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## EVALUATING A NEW APPROACH TO THE LINGUISTIC DATING OF BIBLICAL TEXTS

*A review of:*

**Kim, Dong-Hyuk, *Early Biblical Hebrew, Late Biblical Hebrew, and Linguistic Variability: A Sociolinguistic Evaluation of the Linguistic Dating of Biblical Texts* (VTSup, 156; Leiden: Brill, 2013). Pp. xvii, 184. Hardcover. €98.00 \$133.00 ISBN 978-90-04-23560-1.**

### Summary

This slender monograph is a revision of a Yale University Ph.D. dissertation (2011) written under the supervision of Robert R. Wilson. The catalyst for the research was the decade-long (and continuing) debate between Avi Hurvitz and other “consensus” scholars or “traditionalists” on the one hand and Ian Young, Martin Ehrensverd, myself, and other “challengers” on the other, regarding the possibility of determining the dates of origin of Biblical Hebrew (BH) writings on the basis of their linguistic characteristics (pp. 1–2). Aside from the standard front and back matters, the body of the book has six chapters.

Chapter 1 (“Introduction”) introduces the problem (“Can we date biblical texts [to the preexilic vs. postexilic period] only on the basis of linguistic evidence?”), proposes that a (historical) sociolinguistic variationist approach can help to clarify the relationship between Early Biblical Hebrew (EBH) and Late Biblical Hebrew (LBH) linguistic variables (e.g.,  $\tau\epsilon\eta/\zeta\epsilon\eta$ ), and illustrates with an example from modern English (the pronunciation of the verb ending *-ing* as [n] or [ŋ] in Norwich, England) how a variationist analysis can be helpful for understanding language variation. (In the present context, a simplified definition of the “variationist approach” might be “a quantitative analysis of two or more linguistic variables, or ways of saying the same thing, as a way of detecting language change.”)

Chapter 2 (“Linguistic Dating of Biblical Hebrew Texts: A Survey of Scholarship”) reviews research on the linguistic development of BH,

beginning with the period from Wilhelm Gesenius (usually considered the father of the diachronic study of BH) to Yechezkel Kutscher (Hurvitz's teacher), followed by a discussion of the work of Robert Polzin and a longer treatment of the work of Hurvitz, and their followers, and continuing with a summary of the work of scholars who have challenged various fundamental presuppositions and methods in previous scholarship. The chapter concludes with a list of seven points of agreement and (mainly) disagreement between Hurvitz and his followers and the challengers.

Chapter 3 ("The Variation Analysis of the Hebrew Bible Corpus: The Method"), the second longest chapter in the book, aims to establish the methodological framework of the study. Kim discusses several of the foundational ideas of sociolinguistics (the discipline dealing with the relationship between language and society) and historical sociolinguistics, such as linguistic variation and variationist analysis, and he explains how he will apply such concepts to the corpus of BH. In particular, he introduces the distinction between linguistic or dependent variables on the one hand, and independent variables or factors which condition variant linguistic forms/uses on the other. Independent variables include a speaker's/writer's age, gender/sex, social class/rank/status, region, style, etc. However, very crucial for Kim's investigation of BH are the independent variables of time period and genre/text type. (I discuss these independent variables in more detail below.)

Chapter 4 ("Variability, Linguistic Change, and Two Types of Changes: A Theoretical Assessment") moves from method to theory, arguing that a variationist analysis is able both to explain the ambiguous distribution of linguistic data in BH and to accommodate the seemingly irreconcilable opinions of the traditionalists, in whose chronological model linguistic dating *is* possible, and of the challengers, in whose stylistic model linguistic dating *is not* possible. Important concepts introduced in this chapter include the distinction between early and late adopters of a

linguistic innovation and those who fall somewhere in between, and, also very crucial for Kim's investigation of BH, William Labov's distinction between linguistic changes introduced consciously, or from above social awareness, versus those introduced unconsciously, or from below social awareness. (I discuss changes from above/below in more detail below.)

Chapter 5 ("Variables of Biblical Hebrew: A Sociolinguistic Analysis of the Purported EBH and LBH Features"), the longest chapter of the book which comprises a third of the total pages, evaluates eight sets of morphological, syntactical, lexical, or phraseological variables in BH. They are:

1. -םהיתו vs. -מתו;
2. כ/ב + ו + inf. const. vs. כ/ב + ו + inf. const.;
3. דלמה + king's name vs. king's name + דלמה;
4. ל...יב... vs. יבו...יב...;
5. הוהי תיב vs. מיהלאה תיב;
6. תוכלמ vs. הכלממ;
7. להק vs. הדע;
8. קעז vs. קעצ.

Kim's empirical assessment of the BH data comprises a variationist analysis in which he attempts to correlate the linguistic (or dependent) variables with the independent variables of time period and text type. His objective is to arbitrate between the views of the traditionalists and the challengers, that is, between the arguments that these sets of linguistic variables are either diachronic or stylistic variants in biblical writings, respectively. His conclusion is that seven of the eight pairs represent authentic linguistic changes in progress in BH (all but 7), of which three represent conscious changes from above social awareness (3, 5, 6), three represent unconscious changes from below social awareness (1, 2, 8), and the direction (i.e., from above or below) of one change is unclear (4).

