Can the Ages of Biblical *Literature* be Discerned Without *Literary* Analysis?

Review-Essay of
Ronald Hendel and Jan Joosten
How Old is the Hebrew Bible? A Linguistic, Textual, and Historical Study
(The Anchor Yale Bible Reference Library; New Haven/London: Yale University Press, 2018; xvi + 221)

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At the core of scholarship stands dialogue—
dialogue with evidence and dialogue with interpretations of evidence.

1. Introduction

Since the early 2000s we have witnessed a fierce debate among Hebraists and biblicists concerning the feasibility of dating biblical writings, by their linguistic characteristics, to the preexilic, exilic, or postexilic period. The controversy has exhibited itself in a multitude of monographs, book essays, journal articles, and conference presentations. Now, Ronald Hendel and Jan Joosten join the fray with their book *How Old is the Hebrew Bible?*, which evolved out of workshops that RH-JJ hosted at the University of California, Berkeley and the Université de Strasbourg. This major contribution to the recent discussion by two distinguished scholars is welcomed.

2. Objectives

The main thrust of the book is the argument for a consilience or convergence of different types of evidence that together enable us to discern the ages of biblical writings. The three classes of data that RH-JJ focus on are linguistic, textual, and cultural/historical. The balance of the book deals with the matter of linguistic history (chs. 1–7, appxs. 1–2), whereas textual history is treated above all in ch. 4, and cultural history takes center stage in ch. 8.

At various points RH-JJ underscore their objectives: "The idea of this book is to reinscribe historical research on the Hebrew language where it belongs: at the heart of biblical studies. We aim to gather the fruits of recent research on Biblical Hebrew [BH] and to present them to the student and scholar in an accessible way. The book blends the functions of introduction, synthesis, and scholarly dialogue" (p. x). "In this book, we elucidate some of the features of linguistic and textual change that allow us to discern different temporal states of the Hebrew Bible [HB]. This is a type of historical inquiry that has occasioned controversy and skepticism in recent years. Our aim is to show how this field of knowledge, when pursued with methodological rigor, is viable and illuminating. It allows us, within limits, to discern the ages of biblical literature" (p. 1). "The question we pose in this book is whether the historical linguistics of Biblical Hebrew is useful in determining the age of the biblical writings" (p. 5). "One of the reasons we wrote this book is to bridge this gap [between scholars who specialize either in biblical exegesis or in Hebrew language]" (p. 5). "We aim to show how Hebrew linguistics and other types of historical inquiry work together to provide a 'thick description' of the ages of biblical literature" (p. 99).

As noted, most of the book is concerned with linguistic history, and more pointedly, linguistic dating (pp. ix–x, 5–6, etc.). In this regard, their conclusions have a familiar ring: "To sum up our book so far, we have shown that Biblical Hebrew—like all languages, literary and spoken—has a history that we can trace, even among the welter of other variations due to textual transmission, dialects, and literary style. We are able to specify three main phases or 'chronolects' of Biblical Hebrew: classical (CBH), transitional (TBH), and late (LBH). The similarities between CBH and preexilic Hebrew inscriptions allow us to locate the historical context of CBH in the preexilic period. The characteristics of TBH and the books written in it indicate that its historical context is the 'long' sixth century BCE. The phenomenon of pseudoclassicisms and the other features of LBH illuminate its context in the postexilic period. We have established a relative and absolute dating for these chronolects and have described their distinctive features, focusing on changes in morphosyntax and semantics. As our many examples illustrate, this model of the history of Biblical Hebrew has obvious implications for the dating of biblical texts" (p. 98; cf. pp. 121–125).

3. Contents

The book contains eight chapters (pp. 1–125) and two appendixes (pp. 127–144) between the front matters (pp. i–xvi) and the endnotes, bibliography, and indexes (pp. 145–221). Hendel is the

primary author of chs. 1, 2, 8, and appx. 2, and Joosten is the primary author of chs. 3–7 and appx. 1 (cf. p. 10). For a concise summary of the contents and arguments of each chapter and appendix see our Appendix 1.[1]

4. Commendations

RH-JJ have gathered many of their previous publications on various linguistic and textual issues and integrated them into a coherent argument about the history of BH. The book is chock-full of insightful observations on a multitude of linguistic, textual, and cultural/historical phenomena. This is not unexpected since RH-JJ have a keen eye for detail. The result though is not a jumbled compilation of details but a succinct, flowing, and clear presentation. This is aided by the relatively short main body (125 pages, excluding two appendixes) and the placement of the many notes (36 pages) at the end (i.e., endnotes; pp. 145–180).

In the framework of the conventional view of diachrony in BH and the linguistic dating of the HB, this book easily surpasses in scope and sophistication some other histories that deal with Biblical (and Postbiblical) Hebrew.[2] While there is plenty of discussion of particular lexical features, the large amount of attention paid to changes in morphosyntax is uncommon but refreshing.

As pointed out previously, the contents and conclusions of the book will have a familiar ring to those who are up to speed with the recent discussion (until about five years ago; see further below). The main exception to this for most linguists will be ch. 8 on consilience and cultural history (see the summary in our Appendix 1). This is an interesting and valuable injection into the linguistic dating debate. While the chapter has the feel of being somewhat tacked-on, RH-JJ are nevertheless correct that the most promising and convincing approach and answer to the main question of the book, "How Old is the Hebrew Bible?" (book title) and its constituent writings, will be one that involves, at a minimum, "A Linguistic, Textual, and Historical Study" (book subtitle), rather than an approach that concentrates only or mainly on one of these. This chapter is a welcome improvement to other approaches that might regard language as the only or most

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[1] https://www.academia.edu/38112859/2019b_Young_Rezetko_Can_the_Ages_of_Biblical_Literature_be_Discerned_Without_Literary_Analysis_Appendix_1.

[2] E.g., Gesenius 1815, Hoffman 2004, Kutscher 1982, Sáenz-Badillos 1993, Schniedewind 2013.

objective and dependable criterion for dating the origins of biblical literature, or that might minimize linguistic evidence or dismiss it altogether.

5. Reservations

It will hardly come as a surprise to scholars involved in the recent debates or acquainted with them that we should react strongly to this book and find many substantial problems with it. Our main concern is that RH-JJ have not done what the book series (The Anchor Yale Bible Reference Library)[3] and authors[4] claim to want to do: RH-JJ do not engage with contemporary scholarship, they do not gather or present the fruits of recent research, and they steer clear of literary criticism in particular; in short, there is a striking absence of scholarly dialogue.

The extensive remarks that follow engage many general and specific aspects of RH-JJ's book and revolve around the topics of contemporary scholarship/recent research, linguistic analysis, textual analysis, and their model of consilience. We fully realize that we are making strong claims about the nature of this book, and we invite other scholars to verify for themselves that we have fairly represented its contents and arguments.

5.1. RH-JJ often do not engage data and analyses relevant to how they construe and employ particular linguistic features.

Many, probably most, of the linguistic phenomena in the book were published previously by RH-JJ, and many of them have also been examined by us and others. Of course, there is nothing inherently wrong with recycling examples and case studies in support of one's arguments, but it is debatable whether one should do that silently without acknowledging or answering counterarguments. This is a constant and conspicuous problem in the book. To be fair, RH-JJ do not always overlook other treatments of particular linguistic features.[5] However, in some instances,

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[3] "It aims to present the best contemporary scholarship in a way that is accessible not only to scholars but also to the educated non-specialist. It is committed to work of sound philological and historical scholarship, supplemented by insight from modern methods, such as sociological and literary criticism" (p. ii).

