

Offprint from:

Diachrony in Biblical Hebrew

Edited by

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Winona Lake, Indiana

EISENBRAUNS

2012

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www.eisenbrauns.com

Library of Congress Cataloging-in-Publication Data

Diachrony in biblical Hebrew / edited by Cynthia L. Miller-Naudé and Ziony Zevit.
p. cm. — (Linguistic studies in ancient West Semitic ; 8)

Includes bibliographical references and indexes.

ISBN 978-1-57506-253-2 (hardback : alk. paper)

1. Hebrew language—Grammar. 2. Bible O.T.—Language, style.
3. Linguistic change. I. Miller, Cynthia L. (Cynthia Lynn), 1957– II. Zevit,
Ziony.

PJ4556.D485 2012

492.4'5—dc23

2012028645

The paper used in this publication meets the minimum requirements of the American National Standard for Information Sciences—Permanence of Paper for Printed Library Materials. ANSI Z39.48-1984.®™

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Historical Linguistics and Biblical Hebrew

ROBERT D. HOLMSTEDT

1. Introduction

“Variety is the spice of life,” goes the old proverb, and life in Hebrew historical linguistics has become very spicy indeed. For generations of Hebraists who grappled with linguistic variation in the Hebrew Bible and other ancient Hebrew sources, situating a given feature *diachronically* was often the first and primary concern. Along the way, some features were given other, nonchronological explanations, such as northern-versus-southern dialects and literary-versus-colloquial registers, but these sorts of explanation were often proposed if and only if a chronological explanation was not apparent. Now, thanks to the tenacious work of certain “Young Turks,” there is an alternative explanation for the variation in the Hebrew Bible that is at its core nonchronological:

LBH [is] merely one style of Hebrew in the Second Temple and quite possibly First Temple periods. Both EBH and LBH are styles with roots in preexilic Hebrew, which continue throughout the postexilic period. ‘Early’ BH and ‘Late’ BH, therefore, do not represent different chronological periods in the history of BH, but instead represent coexisting styles of literary Hebrew throughout the biblical period. (Young and Rezetko 2008: 2.96).

To switch from spice to the high seas, this is not the proverbial shot across the bow but a full broadside against diachronic Hebrew studies. The implications are enormous: in the process of arguing that it is impossible to date texts linguistically, these authors have effectively blocked access to diachronically meaningful data, and the entire history of Hebrew has been rewritten. That is, within their alternative explanation, there is no discernible linguistic history in the Hebrew Bible. In the face of this broadside, the question is, of course, will the “diachronic” ship sink?

2. Methodological Issues

One of the healthiest, most productive activities following a strong challenge to a consensus is a careful, agenda-free assessment of methodological principles and practices. We should put aside the Hebrew data and analyses for a long moment and revisit the linguistic literature that addresses diachronic

issues. We should be asking ourselves regularly, “How is this done in general?” and “How can this be applied to Hebrew, if at all?” Although there may be a few new sets of data to analyze, most of the issues are well known and certainly are not going to change (now). Thus, we should take this opportunity to reestablish an explicit and sound methodology for discussing variation and language change in ancient texts.

2.1. Artifacts, Reconstructed Texts, and Grammars: What Is the Object of Study?

A primary challenge to carrying out historical linguistic research on biblical texts is the nature of the texts themselves. Linguistic research on ancient Hebrew texts requires the researcher to be both philologist and linguist. We must not only deal with the challenges posed by paleography, orthography, textual lacunae, and—especially for the Bible—the complexities of composition and textual traditions, we must also navigate the theories and terminology of general linguistics. In concrete terms, this means, for example, that one cannot simply pick up a Hebrew Bible, turn to some datum in Isaiah 45, take it as evidence from the late *eighth* century B.C.E., and work it into a linguistic analysis. That is, we must be aware of the majority positions in biblical studies: namely, that Isaiah 45 is typically understood to have late-sixth-century origin, based on, among other clues, the mention of Cyrus (550–530 B.C.E.), the Persian ruler who took Babylon in 539 B.C.E. Given the complexity of Hebrew historical linguistics, how can we go about this task?

Mark Hale, in his *Historical Linguistics: Theory and Method* (2007), presents what I consider to be one of the clearest discussions of philology as it deals with linguistic artifacts.¹ Hale attempts to clarify the *real object* of historical-linguistic study when we are working with ancient languages. We all begin with historical artifacts as the source of linguistic data, whether an inscription, a scroll from the Qumran caves, or a medieval Masoretic codex, but these are not in fact the primary object of *linguistic* study. Due to the accidents of history, the chronological sequence of artifacts may not reflect the chronological sequence of the linguistic data they contain. For example, most of us think that Masoretic codices, such as B19a (the Leningrad Codex), often preserve earlier data than the corresponding manuscripts from Qumran, written a millennium earlier. Thus, philologists establish “the attributes of a text, many of which may be relevant for subsequent linguistic analysis” (Hale 2007: 21). The product of philological analysis is localized and dated texts (dated relative to other sources, at least). Yet even this reconstructed philological text is not the object of historical-linguistic analysis.

1. On the distinction between *philology* and *linguistics*, see also Holmstedt 2006. On the challenges in reconstructing ancient languages, see Miller 2004.

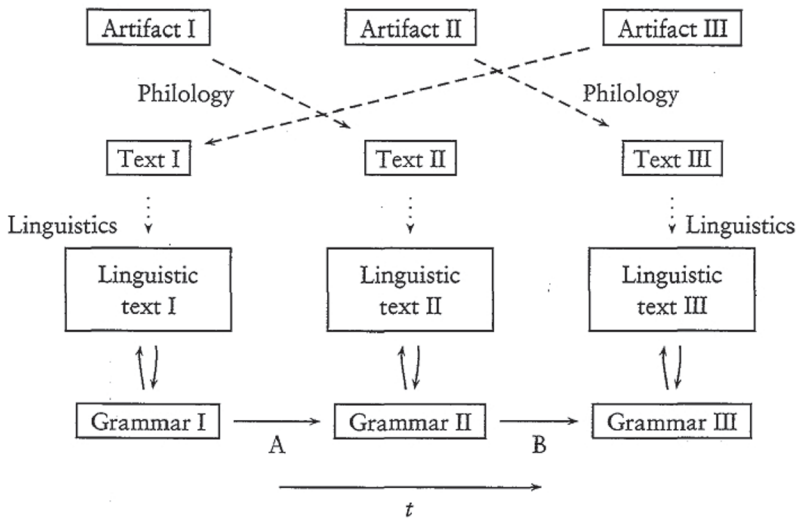


Figure 1. The relationship between grammars (Hale 2007: 25, fig. 2.6). Reproduced with permission.

Historical linguists are not interested in the “texts” but what the texts contain, specifically the relationships between linguistic structures contained within the different texts (Hale 2007: 22 n. 5). Hale asserts that “a rigorous linguistic analysis of the text which was philologically established from a given historical artifact would lead, in principle, to a hypothesized *linguistic* structure for the relevant aspects of that text” (2007: 23). This *linguistic text* presents us with a set of linguistic representations for the analyzed philological text. Again, it is not the *texts* (whether the artifact, the philologist’s reconstruction, or the linguist’s reconstruction) that are compared in historical linguistics but the *grammatical features*—and the grammars behind these features—represented within the texts (see 2007: 25). Figure 1 presents Hale’s visualization of the layers involved in getting to the real object of historical linguistic analysis.

Figure 1 illustrates the complexity of the historical linguist’s task. In the top row are the artifacts, the concrete texts existing on some physical surface. Assuming that each artifact can be dated, even roughly, by some external factor (such as archaeological context, scribal note, historical reference on the manuscript), the next challenge is determining the status of the text represented by the artifact. The crossing dotted lines in Hale’s figure represent the not uncommon situation in which an artifact (for example, the physical manuscript on which text exists) is externally dated to a period later than the philologist dates the text on the basis of internal language clues or historical references. Perhaps

the artifact preserves a copy of the text that was composed much earlier and preserved via a conservative scribal process. Thus, a great part of the philologist's task is to determine the relationship of the artifact to the text. Another part of the philological task is to reconstruct any parts of the text that have suffered from, for example, scribal errors, weather, or other physical damage. The result of this activity is the "philological text" in the second row of fig. 1.

The linguist uses the philological text to reconstruct a "linguistic text," represented in the third row of fig. 1. It is from this linguistic text that the linguist deduces the grammar (in the fourth row) that produced the language "output" represented in the linguistic text. The grammar itself is constrained by grammatical principles arising from theoretical and cross-linguistic research. Thus, the grammar can in turn influence the reconstruction of the linguistic text. This relationship is represented by the bidirectional arrows between the third and fourth rows in fig. 1. Once the grammar of each text is established with reasonable confidence, the grammars may be compared with each other and, using both external and internal information, situated relative to each other chronologically.

Note that *text* in this discussion is not the same thing as *composition*, as the term is often used in biblical studies. For instance, if we took the book of Ezekiel in B19a and began reconstructing the philological text, the result would not necessarily be the prophet Ezekiel's work. Rather, the philological text should be very much like the text-critical goal of the *last redaction*.² This suggests very strongly that text critics and linguists should work much more closely than is often the case.³

The text-critical argument is sometimes set up as an obstacle to historical linguistics, in general, and to the dating of texts, in particular. Admittedly, the

2. This accords with Tov's approach (2001: 288):

as a rule, this branch of textual criticism aims neither at the compositions written by the biblical authors, nor at the previous oral stages, if such existed, but only at that stage of the composition which is attested in the textual evidence. Textual analysis does not aim at oral or literary stages beyond this evidence.