Chapter 6 ("A Sociolinguistic Evaluation of the Linguistic Dating of Biblical Texts: Summary and Conclusions") draws out the implications of Kim's

analyses for the current debate over the linguistic dating of biblical writings. In summary, first, against the challengers, EBH and LBH are not completely stylistic, because seven of the eight linguistic variables show a meaningful correlation between the choice of variant and the independent variable of time period; however, second, against the traditionalists, linguistic dating is not viable because it is impossible to distinguish between early and late adopters of any given linguistic innovation in BH and because linguistic changes which are conscious or from above social awareness are close to, if not the same as, "stylistic," and so they are unreliable criteria for tracking the linguistic chronology of BH.

## **Evaluation**

I find much to commend in this short study. It is well-written, clear and concise in its argumentation, relatively free of editorial mistakes, and well-illustrated with 31 figures and tables. Given the heated debate between traditionalists and challengers over the past ten years, I also find it refreshing that Kim manages to maintain a positive attitude toward both sides throughout his volume, and, in my opinion, succeeds in representing fairly and analyzing even-handedly the views of both Hurvitz et al. and Young et al. In terms of theory and method, Kim's monograph is the first significant attempt to examine and explain linguistic variation in BH from the standpoint of a historical sociolinguistic variationist approach.<sup>[1]</sup> As such it represents a fresh analysis which offers some new insights on the same old data many of us have invested so much time in studying. Personally, as one of the so-called challengers, the book has challenged me to reconsider, revise, and/or restate some of my arguments. And I should acknowledge up front that several of his conclusions are congenial to my own views: BH exhibits linguistic changes in progress; linguistic dating of BH writings is hardly possible. So, for example, I am well-disposed to a statement like this one:

For example, Hurvitz's linguistic dating of P to the preexilic period can in theory be valid *if* P was neither an early adopter nor a conservative with regard to most of the linguistic shifts from EBH to LBH—a proposition that we cannot defend empirically. If, however, P had been generally conservative in following most of the individual changes of the period—again, we cannot prove this—P could theoretically be placed to the exilic period or later, the position that many biblical scholars subscribe to (pp. 88–89).

I also think the discussion of “further implications” (pp. 155–60) is insightful and should prompt more detailed research on issues like so-called “transitional” BH.

However, and this a rather large however, although I totally agree with Kim that BH does exhibit linguistic changes in progress, I continue to believe that the distribution of many linguistic variables in BH, and on a larger scale the linguistic profiles of EBH and LBH, *are largely stylistic*, for reasons I give below. So, following brief comments on some relatively less significant issues and lengthier discussions of several key theoretical and methodological matters, I return to the issue of style.

*Minor quibbles:* An index of subjects would have enhanced the usefulness of the book. And more s-shaped diffusion curves would have been nice too. This concept is briefly introduced in chapter 3 (pp. 58–59) but the study of variables in chapter 5 has only two (incomplete) scatter plots (pp. 104, 126).

*Lateness of Chronicles:* To clarify, Auld and I have not challenged the overall lateness of Chronicles (pp. 31–33, 41–42), but rather the absolute lateness of many literary, textual, and linguistic details in Chronicles relative to Samuel–Kings. I have argued on the basis of literary-critical and text-critical criteria that Samuel–Kings cannot be reduced to the early or preexilic period only, and therefore, to some degree the production of Samuel and Kings was contemporary and even later than Chronicles. [2]

*Language change:* To clarify, Young, Ehrensverd, and I have never denied that BH changed through time (pp. 41, 43, 152–53; cf. discussions of “the challengers’ argument” in Kim’s chapter 5), or believed or asserted that BH is “the eternal language of creation” or anything similar, or advocated an “anti-diachronic” or “non-diachronic” approach to BH or the Hebrew Bible.[3] I am willing to admit, however, that in our zeal to disprove the possibility of linguistic *dating*, we have sometimes used unpropitious language, misjudged or underplayed the potential relevance of different proportions of linguistic variables in biblical writings, and in some instances even over-argued our synchronic or stylistic interpretation.

*Masoretic Text:* Kim remarks: “This conclusion of ours [against the linguistic dating of biblical writings], of course, is based on the discussion that has chosen not to consider text-critical issues. Considering them, no doubt, would work further against the validity of linguistic dating” (p. 157 n. 6). And he expresses his awareness of text-critical issues elsewhere when he decides not to base his linguistic arguments on synoptic material in MT Samuel–Kings//Chronicles (p. 71). Nevertheless, despite his neutral discussion of linguistic analysis of BH based mainly or only on the MT (pp. 21, 26, 35–37, 44, 63), in the end he decides, mainly for pragmatic reasons, to follow Hurvitz’s maxim that a linguistic study of BH should be based on “actual texts” rather than “reconstructed texts” (pp. 65, 67 n. 71, 161). I appreciate Kim’s sensitivity and practicality, but I consider his approach undesirable given the challenging nature of the texts of the Hebrew Bible and normal practice in historical linguistics.[4]

*Extra-biblical Hebrew:* Kim correctly explains the significance of extra-biblical Hebrew in Hurvitz’s dating methodology (pp. 14–15, 19, 22, 41, 46–47; cf. 30–31, 38–39, 43–44, 152), yet he decides to exclude the Hebrew inscriptions, Dead Sea Scrolls/Qumran Hebrew, Ben Sira, Bar Kochba, and Mishnaic Hebrew from his quantitative analysis, “mainly because our data from the Hebrew Bible are relatively well-defined in terms of time and genre” (p. 64; cf. 64–65), and therefore in his case

studies he says relatively little about the language of these writings (pp. 100–101, 103–108, 117, 119, 122, 129, 134 n. 85, 138 n. 95, 141). As with his decision regarding the MT, this decision by Kim is permissible for pragmatic reasons, yet a variationist analysis of BH should preferably, and ultimately must, include the entire corpus of ancient Hebrew, given the overall scarcity of the data at our disposal.