[4] "We aim to gather the fruits of recent research on Biblical Hebrew and to present them to the student and scholar in an accessible way. The book blends the functions of introduction, synthesis, and scholarly dialogue. We hope that it will instruct and inspire others to engage with this field of research" (p. x).

[5] For example, studies by Revell (p. 149 n. 36) and Woodhouse (p. 148 n. 14).

they refer to other publications but seem not to realize that they do not really support their claims.[6] And more commonly, relevant data and analyses by others and ourselves, for example in our *Linguistic Dating of Biblical Texts* (LDBT) and *Historical Linguistics and Biblical Hebrew* (HLBH), are simply overlooked.[7] Examples are presented as clear-cut and certain without discussion. This is the tip of an iceberg.

5.2. RH-JJ often do not engage alternative treatments of the language of specific passages and books.

RH-JJ repeatedly sideline publications that do not cohere with their views on the language of specific texts. This is particularly evident in, but not limited to, the bibliographical survey in appx. 1 (pp. 127–133). According to RH-JJ: "No comprehensive and systematic survey of the biblical corpus from an historical linguistics perspective has been attempted in scholarship. The publication that comes closest to offering such a survey is Samuel R. Driver's *Introduction to the Literature of the Old Testament*" (p. 127). They discount our 71-page survey in LDBT because, they claim, we "reject all relevance of language to dating issues" and therefore we "are unable to give an idea of what is more or less plausible" (p. 178 n. 1). (We will return below to RH-JJ's claim.) In contrast, it seems to us that RH-JJ's idea of implausibility often boils down to any publication that would challenge their own conclusions. For example, they say about the Song of Songs: "Although Song of Songs is not part of the core group [of LBH writings], Chip Dobbs-Allsopp has recently shown that it should be considered LBH [i.e., late]" (p. 132). In our thinking, at a minimum, Noegel and Rendsburg 2009 also merits mention.[8] Its omission is indicative of a larger situation. Earlier

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[6] For example, studies by Bekins (p. 153 n. 89) and Holmstedt (p. 151 n. 58; cf. Rezetko 2016d in response to Fassberg).

[7] Some examples are iterative *weqatal* (pp. 44, 75): LDBT 1:53; HLBH, 184–192; Rezetko 2003: 233–234; 2013: 38–40; ויהי (*wayehi*) with a circumstantial clause (pp. 45, 64–65): HLBH, 148–152; Rezetko 2003: 235–237; 2013: 36–37; imperative use of the infinitive absolute (pp. 38, 44, 69–70, 80): LDBT 2:128–132; paronomastic use of the infinitive absolute (pp. 70, 80): LDBT 2:132–141; *he* locale in general and following a noun in the construct state (pp. 45, 54, 67–68, 83, 144, 167 n. 12): LDBT 1:78–80, 169–170, 350–351; HLBH, 182–184, 374–394; Rezetko 2013: 48–56; Jacobs 2016 passim, 2018 passim; *lamed* to express movement toward a location (pp. 77, 78, 83): HLBH, 390–391; נא (*na'*) and הנה-נא (*hinneh-na'*) (pp. 39, 44, 54, 76, 82): LDBT 2:118–123; HLBH, 563–564; paragogic *nun* (p. 133): LDBT 2:123–126; HLBH, 136, 497–498; pseudoclassicisms מלא יד (*mille' yad*) and לבוא (*lebo'*) (pp. 86–89, 92, 118, 167 nn. 11–12): HLBH, 98–99 (cf. 96–99); LDBT 2:106–108 (cf. 1:69–70, 78–80); Rezetko 2016b in response to Joosten.

[8] Some other examples are: Isaiah: Hays and Paul are cited (p. 131; elsewhere also Kutscher), but Rooker 1996, 2015, and Young 2013 are not (note that Rooker is cited on the language of Ezekiel; cf. Rezetko 2016c in response to Rooker); Jeremiah: Hornkohl is cited (p. 131; elsewhere also Joosten), but

in the book, RH-JJ discard Carr (who follows Young) with the view that the "late" language of the Song of Songs may represent an early dialect; rather, RH-JJ say: "As a consequence of these and other judgments, he [Carr] argues for 'a dating of some of its parts in or close to the tenth century.' This despite the density of the book's LBH features, which are uniformly distributed throughout the book, including Persian loanwords, Aramaisms, and late syntax [citing Dobbs-Allsopp and Fox, p. 146 n. 13]. The distinctive LBH profile of the book has no purchase in this approach" (p. 7). RH-JJ are content to refer to Noegel and Rendsburg's book in a note, commenting: "These features are identified as dialectal (Israelian Hebrew) and the book dated to circa 900 BCE by [Noegel and Rendsburg]; see our discussion of dialects in chapter 3" (p. 146 n. 13).[9] Here, as elsewhere, RH-JJ stop short of discussing linguistic data and other viable explanations. They portray the situation as more clear-cut and certain than it is in reality. (We will return below to RH-JJ's treatment of the Song of Songs.)

5.3. RH-JJ often do not engage recent dialogue on historical linguistic theory and method.

The book has a weak footing in contemporary historical linguistics.[10] RH-JJ do not bring up substantive issues that are vigorously debated in other recent publications: the framework and objective of the research, whether linguistic dating as conventionally undertaken in biblical and Hebrew studies (as by RH-JJ) or a more vigorous descriptive approach that is "marked by both methodological rigor and theoretical awareness beyond Hebrew studies, including informed statistical analysis" (Holmstedt 2012: 120); the notion of language periodization and difficulties with conventional thinking on language states and transitions (e.g., so-called Transitional Biblical Hebrew); language variation, change, and diffusion, including (mis)use of the s-curve; corpus linguistics; variationist approaches to language change; quantitative methods; and so on. Let us elaborate.

Rezetko 2016 and Stipp 1997 are not; Job: Joosten is cited (p. 133; elsewhere also Hurvitz), but Young 2009 is not; and so on.

[9] In ch. 3, RH-JJ supply *two paragraphs* on dialects (pp. 32–33), concluding "the results of the approach often remain uncertain" and "many uncertainties remain."

[10] And there is minimal interaction with general historical linguistic scholarship, mainly a few short references to Campbell, Dresher, Hale, Schneider, and Thomason (pp. 11–12, 23, 136, 138, 141, 143, 147 nn. 1–3 and 6, 151 n. 59, 179 nn. 8 and 22, 180 nn. 23 and 40).

5.3.1. RH-JJ cite several contributions in Miller-Naudé and Zevit 2012.[11] Nevertheless, aside from their brief reference to the treatment of the s-curve and diffusion in Cook 2012 (pp. 22–23; cf. p. 151 n. 58) and their references to several remarks in Dresher 2012 and Holmstedt 2012 in RH-JJ's critique of our work (pp. 136, 138, 141, 143), RH-JJ overlook the issues mentioned above and others that are treated in the section "Theoretical and Methodological Perspectives on Diachrony" (Miller-Naudé and Zevit 2012: 19–124).[12] For example, the basic yet essential concept of diffusion (see further below) plays no part in the book outside the passing reference to Cook's treatment (p. 22).

5.3.2. Kim 2013, another significant work, is cited with regard to the appearance of innovations "first in speech and only much later in written literature" (p. 164 n. 62), but the essence of Kim's work, which aims to adjudicate between Hurvitz et al. and Young et al., is variationist analysis, and that concept too is disregarded by RH-JJ. Furthermore, RH-JJ seem unaware that the point on which they cite Kim, the appearance of innovations "first in speech and only much later in written literature" (pp. 70–71), relates to only one kind of change that Kim discusses, change from below, and that change from above is an equally important issue in Kim's book. Unfortunately, change from below and change from above are concepts that do not enter into RH-JJ's book. Finally, as a general indication of RH-JJ's lack of engagement with contemporary scholarship, it is extraordinary that Kim 2013 is only cited in this one place.