Moreover, earlier in the book, Tov makes it clear that, even when he discusses the "single original text," he is not referring to "the most ancient form or earliest literary strand of a biblical book nor to the earliest attested textual form, but rather to the text or edition (or a number of consecutive literary editions) that contained the finished literary product and which stood at the beginning of the process of textual transmission" (Tov 2001: 171).

3. A related point about the Masoretic tradition is critical. If the Masoretic textual tradition has preserved linguistic elements that are no longer part of whatever Hebrew the Masoretic scribes knew, then we have historical-linguistic evidence that can be used to build profiles of linguistic change. The result in most scribal traditions is a text in which it is possible to recognize the various linguistic layers that have accreted (see, for example, Ó Buachalla 1982). The linguist's task is therefore not merely extracting linguistic data in a naïve way but, rather, dating (relatively) the discernible layers and establishing a (relative) linguistic chronology.

reconstructive process is challenging, whether the goal is textual or philological; but the challenge should not be exaggerated. For example, while Young and Rezetko may be right to criticize the general disconnect between Hebrew historical study and text-critical study, it does not follow that “textual stability is essential to linguistic dating of texts. . . . If texts were modified to any serious degree, then we cannot use language as a criterion for talking about the original language of biblical books or the dates of original authors” (2008: 344). If the artifacts from which philological texts are reconstructed can be placed in sequential order, then the differences between them are prime evidence for language change. Moreover, even Young and Rezetko discuss numerous examples where differences among the textual evidence show “that the language was subject to constant revision at the hands of editors and scribes who passed down the biblical tradition through many generations” (2008: 359; also see p. 351). If they can identify textual differences that stand in an obvious chronological relationship, then they have engaged in the very reconstruction work they seem to disallow. It comes down to a simple principled position: either we are limited to the historical artifact, or we are allowed to access information, whether textual or linguistic, “behind” the historical artifact by means of reconstruction.

2.2. Language Change

Now that I have briefly discussed the object of study from a philological and linguistic perspective, let us turn to the issue of language change. The nature of language change has not been discussed with necessary clarity in ancient Hebrew studies. First, the categories of “Archaic BH,” “Standard/Classical/Early BH,” and “Late BH” are not only unhelpful, they have no empirical status.

As Naudé reminds Hebraists in his 2003 study of the transitions of Biblical Hebrew, it is only the formal grammar represented in the output of an individual, the I-language represented by an idiolect, that is a discrete object of scientific study (Naudé 2003: 197; see also Hale 2007: 3–18). From an I-language perspective, “change results when transmission is flawed with respect to some features. When transmission is not flawed (with respect to some feature), there has been no change in the strict sense” (Hale 2007: 36). This feature, the product of imperfect transmission in the acquisition process, spreads or becomes *diffuse*, when it is accurately acquired by another speaker.

This view of change and diffusion has a number of implications for how we may even talk about the history of Hebrew. First, as Naudé rightly points out, the notion of a “transitional” stage between SBH and LBH is not justifiable (2003: 202). Each I-language, represented by the idiolect that is itself represented in the language of the philologically reconstructed texts of the biblical books, is its own “stage,” as it were. The idea underlying this approach can be expressed by borrowing and modifying the dialectological dictum slightly:

every change and its resulting diffusion (if it becomes diffuse) has its own history. It is unlikely, therefore, that any two change-and-diffusion features will have the same origin. It is also unlikely that any two I-languages will reflect the same cluster of change-and-diffusion features, which implies that the *exact* order of texts may vary for each feature analyzed. But no single feature set can be determinative for a relative order, since the texts (or, the I-languages represented within the texts) do not stand in a two-dimensional line; rather, since each I-language is a unique constellation of features, some I-languages/texts will stand to the “left” or “right” of any two-dimensional line of descent. (For further discussion of statistical analysis and relative dating with regard to Biblical Hebrew, see Andersen and Forbes 1986.)

Two more features of change-and-diffusion are critical before a coherent picture emerges, from which we may derive an analytical framework for Hebrew. First, it necessarily follows from the acquisition-related change-and-diffusion framework that a new form representing the acquisition and diffusion of a given change will coexist with the older form within the speech community, perhaps for many generations, such as in the case of a dialect of an elderly or immigrant community coexisting with their children’s and grandchildren’s dialects. In other words, it may take generations for a single change to become diffuse throughout an entire speech community (and thus to be reflected in every text coming from that community). Indeed, this precise pattern has been observed many times over. As Wolfram and Schilling-Estes point out:

Speakers do not suddenly adopt a new form as a categorical replacement for an older form, whether the form involves a gradual imperceptible change in the phonetic value of a vowel within a continuum of phonetic space or an abrupt, readily perceptible change involving the metathesis of consonants or the linear realignment of constituents within a syntactic phrase. Instead, there is a period of variation and coexistence between new and old forms in the process of change. This transitional period of fluctuation has often been ignored in historical linguistics under the assumption that language change cannot be directly observed. (Wolfram and Schilling-Estes 2003: 715–16)

Second, the historical linguist Charles Bailey observed that the diffusion of changes over time follows a *Sigmoid*, or “S”-shaped, curve:

A given change begins quite gradually; after reaching a certain point (say, twenty per cent), it picks up momentum and proceeds at a much faster rate; and finally tails off slowly before reaching completion. The result is an S-curve: the statistical differences among isolects in the middle relative times of the change will be greater than the statistical differences among the early and late isolects. (Bailey 1973: 77; see also Kroch 1989; Pintzuk 2003)

An idealized example of such an S-curve is provided in fig. 2.

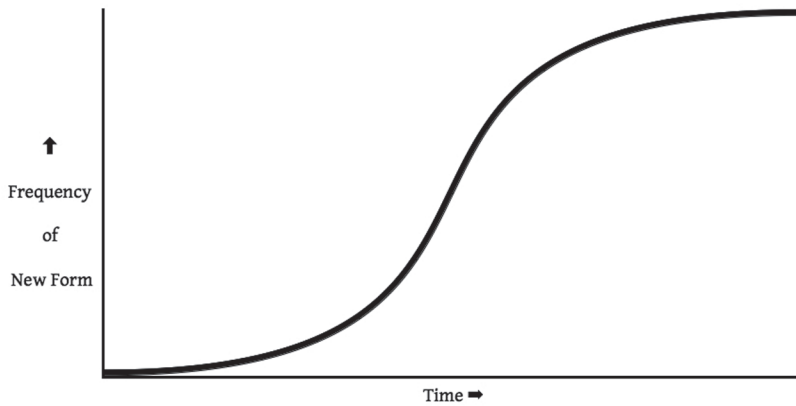


Figure 2. The S-curve of diffusion.

Although the S-curve has not been invoked in ancient Hebrew studies, it is quite likely that, if the features commonly cited were plotted along the dimensions of time and frequency, an S-curve such as the one above would emerge, and the relative order of books would match the general order achieved in more traditional analyses. Thus, the common refrain from the challengers that “the existence of a supposed late feature in a supposedly earlier text invalidates the entire approach” falls on its face for lack of linguistic awareness. Again, it is simply a fact that old and new forms do coexist, often for hundreds of years.⁴ What is needed is a reevaluation of all previous results in which the features are each plotted separately and the results are overlaid, increasing with each additional layer the statistical probability of the accuracy of the synthesis.⁵

Now, to return to the claim at the outset of this section: the reorientation required concerns the traditional categories of “Archaic BH,” “Standard/Classical/ Early BH,” and “Late BH.” The change-and-diffusion framework, with the S-curve describing the temporal path of diffusion, suggests that any categories such as “Standard” or “Late” are at best conveniences and at worst

4. A commonly cited syntactic example is the development of ‘do’-support in Middle English, in which ‘do’ appears as an auxiliary verb (or better, as the finite verb carrying the bundle of inflectional features) in questions (‘do you want?’), clauses with an initial adverb (‘rarely did they want’), and other restricted environments. This development began in a restricted environment and then spread to other contexts. Moreover, non-‘do’-support clauses coexisted with the newer construction for over 300 years, until finally being replaced entirely by the ‘do’-support construction (see Lightfoot 1979; Kroch 1989; 2001; and Dresher in this volume).

5. DeCaen makes a similar argument about the use of statistics on historical Hebrew analysis: “One form or one contrast yields precious little, but *all possible variants* statistically correlated should yield much” (DeCaen 2001: 23).

inaccurate and misleading. These labels represent generalizations that obscure the presence of numerous cases of change-and-diffusion within the corpus that the label supposedly covers. Changes, and thus the dialects within which they occur, can only be described as “earlier” or “later” relative to each other.

Considering two categories of language change, with examples, will help to clarify the diachronic discussion: *exogenous*, or externally-motivated changes, and *endogenous*, or internally-motivated changes. Externally-motivated changes are due to language contact, such as borrowing or interference due to acquisition of a second language (see Thomason 2003). Internally-motivated changes are due to the nature of language acquisition itself: the “perfect” transmission of one parent’s grammar to a child is precluded by the reality of the output-input situation.