*Combining biblical books:* This and the following points are my main misgivings about Kim's argumentation. Kim intends his book to be chiefly a study of method or procedure (pp. 8, 161; cf. chapter 3 and pp. 153–54), and at its core it is a quantitative or statistical study of linguistic variables in a variationist framework.<sup>[5]</sup> One way that Kim seeks to boost the statistical reliability of the data is by combining biblical books. First I say a few words about grouping together different biblical writings, and then I look specifically at several matters related to statistics in particular. As we have discussed elsewhere, two persistent problems in historical linguistic research on BH are (1) overestimation of linguistic contrast between books written in EBH and LBH and (2) overestimation of linguistic uniformity in EBH books on the one hand and in LBH books on the other.<sup>[6]</sup> Very often it is the case when biblical books are combined that the patterns and preferences of individual books are obscured, and this clearly affects the appraisal of the linguistic data in one way or another. (I give some examples below.) Take, for example, Kim's treatment of Joshua–Kings, in addition to his and others' more typical groupings of EBH and LBH books. Kim mentions linguistic characteristics of the individual books of Joshua, Judges, Samuel, and Kings very infrequently in his case studies, and then usually only when quoting the discussions of others (pp. 108–109, 117, 123, 128–30, 134–35, 145–46, 152). His justification for this all-inclusive treatment of Joshua–Kings relates to his view of the Deuteronomistic History (DtrH) as a consistent and integrated composition of which the original sources and the canonical boundaries are unclear, “because of the Deuteronomist's freer adaptation of his sources” (p. 70).

This approach is unacceptable for a number of reasons. First, and this is mainly an observation, as far as I know Kim is alone in taking this approach in a diachronic linguistic analysis of BH. Second, in the MT Bible and Qumran scrolls, for example, we can see clearly the individual books of Joshua–Kings, but the DtrH is a scholarly construct, and some recent scholars doubt it ever existed. Third, it seems rather arbitrary to work with a DtrH but not a Pentateuch or Torah group, yet Kim is perfectly open to discussing the linguistic profiles of J, E, J/E, non-P, and P. Fourth, Joshua–Kings as a unit has nearly 70,000 graphic units and makes up almost 1/4 of the Hebrew Bible, thus it constitutes a disproportionately large group when compared to the other books of the Hebrew Bible. Fifth, in many, probably most appraisals of the DtrH (e.g., Martin Noth's), a preponderance of material in DtrH is traditional or pre-DtrH and the Deuteronomist's (Dtr) role relates more to the selection and arrangement of the diverse sources than to their (re)writing or revision.<sup>[7]</sup> Sixth, and most importantly, Kim's approach occasionally obscures preferences in the individual books for linguistic variables which are more characteristic of LBH than EBH or DtrH as a whole, such as Samuel's preference for מִהִיָּתוּ (cf. p. 103; case study 1), Kings' for לֶהֱקֵ (cf. p. 143; case study 7), and Judges' and Samuel's for קָעַז (cf. p. 147; case study 8), in which the ratios of usage in these books align closely with those of LBH books.

In his eight case studies Kim always groups and evaluates the biblical books according to the following four time periods (I say more on this below): preexilic, preexilic to early exilic, exilic, and postexilic (chapter 5, *passim*; cf. p. 79). Why does he do this? He gives the clearest statement of his reasons in his second case study. First, he says: “Most of these statistics, however, are not helpful, since they are based on too few occurrences of the variants. So I combine the numbers for each period” (pp. 114–15). Elsewhere he groups the books “[i]n order to exploit the data as much as possible” (p. 125), “[a]s an attempt to incorporate all the relevant data” (p. 132), and “[t]o exploit the data as much as possible”

(p. 138). In other words, given the small number of tokens of linguistic variables in many and often most biblical books, Kim combines the (statistically unreliable) tokens of individual books with the intention of creating a statistically reliable subcorpus of tokens. But, is this a legitimate procedure? (I say more on this below.) Second, he says: "One might raise a question about my decision to combine data from different books/texts. However, when we group data according to time periods, we do not mingle dissimilar elements. The data from each group share, and are defined by, the property of coming from the same time period" (p. 114 n. 42). Again, I ask, is this explanation adequate? Is it justifiable to group books into collections from the same period when the dates of the individual books are disputed? (I say more on this below.) Here I should point out that ten occurrences of linguistic variables (e.g., קעז + קעצ = ten or more in one book/corpus) is usually sufficient in Kim's mind to ensure statistical reliability (pp. 102, 104–105, 113, 115, 124, 127 n. 69, 132, 143, 148, 149 n. 22).