5.3.3. Rezetko and Young 2014 dialogues with the preceding books and others on the fundamental issues that are mentioned above, yet our book goes unmentioned *until* appx. 2. Furthermore, the historical sociolinguistic methods that are the essence of our book, cross-textual variable analysis and variationist analysis, *go unmentioned even in* appx. 2. RH-JJ somehow manage to criticize our work *without even mentioning* our methods.

5.3.4. Many other publications that have challenged aspects of the traditional linguistic dating approach, or have otherwise advanced our understanding of diachrony in BH, are missing, including ones by Forbes, Holmstedt, Naudé, ourselves, and others.[13] Or, RH-JJ might have

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[11] Cook 2012 (pp. 22, 150 n. 55, 151 n. 57), Dresher 2012 (pp. 136, 141, 143, 179 n. 8, 180 n. 40), Holmstedt 2012 (pp. 138, 179 n. 21), Notarius 2012 (pp. 103, 171 n. 25), Pat-El 2012 (pp. 24, 29, 151 n. 64, 153 nn. 89 and 91–93), Paul 2012 (pp. 131, 165 nn. 10 and 19).

[12] Cook 2012, Dresher 2012, Givón 2012, Holmstedt 2012, Naudé 2012.

[13] Forbes 2012, 2017a, 2017b, Holmstedt 2006 and other publications by him, Naudé 2003, 2004, 2010, 2012, and other publications by him, Rezetko and Naaijer 2016a, 2016b, and many other papers and

considered a recent issue of *Hebrew Studies*, "Symposium: Does Archaic Biblical Hebrew Exist?" (Barmash 2017a),[14] or a recent issue of the *Journal for Semitics*, "Historical Linguistics, Editorial Theory, and Biblical Hebrew: The Current State of the Debate" (Naudé and Miller-Naudé 2016a),[15] or several sections of the recent book *Advances in Biblical Hebrew Linguistics* (Moshavi and Notarius 2017a), "Linguistics and Philology" and "Historical Linguistics and Language Change."[16] All of these are omitted. Beyond their own publications and some by others which promote similar views, RH-JJ typically bypass other scholars, publications, ideas, and data, with the main exception of their critique of our work in appx. 2 (on which more is said below).

To this point, we have argued that RH-JJ often do not engage data and analyses relevant to how they construe and employ particular linguistic features, or alternative treatments of the language of specific passages and books, or recent dialogue on historical linguistic theory and method. In sum, they do not engage with contemporary scholarship, they do not gather or present the fruits of recent research. How then do they present the data that they do cite?

5.4. RH-JJ provide inadequate documentation for the linguistic phenomena they cite.

In recent research (2012–2018), Hebraists have had increased recourse to the notion of diffusion, or spread, of innovations and the measurement of the diffusion of individual linguistic features in BH and ancient Hebrew generally. They have done this as a way to move beyond broad generalizations and intuitional judgments about the variation and distribution of specific linguistic features. Methods of variationist and quantitative analysis have played a prominent role in the new research agenda (see, for example, the articles in a recent issue of *Journal for Semitics*). As noted above, RH-JJ have not taken account of this advance, and so like a lot of previous research, their book is full of unsubstantiated claims about the locations and frequencies of "early" and "late" linguistic items. This is especially obvious when they speak about the linguistic characteristics of CBH, TBH, and LBH writings generally without differentiating the writings within these groups or observing how a particular linguistic item is distributed among the individual constituents. Other

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publications by us. It is remarkable that insights from Forbes and Naudé are completely absent from RH-JJ's book.

[14] Barmash 2017b, Korchin 2017, Notarius 2017a, Young 2017.

[15] Dean 2016, Forbes 2016, Hornkohl 2016, Jacobs 2016, Klein 2016, Naaijer and Roorda 2016, Naudé and Miller-Naudé 2016b, Samet 2016, Young 2016. Young 2016 is cited once in a footnote (p. 180 n. 43).

[16] Block 2017, Hornkohl 2017, Mizrahi 2017, Moshavi and Notarius 2017b, Notarius 2017b, Schniedewind 2017.

points that could be discussed here—again, because they have been over the past decade—are: the presupposition of corpora (or sub-corpora) called CBH, TBH, and LBH rather than their empirical corroboration; generalization about the cohesiveness or distinctiveness of either CBH or LBH writings or, in other words, oversimplification regarding linguistic distribution and opposition in and between these presupposed corpora; distortion in data evaluation due to incomplete datasets, that is, complete sets that include both the feature under study as well as its opposite or contrast; and so on. It is remarkable that RH-JJ provide a significant dataset in only one of their examples: אני/אנכי (*’anoki/’ani*) ("I").[17] As for the linguistic feature that they discuss in greatest detail, the *qal* passive (see further below), they are content to say: "See the catalog of probable passive *qal*s in [Waltke and O'Connor's *Introduction to Biblical Hebrew Syntax*], 374–375nn29–36" (p. 149 n. 31).

5.4.1. An illustration will prove helpful at this point. In ch. 3 on "how to handle linguistic variation," RH-JJ discuss "sources of synchronic variation," "the way out: the contrast between LBH and CBH," "the criterion of distribution," and "the (positive) identification of CBH." At the end of the third section, on distribution, they say this: "In contrast, where the distribution of a feature aligns neatly with the demarcation of the CBH and LBH corpora, it provides a firm basis for language-historical claims. Features attested exclusively in LBH, with a distinct counterpart in CBH, show concretely, within the limits of the available evidence, how the language evolved. The same can be said for features attested only in CBH but never in LBH" (p. 43). Note the language "aligns neatly with," "attested exclusively in," "a distinct counterpart in," and "attested only in...but never in." On the next page, RH-JJ say: "As we have illustrated, there are many usages that occur repeatedly in the CBH corpus, but never in LBH. In some cases this distribution may be due to chance. However, when it can be shown that LBH employs a different feature in place of the CBH one, the absence in the late corpus must be judged to be significant. If additionally the disappearance of the feature can be explained from linguistic typology, the probative value is strengthened even more. Examples include the features enumerated above" (p. 44). Again, note

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[17] Pp. 17–19; cf. pp. 58, 79, 82, 133, 149–150 nn. 32–43, 160 n. 43, 166 n. 43. While there is a clear reduction from two pronouns to one through the history of ancient Hebrew—*i.e., diachronic development*—the distributions, functions, and other complications are more complex than admitted by RH-JJ. For discussions of these issues, see HLBH, 465–467; Hornkohl 2014: 108–111; Loder 2016; Rezetko 2003: 225–226; 2016: 261–266.

the language "occur repeatedly in...but never in," "in place of," "the absence in," and "the disappearance of."

5.4.2. Before looking at the illustration, it is also important to underline that not only do RH-JJ neglect to supply complete datasets, many of their claims about the distribution of the linguistic data are surprisingly imprecise. Consider, for example, the seven features they highlight in their CBH (Gen 12:10–20) and LBH (Esth 4:1–8) sample passages (pp. 38–42; cf. p. 44) related to the contrast between CBH and LBH: "This beautiful piece of prose [Gen 12:10–20] exhibits several expressions typical of CBH that are never found in the LBH corpus," which is supported by "but not once in LBH" (item 1), but which is at odds with "but virtually absent from LBH" (item 2) and "almost completely lacking from LBH" (item 3). Which is it, *never found in* or *sometimes found in*? Similarly: "Several features typical of LBH, and completely absent from the CBH corpus, occur in this short passage," which is supported by "none of them is found in CBH" (item 1), "but never in CBH" (item 2), "limited to LBH" (item 3), and "but never in CBH" (item 4), but which is at odds with "but practically never in CBH" (item 3) and "but extremely rare in CBH" (item 4). Again, which is it, *completely absent from* or *sometimes absent from*?