2.2.1. Exogenous Change

The most commonly studied type of exogenous change, at least for ancient languages, is borrowing. This is certainly true for Hebrew studies: there are numerous works devoted to the topic and any biblical commentary worth its salt discusses the issue at some point. But, while identifying borrowed words in biblical books and discussing the possible implications for the temporal, geographic, or social origin of the book in question is almost commonplace, the types of borrowing and its motivation are rarely discussed. Thus, it is worth considering the nature of linguistic borrowing and the closely related phenomenon of code-switching.

Languages borrow words from other languages for two primary reasons: need and prestige (Campbell 2004: 64–65).⁶ The word ‘coffee’ is a good modern example of need-based borrowing: European languages borrowed the word from Arabic through Turkish.⁷ A likely example of need-based borrow-

6. A third category of borrowing that relates to Hebrew is loan translations or “calques.” In contrast to borrowed words, for which something of the borrowed item’s phonetic shape and meaning continue into the borrowing language, loan translations use native words to translate a borrowed concept. Thus, Modern English ‘gospel’ is derived from Old English *gód spel* ‘good tidings’, which was a translation, through Latin *evangelium*, of the Greek *εὐαγγέλιον* ‘good tidings’. A possible Hebrew example is the use of **בְּצֵל** in Qoh 7:12 (see also 6:12, 8:13; see Wise 1990 for the full argument). In this verse, instead of the normal Hebrew meaning ‘in the shadow (of)’, **בְּצֵל** may be a loan translation of the cognate Aramaic **בטלל**, which, unlike the Hebrew phrase, went through a series of semantic shifts: ‘in the shadow (of)’ > ‘with the help (of)’ > ‘because (of)’. Thus, in Qoheleth, the Hebrew phrase **בְּצֵל** has neither the normal denotation of ‘in the shadow (of)’ nor the metaphorical meaning ‘in the protection (of)’ but is a loan translation of what the cognate Aramaic phrase had become, ‘because’.

7. The etymological entry in the *Oxford English Dictionary* explains its background and notes its diffusion as a borrowed word:

Arab. *qahwah*, in Turkish pronounced *kahveh*, the name of the infusion or beverage; said by Arab lexicographers to have originally meant ‘wine’ or some kind of

ing in Hebrew is the word קֹהֵל ‘monkey’. Since monkeys were not native to ancient Israel, it is understandable that Hebrew had no native word for the animal. However, when the need arose to mention this particular animal (1 Kgs 10:22 // 2 Chr 9:21), the word was borrowed into Hebrew, perhaps via Egyptian (although its origin may be Sanskrit *kapi*; see *HALOT*, s.v.). Few examples of need-based borrowing figure into the reconstruction of Hebrew diachrony.⁸

It is prestige-based borrowing that figures prominently in ancient Hebrew studies. Prestige-based borrowing reflects a sociolinguistic situation in which a foreign language, whether closely related or not, is associated with higher social or political status or is simply a dominant linguistic cultural influence (for example, a lingua franca). For example, during the Norman French dominance in England (1066–1300), many French words were borrowed into English (for example, ‘pork’ > Fr. *porc*) even though English already had serviceable terms (for example, *pig meat*; Campbell 2004: 64). The reason was that at that time French was considered more prestigious than English.

In Hebrew, prestige borrowing is often invoked to explain the increasing number of Aramaisms (for example, זְמַן ‘time’) as well as the few Persianisms (for example, סַרְדֵּן ‘garden’, ‘royal enclosure’) found in some biblical texts. The prestige status for Aramaic came from its role as the administrative language of both the Neo-Babylonian and Persian empires; for Persian, the prestige status no doubt derived from the political dominance of the Persians from the sixth to fourth centuries B.C.E.

Whether words are borrowed due to need or prestige, it is important to recognize that the borrowed item is normally adapted and accommodated to the borrowing language’s phonology and morphology (see Campbell 2004: 65–69). For instance, although the Hebrew הַשֵּׁץ ‘conjurer’ entered either via Aramaic הַשֵּׁץ or Akkadian (*w*)*āšīpu*, the Hebrew word is the only version of the word that reflects gemination of the root’s middle consonant, which is

wine, and to be a derivative of a vb.-root *qahiya* ‘to have no appetite.’ Some have conjectured that it is a foreign, perh. African, word disguised, and have thought it connected with the name of *Kaffa* in the south Abyssinian highlands, where the plant appears to be native. But of this there is no evidence, and the name *qahwah* is not given to the berry or plant, which is called *bunn*, the native name in Shoa being *būn*.

The European langs. generally appear to have got the name from Turkish *kahveh*, about 1600, perh. through It. *caffè*; cf. F., Sp., Pg. *café*, Ger. *kafee*, Da., Sw. *kaffe*. The Eng. *coffee*, Du. *koffie*, earlier Ger. *coffee*, *koffee*, Russ. *kophe*, *kophei*, have *o*, app. representing earlier *au* from *ahw* or *ahv*.

8. Note that personal names are a special case since they do not necessarily have a continued presence in the language. In fact, while they are often adapted to the phonology and morphology of the recipient language, personal names used of specific individuals within a text (for example, אֲרִיִּין in Gen 14:1, 9; contra Young and Rezetko 2008: 1.307–8) are arguably not cases of borrowing because they may simply match the individual’s foreign-origin name. This is in contrast to, say, a personal name that becomes a common name within the speech community, such as בְּרִיָּה, the name of Moses’ sister.

likely because the word was imported as a *qattāl*-pattern noun, the nominal morphological category used for ‘nouns of profession’ (Joüon and Muraoka 2006: §88Ha and n. 49; also §87d).

The problem that adaptation and accommodation raise is the problem of identification: most cases of discernible borrowings in Hebrew come from other Semitic languages, which share a similar phoneme inventory and the triradical root morphology. For all practical purposes, then, we should begin by limiting ourselves to words the shape of which falls outside the paradigmatic margins. As Campbell suggests, “Words which violate the typical phonological patterns (canonical forms, morpheme structure, syllable structure, phonotactics) of a language are likely to be loans” (Campbell 2004: 70). An example of a borrowed word that has been phonologically adapted but must have been borrowed is **כְּתָב**. The vocalization tradition for the word indicates that it was not affected by the Canaanite shift (ca. 1400 B.C.E.) in which $\bar{a} > \bar{o}$. If it had been affected, it would be vocalized like **חִמּוֹר** (< **himār*).⁹ Moreover, if the word reflected the Hebrew pattern *qatal*—that is, if it were not an Aramaism and the final syllable did not reflect the vowel \bar{a} —it would be vocalized like **דָּבָר** (< **dabar*), that is, without the reduction of the vowel in the first syllable to *šēwa*. Since **כְּתָב** does not fit the phonological and morphophonological patterns of Hebrew, it not only must have been borrowed but it must have been borrowed after the Canaanite shift.

One example of a word that exhibits morphological adaptation is the adjective **אַחַשְׁתָּרְנַיִם** ‘royal’ (Esth 8:10, 14). This word clearly does not have a triradical root and is considered a Persian borrowing, yet having been fitted into Hebrew inflectional noun morphology, it bears the normal Hebrew masculine-plural suffix **ים**.

A source of exogenous change related to borrowing is code-switching. Code-switching is “the use of more than one language in the course a single communicative episode” (Gordon and Williams 1998: 75–76; see also Thomason 2003: 695–99). Examples of code-switching in the Hebrew Bible range from easy-to-identify phrases (**יְגֵר שְׁהֵרִיתָא** in Gen 31:47) or verses (Jer 10:11) to multiple chapters (Ezra 4:8–6:18; Dan 2:4b–7:28). It is also possible that more-nuanced examples of code-switching were used for rhetorical effect (Rendsburg 1992; Greenstein 2003: 653–57).

9. The accuracy of the Masoretic vocalization vis-à-vis ancient Hebrew phonology is a complicated issue. For numerous features, it is clear that the Masoretes received and transmitted an old vocalic tradition that reflects a phonology quite different from the phonology represented by texts closer to the Masoretic period (such as Amoraic texts, rabbinic commentaries). At the same time, small but grammatically significant details could easily have been changed, whether by intention or not. An example would be the articular /a/ vowel under many prefixed prepositions: it is not clear in many cases that the assimilated definite article was a native feature of the “biblical” text (see Barr 1989: 325–33).

In fact, the line between borrowing and code-switching is fuzzy. Both refer to linguistic features that are not native to the primary language being used. The motivation for using both in literature relates to rhetorical effects—from simple aesthetics to a signaling of political or social agendas. Borrowing typically refers to single lexical items or grammatical features, whereas code-switching may refer to entire phrases, sentences, or longer stretches of discourse. Both borrowed and code-switched features, however, may actualize the change-and-diffusion process; that is, both may cease to be overtly “foreign” and may be acquired as a native part of the recipient grammar and lexicon (Thomason 2003: 696).