My first observation is that the very fact that Kim—and by no means is he alone in his method—must combine data from different books in order to boost their statistical reliability should immediately throw up red flags. The quantity of BH data which suffices for diachronic linguistic analysis is quite small and inconsistent. My remark applies to both the small sizes of many books (most of the Twelve, Ruth, Song of Songs, etc.) and the small numbers of tokens in these and many other books.<sup>[8]</sup> The situation nicely illustrates Labov's well-known adage about historical linguistics "as the art of making the best use of bad data," except that "very bad data" is probably a better description of the situation in BH. As to the minimum number of tokens for each book, Kim realizes there is no magic number (p. 55 n. 39), but he settles on ten or more. This is a frequently cited number in sociolinguistic literature, though thirty or more is often considered ideal.<sup>[9]</sup> Of course, in the case of the Hebrew Bible, or for that matter any other ancient/pre-modern writings, the fortuitous preservation

of the sources and distribution of the data in them must be factored into the equation. It is an uneasy situation: having sufficient tokens for statistical reliability versus not silencing written “voices” which may give true testimony to divergent tendencies. This is pointed out nicely in Suzanne Romaine's discussion of “the problem of sampling” in which she summarizes different sociolinguistic studies of randomness versus representativeness and of relationships between individuals and groups.<sup>[10]</sup> Most interesting in the present context are Gregory Guy's study of final stop deletion in Philadelphia English, in which he “showed that most of the individual deviations from majority patterns occurred when there were fewer than 10 tokens; above this number, there was 90 percent conformity with the expected pattern. Above 35 tokens, there was 100 percent,” contrasted with Xavier Albó's study of a number of different variables in Cochabamba Quechua, in which he “concluded that there was no single criterion to determine the number of occurrences necessary to produce representative results for a given variable for an individual speaker. In some cases more than 100 occurrences may not be enough, while in others fewer than 10, and even 2 occurrences might show contrastive patterns of usage.”<sup>[11]</sup> Results like these in sociolinguistic studies of contemporary speech might cause us to be a little more open-minded when a BH book, small or large, has only, say, four or six or eight tokens of any given set of linguistic variables instead of the “minimum” ten. Another outcome of such studies might be that we should resist combining biblical books, whether in EBH, LBH, DtrH (see above), or whichever, whose ratios of usage of particular variables contrast, or diverge very much. In conclusion, my opinion is that Kim's pragmatic decision to combine biblical books in his quantitative analyses is highly problematic and should be avoided.

*Independent variable of time period:* Kim's variationist analysis aims to correlate BH linguistic variables with the independent variables of time period and text type. My interest is mainly the latter but I want to make

several brief remarks on the former. Is Kim's independent variable of time period really independent (pp. 63, 68 n. 73, 73–79, 84, 96, 98–99)? Yes and no. In the sense that he assigns dates of origin to the biblical books on non-linguistic grounds, he is correct, and his argumentation does not amount to circular reasoning (p. 63 n. 57, 74). However, let's not fool ourselves. First, the sources for the diachronic study of BH do not even remotely approximate the ideal (and often, for other languages, the reality) of authentic non-composite manuscripts which are localized in time and place (cf. the discussion below of personal/private letters). Simply stated, we lack even a single early manuscript anchor for the Hebrew Bible or BH. Second, with the exception of the small number of undisputed postexilic books (Esther–Chronicles), the dates of origin of all other biblical writings are disputed.[12] I for one would date the final production of the books of the Pentateuch and Former Prophets to the postexilic period, rather than accept a “disputed” date for P or “late preexilic to early exilic period” date for “DtrH.” The independent variable of time period crumbles when it becomes dependent on the individual perspective of the Hebraist or biblical scholar.[13]

*Independent variable of text type:* The most innovative aspect of Kim's variationist analysis of BH is his correlation of the independent variable of genre or text type, speech or narration, with Labov's ideas of linguistic changes introduced unconsciously, from below social awareness, or linguistic changes introduced consciously, from above social awareness, respectively.[14] (See pp. 79–84 on text type, pp. 89–96 on changes from below/above, and pp. 95–96, 98–99, 155–56 on their correlation.) In short: “When the change [in BH] is more prominent in the oral-based text type (i.e., recorded speech), we shall understand that this change is a change from below. When the change is more prominent in the written-based text type (i.e., narration), it shall be considered a change from above” (pp. 95–96). In Kim's estimation, only changes introduced *unconsciously* or *from below social awareness* may be considered to be

reliable indicators of the chronology of BH. (As noted above, Kim concluded that only three of the eight frequently-occurring variables which he studied represent unconscious changes from below social awareness: 1, 2, and 8.) In my view there are three difficulties with Kim's analysis. First, and this is mainly an observation, Labov's original formulation of linguistic changes from above/below was based on the social stratification of *spoken* English in New York City. It dealt with linguistic processes in the *speech* community, namely the spread of the pronunciation of the postvocalic /r/.<sup>[15]</sup> And it was unrelated to distinctions between speech (oral genres) and writing (literate genres). Second, studies applying Labov's above/below concept and many other sociolinguistic concepts to historical data have usually focused on written texts that mirror the informal spoken language as closely as possible. The underlying idea is that some written text types resemble spoken language more than other written text types do. What kinds of texts are those? Edgar Schneider, in an important article on investigating variation and change in written documents, summarizes four basic requirements for written documents to be useful for a variationist analysis, the first of which is "texts should be as close to speech, and especially vernacular styles, as possible" and "[t]his condition largely excludes formal and literary writing."<sup>[16]</sup> Then he proposes a taxonomy of written genres according to their proximity to speech. He begins with interview transcripts and trial records, followed by ex-slave narratives, letters, diaries, and commentaries, and he ends with literary dialect since it sits farthest away from speech.<sup>[17]</sup> In particular, personal/private letters offer certain advantages and rank as one of the best text types available for historical variation studies, including studies of Akkadian, pre-modern English, and many other languages.<sup>[18]</sup> It hardly needs to be pointed out that biblical writing—speech and narration together—is *literary writing*. Third, while there are linguistic differences between BH speech and narration,<sup>[19]</sup> and while the text type of speech may be closer to the typical vernacular and the text type of narration to the typical writing (p. 154), BH speech is literary or written speech and