We are not splitting hairs: The issue at stake is the accurate and transparent reporting of the distribution of linguistic data. As one reads through RH-JJ's book and attempts to understand the distribution of linguistic phenomena, one encounters vague generalizations one after another in relation to different books, different groups of books, and different states of language, and their comments about difference and change in frequency, and occurrence/presence and non-occurrence/absence, and partial and systematic replacement, must be taken with a grain of salt. Differences and similarities, contrasts and continuities, are underreported or distorted. Caveat emptor! Short and clear summaries like this one obscure the facts: "As illustrated by these samples [Gen 12:10–20 and Esth 4:1–8], the linguistic differences between CBH and LBH are extensive and profound. Hurvitz's recent *Concise Lexicon of Late Biblical Hebrew* lists eighty items characteristic of LBH for which CBH uses a different word. To this number, many syntactic usages should be added. The linguistic differences between the two corpora are also systemic: LBH features are typically attested in more than one of the LBH books, and similarly, CBH features are usually found throughout the CBH corpus" (p. 41). The comment that Hurvitz's Lexicon "lists eighty items characteristic of LBH *for which CBH uses a different word*" is wildly misleading,[18]

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[18] See Rezetko and Naaijer 2016a, 2016b.

and so are the comments that "LBH features are typically attested in more than one of the LBH books, and similarly, CBH features are usually found throughout the CBH corpus." Let us illustrate this.

5.4.3. The first feature that RH-JJ discuss, the syntagm (= linguistic unit) הנה־נא (*hinneh-na'*) (BDB: "behold, I pray"), is one of many potential illustrations. They say: "The syntagm הִנֵּה־נָא [*hinneh-na'*] (v. 11 [of Gen 12]) is attested twenty-two times in the CBH corpus and four times in the poetry of Job, but not once in LBH. These statistics are not accidental; they are symptomatic of wider-ranging developments. The modal particle נָא [*na'*] is vastly more frequent in CBH than in LBH. Moreover, while in CBH it is syntactically flexible, being attached to verbs, particles, and even once to a noun in the vocative, in LBH its use is severely restricted: it occurs only immediately following a modal verbal form (imperative, jussive, cohortative)" (p. 39; cf. pp. 44, 76). For the sake of economy, our observations here will be limited to the syntagm הנה־נא (*hinneh-na'*) and not deal with the "wider-ranging development" related to נא (*na'*).[19] As RH-JJ observe, the syntagm occurs twenty-two times in the CBH corpus and also four times in the [TBH] poetry of Job. What they do not give, however, are real data, a dataset, references, or the distribution of the syntagm in the CBH corpus. In other words, as is usually the case in their book, their only observation is a generalization: CBH vs. LBH. In actual fact, the syntagm occurs nine times in Genesis (12:11; 16:2; 18:27, 18:31; 19:2, 8, 19, 20; 27:2), twice in Judges (13:13; 19:9), four times in Samuel (1 Sam 9:6; 16:15; 2 Sam 13:24; 14:21), and eight times in Kings (1 Kgs 20:31; 22:13; 2 Kgs 2:16, 19; 4:9; 5:15; 6:1). (The references for Job are 13:18; 33:2; 40:15, 16.) This distribution leads to various observations and questions. For example, how and why do RH-JJ speak about CBH as a cohesive or unified corpus when the syntagm is absent from more books than where it is present? Yes, הנה־נא (*hinneh-na'*) is used in Genesis, Judges, Samuel, and Kings—note the different rates of usage—but it is not found in Exodus, Leviticus, Numbers, Deuteronomy, and Joshua. That means that in terms of actual distribution, the latter five CBH books are identical in (non-)usage to, for example, the five LBH books of Esther–Chronicles. As an aside, and this pertains also to the larger issue of נא (*na'*), non-P in the Pentateuch attests the particle 110 times, but P only twice, thus the rates of (non-)occurrence of נא (*na'*) and הנה־נא (*hinneh-na'*) in P match LBH rather than CBH usage. Therefore, a first observation is that RH-JJ's generalization about

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[19] On נא (*na'*), see our discussions cited above in n. 7.

CBH vs. LBH obscures the distribution of הנה־נא (*hinneh-na'*) in BH. Their prior assumption of a clear difference between CBH and LBH has led them to misrepresent the data to make it seem as if it supported such a division. A second observation is that they do not follow through with the second criterion of Hurvitz's method, linguistic contrast (pp. 31, 156 n. 51). If הנה־נא (*hinneh-na'*) is an early syntagm, then what is its late equivalent? Is it הנה (*hinneh*) (1061 times in BH)? If so, then it is still incumbent on RH-JJ to explain the distribution of the two particles throughout BH. If the late equivalent of הנה־נא (*hinneh-na'*) is not הנה (*hinneh*)—that would seem to be the case, due to the frequency of הנה (*hinneh*) throughout BH and because the two items actually function differently; cf. Wilt 1996: 252–253—then it is also incumbent on RH-JJ to identify what that late equivalent is and to show that the non-occurrence of הנה־נא (*hinneh-na'*) in LBH is due to its replacement by some other feature and not due to some other factor (no context to use it, dispreferred by some authors for some reason, etc.). In summary, as with הנה־נא (*hinneh-na'*), generalizations by RH-JJ about other linguistic features very often obscure the actual facts of linguistic distribution and contrast. Their generalizations could reach higher levels of accuracy and persuasiveness if they incorporated methods of variationist and quantitative analysis in their linguistic research. And, without such solid data, their conclusions (which include such sweeping and controversial claims that CBH and LBH are "essentially different" [pp. 38, 40] or "the linguistic differences between the two corpora are also systemic" [p. 41]) remain simply assertions. It is issues like this one that the recent debates in our field have been all about, but RH-JJ choose to take no part in them when it comes to their linguistic research.

5.5. RH-JJ duck the recent discussion about what constitutes an accumulation of late (or early) features (which features? how many?).[20]

RH-JJ actually do not say much about the fourth criterion of Hurvitz's method, linguistic accumulation (pp. 41, 53–55, 59, 156 n. 51). However, they do talk about "the systematic accumulation of relatively early features in the CBH corpus and relatively late features in the LBH corpus" (p. 41), and they remark: "The profiles of CBH and LBH are distinct...The criterion of accumulation allows historical linguists to tell them apart" (p. 59). In terms of specific texts, however, they only comment on the density of LBH features in the prose tale of Job (p. 75; "Seven

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[20] We dialogue with the recent and trenchant remarks on this matter by Forbes and Hornkohl in our "Currents in the Historical Linguistics and Linguistic Dating of the Hebrew Bible."