Critically, borrowing and code-switching presume a necessary level of “intensity of contact.” That is, if non-native items are identified and they are not need-based lexical items, they must reflect intense contact between the language in question and the other language that is the source of the feature. “Among the factors that contribute to greater intensity of contact are a high level of bilingualism, socioeconomic and/or political pressure on one speaker group in a two-language contact situation to shift to the other language, length of contact, and relative sizes of the speaker population” (Thomason 2003: 689). Notable among this list is bilingualism; unless an author used foreign words as a barrier, as James Joyce did, the use of prestige-based borrowing or code-switching presumes that both the author *and the audience* understood such elements. For Hebrew texts from ancient Israel, this raises an important issue: unless some texts were aimed only at the highly educated, the increasing presence of Aramaic words or the use of any Persian words presupposes a multilingual environment for the general Judean audience.¹⁰ General contact, political or economic, would not necessarily create a sufficient level of intensity of contact for prestige-based borrowing. Thus, it is extremely unlikely, as has been suggested in Young and Rezetko 2008 (1.296–98), that the presence of Persians serving in the army of Ashurbanipal in the mid-seventh century B.C.E. or in the army of Tyre in the early sixth century B.C.E. would have provided enough contact between Hebrew and Persian before the Achaemenid period to allow for Persian borrowings in early texts.

In summary, when we are discussing Aramaic or Persian features in Hebrew texts, we must ask a number of questions to sort carefully through the issues. It is not simply a matter of saying either “this word is borrowed from Aramaic and thus the text is late” or “there is an Aramaic word in an early text and so Aramaic borrowings cannot be used for dating” (see Young and Rezetko 2008: vol. 1, chaps. 8, 11). Minimally, we should ask these six questions:

10. If the audience were not actually multilingual, they would at least have had to be passively familiar with the source languages of the borrowed items (see Thomason 2003: 699 on passive familiarity as a mechanism of “interference”).

1. Is the feature clearly at odds with the paradigmatic grammar of the recipient language? See, for example, שְׁנֵהָבִים ‘ivory’ (1 Kgs 10:22//2 Chr 9:21)
2. Is there a clear need for the feature? See, for example, קֶפֶים ‘monkeys’ (1 Kgs 10:22//2 Chr 9:21)
- 3a. If there is no clear need, can the feature be set within an appropriate “intensity of contact” situation as a prestige item? See, for example, כְּתָב in Ezek 13:9 (in the context of Aramaic, either as an emerging lingua franca or as the dominant contact language in the exile, where Ezekiel the prophet was situated).
- 3b. If there is no clear need, can the feature be set within an appropriate “intensity of contact” situation so that it can be *interpreted* as a literary device? See, for example, שֶׁ in Jonah 1:7, 12; 4:10 (see below, §3 and n. 35).
4. If there is no apparent “prestige” role, is the frequency of usage high enough and the intensity of contact strong enough that the feature reflects change and diffusion (that is, “normalization” into the recipient grammar)? See, for example, כְּתָב (Esth 1:22; 3:12, 14; 4:8; 8:8, 9, 13; 9:27; Dan 10:21; Ezra 2:62; 4:7; Neh 7:64; 1 Chr 28:19; 2 Chr 2:10; 35:4).
5. If there is a contrast with an older item, is there a noticeable semantic shift in the older item? For כְּתָב and סֵפֶר, this appears to be so: סֵפֶר shifts from a general lexeme for a product of writing to a word that is specifically associated with book titles (for example, “the Book of Moses” [Neh 13:1], “the Book of Meditation” [CD 13:2]) or holy scrolls; whereas, כְּתָב takes over as the general entry for the product of writing.¹¹
6. If there is eventual replacement, how does the frequency of the new feature plot on an idealized S-curve? (This does not apply to כְּתָב and סֵפֶר since the borrowing of כְּתָב does not affect the replacement of סֵפֶר but reflects its semantic shift.)

Finally, we must recognize that borrowing and code-switching can be avoided for reasons similar to those that often motivate their use. On the level of text, the use or avoidance of features perceived to be non-native may serve as boundary-leveling or boundary-maintaining strategies—that is, may serve to create “in groups” and “out groups” (Gordon and Williams 1998: 80–81). On the level of speech community, intensity of contact may be resisted, and borrowing and code-switching may be avoided for religious, ethnic, or nationalistic concerns (see Thomason 2003: 689). For example, the lack of Aramaisms in a text such as Haggai may reflect an audience-centered rhetorical stance to focus the audience on restoring the “Hebrew” institutions and life in Jerusalem. The lack of borrowing during a period of sufficient intensity of contact does not necessarily mean that the text must come from a period before or after the period of contact (so also Young and Rezetko 2008: 1.218, 293–95). And yet the ability of authors to avoid foreign elements does not invalidate the use of carefully identified borrowings in dating their texts relative to other

11. Hurvitz has argued repeatedly that סֵפֶר is replaced by אִגְרָת (see, for example, Hurvitz 1997); he does not discuss כְּתָב.

Table 1. Example of an Internally-Motivated Change

	Non-III-ה Root	III-ה Root
<i>Input</i>	ואכתב	ואבכה
<i>Rule 1: Apocopation</i>	—	ואבך
<i>Rule 2: Suffixation (Reanalysis)</i>	ואכתבה	ואבכה

texts that do use borrowed items (other features will determine the place in the relative chronology of the texts that lack borrowings).

2.2.2. Endogenous Change

Hale describes this output-input situation by using the fact that his grammar differs from his mother's grammar:

[I]n the case of language change, we must confront the fact that there is, in a very real sense, a different object (a different grammar) with each new generation. The grammar of my mother did not change into my grammar: I engaged in an ultimately successful process of grammar construction using her output (in part) as the basis for the construction of my grammar. . . . It is inevitable, in my view, that this process will give rise to a grammar which differs in some ways from the grammar which my mother constructed on the basis of her analysis of the input she received during her own early life. Quite aside from the "messiness" of output (which forms a potentially nondeterministic basis for my grammar construction), it seems unlikely that she will have received the evidence for her grammar in the same order as I did, or that she would have been exposed to various features of the grammar with the same frequency that I was. (Hale 2007: 33–34)

Hale identifies three factors that influence the shape of the constructed grammar: noise in the channel (that is, disruption introduced by the speaker's body, the receiver's body, or the environment), the order in which the grammatical data are presented, and the frequency (and thus salience) of various constructions. He concludes that, "since 'perfect' grammar transmission is precluded by these factors, it is inevitable that my grammar will differ from that of my input sources. Such differences are change" (Hale 2007: 34).

For Biblical Hebrew, the apocopation and then suffixation of a final ה in first-person *wayyiqtol* forms is a possible example of an internally motivated change, as argued in DeCaen 2001. DeCaen observed that the problem of the presence or absence of the final ה on the first-person *wayyiqtol* forms could be accounted for by two morphophonological rules, *applied in order*. He provided the initial diagram, presented here in table 1, to illustrate the ordering (modified slightly from DeCaen 2001: 15, §8.3): DeCaen's proposal is that apocopation was first applied to the first-person *wayyiqtol* forms, a process that

Table 2. Final ה and the First-Person Wayyiqtol

<i>Text</i>	אבכה ^a	אבך ^b	% New	אכתב ^c	אכתבה ^d	% New
Kings	2	0	0	17	0	0
Amos	1	0	0	7	0	0
Chr	3	0	0	7	0	0
Isa	1	1	50.0	13	0	0
Hos	1	1	50.0	1	0	0
Deut	2	11	84.6	41	0	0
Exod	0	2	100.0	8	0	0
Num	0	2	100.0	5	1	16.7
Gen	1	3	75.0	39	3	7.1
Judg	1	1	50.0	9	5	35.7
Josh	3	2	40.0	19	1	5.0
Jer	6	4	40	51	2	3.8
Job	1	1	50.0	9	6	4.0
Ezek	11	11	50.0	64	3	4.5
Zech	4	7	63.6	29	1	3.3
Neh	5	6	54.5	35	31	50.0
Ps	3	4	57.1	9	12	57.1
Dan	6	1	14.3	8	10	55.6
Sam	7	0	0	17	7	29.2
Ezra	2	0	0	4	18	81.8
Qoh	2	0	0	0	1	100.0

a. Gen 24:48; Deut 1:16, 18; Josh 7:21 (*Kethiv*); 9:24; 24:3 (*Qere*); Judg 12:3; 1 Sam 10:14; 13:12; 26:21; 2 Sam 7:9; 11:23; 12:22; 22:24; 1 Kgs 8:20; 11:39; Isa 6:1; Jer 13:2; 25:17; 31:26; 32:9, 13; 44:17; Ezek 1:1, 28; 2:9; 8:2, 7, 10; 10:1, 9; 11:1; 16:8; 20:14; Hos 11:4; Amos 4:10; Zech 5:1; 6:1; 11:7 (2x); Ps 69:11, 21; 102:8; Job 7:20; Prov 8:30 (2x); 24:32; Qoh 4:1, 7; Dan 8:2 (2x), 3, 27; 9:4; 10:8; Ezra 8:15, 17 (*Qere*); Neh 1:4; 3:38; 7:2; 12:31; 13:25; 1 Chr 17:5, 8; 2 Chr 6:10. Of 9 total III-ה wayyiqtol forms in the Qumran texts, 8 have the final ה (see 1QHa 10:10, 12, 16, 17; 11:8; 14:27; 16:28; 4Q364 frg. 24a–c:15).