lacks many of the characteristics of actual or spontaneous spoken speech. Reported speech in biblical literature is highly stylized, thus attempting to discern colloquial Hebrew within reported speech is misguided. “[I]t is clear that when Biblical authors composed their works they couched everything, including direct speech, in the classical language” and “when reproducing the dialogues of Biblical characters, the authors couched their words not in colloquial Hebrew but in the standard idiom reserved for literary composition.”[20] To sum up, Kim's application of Labov's above/below distinction to biblical text types is unpersuasive, mainly because there is no compelling reason to believe that recorded speech in literary BH is an actual specimen of informal or spontaneous spoken language.[21]

Nevertheless, in my mind Kim has introduced an important idea into the ongoing debate on the historical development of BH and the linguistic dating of biblical writings. At this point I want to return momentarily to the issue of style,[22] and I want to suggest that while Kim correctly identifies linguistic changes which are introduced from above social awareness as close to, if not the same as, something that we call “stylistic” (p. 156; cf. 95), *he underestimates the degree of the biblical writers' conscious involvement in selecting one or another linguistic variable from among several available/known ones.*

*Style:* Style and stylistic variation are ubiquitous in Labov's sociolinguistic model.[23] His analysis is founded on the notion of *consciousness* which relates in turn to the *formality* of the context. The basic idea is that a more formal context triggers more attention to language, hence it is more aware, careful, intentional, and so on.[24] Aesthetics also plays a role in stylistic variation: a speaker or writer (or a group of speakers or writers) often has (have) attitudes about what constitutes “good style,” resulting in the manipulation of language for aesthetic purposes.[25] On the scale of formality, writing is usually, but not always, more formal than speech, and literary writing is habitually more formal than other genres of writing

(cf. the remarks above on text type). Thus literary writing in itself triggers careful attention to language. Given that BH is written language, and literary language, and scribally-learned language,[26] and also serves religious purposes, it stands to reason that the distribution of many linguistic variables in BH are the outcome of conscious choices. They are “changes from above” or “stylistic variants.”

Another avenue of argumentation also seems to support my proposal. All language change involves variation between several or more linguistic variables. “Completed” language change may be defined as either stable variation between linguistic variables, thus exhibiting no inclination to move to completion, or replacement of one variable by another, thus resulting in the elimination of the earlier variable.[27] What do we see in BH—which may have been written and changing for a thousand years? There are very few lexemes, phrases, or semantic developments, and very few, if any, grammatical forms or uses, which are attested (and certainly not frequently attested) in “late” BH only or without their contrasting “early” BH variable in “late” BH also.[28] In other words, we can hardly speak of “completed” change, only change which is ongoing or in progress. This suggests that in the case of many linguistic variables attested in the Hebrew Bible, the writers (authors, editors, and scribes) of BH had to make conscious choices between “competing” linguistic variables. And more often than not they made the same choices. The result is that there is remarkable homogeneity (continuity, uniformity) in BH.

But then the question arises: to what should we attribute the intermittent stylistic variants which we do find between EBH writings and LBH writings on the whole? The argument in our *Linguistic Dating of Biblical Texts* was that EBH and LBH are best taken as representing two tendencies among authors/editors/scribes of the biblical period: conservative and non-conservative. The writers who produced works in EBH exhibit a tendency to “conservatism” in their linguistic choices, in the sense that they only

rarely use items outside a narrow core of what they considered literary language. At the other extreme, the LBH writers exhibited a less conservative attitude, freely adopting a variety of linguistic items in addition to (not generally instead of) those favored by the EBH writers.[29]

But then the additional question arises: to what should we attribute the different attitudes of EBH and LBH writers? Konrad Schmid suggests that

the choice of language also indicates conceptual closeness to or distance from normative core traditions in the Torah: the books of Job or Qoheleth use a Hebrew that does not conform to the Torah because of their theological dissidence, while late Joshua or Judges texts can be closely paralleled with the classical biblical Hebrew of the Torah and the older parts of those same books.[30]

Similarly Ehud Ben Zvi argues that

the shift from SBH to LBH as the language of writing religious texts conveyed at some point in the late Persian period an ideological image of conceptual clusters and boundaries. On the one hand, texts associated with “Judahite” language and characters (including, by extension and appropriation the figure of Moses, but certainly not that of Ezra). These texts appeared in the mentioned triad of collections (or mental shelves) [“the pentateuchal books, the so-called deuteronomistic history, and the prophetic books”] and stood at the ideological core of the “text-centered” community construed (and imagined) by the literati in the late Persian period. These books were associated with earlier times, from an era preceding the settlement in the land to the loss of the land and exile. On the other hand, texts associated with LBH were considered to be less central to the community, outside the triad mentioned above, later, and as all postmonarchic Israel within this discourse, as carrying a strong Babylonian returnee voice.[31]