features are not much in nearly three chapters"), the Song of Songs (p. 7; but see above), and Esther (p. 38; vs. Genesis, specifically Esth 4:1–8 vs. Gen 12:10–20). Later in the book, they make these comments on Esther and Genesis: "As we saw in chapter 3, a short extract from the book of Esther exhibited at least seven late features. A text of any length written in LBH will show at least a handful of features" (p. 53); "If the Joseph story were coeval with the LBH corpus, one would expect it to exhibit not two or three late elements but twenty or thirty" (p. 53). Two points have to be made here. First, the accumulation of late (or early) language features can be quantified in different (early and late) texts and then the texts can be compared. Second, RH-JJ talk about "a handful of [LBH] features" and "twenty or thirty [LBH features]," but again these are generalizations without an empirical basis. We have argued that there are in fact late biblical and postbiblical texts that do not have an accumulation of late language features that distinguishes them from CBH writings (e.g., LDBT 1:111–142). Since RH-JJ clearly would disagree with us, it would be helpful if they formulated a method for measuring accumulation that has a basis in specific text samples. A marker that their book is largely a restatement of the traditional linguistic dating approach is that the books with unusual and in many instances likely more chronologically-developed language like Esther are always assumed to be the measure of what late Hebrew looked like. At no point do RH-JJ engage with the argument mounted in the last decade that these books are in fact atypical, since much of the other evidence from the postmonarchic period displays a quite different, more "classical" linguistic profile, whether biblical (e.g., Joel, Haggai, Zechariah) or nonbiblical (e.g., Ben Sira, Pesher Habakkuk[21]). The chapter on Qumran focuses instead largely on pseudoclassicism, and as elsewhere in the book, there is no acknowledgement or engagement with the fact that RH-JJ's work in this area has been seriously questioned.[22] A further marker of how out of touch RH-JJ are with contemporary scholarship is the reliance on Qimron's fine, but old (1986) and incomplete, book on QH, and the absence of references to more recent work, for example, by Abegg, Geiger, Holst, Penner, especially Reymond (2014), and so on.

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[21] "Ian Young has shown that Pesher Habakkuk contains very few LBH features and is written almost entirely in Classical Hebrew" (p. 94).

[22] On pseudoclassicism, see our discussions cited above in n. 7.

5.6. RH-JJ misrepresent the views of their two major "sparring partners" (Carr, Young/Rezetko).

5.6.1. In ch. 1, RH-JJ formulate a framework for linguistic change and historical method (pp. 5–10), and they take issue with scholars who, they assert, ignore or dismiss the relevance of linguistic inquiry for determining the ages of the biblical writings. They cite as examples some works by Baden, Carr, Kratz, and Schmid, and then they narrow their focus to Carr's *The Formation of the Hebrew Bible* (Carr 2011). They make three claims about Carr's book: first, he "set[s] aside the evidence of language in his inquiry" (pp. 5–6), he "set[s] aside evidence of language in toto" (p. 7); second, he "dates most biblical texts to the exilic, Persian, and Hellenistic periods, with some having roots in the Neo-Assyrian period" (p. 7); and third, he "argues that the Song of Songs may—in agreement with its superscription—be Solomonic" (p. 7; cf. p. 9: "It is possible, as Carr maintains, that some or all of the Song of Songs was written in the tenth century BCE, perhaps by Solomon"). In response, first, it is true that Carr judges language to be less decisive for dating biblical writings than RH-JJ believe, but his thinking on the issue is not simplistic and tendentious; rather, it relates to his broader view on the complexity of biblical text composition, redaction, and transmission. In fact, he addresses the evidence of language many times in his book (and he makes many trenchant comments on linguistic dating).[23] Second, Carr clearly and explicitly (many times) does not date most biblical writings to the Persian and Hellenistic periods. For example: "The Samuel–Kings work now found in the Bible appears to be largely an exilic work at the latest, aside from a variety of small-scale additions of uncertain date" (Carr 2011: 244); or, more generally: "Yes, it is true that the Hebrew Bible is in large part a Persian-into Hellenistic-period recension, yet I see those periods as times primarily of coordination, reframing, and extension of earlier Torah and prophetic material (the latter broadly construed), *rather than the creation of the bulk of the Hebrew Bible* (but cf. Haggai, proto-Zechariah, P and H, Chronicles, Nehemiah, and Rebuilding-Ezra materials)" (Carr 2011: 489; emphasis added). Third, it is true that Carr dates the Song of Songs to an early period—like other linguists such as Young and Noegel and Rendsburg (see above), and also S. R. Driver (1913: 448–450) and Rabin (1973–1974)—but on the "Solomonic" authorship of the Song, Carr concludes: "Comparative

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[23] Carr 2011: 6, 33, 41, 49, 55, 58–59, 61, 63, 65, 125–133, 137, 148, 163, 182–183, 193–194, 196, 206–208, 220, 222, 297–298, 344–346, 349–350, 360, 376, 381–382, 389, 391, 394, 396, 400–401, 408, 413, 432–434, 439, 441–447, 452–455, 463, 482

evidence suggests that such love poetry generally circulated anonymously, but the material in this book may have gained an instruction-like attribution to Solomon when it was authored and/or reshaped for educational use, much like the attributions put on instructional material throughout the book of Proverbs" (Carr 2011: 455). In short, RH-JJ do not portray Carr's views equitably.

5.6.2. Another notable example of RH-JJ's misuse of others' writings is the case of our own work. There are two references to our work in the main body of the book and some additional ones in notes,[24] but RH-JJ save our most recent book (Rezetko and Young 2014) for appx. 2, and several preceding comments give an indication of what is in store for there: "Young's article [on Hebrew inscriptions] offers rich material but is marred by a tendentious approach" (p. 160 n. 1); "because [Young, Rezetko, and Ehrensvärd] reject all relevance of language to dating issues, they are unable to give an idea of what is more or less plausible" (p. 178 n. 1). It is interesting, to say the least, that RH-JJ contend with our work only in an appendix; they completely overlook the major theoretical and methodological issues that we (and others) address; and they somehow manage to criticize our work *without even bringing up* the historical sociolinguistic methods that are applied through our entire book (cross-textual variable analysis and variationist analysis). But we will focus on other points here.

RH-JJ call our model "revisionist" and "non-chronological" (pp. 135–136, 139, 143–144, 179 n. 6). Revisionist, compared to RH-JJ's traditional linguistic dating model, yes, but non-chronological, no, not in the way RH-JJ construe it. We have addressed such straw man caricatures of our work elsewhere.[25] In addition, RH-JJ conflate and thereby misrepresent our two major publications (LDBT, HLBH). We have explained how the contents, objectives, arguments, and theoretical and methodological foundations of the two works are similar and different from one another (HLBH, 3–5, 14–21, 596–598). For this reason also, RH-JJ's use of Dresher's words as an argument against HLBH (pp. 136, 141, 143), when Dresher was evaluating LDBT, is a doubtful tactic (see further below). Another instance of RH-JJ's misrepresentation of our views is their claim that we argue that the *historical linguistics* of BH is "infeasible" (pp. 135, 139; cf. p. 141) and "impossible" (pp. 135, 139) and "cannot be properly done" (p. 135) and we are given to a

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[24] Pp. 7, 94, 146 n. 10, 150 n. 41, 153 n. 1, 155 n. 28, 156 n. 49, 157 n. 3, 159 n. 51, 160 n. 1, 161 nn. 12 and 16, 162 nn. 25 and 33, 163 n. 51, 164 n. 61, 169 n. 30, 178 n. 1.

[25] HLBH, 594–596; Young, Rezetko, and Ehrensvärd 2016.

"disavowal of historical linguistics" (p. 136).[26] Those familiar with HLBH will have a difficult time accepting such a verdict. This is what we really say: "The truth of the situation as we see it is that it is largely impossible to undertake 'normal' or conventional historical linguistic research on ancient Hebrew simply because the nature of the sources of data does not permit it. The study of language variation and change is complex seeing that it must embrace internal and external constraints, linked to senders, receivers, and settings and their interaction, individually and corporately, and involving diverse psychological and social dimensions. But we can do this only to a limited degree with BH, compared to contemporary languages, and even when compared to other premodern language varieties of English, French, Spanish, Akkadian, Hittite, and so on, which are far better documented by many non-literary writings that are authentic, non-composite, dated, and localized..." (HLBH, 242). Here lies the problem: RH-JJ ignore the words "normal" and "conventional" in our statement and the reasons behind that statement that have to do with the "nature of the sources" as discussed on that page and more extensively earlier in our book (HLBH, 21–45). It is obvious that we are fully committed to historical linguistic research on ancient Hebrew—that is what we do all through HLBH, that is what cross-textual variable analysis and variationist analysis are!—and any claim to the contrary is mind-boggling. Lastly, RH-JJ discuss almost none of the actual content of HLBH, and when they do their comments tend to be inadequate or misrepresentative. Two examples follow.