b. Gen 24:46; 31:10; 41:22; Exod 6:3; 9:15; Num 13:33; 23:4; Deut 2:1, 8, 33; 3:1 (2x), 18; 9:15, 16; 10:3 (2x), 5; Josh 7:21 (*Qere*); 24:3 (*Kethiv*); Judg 18:4; Isa 64:5; Jer 3:8; 15:6; 20:7; 35:10; Ezek 1:4, 15, 27; 11:16; 12:7; 20:9, 22; 23:13; 24:18; 43:8; 44:4; Hos 13:7; Zech 2:1, 5; 4:4, 11, 12; 5:9; 6:4; Ps 18:24; 38:15; 69:12; 73:14; Job 30:9; Prov 7:7; Dan 10:5; Neh 1:4; 2:11, 13, 15 (2x); 4:8. Of 9 total III-ה wayyiqtol forms in the Qumran texts, only 1 does not have the final ה: see 4Q364 frg. 26b i:6.

c. There are 406 occurrences in the Hebrew Bible: Gen 3:10 (2x), 12, 13; 12:19; 16:5; 20:6, 13; 24:39, 42, 45, 47 (2x), 48 (2x); 26:28; 27:33; 31:10, 11; 32:5; 39:14, 15, 18; 40:11 (3x); 41:12, 21, 24; 42:31; 43:7, 21; 44:20, 22, 24, 26, 28; Exod 3:8, 17; 4:23; 6:5; 19:4 (2x); 31:3; 32:24; Lev

7:34; 17:14; 18:25; 20:23, 24, 26; 26:13 (2x); Num 8:18; 20:15, 16; 21:30; 31:50; Deut 1:9, 15 (2x), 19 (2x), 20, 23, 29, 43; 2:1 (2x), 8 (2x), 13, 26, 34 (2x); 3:4, 6, 8, 23, 29; 9:9, 15, 17, 18, 20, 21 (2x), 25, 26 (2x); 10:3, 5 (2x); 22:14; 26:7; 29:4, 7; Josh 2:11; 7:7; 9:24; 14:7; 22:26, 28; 24:3 (2x), 4, 5 (2x), 6, 8, 10, 11, 12, 13; Judg 2:1; 6:8, 9 (2x); 10:10; 12:2; 19:18; 20:6; 1 Sam 1:15; 10:14, 18; 12:1, 10; 13:12 (2x); 15:20, 24; 24:11; 25:35; 28:21 (2x); 2 Sam 1:7, 10; 1 Kgs 2:8, 42; 3:17, 21 (2x); 8:16, 20 (2x), 21; 14:8; 18:13; 19:10, 14; 21:6; 2 Kgs 6:29 (2x); 10:13; Isa 6:5, 8 (2x), 11; 8:3; 24:16; 41:9; 45:4; 48:5; 50:7; 51:16; 64:4, 5; Jer 1:6, 11, 13; 2:7; 3:7, 8, 19; 4:10; 5:7; 7:13 (2x), 25; 8:6, 13; 11:5, 8; 13:2, 5, 7 (3x); 14:13; 15:17; 18:3; 23:38; 24:3; 25:3, 17; 31:18; 32:8, 10 (4x), 11, 12, 16; 35:3, 4, 5, 8, 10 (2x), 11 (2x), 15, 17; 42:21; 44:4, 17; Ezek 1:24, 28 (2x); 2:2; 3:2, 12, 14, 15, 23 (2x); 4:14; 8:5, 8, 10; 9:8 (2x); 10:20; 11:13 (3x), 25; 16:6 (3x), 8 (4x), 9, 12, 27, 50; 20:5 (3x), 7, 8, 11, 13, 18, 21, 26, 29; 21:5; 22:26, 30, 31; 23:43; 24:18, 20; 28:18; 31:15 (2x); 36:18, 19, 21; 37:3; 39:23–24; 43:3, 6; 44:4; Hos 3:3; Amos 2:9, 10, 11; 7:2, 5, 8; 8:2; Mic 3:1; 6:4; Hag 1:11; Zech 1:9; 2:1, 2, 4, 5, 6; 3:5; 4:4, 5, 11, 12, 13; 5:1 (2x), 2, 6, 9, 10; 6:1 (2x), 4; 11:7, 10 (2x), 12, 13, 14; Mal 1:2, 3; Ps 18:24; 20:9; 44:21; 55:7; 73:13; 77:11; 95:10; 119:52; 139:11; Job 5:3; 19:10; 29:18; 30:19; 31:34; 32:6; 38:10 (2x), 11; Lam 3:18; Dan 8:3, 16, 27 (2x); 10:5, 9, 16; 12:7; Ezra 8:21, 32 (2x); 10:2; Neh 1:5; 2:1, 2, 3, 4, 5, 7, 9, 11, 12, 14, 15 (3x), 17, 18, 20 (2x); 4:3 (2x), 7 (2x), 8 (2x), 9, 13; 5:7, 12; 6:4; 7:1, 5 (2x); 13:7, 15, 25; 1 Chr 17:8, 10; 2 Chr 6:6 (2x), 10 (2x), 11. There are only 5 (out of 28 total) examples without the ה in the Qumran texts (see 4Q364 frg. 23a–b i:6, 14; frg. 24a–c:8; frg. 26b i:8; 4Q386 frg. 1 ii:2; 4Q389 frg. 2:2).

d. There are 101 occurrences in the Hebrew Bible: Gen 32:6; 41:11; 43:21; Num 8:19; Josh 24:8; Judg 6:9, 10; 10:12; 12:3 (2x); 1 Sam 2:28; 28:15; 2 Sam 4:10; 7:9; 12:8 (2x); 22:24; Jer 11:18; 32:9; Ezek 3:3; 9:8; 16:11; Zech 11:13; Ps 3:6; 7:5; 69:12, 21; 73:16; 90:10; 119:55, 59, 106, 131, 147, 158; Job 1:15, 16, 17, 19; 19:20; 29:17; Qoh 1:17; Dan 8:13, 15, 17; 9:3, 4 (2x); 10:16 (2x), 19; 12:8; Ezra 7:28; 8:15, 16, 17 (2x, *Kethiv*), 23 (2x), 24, 25 (2x), 26, 28, 31; 9:3 (2x), 5 (2x), 6; Neh 1:4; 2:1, 6, 9, 13; 5:7 (2x), 8, 13; 6:3, 8, 11, 12; 7:5; 12:31; 13:7, 8, 9 (2x), 10, 11 (2x), 13, 17 (2x), 19 (2x), 21 (2x), 22, 30. Of 28 first-person *wayyiqtol* forms in the Qumran texts, 23 have the ה (see 1QH^a 12:37; 14:9, 10; 15:23; 17:9, 10; 1Q49 frg. 1:1; 4Q216 5:10; 4Q385 frg. 2:9; 4Q385a frg. 1a–b ii:1, 6, 7; frg. 15 i:5; 4Q387 frg. 1:7; 4Q387a frg. 4:2; 4Q389 frg. 2:4, 5; frg. 6:1; 4Q390 frg. 1:6 (2x); 4Q437 frg. 2 ii:13; 4Q504 frgs. 1–2 rev:17; 11Q19 65:8).

only affects III-ה verbs, producing the “short” form without the ה at the end.¹² Following this, at some point the second rule applied (and became diffuse), so that the modal (or “paragogic”) ה suffix was reanalyzed as non-modal and applied to all first-person *wayyiqtol* forms. The conclusion that DeCaen draws is that, “. . . the diachronic distribution of the final *h* must show an apparent regression to norm [that is, return to the earlier form; RDH]. First, we should see the *h* of glide-final roots disappearing; but then we should see that same final *h* reappearing (hence regression)” (DeCaen 2001: 16, §8.5). Although DeCaen does not provide the raw numbers, I have collected and sorted the data and provide the statistics in table 2:

12. Although the ה on III-ה verbs is vocalized as /e/ and the modal (or paragogic) ה on non-III-ה verbs is vocalized as /a/ in the Masoretic tradition, DeCaen asserts that this distinction must be ignored as inaccurate. Presumably, DeCaen would suggest that at some point all III-ה verbs with a final ה were leveled within the tradition that the Masoretes received. For more on the Masoretic vocalization, see n. 9 above.

Since the data in table 2 represent two replacement changes with partial overlap of the process of diffusion, they are difficult to represent with a single S-curve. However, two items stand out from the tabulation: first, even without dividing many of the books into the typically accepted components (for example, First Isaiah, Second Isaiah, the prose Job, the poetic Job, individual psalms), the general order accords well with the typical relative chronological order in mainstream Hebrew studies; second, the positions of Chronicles and Samuel, which are precisely opposite expectations, require further investigation. However, the Samuel-Chronicles issue does not invalidate the results that I have presented here; rather, it simply suggests that we are not yet seeing the whole picture.

2.3. Style

To close the discussion on theory and methodology, we must briefly discuss the issue of “style.” At the very heart of the alternative model proposed in Young and Rezetko 2008 is the notion that two contemporaneous “styles” provide a better account for the linguistic variation in the Hebrew Bible than diachrony. They compare the use of two “dialects” of Achaemenid-period Aramaic by the Jewish population in Egypt: a few letters written to Persian officials use the “Eastern Official” dialect while the remaining documents from the Elephantine Jewish community use the “Western” dialect (Young and Rezetko 2008: 1.294–95). The conclusion drawn by Young and Rezetko is:

The important factor was the perceived audience or purpose of the document. Thus, what we may have is a conscious attempt to distance this style of literature from literature produced in the EBH style. Rather than geographical or chronological distance, we would have intellectual or ideological distance. (Young and Rezetko 2008: 2.99)

In this proposal that carries so many implications for our study of Hebrew diachrony and biblical texts, there is a disturbing lacuna: the line between “style” and “dialect” is left unacceptably fuzzy. Are EBH and LBH *different registers*, that is, variations of the same grammar; or *different dialects*, that is, different formal grammars, so that the community was bilingual? The difference between these two options carries implications for the way we explain an author’s choice to use one versus the other. Young and Rezetko do not clearly distinguish between register and dialect.