In short, these authors argue that the biblical writers' choice of language arises from particular conceptual or ideological motivations.[32] My specific suggestion, therefore, is that EBH writers were conservative in their linguistic choices because their core literary language was the language of Torah. By contrast, LBH writers had a less traditional attitude toward Torah and its language, so they *sometimes* embraced, more or

less frequently, non-conservative (or non-traditional or non-standard) linguistic forms/uses (e.g., תוכלם rather than הכלמם). In other words, EBH and LBH language/writing styles are acts of identification.[33] To sum up, Kim is right to link linguistic change from above social awareness with stylistic variation, but his method for distinguishing change from above and below is unconvincing, and the written/literary/scribal/religious nature of the Hebrew Bible, and the remarkably homogeneous distribution of linguistic data in BH, argue strongly that the distribution of the EBH and LBH language variables (including frequencies of occurrence) and the linguistic profiles of the EBH and LBH books are largely the outcome of conscious choices, or stylistic variation.

*Case studies:* It is impractical in this context to scrutinize the details of Kim's eight case studies.[34] The preceding remarks have taken issue with some general methodological procedures in Kim's book which have direct implications for all eight of his illustrations: absence of s-shaped diffusion curves, dismissal of text-transmission issues, inattention to extra-biblical Hebrew (usually), grouping biblical writings and statistics (e.g., DtrH), presupposing dates of origin of biblical writings, avoiding data of books whose dates are "disputed" (e.g., P), and distinguishing changes from above/below on the basis of biblical narration/speech. Although I would make some corrections of fact here and there, and would nuance differently some of the argumentation in the light of the general criticisms I have given above, I do not deny that these linguistic variables are plausible examples of language change in progress in BH. But, whereas we agree that they are unhelpful for linguistic dating, namely because it is impossible to distinguish between early and late adopters and others in between, we disagree on the direction of change of some of the variables: Kim thinks they are examples of change from both above and below, whereas I find no evidence for interpreting any of them as examples of change from below.

In conclusion, it is clear that I have mixed feelings about Kim's results. However, to repeat what I said above, personally, as one of the so-called challengers, the book has challenged me to reconsider, revise, and/or restate some of my arguments. Kim describes his book as "a pilot study" which seeks "to establish a method" that can be used or revised by others, and "as a stepping-stone for those who wish to continue the exploration" (pp. 8, 161). In my mind he has given all of us involved in the historical linguistics/linguistic dating debate a lot to think about.

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[1] Several of the articles in C. Miller-Naudé and Z. Zevit (eds.), *Diachrony in Biblical Hebrew* (LSAWS, 8; Winona Lake, Ind.: Eisenbrauns, 2012) also make use of a variationist approach (see my forthcoming review in *RBL*), and the method plays an important part in R. Rezetko and I. Young, *Historical Linguistics and Biblical Hebrew. Steps Toward an Integrated Approach* (expected publication data: ANEM/MACO; Atlanta: Society of Biblical Literature, in preparation). 

[2] See R. Rezetko, *Source and Revision in the Narratives of David's Transfer of the Ark. Text, Language, and Story in 2 Samuel 6 and 1 Chronicles 13, 15–16* (LHB/OTS, 470; London: T&T Clark, 2007); cf. R. F. Person, Jr., *The Deuteronomistic History and the Book of Chronicles. Scribal Works in an Oral World* (Ancient Israel and Its Literature, 6; Atlanta: Society of Biblical Literature, 2010). 

[3] See I. Young, R. Rezetko, and M. Ehrensverd, *Linguistic Dating of Biblical Texts. Volume 1. An Introduction to Approaches and Problems, Volume 2. A Survey of Scholarship, a New Synthesis and a Comprehensive Bibliography* (BibleWorld; London: Equinox, 2008), 1:4 and 2:94 on language change; 1:63–64, 341–48, and 2:1–71 on textual and literary diachrony.



[4] See, provisionally, Young, Rezetko, and Ehrensverd, *Linguistic Dating of Biblical Texts*, 1:16–18, 60–64, 341–60, 2:100–102; R. Rezetko, "The Qumran Scrolls of the Book of Judges. Literary Formation, Textual Criticism, and Historical Linguistics," *JHS* (2013), volume 13, article 2; and the thorough treatment of this issue in Rezetko and Young, *Historical Linguistics and Biblical Hebrew* (in preparation). 

[5] Note the use throughout of terms such as quantitative, statistics, statistical, rate, ratio, percentage, proportion, frequency, trend, tendency, and preference. 

[6] See, for example, Young, Rezetko, and Ehrensverd, *Linguistic Dating of Biblical Texts*, 1:83–90, 111–19; R. Rezetko, “What Happened to the Book of Samuel in the Persian Period and Beyond?,” in E. Ben Zvi, D. V. Edelman, and F. H. Polak (eds.), *A Palimpsest. Rhetoric, Ideology, Stylistics and Language Relating to Persian Israel* (Piscataway, N.J.: Gorgias, 2009), 237–52 (241–51). 

[7] For a synopsis of Noth's views on pre-DtrH sources, Dtr intervention, and post-DtrH additions, see A. F. Campbell, “Martin Noth and the Deuteronomistic History,” in S. L. McKenzie and M. P. Graham (eds.), *The History of Israel's Traditions. The Heritage of Martin Noth* (JSOTSup, 182; Sheffield: Sheffield Academic Press, 1994), 31–62 (58–62). 