5.6.3. RH-JJ's first three criticisms of our work relate to alleged inaccuracies in our analyses of individual linguistic features (p. 135); our alleged reliance on an uncritical Masoretic Text (MT)-only approach which skews our data and results (pp. 136–139); and our alleged misconstrual of the nature and implications of ancient textual variation, including the degree of fluidity in the transmission of the biblical texts, which is incorrect according to the normal procedures of textual criticism (pp. 136, 139–141). RH-JJ omit that we have responded to the first and second criticisms.[27] RH-JJ's second and third criticisms relate to textual matters. They say, for example: "The degree of fluidity in the transmission of the biblical texts is not as great as they suggest. Part of the problem is that their non-chronological approach does not allow them to distinguish between earlier and later readings. All variants, in the eyes of their method, are stylistic choices. There is

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[26] On the other hand, we do believe that the *linguistic dating* of biblical writings is infeasible and impossible.

[27] Our responses to these claims (in, for example, Hendel 2011, Joosten 2012), can be found in Rezetko, Young, and Ehrensvärd 2011, Young 2016, and Young, Rezetko, and Ehrensvärd 2016.

no room for scribal modernizations or secondary readings. For instance, when they compare, in their second book, the different readings for 'kingdom' in MT and 4QSam[b] of 1 Sam 20:31 (מַלְכוּת [*malkut*] and ממלכה [*mamlakah*] ["kingdom"], respectively), they make no text-critical judgments. These are stylistic variants, part of the endless proliferation of fluid texts. Their method has no room for textual criticism of variant readings" (p. 139). RH-JJ seem unaware that we explicitly address the evaluation of variants, especially linguistic variants, in textual criticism (HLBH, 77–79), we discuss earlier and later readings related to words for "kingdom" in one of our case studies (HLBH, 346–348, 568), and we speak about potentially earlier and later readings in many other places in our book, including in our extensive studies of parallel passages and Samuel manuscripts (HLBH, 145–210, 413–591). Also, as for this statement, "[t]he degree of fluidity in the transmission of the biblical texts is not as great as they suggest" (p. 139), RH-JJ do not interact at all with our cross-textual variable analysis and statistical analysis of Samuel manuscripts (HLBH, 171–210) or our commentary on linguistic variants in MT and Qumran Samuel (HLBH, 453–591). (Nor, for that matter, do RH-JJ interact with our cross-textual variable analysis and commentary on linguistic variants in parallel passages in the MT [HLBH, 145–169, 413–452]). Given that RH-JJ do not interact with our analysis of some 200 linguistic variants in Samuel, how can they make such a claim about our work? In fact, RH-JJ discuss just five cases of textual variation in Samuel. They conclude twice that 4QSam[a] reflects later language (than MT; 1 Sam 10:27; 2 Sam 3:2; pp. 1–4, 7, 42, 159 n. 46) and three times that MT reflects the later situation (than 4QSam[a] or 4QSam[b]; 1 Sam 3:7; 20:31; 2 Sam 13:39; pp. 49–52, 137, 139). Besides their remark on "degree of fluidity," RH-JJ's other remark on Samuel manuscripts is: "The rewriting of the Samuel passage in Chronicles reflects the same language-state that we see in 4QSam[a]" (p. 4). However, even their own findings suggest that such a conclusion is only true sometimes, and not necessarily even half the time. The situation is more complex, and the data are more diverse, than RH-JJ seem to envision. In actual fact, most textual variants do not involve linguistic features with CBH vs. LBH import, and consequently it happens only occasionally that textual criticism and historical linguistics could and might "complement" or "reinforce" each other (pp. 50–51, 59), or that textual criticism "comes to the aid of the historical linguist" or "has the global effect of confirming the diachronic approach" (pp. 50, 59). RH-JJ select and underline examples that sustain their argument, and thereby they commit the fallacy of inferring something about a whole class of things on the basis of some instances of that thing, that is, the fallacy of insufficient evidence or sample.

RH-JJ might benefit from our detailed analysis of linguistic variants in Samuel manuscripts (HLBH, 145–210, 413–591; cf. also the helpful but less complete and somewhat problematic study of linguistic exegesis in Samuel manuscripts in Driesbach 2016: 102–131).

Given how experienced RH-JJ are in dealing with ancient texts, the simplicity of their approach to textual variation is surprising. (Their main discussions of textual criticism are on pp. 1–4, 47–59.) After a promising start where they acknowledge, with conventional scholarship, that "[a]ny biblical book may turn out to contain strata and fragments composed at wholly different periods" (p. ix), they then proceed to talk perplexingly as if most biblical compositions have individual authors at specific times, the specific linguistic details of whose original writing have been preserved in scribal transmission. They aim to determine "a chronology for individual writings" (p. x), because "linguistic discourses contain evidence of their time of composition" (p. 10), the "original writing" can be recovered for linguistic analysis (p. 50), and minor details of the compositions are regularly assumed to have been transmitted exactly over many hundreds of years. In the face of the amount of contrary data presented by us, such statements remain mere assertions.

5.6.4. RH-JJ's fourth criticism of our work relates to the inferences we draw from our statistics which are allegedly incorrect according to the normal procedures of historical linguistics (pp. 136, 141–144). As remarked above, RH-JJ use Dresher's critique of LDBT as their argument against HLBH (pp. 136, 141, 143). However, in HLBH we acknowledge or respond to Dresher's major criticisms of our argument in LDBT (cf. the index of modern authors, HLBH, 647). And, given RH-JJ's use of Dresher's words against our work, it is somewhat ironic that we actually reach a conclusion similar to Dresher's on מלכות (*malkut*) as a generally later form: "On the face of it, it seems likely that in the history of ancient Hebrew מלכות [*malkut*] gradually became more predominant, and probably due to Aramaic influence" (HLBH, 350; cf. 329–350 for the complete case study). This is clearly a chronological statement, not a "non-chronological" one. Instead, RH-JJ say this about our argument: "They conclude: 'words for "kingdom" could be "freely" selected *in any period*'" (p. 141; emphasis added). That, however, is far from what we say. On those pages we discuss overlapping fragments of the pseudepigraphic work Pseudo-Moses where in the same context the fragments alternate between either ממלכה (*mamlakah*) or מלכות (*malkut*) (HLBH, 345–346). About *that* situation in those contemporaneous fragments, we say: "This evidence suggests that these two nouns are synonymous, or can be used synonymously. It also shows that words for 'kingdom' could be 'freely' selected *and could alternate in manuscripts of a single composition*"

(HLBH, 346; emphasis added). RH-JJ have removed our statement from its original context, shortened it, and inserted their invented phrase "in any period," but we actually arrive at a substantially different—*and chronological*—conclusion about the use of מלכות (*malkut*) in ancient Hebrew.