If EBH and LBH were simply two registers—say, a formal register and an informal—the conscious use of both in writing by the same community at the same time should be contextualized. To make this a plausible scenario, it would be necessary to reconstruct the recurrent situations in which each register would have been used. To mention only “perceived audience or purpose” is tantamount to admitting the inability to explain an author’s choice to use EBH or LBH for a given text.

Appealing to an author's "conscious attempt to distance" the linguistic style of a given text from another known style raises critical methodological issues. How can this sort of claim be supported? In his study of stylistics, the linguist David Crystal asserts that, before "style" can be discussed, all regional, historical, and social dialectal factors must be eliminated. To understand an author's stylistic use of language, one must first know the variety of language used on its own terms (1970: 100–101; 1987: 201, 205–6). That is, to study deviation from the norm, one must first describe the norm. For Hebrew, this would minimally require a full-scale grammatical description of some biblical text in which a "norm" is posited, and identified divergences are characterized as "stylistic variation." The speculative nature of such an endeavor highlights the highly subjective and methodologically precarious use of "style" in explaining linguistic variation in Biblical Hebrew.

3. Variation in Ancient Hebrew as Language Change: A Case Study

To conclude this methodological discussion, I provide one full example of the way I think the preceding discussion should contribute to the careful historical-linguistic analysis of any given feature. The feature I consider is one of the best known in ancient Hebrew diachronic studies: the use of two relative terms, אֲשֶׁר and שֵׁ.

While there are about 5,500 אֲשֶׁר clauses in the Hebrew Bible, there are only 139 occurrences of שֵׁ. Of these, 68 are in Qoheleth and 32 are in Song of Songs.¹³ Twenty-one are in various psalms from Psalm 122 onward, and the remaining 18 are scattered in the Hebrew Bible, literally, from beginning to end.¹⁴

The distribution of אֲשֶׁר and שֵׁ in nonbiblical texts is somewhat similar. In epigraphic texts from the first millennium, there are 30 clear occurrences of אֲשֶׁר and none of שֵׁ.¹⁵ The Hebrew text of Ben Sira contains 29 cases of

13. Qoh 1:3, 7, 9 (4×), 10, 11 (2×), 14, 17; 2:7, 9, 11 (2×), 12, 13, 14, 15, 16, 17, 18 (3×), 19 (2×), 20, 21 (2×), 22, 24, 26; 3:13, 14, 15, 18 (šš), 22; 4:2, 10; 5:4, 14 (2×), 15 (2×), 17; 6:3, 10 (2×); 7:10, 14, 24; 8:7, 14 (2×), 17 (šel); 9:5, 12 (2×); 10:3, 5, 14, 16, 17; 11:3, 8; 12:3, 7, 9. Song 1:6 (3×), 7 (2×), 12; 2:7, 17; 3:1, 2, 3, 4 (4×), 5, 7, 11; 4:1, 2 (2×), 6; 5:2, 8, 9; 6:5 (2×), 6 (2×); 8:4, 8, 12.

14. Pss 122:3, 4; 123:2; 124:1, 2, 6; 129:6, 7; 133:2, 3; 135:2, 8, 10; 136:23; 137:8 (2×), 9; 144:15 (2×); 146:3, 5. Gen 6:3; Judg 5:7 (2×); 6:17; 7:12; 8:26; 2 Kgs 6:11; Jonah 1:7, 12; 4:10; Job 19:29; Lam 2:15, 16; 4:9; 5:18; Ezra 8:20; 1 Chr 5:20; 27:27.

15. Excluding questionable readings, reconstructions, and unprovenanced texts, there are 22 epigraphic occurrences of אֲשֶׁר and none of שֵׁ (see Dobbs-Allsopp et al. 2005 for the full texts): Arad 8.9; 18.6–8; 29.7; 40.4–5; 71.2; Kuntillet Ajrud 16.1; Lachish 2.5–6; 3.4–6, 10–12; 4.2–3, 3–4, 11–12, 9.4–9; 18.1; Lachish Bulla 6.2, Yavneh Yam/Mesad Ḥashavyahu 1.6–8, 8–9; Papyrus Murabba'at 17a 1.2; Naḥal Yishai 1.1; Samaria Basalt 1.1; Silwan 2.1, 2–3; 3.2. Additionally, there are 8 examples in unprovenanced texts, mostly seals, within private collections (again, see Dobbs-Allsopp et al. 2005 for the full texts): Moussaieff Ostraca 1.1, 2.4–6; Avigad-Hecht seal 1.1–2; Avigad, *Hebrew Bullae* 1, 2, 3. Finally, to this

Table 3. אִשֶׁר and שׁ in Ancient Hebrew ^a

<i>By Frequency</i>	אִשֶׁר (Old)	שׁ (New)	% New	<i>By Frequency</i>	אִשֶׁר (Old)	שׁ (New)	% New
Epigraphic Texts	38	0	0	Qumran Majority Sectarian	1864	0	0
Exodus	309	0	0	Kings	696	1	0.14
Leviticus	309	0	0	Genesis	411	1	0.24
Numbers	295	0	0	Chronicles ^b	345	2	0.58
Deuteronomy	584	0	0	Ezra–Nehemiah	120	1	0.83
Joshua	265	0	0	CD	132	2	1.50
Samuel	428	0	0	Job	40	1	2.40
Isaiah	171	0	0	Judges	177	5	2.70
Jeremiah	461	0	0	4Q266	67	2	2.90
Ezekiel	342	0	0	11Q20	12	1	7.70
Hosea	12	0	0	Qumran Set A (4Q385, 521, 522)	23	3	11.50
Joel	12	0	0	Jonah	12	3	20.00
Amos	18	0	0	Qumran Set B (4Q222, 11Q5)	6	2	25.00
Obadiah	4	0	0	Ben Sira	71	26	26.80
Micah	16	0	0	Lamentations ^c	9	4	30.80
Nahum	2	0	0	Qoheleth	89	68	43.30
Habakkuk	3	0	0	4Q448	1	1	50.00
Zephaniah	6	0	0	Psalms B (135, 144, 146)	6	7	53.80
Haggai	7	0	0	Song of Songs	1	32	97.00
Zechariah	44	0	0	4QMMT (394–99)	2	67	97.10
Malachi	13	0	0	Mishnah	69	11690	99.40
Psalms A (1–121, 125–28, 130–32, 134, 138–43, 145, 147–50)	96	0	0	Psalms C (122–24, 129, 133, 136–37)	0	14	100.00
Prov	12	0	0	3Q15	0	56	100.00
Ruth	42	0	0	Qumran Set C (4Q302, 322, 322a, 324, 332, 333, 4681)	0	7	100.00

Esther	99	0	0	Naḥal Hever (and Naḥal Şe‘elim)	0	71	100.00
Daniel	47	0	0	Wadi Murabba‘at	0	47	100.00

a. For this study, I divided the Psalms into three categories following the distribution of **ש** and **אשר**, primarily for convenience. Of course, each psalm should be assessed separately, as a distinct text. Moreover, that what I have done here with the Psalms is methodologically disallowed is made all too clear by my own student, Andrew Jones:

[I]n order for groups of texts to be useful in statistical analysis as a random sample, it is not permitted to organize the groups ahead of time to match the conclusions being sought by the research. Any groups of texts need to be formed for reasons independent of the distribution of the grammatical forms at hand, such as dating that is based on historical considerations. (Jones 2010: 5)

b. The frequency in the table is for all of Chronicles. However, if only nonparallel passages are considered, only 36 **אשר** are in same passages, resulting in a higher (5%) frequency of **ש**. For an argument against the validity of taking the nonparallel passages as indicative of the Chronicler’s language (versus that of the Samuel–Kings source), see Young and Rezetko 2008: 2.78–79.

c. On the unity of the five songs, see Dobbs-Allsopp 1998; 2003; and Longman 2008.

ש¹⁶ (and also 67 of **אשר**).¹⁷ In the Qumran nonbiblical texts, **ש** (including **של**) occurs 145 times, but 124 of these are in just 2 texts: 57 in the *Copper Scroll* [3Q15]¹⁸ and 67 in 4QMMT^{B,C} [4Q394–99]¹⁹; the remaining 21 examples are

latter group, we can add ostracon 2 (lines 6 and 9) in the unprovenanced texts published in Lemaire and Yardeni 2006.

16. Sir 3:22^A; 14:16^A, 18^A; 15:17^B; 16:3^{AB}, 15^A; 25:8^C; 26:17^C; 30:12^{B(2x)}, 19^B, 34^E, 36^E; 34:10^B, 15^B, 16^{B(2x)}, 20^B, 27^B; 37:3^B; 44:9^M; 51:30^B. This includes one conjectured reconstruction (30:19^A) and two occurrences of **של** (13:5^A, 30:28^E). Also, the following cases exhibit alternation between the manuscripts: 3:22 (A = **במה שהורשית**; B = **באשר שהורשיתה**); 15:17 (A = **אשר יחפץ**; B = **שיחפץ**); 34:15 (B = **ורככל ששנאת**; margin = **אשר ששנאת**); 44:9 (B = **יריש מהם אשר אין לו זכר**; M = **יריש מהם שאין לו זכר**). And finally, the following is the one case of the two relative words used in the same verse: 16:15^A: **ידעו שמעשיו מגולין תחת השמים**.

