

produced a volume of consistently high quality. In the Introduction acknowledgment is made to Cynthia Thompson, of Westminster Press, as the person most responsible for conceiving and encouraging the project.

The editors express their faith and hope in the project by saying, "There is a great sense of empowerment . . . that comes from reading the Bible as a woman in the company of other women. That is an experience that this volume is intended to assist" (p. xv). In my judgment *The Women's Bible Commentary* is an important statement, equally imperative for men as well as for women to read and ponder.

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Studies in Hebrew and Aramaic Orthography, by David Noel Freedman, A. Dean Forbes, and Francis I. Andersen. Biblical and Judaic Studies from the University of California, San Diego 2. Winona Lake, IN: Eisenbrauns, 1992. Pp. xii + 328. \$32.50.

In the Preface to this book, series editor William H. Propp explains that the volume (henceforth *SHAO*) grew out of a conference held at the University of California at San Diego in April, 1986. Among the featured speakers at the conference were David Noel Freedman, A. Dean Forbes, and Francis I. Andersen, all of whom are well known for their previous work on the orthography of the Hebrew Bible. The latter two, of course, are the authors of an earlier volume entitled *Spelling in the Hebrew Bible* (BibOr 41; Rome: Biblical Institute Press, 1986) (henceforth *SHB*). The fifteen individual chapters and an appendix are written by Freedman, Forbes, and Andersen separately, or by the combinations of Andersen-Forbes and Andersen-Freedman.

To a great extent the principal findings of the present book already may be found in *SHB*. Accordingly, I am forced to state that much of *SHAO* is simply a rehashing of *SHB*. The 1986 book truly was a pathfinding and important volume; one cannot say the same about the volume under review. For example, the most general conclusion we learn is that as a whole the Masoretic Text "preserves a type of spelling that was normative in the 5th–4th centuries, which coincided with a great revival of classical Hebrew, clearly post-exilic and just as clearly pre-Qumran or Maccabean" (Freedman, p. 15). We learn further that the Pentateuch has a more conservative spelling (i.e., it uses fewer *matres lectiones*) than the other sections of the Bible, and among individual books Exodus has the most conservative spelling. But all of this may be found in *SHB* (pp. 312–314).

There are of course new discussions in *SHAO*, including detailed treatments of various documents, e.g., the Tell Fekheriyeh inscription in Aramaic, several Qumran texts, and the Kasaru-Bazar Codex (Petersburg, Russia) of the Latter Prophets (if not indicated to the contrary, the discussions in *SHAO* that deal with the Bible refer to the famous Leningrad Codex which serves as the basis for *BHS*). A fascinating study is the appendix by Andersen and Forbes, "What *Did* the Scribes Count?"; referring to the counts of letters, verses, middle letters, middle verses, etc., in traditional Jewish sources.

Since I have praised *SHB* as a pathfinding and important piece of research, and since I have stated that the new book presents much of the same material, it is clear that my general impression of *SHAO* is positive. It indeed makes for interesting reading.

I apologize, therefore, if the remainder of this review raises three critiques. I raise them with the hope that they will advance the discussion in the future.

I would take issue with the previously cited phrase “a great revival of classical Hebrew” in the Persian period. I know of no evidence that suggests this is the case. All of the books usually dated by scholars to this period reflect Late Biblical Hebrew, not the Standard Biblical Hebrew of the pre-exilic period. For a revival to occur, there must be an intervening period of decline.

Is Freedman implying that the sixth century, let us say, reflects such a decline? Works such as Ezekiel and Second Isaiah show already some of the signs of Late Biblical Hebrew, but the works of the fifth-fourth centuries (Chronicles, Ezra-Nehemiah, etc.) reflect this stage of the language even more so. Furthermore, even if Freedman were correct, it is not entirely clear to me that a revival in the *language* (a linguistic phenomenon) would necessarily have an effect on or be paralleled by *spelling* practice.

A basic premise of all three authors is that the Masoretes performed the task of vocalizing the consonantal text before them. This premise should be questioned. Recent studies, mainly in Israel, have concluded that Masoretic activity was chiefly one of *pointing*, not of *vocalizing*. That is to say, the Masoretes of the ninth century CE were simply recipients of reading traditions of great antiquity. The Tiberian Masoretes invented a series of dots and dashes to mark the vowels of one such reading tradition, but they did not determine what the vowels were to be. Thus, a statement such as the following needs to be rethought: “2 Kings 19:23 has *mbhw*, which can only be pointed *mibhō*, whereas the parallel text in Isaiah 37:24 has *mbhr*, which could have been pointed *mibhō* but was pointed *mibhār* [*sic*: the word actually appears with *pataḥ* not *qames*]” (Andersen, p. 66). The statement that *mbhr* “could have been pointed” *mibhō* suggests that the Masoretes were making decisions on how to pronounce individual words. I do not think this was the case. Rather, as intimated above, they simply were heirs to a reading tradition that read *mibhar* in Isa 37:24, and thus they pointed the word with *pataḥ*. Now it is possible that at some earlier stage in the tradition, the presence of the *waw* in at least one manuscript of 2 Kgs 19:23 led to the pronunciation of the word in question with the [o] vowel, as opposed to the [a] vowel with which the parallel word in Isa 37:24 was read. But this would be pre-Masoretic, at least insofar as the term “Masoretic” is used and understood by most scholars.

