that looks idealistic. In “Hebrew Poetic Structure as a Basis for Dating,” Terry Fenton argues for an early use of the “staircase tricola” and “forked parallelism,” known from Ugaritic poetry, which were then subsequently developed and altered. It thus serves as a device for the general dating of some texts.

This is a valuable collection that brings together a good deal of recent scholarship, especially from the United Kingdom, and will serve as a “state of scholarship” survey in some ways. But there are three ways, in my opinion, in which it exhibits some significant weaknesses of which readers should be aware. First, its onslaught on “the revisionists” is not well conceived. There is no “school of revisionists.” T. L. Thompson and N. P. Lemche might be willing to be a “Copenhagen school,” despite some differences between them, but Philip Davies has a rather different approach. Keith Whitelam has some things in common but also other quite different concerns. Herbert Niehr is brought into the fold by Mastin. As for Israel Finkelstein—not normally mentioned in this context but insisted upon by Dever—he has many differences from Thompson and Lemche. Yet Dever even

speaks of Nadav Na'aman as “a ‘revisionist’ of sorts,” though he then turns around and identifies himself as a revisionist! Nicholson also comments at length about constant revision in scholarship as a whole (1–6). So why this crusade to attack “the revisionists”? Dever’s attempt to contrast them with “‘revisionists’ in the proper sense, constantly revising their histories in the light of new discoveries” (70), suggests that “revisionist” has simply become a slogan to stigmatize and dismiss those with whom one disagrees. The named individuals are all serious scholars. One may not agree with them, but they have been influential on the field because they have had important things to say.

The second problem is a related one: that of an overall British conservatism. As one might expect, most—though by no means all—contributors are British, and British scholarship has been characterized by a moderate to conservative approach in many areas. There are many exceptions to this statement, but as someone whose roots are well outside the British tradition, this is my reading of the situation. The result is a certain bias against any attempts to change the consensus. Regardless of whether this analysis is correct, though, the impression is given to the reader that the contributors were invited “to get the ‘revisionists’” or chosen because they would take an “antirevisionist” approach. As already noted, not all the contributors have such a tone to their papers, but a number do. That does not sit well with the statement that the aim was not to approach the topic from “any reactionary standpoint but from a thoroughly reasoned, critical point of view”—especially if the slant of the papers was determined in advance.

Finally, I am sorry that the editor (or someone else) has not given a proper introduction to the volume. Those who have experience with editing know that the amount of work needed just to get a collection of papers between two covers should not be belittled. Nevertheless, it seems to me that the editor also has the duty of summarizing the papers or at least indicating the main themes and how different individual papers relate to the themes. This would have made the volume more usable to those who wanted only to read certain topics but not necessarily the whole collection.

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