17. Sir 3:22^C; 6:37^A; 7:31^A; 8:9^A, 14^A; 10:9^A; 12:15^A; 13:2^A, 7^A; 15:11^{A(2x)B}, 16^{AB}, 17^A; 16:7^{AB}, 15^A; 18:32^C; 30:19^B, 20^B; 33:4^{B+margin}, 5^B; 34:15^{B[margin]}, 16^B; 36:31^{B(2x)C(2x)D(2x)}; 37:12^{B(2x)D}, 15^{BD}; 38:13^{B+margin}, 14^B, 15^B, 27^B; 40:11^B; 44:9^{B(3x)M}, 20^B; 45:23^B, 24^B; 46:1^B, 11^B; 47:13^B, 23^{B(2x)}; 48:1^B, 4^B, 11^B, 15^B; 49:10^B; 50:1^B, 2^B, 3^B, 24^B, 27^{B(2x)}; 51:8^B. This includes 4 conjectured reconstructions of the text: 30:19^B; 36:31^C; 37:12^D; see *The Book of Ben Sira* (Ben-Hayyim 1973: 99–100).

18. There are 32 occurrences of **ש**: 3Q15 1:1, 6; 2:1, 5, 7, 9, 10, 13; 3:8, 11; 4:1, 6, 9, 11; 5:5, 12; 6:14; 7:8; 8:1, 2, 14; 9:1, 14, 16, 17; 10:3, 5, 9; 11:7, 8, 14; 12:10. There are also 25 cases of **של**: 3Q15 1:9, 10, 13; 2:11; 3:2; 4:13; 5:6, 8; 6:1, 7, 8; 7:3, 8, 10, 14; 8:8, 10, 14; 9:14; 10:8, 15; 12:4, 6, 7, 8.

19. In the B manuscript, there are 43 occurrences of **ש**: 4Q394 frgs. 3–7 i:4, 5, 9, 12 (2x), 13, 14, 15, 19; frgs. 3–7 ii:14, 16; frg. 8 iv:2, 3, 5 (2x), 8, 11; 4Q395 frg. 1:6; 4Q396 frgs. 1–2 i:3, 5; frgs. 1–2 ii:1, 5, 7 (2x), 10; frgs. 1–2 iii:1, 6 (2x), 10; frgs. 1–2 iv:2 (2x), 5, 6, 7, 8, 9; 4Q397 frgs. 1–2:4; frgs. 6–13:11 (2x), 12, 13 (2x), 14. There are 3 cases of **של**:

so spread out that no one text uses Ψ more than twice.²⁰ The Bar Kokhba texts from Naḥal Ḥever and Wadi Murabbaʿat contain 118 occurrences of Ψ and none of $\Psi\text{א}$.²¹ Finally, Ψ dominates in the Mishnah, where $\Psi\text{א}$ is used only 69 times, all in biblical quotations or allusions.²² The raw numbers of $\Psi\text{א}$ and Ψ by increasing frequency (that is, the ratio of new forms over the sum of old forms and new forms) of Ψ are presented in table 3.

What kind of explanation can we provide concerning these data and their distribution? Accounting for the variation between $\Psi\text{א}$ and Ψ typically weaves diachrony, dialect, and stylistics together. Since Gotthelf Bergsträsser's (1909) "Das hebräische Präfix Ψ ," it has been the scholarly consensus to trace the etymology of Hebrew Ψ from the Akkadian relative $\check{s}a$.²³ It has since become generally accepted that the route between Akkadian $\check{s}a$ and what we find in the Hebrew Bible goes through northern Canaanite (for example, Phoenician) and then northern Hebrew. The northern connection has been suggested in particular to account for the appearance of Ψ in Judges 5–8 (5:7; 6:17; 7:12; 8:26), as well as the single occurrence in 2 Kgs 6:11.²⁴ By combining the northern origin view with diachrony, the following reconstruction is common: Ψ became the relative word of choice (perhaps originally by borrowing from Phoenician), by change and diffusion, within some Hebrew grammar in the north (which presumably also already had $\Psi\text{א}$), from which it influenced some southern

4Q394 frgs. 3–7 i:15, 19; 4Q395 frg. 1:10. In the C manuscript, there are 19 occurrences of Ψ : 4Q397 frgs. 14–21:2, 10, 12, 15, 16; frg. 22:2; frg. 23:2; 4Q398 frgs. 11–13:3, 4 (2×), 6, 7; frgs. 14–17 i:5; frgs. 14–17 ii:1, 3 (2×), 4, 6; 4Q399 frg. 1 i:11. There are two cases of Ψ $\bar{\Psi}$: 4Q397 frg. 23:2; 4Q398 frgs. 14–17 ii:6.

20. There is 1 case of Ψ (4Q385 frg. 6:9) and 20 occurrences of Ψ : CD 15:11; 20:4; 4Q222 frg. 1:7; 4Q266 frg. 10 i:1; frg. 10 ii:2; 4Q302 frg. 8:3; 4Q322 frg. 1:3; 4Q322a frg. 1:9; 4Q324 frg. 1:6; 4Q332 frg. 2:3; 4Q333 frg. 1:3; 4Q448 3:5; 4Q4681 frg. 1:2; 4Q521 frg. 2 ii + 4:11; 4Q522 frg. 9 i + 10:10; 11Q5 28:13; 11Q20 12:14; KhQ 3 1:1 (2×), 4.

21. From Naḥal Ḥever, there are 51 occurrences of Ψ in the texts collected in Yadin et al. 2002 (44.5, 6, 8, 9, 10, 11, 12 (2×), 13 (2×), 14, 15, 16, 18, 20, 23, 25, 26; 45.7, 8, 10, 15, 16, 18 (2×), 19; 46.3 (3×), 4, 5 (3×), 7 (3×), 9, 11; 49.5, 6, 7 (3×), 8 (3×), 11, 13; 51.5; 61.4) and 12 Ψ (44.7, 10, 11; 45.7, 12, 20, 21; 46.7 (2×), 8; 51.6) and, from the Naḥal Ṣeʿelim (Cotton and Yardeni 1997) we may add 7 Ψ (8.9 [2×], 8e–k.9, 30.7, 49.8 [2×], 12) and a single occurrence of Ψ (8.9). From Wadi Murabbaʿat (Yardeni 2000), there are 39 occurrences of Ψ (22.5–6; 22h.1; 24b.4, 11, 12, 15; 24c.4, 7, 9; 24d.4; 24e.3, 6, 9; 30.3, 15, 19 [2×], 23 [3×], 36; 42.2, 3 [2×], 4 [2×], 5, 6; 43.5, 6; 44.2, 9; 45.3; 46.3, 4, 7, 8, 9; 47.5) and 8 Ψ (30.26; 22f.2, 5–6; 24.6; 42.1, 4; 46.7; 47.3).

22. Segal 1927: 42; Pérez Fernández 1999: 50.

23. Within Northwest Semitic, we have evidence of a relative \check{s} in the alphabetic cuneiform text from Tanaach, an Ammonite amulet/seal ca. 600 B.C.E., and a Philistine text. Additionally, the determinative pronoun/genitive marker in Punic and a few late colonial Phoenician inscriptions are often considered to be cognate, and I take the view that the relative \check{s} in Phoenician (Standard though late Neo-Punic) and Ammonite are cognate. For the full range of comparative evidence, see Huehnergard 2006 and Holmstedt 2007.

24. On the identification of Judges 5–8 and 2 Kings 6 as northern, or "Israelian" Hebrew, see, among others, Driver 1913: 449 n.; Gordon 1955; Rendsburg 1992.

grammar, particularly after 722 B.C.E., so that eventually it replaced אֲשֶׁר (see Kutscher 1982: 32, §45; Davila 1990; Rendsburg 2006; compare Bergsträsser 1909).²⁵

Alongside the dialectal and diachronic perspectives (and in some tension with them) are the register and style proposals. For register, some have identified ׀ as the colloquial Hebrew relative word and אֲשֶׁר as the literary choice (Bendavid 1967: 77; see also Joüon 1923: 89; Segal 1927: 42–43). Taking this a step further, Gary Rendsburg has situated this variation within a diglossia analysis, suggesting that אֲשֶׁר reflects the “High” variety and ׀ reflects the “Low” variety that had somehow made its way into a formal, written context (Rendsburg 1990: 116–18; Davila 1994).²⁶ For style, Davila has suggested that the use of both אֲשֶׁר and ׀ is a literary device: “the impression we get [of the author of Qoheleth] is that he was a proud iconoclast, and it is not hard to imagine him as a sage who insisted on talking like real folks and not the highbrows in Jerusalem” (Davila 1994). In Young and Rezetko 2008, the stylistic analysis is taken a step further: the use of ׀ is identified as “substandard” Hebrew in the service of the “unconventional writing” of an “unconventional thinker” (2008: 2.65) and denied any diachronic relevance (2008: 1.214, 227, 247).²⁷

The obvious question is: Why must it be either-or? It does not necessarily follow that one or two cases in which a borrowed word is used for style undermines its diachronic explanation. Consider the use of אֲשֶׁר in the majority of the Qumran texts but the use of ׀ in the *Copper Scroll* (3Q15) and the *Halakhic Letter* (4QMMT). It is reasonable, given the stance toward “Scripture” at Qumran that the use of אֲשֶׁר reflects a religiously oriented archaizing—a very specific stylistic choice. This parallels the Mishnah’s use of אֲשֶׁר only in biblical quotations or allusions. The use of ׀, then, in 3Q15, 4QMMT, and the

25. As Rendsburg already notes, one of the problems with such a simple “north versus south” picture of ׀ and אֲשֶׁר is that אֲשֶׁר is used in the same texts identified as northern (for example, Judges 4–8, Hosea; Rendsburg 1990 :116). In his study of the grammar in Qoheleth, Isaksson concludes his overview of the use of ׀ with a virtual shoulder shrug: “the immense use of *šæ-* in the Book of Qoheleth probably is due to the influence of a Northern dialect, if not a northern origin. The mixed use of two seemingly interchangeable relative particles remains unexplained. Possibly, we should also reckon with Aramaic influence” (Isaksson 1987 :161).