[8] I can mention here as a related illustration the discussion of *the relative infrequency of the most frequent LBH lexical items* given in Young, Rezetko, and Ehrensverd, *Linguistic Dating of Biblical Texts*, 1:113–17. Also, note that one reason Kim selected the linguistic variables which he studied in chapter 5 is because “... of hundreds of the purported EBH and LBH contrasts, these eight items are among the most frequently used. This situation will enable us to obtain statistically more reliable results” (p. 98; cf. 72). Finally, although it contains many errors and misrepresentations, A. D. Forbes, “The Diachrony Debate. Perspectives from Pattern Recognition and Meta-Analysis,” *HS* 53 (2012), 7–42, at least has some helpful observations on the basis of statistical pattern recognition. 

[9] See, for example, S. A. Tagliamonte, *Variationist Sociolinguistics. Change, Observation, Interpretation* (Language in Society, 40; Chichester: Wiley-Blackwell, 2012), 136. 

[10] S. Romaine, *Socio-Historical Linguistics. Its Status and Methodology* (Cambridge Studies in Linguistics, 34; Cambridge: Cambridge University Press, 1982), 105–14. 

[11] Romaine, *Socio-Historical Linguistics*, 111; cf. 110–11. 

[12] See the survey in Young, Rezetko, and Ehrensverd, *Linguistic Dating of Biblical Texts*, 2:1–71. 

[13] See the discussions of literary-linguistic circularity in Young, Rezetko, and Ehrensverd, *Linguistic Dating of Biblical Texts*, 1:65–68, 81, 93–94, 118, 140, 2:97. Kim himself comes close to admitting this when he says, “...if one is dissatisfied with the periodizations of biblical books and texts which we have based on classical datings, one can redefine them and start one's own new analysis” (p. 161). 

[14] At the risk of overgeneralization, one might distinguish the two types of change this way (not every element applies to BH, of course): *Change from below* occurs below social awareness, is unconsciously chosen, is socially unnoticed, appears first in vernacular or

spontaneous speech and then moves to more formal varieties of speech, represents the operation of internal linguistic factors, is unnoticed until nearing completion, is introduced by any social class but mostly inner classes including lower middle and upper working classes, is acquired in childhood, diffuses in a more natural and even direction. Also, its distribution of old and new forms/uses is more predictable and systematic, is definitive and irreversible, is a reliable indicator of chronology, and appears in written documents in oral text types. *Change from above* occurs above social awareness and often with full public awareness, is consciously chosen, is socially noticeable and considered prestigious, appears first in careful speech or more formal styles, represents the operation of external linguistic factors, is noticed from the beginning, is introduced mostly by the dominant (not necessarily the highest) social classes, is acquired in adulthood, diffuses in a less natural and uneven direction. Its distribution of old and new forms/uses is less predictable or sporadic and unsystematic, is indefinite and reversible, is not a reliable indicator of chronology given that it is largely stylistic, and appears in written documents in literate text types. 

[15] W. Labov, *The Social Stratification of English in New York City* (2d ed.; Cambridge: Cambridge University Press, 2006), 203–209 and 240 n. 24 on change from above/below. 

[16] E. W. Schneider, "Investigating Variation and Change in Written Documents," in J. K. Chambers, P. Trudgill, and N. Schilling-Estes (eds.), *The Handbook of Language Variation and Change* (Blackwell Handbooks in Linguistics; Oxford: Blackwell, 2002), 67–96 (71). This does not mean, however, that literary language has no place in historical (socio-) linguistic research, as Kim (pp. 56–57), Schneider (pp. 79–81), and others have pointed out. Even so, literary writings do have some limitations, but this is not the place to discuss those. 

[17] Schneider, "Investigating Variation and Change in Written Documents," 71–73; cf. 73–81 for deeper discussion of the text types. Observe that Kim's extra-biblical examples of oral and literate genres/text types are *completely separate* sets of writings; speech embedded in literary writing is not mentioned (pp. 60, 80–81). 

[18] On Akkadian see G. Deutscher, *Syntactic Change in Akkadian. The Evolution of Sentential Complementation* (Oxford: Oxford University Press, 2000). On pre-modern English see A. Bergs, *Social Networks and Historical Sociolinguistics. Studies in Morphosyntactic Variation in the Paston Letters (1421–1503)* (TEL, 51; Berlin: Mouton de Gruyter, 2005); T. Nevalainen and H. Raumolin-Brunberg, *Historical Sociolinguistics. Language Change in Tudor and Stuart England* (Longman Linguistics Library; Edinburgh: Pearson Education, 2003). For additional general discussion see J. C. Conde Silvestre, *Sociolingüística Histórica* (Madrid: Gredos, 2007), 42–46, 51–52; and chapters 7–11 dealing with various text types in J. M. Hernández Campoy and J. C. Conde Silvestre (eds.), *The Handbook of Historical Sociolinguistics* (Blackwell Handbooks in Linguistics; Chichester: Wiley-Blackwell, 2012). 

[19] J. MacDonald, "Some Distinctive Characteristics of Israelite Spoken Hebrew," *BO* 32 (1975), 162–75; F. H. Polak, "The Style of the Dialogue in Biblical Prose Narrative," *JANES* 28 (2001), 53–94. 