Two scholars have been equally productive in recent years on many of the same problems addressed in this book. I would have liked to see some interaction between Freedman, Forbes, and Anderson and the works of James Barr and Saul Levin. The former is quoted only rarely and summarily dismissed (e.g., on p. 69); the latter is not referred to at all. This is most unfortunate. Scholarship progresses through the give-and-take among colleagues, and everyone benefits therefrom. Levin’s hypothesis is that the presence or absence of *matres lectionis* does not indicate simple spelling variation, but rather indicates actual differences in pronunciation. Thus, the appearance of *w* and *y* after /o/ and /i/ vowels, respectively, indicates the presence of an off-glide, either [o^w] or [i^y].

The statistics which Levin has presented in his various publications (see most importantly “The Hebrew of the Pentateuch,” *Fucus: A Semitic/Afrasian Gathering in Remembrance of Albert Ehrman* [ed. Y. L. Arbeitman; Amsterdam: John Benjamins, 1988], pp. 291–323) show a remarkable consistency, with factors such as stress determining the presence or absence of *w* or *y*. That stress may influence the presence

or absence of *w* or *y* is countenanced at one point in the book. In a discussion of the spelling of *māqôm*, we learn that the word is spelled *plene* 360x and *defective* 41x, with the further comment: "In forty cases the *ō* is not stressed, and allowance must be made for the influence of stress on spelling choice" (Andersen, p. 71). This comes very close to the kind of work undertaken by Levin; it is a pity that the authors of the book under review did not interact more directly with Levin's position.

If Freedman, Forbes, and Andersen are to return to the subject of ancient Hebrew orthography in future studies, I would urge them to utilize their advanced computer technology to test Levin's theory and findings, especially if Andersen already is aware of the possibility of stress as a factor in spelling choice. I stated that two books of such a similar nature as *SHB* and *SHAO* create a certain duplication, but I would welcome the opportunity to read still a third volume on the subject, if it moved in the direction here suggested.

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Die Hebräischen Präpositionen Band 1: Die Präposition Beth, by Ernst Jenni. Stuttgart/Berlin/Cologne: Kohlhammer, 1992. Pp. 398. DM 149.

Since prepositions appear over 60,000 times in the Hebrew Bible, an exhaustive analysis requires a little patience with a dose of stamina. The present volume classifies every one of the over 15,500 appearances of the preposition *-כּ*, and Jenni targets two more volumes to treat similarly the some 24,000 appearances of the prepositions *-ל* and *-ב*. A scripture index provides access to the one place where each verse is categorized, but unfortunately the many other pages where a verse may be discussed in more detail are usually inaccessible (e.g., Ps 69:22 is categorized on p. 209 but one can't know that the verse is discussed on p. 86). Even the at times skimpy table of contents does justice neither to the author nor his readers, for there is no way for one to find where, for example, the category *beth normae* is discussed (p. 148). Another omission, perhaps to the author's credit but his readers' loss, is the failure of Jenni's name to appear in the list of authors cited, consequently hindering access to a mine of bibliography for this insightful Hebrew linguist (see footnotes on pp. 11, 21, 37, 77, 101, 302, 307, 309, 313, 319–320, 332, 334).

This is one of those books whose simple profundity stuns the researcher into self-reproof with the query: "Why didn't I see that before?" For generations, each of the biblical Hebrew prepositions has been treated by lexicographers and grammarians as an independent entity whose meaning is lexically determined (e.g., *-כּ* is often perceived as signifying rest in a place and *-ל* direction toward a goal, and peculiar meanings may represent generalizations from an original specific meaning). Jenni's fresh perspective is anchored in contemporary linguistic and particularly semantic theory, prompting him to insist that the Hebrew prepositions can only be comprehended by context: the issue is not the meaning of the preposition itself but the relationship of the preposition to particular semantic classes of nouns and verbs (for the latter, one must be sensitive to distinctions not only between transitive and intransitive verbs but also verbs such as those that depict physical contact, social contact, *verba dicendi*, and others). "The fundamental thesis of the present investigation is that each preposition's usage