26. See Rendsburg’s summary of this position:

During the period of the monarchy, 1000–586 B.C.E., a standard literary Hebrew was utilized in which אֲשֶׁר was the sole relative pronoun. The colloquial form, which existed side-by-side with the classical form, was ׀, which in a very few instances infiltrated literary composition. The upheaval of 586 B.C.E., with the resultant exile and restoration, effected changes in the Hebrew language, and one of these was the further penetration of ׀ into written records. (Rendsburg 1990: 116–17)

27. Young and Rezetko are able to make the assertion that ׀ “actually occurs more often in core EBH than core LBH books” (2008: 1.227) only by setting aside Qoheleth and Song of Songs.

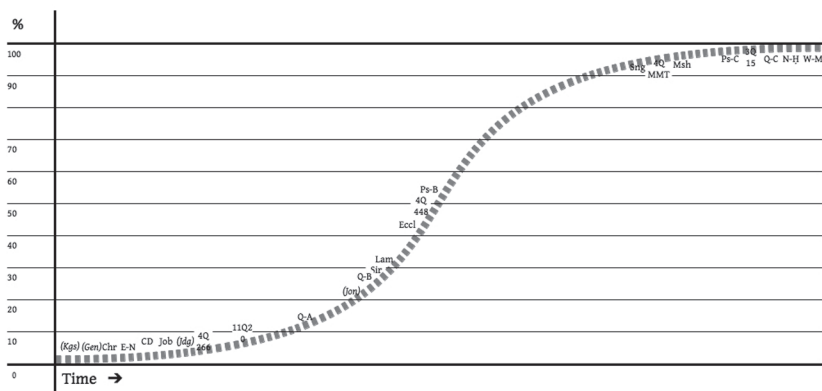


Figure 3. The diffusion of ש.

Mishnah represents the “real” picture of diffusion: ש replaced אשׁר. The same explanation can apply to books that other feature analyses suggest belong later on the scale, such as Daniel—the lack of ש in the Hebrew of Daniel does not necessarily mean that the book is early but simply that the feature had not yet become diffuse in the Hebrew author’s idiolect or that the author was intentionally mimicking the “scriptural” use of אשׁר, just like the majority of the Qumran texts.

In addition to stylistic archaizing, there also appear to be examples of ש used as a literary device to portray a character as “non-Hebrew.” This would include the isolated example in 2 Kgs 6:11, the 3 examples in Jonah, and possibly the 5 examples in Judges (Holmstedt 2006: 16–17; see also Young 1995).²⁸ If this analysis is accurate, it implies first that ש was perceived by the author and intended audience as “foreign,” though intelligible; and second that, as a borrowing for a literary need, such examples may have contributed to the actuation of change and diffusion but do not represent the process of diffusion itself. Thus, they do not sit on the S-curve of change in the position that corresponds to the frequency of the new item.²⁹

28. In 2 Kgs 6:11, ש is placed in the mouth of an Aramean king, even though ש is not used in Aramaic. In Jonah, ש is placed once in the mouth of the sailors (when they speak among themselves), once in Jonah’s mouth (when he addresses the sailors), and once in God’s mouth. The use of ש in God’s mouth alongside an immediately preceding אשׁר suggests that ש is used for rhetorical effect: to support one of the author’s theological points, that YHWH is the God of non-Israelites as well as Israelites.

29. Regarding the single occurrence of ש in Gen 6:4, there is no clear reason for it to reflect prestige-based borrowing. Moreover, as a single occurrence in a lengthy text that otherwise only uses אשׁר, the whole text cannot reflect any sort of diffusion of ש. We are left, then,

After working through the philological and linguistic issues related to this single feature set, the results of plotting the frequencies on an idealized S-curve, which I present in fig. 3, are interesting if for no other reason than the lack of major surprises.

Even though the Psalms and Qumran texts are kept in three general categories and none of the various compositional divisions proposed within biblical studies has been taken into account yet, the relative order of texts falls along familiar lines.³⁰ Overall, the pattern of Ψ 's diffusion supports the traditional explanation but without the empirically problematic divide between SBH and LBH.³¹

4. Conclusion

At the end of the day, where are we with regard to ancient Hebrew diachrony? If we situate the study of ancient Hebrew firmly within the larger enterprise of historical linguistics and, using the most rigorous methods, reconstruct the history of linguistic features carefully, we may well end up with a refined relative chronology of both linguistic features *and* ancient texts. That is, because the linguistic features necessarily exist in texts, a relative chronology of features would be salient for a relative chronology of texts. Whether or not these results accord with anything that previous scholars have concluded is irrelevant to the diachronic enterprise itself. What is critical is that the foundation be firm. If this is so, to return to one of my opening metaphors, the rebuilt ship will have no fear of sinking.

To be fair, the recent work of Young and Rezetko (and Ehrensverd) has ostensibly focused on the dating of *texts*, whether absolute or relative: “We will not say much about the relative dating of linguistic features, or linguistic change, except when it pertains to the dating of the texts” (Young and Rezetko 2008: 4). And yet, as I indicated at the outset, they often go too far in their claims, effectively removing both textual dating *and* linguistic feature (relative) dating from our reach.³² Far from this nihilism, I concur with Waltke and O’Connor’s assessment:

with the possibility that it is a textual error (see Young and Rezetko 2008: vol. 1, chap. 13, on the complexity of the textual history of the biblical text)—that is, that a later scribe accidentally replaced גַּם בְּאִשֶּׁר with בְּשֹׁגַם . Or, more likely, in my opinion, it is possible that the verse is a later interpolation and reflects the much later language of the scribe-editor who inserted it. On this verse, see, among many others, Keil 1878: 107; Gunkel 1997 [1910]: 58.

30. For some texts, such as Daniel, with regard to אִשֶּׁר and Ψ , a given feature may not reveal the entire story. This, of course, is why numerous analyses like these are necessary.

31. For a similar study of אִשֶּׁר and Ψ , focused on the distribution in Qoheleth, see Holmstedt forthcoming.

32. Young and Rezetko (2008: 1.61–62, 90–91) elaborate on their objections to using linguistics to produce a relative chronology of biblical texts by asserting that none of the “preconditions” available in other languages are available in Hebrew, such as clear innovations, datable loanwords, or an adequate control corpus. In their attempt to exclude all dating

[R]elative and absolute dating studies are different endeavors. Relative dating is logically prior to absolute dating; virtually any absolute date entails a relative date, while the converse is not true. In general, absolute dates cannot be derived from linguistic evidence. The second important point is that *abundant material for dating studies exists, both within the biblical corpus and outside it.* (Waltke and O'Connor 1990: 15, italics mine)

I am encouraged, not because we already have many scientifically established answers, but because we are now embarking on a new stage of historical Hebrew language research—a stage that will be marked by both methodological rigor and theoretical awareness beyond Hebrew studies, including informed statistical analysis, all the while maintaining the philological diligence that has always been a hallmark of our discipline. For this renewed attention to diachrony resulting from their work, I am thankful to the “Young Turks” who have recently so vigorously questioned the status quo—not because I agree with many of their methods or conclusions or because I think their “new synthesis” provides any sort of workable pattern for future study, but because the study of Hebrew diachrony will be immeasurably better for the introspection and methodological reevaluation that Young, Rezetko, and Ehrensverd have provoked.

evidence, they set their preconditions unreasonably high and contrary to the real practices in historical linguistics. Moreover, they confuse nonlinguistic historical information that may situate a given text with linguistic information outside the text. That is, because there is virtually no Hebrew linguistic evidence available outside the Bible from the sixth to third centuries B.C.E., they conclude that there is no “control corpus” (2008: 1.90). They can make this claim because at every turn they either dismiss the obvious nonlinguistic evidence that is relevant for dating at least some of the biblical books (for example, many of the prophetic books, or sections thereof) or take a revisionist biblical studies position, that the books are all late, and they couch this position as “mainstream biblical scholarship” (2008: 2.100)—a claim that is both subjective and tendentious. Moreover, they seem to assume that developing a cline based on the biblical features themselves, tested against known types of linguistic change from other languages, and situated historically by nonlinguistic evidence is illegitimate. If so, then most of what is done in historical linguistics in general is illegitimate.

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