[20] G. A. Rendsburg, *Diglossia in Ancient Hebrew* (AOS, 72; New Haven: American Oriental Society, 1990), 19, 161; cf. 18–21, 159–61. See also R. Rezetko, "Diachrony in Biblical Hebrew. Review of an Approach from the Perspective of Paraleipomenon," *HS* 52 (2011), 397–409 (405–406). It is worth pointing out that even in Polak's analysis of biblical styles there is not a one-to-one correspondence between the rhythmic-verbal style and speech and the complex-nominal style and narration. His (early) verbal style and (late) nominal style tend to be consistent in all samples whether representing battle accounts, action narratives, or dialogues, and the rhythmic-verbal style may characterize (early) narrative and the complex-nominal style may characterize (late) speech although it appears less formal than in (late) narrative. See F. H. Polak, "The Oral and the Written. Syntax, Stylistics and the Development of Biblical Prose Narrative," *JANES* 26 (1998), 59–105; "The Style of the Dialogue in Biblical Prose Narrative." 

[21] Naturally there is no evidence for spoken Hebrew in the First and Second Temple periods, thus we are basically guessing what spoken Hebrew might have been like from what has been preserved in written Hebrew. More viable clues are probably to be found in the epigraphic letters from Arad and Lachish, and Tannaitic Hebrew, which is probably a descendant of a dialect of ancient spoken Hebrew that existed independent of and alongside BH. See Young, Rezetko, and Ehrensvärd, *Linguistic Dating of Biblical Texts*, 173–200, 223–49. 

[22] My thoughts here are a sketch of a larger discussion in Rezetko and Young, *Historical Linguistics and Biblical Hebrew* (in preparation). 

[23] For example, in Labov, *The Social Stratification of English in New York City*. For an explicit remark see p. 240 n. 24. 

[24] See, for example, T. Kristiansen, "Attitudes, Ideology and Awareness," in R. Wodak, B. Johnstone, and P. Kerswill (eds.), *The Sage Handbook of Sociolinguistics* (London: Sage, 2011), 265–78 (267–71); N. Schilling-Estes, "Investigating Stylistic Variation," in J. K. Chambers, P. Trudgill, and N. Schilling-Estes (eds.), *The Handbook of Language Variation and Change* (Blackwell Handbooks in Linguistics; Oxford: Blackwell, 2002), 375–401 (378–83). 

[25] D. Biber and S. Conrad, *Register, Genre, and Style* (Cambridge Textbooks in Linguistics; Cambridge: Cambridge University Press, 2009), 18–19. 

[26] Kim also recognizes the connection between scribal culture and changes from above (pp. 157–58; cf. 95). 

[27] L. Campbell and M. J. Mixco, *A Glossary of Historical Linguistics* (Edinburgh: Edinburgh University Press, 2007), 30; cf. Kim, pp. 52–53, especially Table 2. 

[28] One of the few possible illustrations is שׁשׁ (Genesis, Exodus, Ezekiel, Proverbs) vs. שׁשׁ (Ezekiel, Esther, Chronicles). Other popular EBH versus LBH illustrations are arguably not examples of completed change in BH:

- תוכלמ vs. הכלממ
- תרגא vs. רפס
- קשמרד vs. קשמד
- רמש vs. שרד + commandments/torah
- consecutive vs. copulative verb tenses
- הiphil vs. piel מוק
- -בתו vs. -מהיתו

There are many more. For additional discussion and illustrations, I refer the reader, once again, to Young, Rezetko, and Ehrensävrd, *Linguistic Dating of Biblical Texts*, 1:83–90, 111–119. 

[29] For conservative and non-conservative one might substitute traditional and non-traditional or standard and non-standard. For the clearest statements of our view see Young, Rezetko, and Ehrensävrd, *Linguistic Dating of Biblical Texts*, 1:70, 141, 361; 2:96–99. 

[30] K. Schmid, *The Old Testament. A Literary History* (Minneapolis: Fortress, 2012), 31. 

[31] E. Ben Zvi, “The Communicative Message of Some Linguistic Choices,” in E. Ben Zvi, D. V. Edelman, and F. H. Polak (eds.), *A Palimpsest. Rhetoric, Ideology, Stylistics and Language Relating to Persian Israel* (Piscataway, N.J.: Gorgias, 2009), 269–290 (285–86; cf. 287, 289–90). 

[32] A similar thought is expressed in C. Levin, “Review of R. M. Wright, *Linguistic Evidence for the Pre-exilic Date of the Yahwistic Source*,” *RBL* 01/2006, [http://bookreviews.org/pdf/4860\\_5055.pdf](http://bookreviews.org/pdf/4860_5055.pdf) (p. 4). 

[33] See N. Coupland, *Style. Language Variation and Identity* (Key Topics in Sociolinguistics; Cambridge: Cambridge University Press, 2007). For possible social settings/situations of the Deuteronomistic and Chronistic writers in the Second Temple period see Person, *The Deuteronomistic History and the Book of Chronicles*. 

[34] I partially reexamined one of them (םתו- vs. םהיתו-) recently in Rezetko, "The Qumran Scrolls of the Book of Judges," 56–59. And we look again in more detail at two others (הכלממ vs. תוכלמ; and also קעצ vs. קעז) in Rezetko and Young, *Historical Linguistics and Biblical Hebrew* (in preparation). Note that two of these are actually two of Kim's three examples of change from below. 