After their discussion of ממלכה (*mamlakah*) and מלכות (*malkut*), RH-JJ proceed to criticize the final paragraph (HLBH, 394) in our study of directive *he* (HLBH, 374–394). Their aim is to show that there "is a recurring tendency in the revisionist model to cherry-pick the statistical data in a manner that misrepresents their true import" (pp. 143–144), and "the use of statistical arguments in the revisionist model is driven more by rhetorical aims than philological precision" (p. 144). These are big claims. In response, first, RH-JJ do not discuss the larger case study or statistical data on the previous twenty pages of our discussion, or anywhere else in HLBH for that matter. Consequently, their judgment that we cherry-pick the statistical data and simply engage in rhetoric are based on a slim evidentiary basis. Also, strangely, RH-JJ actually base this conclusion about our work on their reading of *one sentence* in Young 2016: 999 rather than the fuller 20-page treatment in HLBH (p. 180 nn. 43–44).[28] Second, RH-JJ omit to acknowledge our own cautious remarks about directive *he* in epigraphic Hebrew: "The inscriptional corpus is *too small to reach solid conclusions on this issue*, but the evidence is suggestive. On the basis of these data, one could suggest *a tentative theory...This highly theoretical discussion* gives an indication of what might be achievable if we had a more adequate corpus of dated and localized evidence for ancient Hebrew..." (HLBH, 394; emphasis added). Third, the case study in HLBH focuses on the verb of movement בוא (*bo'*) ("to go") when used with a place of destination. (The reasons for this focus are explained in HLBH, 377–378.) Hence, RH-JJ's several examples with הלך (*halak*) ("to go"), עלה (*'alah*) ("to go up"), and שלח (*shalach*) ("to send") *might* be relevant in a broader discussion, but they are outside the purview of the case study in HLBH. (However, we do make some brief remarks on עלה [*'alah*] in epigraphic Hebrew on the previous page, HLBH, 393.)

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[28] The quotes given by RH-JJ (pp. 144, 180 nn. 43–44) are from Young 2016: 999, not HLBH, 394. The sentence they quote is: "[W]hile in inscriptions the directive *hê* is always (100%) used with the motion verbs בוא [*bo'*] and עלה [*'alah*] to express movement to a destination, MT Samuel uses it in similar contexts only once, i.e., 3% of the time."

5.7. RH-JJ's model of consilence falls short because they a priori reject the literary analysis of biblical literature.

As noted above, ch. 8 on consilience is a commendable step forward. That said, the model adopted in the book is not new or novel (see, for example, Dever 2001; Halpern 2001: 57–72), and while RH-JJ explicitly exclude from their model *Traditionsgeschichte* (the history of conceptions, ideas, etc.; pp. ix, 7, 98–101; in this they follow Sommer 2011), their inattention to literary criticism is more problematic. The book would be more well-rounded and convincing if its subtitle and contents were "A Linguistic, Textual, *Literary*, and Historical Study." RH-JJ remark: "[I]s the evidence of language relevant for the study of the literary history of the Hebrew Bible? Since the Hebrew Bible consists solely of *linguistic discourse*, it would seem that the answer should be yes" (p. 10; emphasis added). Surely, however, all written discourse is linguistic discourse, and the more significant issue for the HB is that it is *literary discourse* to the core (as opposed to being a documentary source, for example). The implication of this is not "the study of language is a necessary partner for the literary history of the Hebrew Bible" (p. 10), or "we must supplement linguistic inquiry with other kinds of historical scholarship" (p. 98), but rather, "before the text can be used as data for forming hypotheses about specific stages of a language" (Schendl 2001: 15; cf. 14–15) we must first unravel the extralinguistic context, the textual and literary envelope as it were, in which the linguistic data are embedded. In other words, RH-JJ have flip-flopped conventional historical linguistic procedure,[29] which suggests that in their model of consilience (for determining the ages of the HB), linguistic analysis keeps on having a preeminent position (as more unbiased, reliable, and so on), which again is against the normal philological practice of historical linguists. This evaluation is confirmed by several avenues of evidence. First, seven chapters on language precede the final chapter on consilience which gives the impression that it is tacked-on at the end. (Could this also be due to the fact that Joosten wrote chs. 3–7 and Hendel chs. 1–2, 8?) Second, RH-JJ regard their book as an opportunity to educate biblical exegetes/scholars in the Hebrew language (p. 5). (Of course, the other side would say the opposite, that Hebrew linguists should be more in tune with biblical exegesis/scholarship.) Third, their discussions of specific texts are short of literary-critical evidence and analysis. This is true also for

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[29] For lengthy discussion, see HLBH, 21–45, and our "Currents in the Historical Linguistics and Linguistic Dating of the Hebrew Bible." RH-JJ's assertion that "[d]ating texts by their language is a well-established practice in biblical studies *as in many other fields*" (p. x; emphasis added) is manifestly untrue. Which texts in which fields do they have in mind?

the sections of the book where they address "textual criticism and redaction criticism" and "historical linguistics and redaction history."

5.7.1. In their discussion of "textual criticism and *redaction criticism*" (pp. 56–58; emphasis added), RH-JJ comment briefly on Jeremiah (see further below), Job, and Isaiah, before concentrating on the plus in MT Judg 6:7–10 when compared to 4QJudgᵃ. About this text they say: "Another possibility is that a textual minus [in 4QJudgᵃ] indicates an insertion [in MT], but the supplementation occurred at an early date" (p. 57). Following Rofé, they view the insertion as "pre-Deuteronomistic" (p. 58) and "inserted during the monarchic period or soon after...in this early period" (p. 58). The problem is that RH-JJ circumvent all of the literary-critical evidence and analysis, including responses to Rofé's arguments, that suggest that the MT plus (written in CBH) was composed and inserted in the late Second Temple period (e.g., Blum 2006: 103; Rezetko 2013: 20–31; cf. Ausloos 2014).

5.7.2. In their section on "historical linguistics and *redaction history*" (pp. 77–79; emphasis added), RH-JJ concentrate on Jeremiah (cf. also p. 56) followed by a brief comment on Isaiah. Here they conclude: "In a global view, the language of the book suggests that few, if any, of the longer sections were added later than the end of the sixth century" (p. 79). Again, the problem is that RH-JJ do not engage or cite any literature or analysis related to the editorial history of Jeremiah—which is remarkable in a section on redaction history. Moreover, they quietly sidestep the scholarly consensus on the literary formation of the book: "[T]he Masoretic edition of Jeremiah is *post-exilic*, as opposed to the edition included in the LXX" (Tov 2012b: 50; emphasis added; cf. Tov 2012a: 288; Rezetko 2016a).

5.7.3. RH-JJ argue cogently that details such as the Philistine loanwords סרן (*seren*) ("governor") and קובע/כובע (*koba'/qoba'*) ("helmet") and references to the Philistine pentapolis including Gath (pp. 25, 104–106, 172 n. 42), as in Samuel's stories of the ark (1 Sam 5–6) and David and Goliath (1 Sam 17), are imprints of "early monarchical memories in classical Hebrew literature. The explicit and implicit setting of these CBH texts is the Iron Age" (p. 106). The problem again, however, is that RH-JJ neglect to dialogue with textual and literary critics on the book's complex production history (e.g., Auld and Eynikel 2010, Becker and Bezzel 2014, Dietrich, Edenburg, and Hugo 2016, Edenburg and Pakkala 2013, Hugo and Schenker 2010). There is a tendency to confuse sources, compositions, redactions, and the linguistic forms that turn up in the late manuscripts of the writings. Moreover, RH-JJ take an all-or-nothing approach. There

are certainly early peripheral phenomena and retentions of archaisms of various kinds in (MT) Samuel (cf. Rezetko 2007: 10–11 n. 24, 74 n. 137), but those do not demonstrate that the book is only ("compositionally") early and not also ("editorially") late. In short, textual and literary critics frequently argue that the current MT in many aspects reflects editorial revision and expansion beyond the monarchic period into the postmonarchic period, even into the last centuries BCE around the turn of the era (cf. the brief synopsis of the production of Samuel in HLBH, 174–176). RH-JJ may disagree, but they do not have the liberty to overstep completely the relevant scholars, publications, facts, and arguments, and especially not in a book that seeks "the convergence of different bodies of data and inference" (p. 125), among which literary criticism should also play a role. Finally, we venture to suggest that the main factor that keeps RH-JJ from imagining postmonarchic writing in CBH (pp. 58, 82, 124–125)—ultimately this is what the recent debates have been about—is the absence of dialogue with literary critics in the formulation of their model of consilience.[30] In our estimation, RH-JJ's substantial resistance to literary scholarship yields a deeply flawed approach to the history of biblical literature (contrast their alternate claim, pp. 4–10). Their model is not the only one—nor, we believe, the best one—that can accommodate the kinds of peripheral details that they describe. The answer to their question, "How Old is the Hebrew Bible?," with reference to the individual writings of the HB, is, we believe, not all or only old, and not all or only young, but rather, both. The individual writings of the HB are long-duration literature, the final products of a complex (*chronological! diachronic!*) process of composition and

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[30] At various points RH-JJ acknowledge the complex production history and composite literary makeup of biblical writings, e.g., pp. ix, 5–10, 56–58, 74 ("redaction history is uncertain"), 77–82, 98–101, 121 ("insets of...text, as distinct literary layers exist side by side"), 124–125, 145–146 n. 7, 146 n. 17 ("lengthy compositional history"), 150 n. 46 ("late supplement"). However, it is very clear that for them language issues take priority in matters of dating, and at no point do they look to discern literary history using literary-historical methods. RH-JJ concur with Blum that "linguistic arguments should be part of a much more comprehensive historical-philological endeavor" (p. 98; cf. Blum 2016: 305), but the irony is that Blum does not exclude *linguistic or literary-historical analysis and judgments* from his method. Also, RH-JJ take issue with Gesundheit's example of Lucian and other second-century CE Greek writers who "learned to produce literary works in nearly flawless imitations of fifth-century Attic" (p. 124; cf. Gesundheit 2016: 298–299; note that Gesundheit cites the classicist and linguist Eleanor Dickey). However, right or wrong in this case, there is an abundance of additional evidence related to Standard Babylonian, Late Middle Egyptian, Ancient Greek, Classical Sanskrit, Classical Arabic, and Classical Armenian. See our "Currents in the Historical Linguistics and Linguistic Dating of the Hebrew Bible." For example, according to the Indo-European linguist Jared Klein, "there is really no such thing as a diachronic linguistics of Classical Armenian in these [5th–17th] centuries," because "[t]he language of fifth century C.E. Classical Armenian, known as the *grabar* or simply 'literary language,' continued to be used for written Armenian until at least the seventeenth century" (Klein 2016: 867).

transmission, and their contents (including language) are authorial, editorial, and scribal. A "thick description" of the ages of biblical literature cannot ignore literary-historical analysis and judgments. "Language [is] relevant for the study of the literary history of the Hebrew Bible" (p. 10)—and literary criticism is too! We concur with Hendel and Joosten: "One wants a model that successfully accommodates as many different classes of data as possible" (p. 121). The difference between us and them is that we are not reticent to embrace literary analysis among the different methods and classes of data that should be included in the model. In sum, the answer to our question, "Can the Ages of Biblical *Literature* be Discerned Without *Literary* Analysis?," is they should not as a matter of principle, and they can not as a matter of practice.

6. Illustrations

To this point, we have discussed two of RH-JJ's linguistic examples (הנה-נא [*hinneh-na'*], Philistine loanwords סרן [*seren*] and קובע/כובע [*koba'/qoba'*]), and we have also discussed their use of two of our linguistic case studies (מלכות [*malkut*] and ממלכה [*mamlakah*], directive *he*). In addition, we have examined some of their other examples and case studies in other contexts (cf. n. 7).

In the advertisement for their book on The Bible and Interpretation, RH-JJ provide the additional illustration of הֶעֱמַדְתִּיךָ (*he'emadtika*) in Exod 9:16. They prefer to render this verb as "I have let you live" (NRSV) or "I have kept you standing" rather than "I raised thee up" (KJV) or "I have raised you up." We could take issue with several aspects of RH-JJ's argument here,[31] but it is sufficient in this context to underline that their main point about the evolution of עמד (*'amad*) over time from "to be in a standing position" (positional meaning) to "to stand up" (transitional meaning) overlooks, first, that there is some evidence for the latter transitional meaning in CBH, and second, and more importantly, that the former positional meaning continues

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[31] It is uncertain that the construal of the *hiphil* (!) of עמד (*'amad*) as "I have raised you up" is problematic or incorrect (cf. NIV, HALOT, and the use of the *hiphil* elsewhere in CBH, e.g., Judg 16:25); that RH-JJ's comment about the history of religion and predestination to perdition is germane to the interpretation of Exod 9:16 (cf. Clines 1998); or that Paul (or the KJV) misunderstood and "mix[ed] up different types of Hebrew" (cf. Cranfield 1979: 486; Moo 1996: 594–595; Stanley 1992: 107–108; etc.). What seems certain, however, is that the translator of the Septuagint correctly understood העמדתיך (*he'emadtika*) in Exod 9:16 (cf. Wevers 1990: 131–132), which also underlines the point made below about the continuity of the usual meaning of עמד (*'amad*) in the postexilic period.

to be the normal usage in LBH and in some postbiblical writings.[32] Consequently, historical linguistics does not "independently point to" anything about הֶעֱמַדְתִּיךָ (heʿemadtika) in Exod 9:16.

Finally, we have taken the opportunity to comment elsewhere on the linguistic feature that RH-JJ discuss in the greatest detail, the *qal* passive.[33] See our Appendix 2.[34] In brief, we agree with RH-JJ that the form was gradually lost, but their linguistic dating claim, "[a]ll other things being equal, a passage with the *qal* passive may be considered earlier than a passage using the *niphal*" (p. 31), is problematized by a closer analysis of the *qal* passive and related *niphal* data. They make way too much of the far-too-little data that they have cited and examined.

7. Conclusion

This is a book that potentially could have made a significant contribution to the field, but first it would have had to be substantially revised to engage with contemporary scholarship. RH-JJ could have made an effort to dialogue with other Hebraists about actual datasets and on important theoretical and methodological issues. The way it is, their book pays scant attention to recent historical linguistic research on BH, and though its notion of consilience is admirable, it also pays scant attention to other streams of textual and literary research that focus on the complex production history of the biblical writings. The book can hardly be considered a comprehensive study of the dating of the Hebrew Bible. Finally, rather than dealing with many disconnected linguistic, textual, and cultural/historical phenomena, RH-JJ—after thoroughly revising their method and seeking to engage in scholarly dialogue with other linguists and textual and literary critics—might have considered illustrating their model of consilience through detailed application to a particular book(s) (e.g., Judges, Samuel) or passage(s) (e.g., Judges 6, 1 Samuel 17). As it stands, Hendel and Joosten's answer to the question, "How Old is the Hebrew Bible?," is unoriginal because they do little more than offer a sophisticated repackaging of the traditional linguistic dating approach and results, and it is also unsatisfactory because they eschew literary criticism in the formulation of their model of consilience for determining the ages of biblical literature.

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[32] See our detailed case study in HLBH, 294–304.

[33] Pp. 1–4, 16–17, 19, 31, 39, 41–42, 44, 55, 76, 79–80, 82–83, 133, 145 nn. 2–6, 149 nn. 23–31, 156 n. 50.

[34] https://www.academia.edu/38112870/2019c_Young_Rezetko_Can_the_Ages_of_Biblical_Literature_be_Discerned_Without_Literary_Analysis_Appendix_2.

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