

Dating the Language of Ruth: A Study in Method¹

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1. Introduction

There can be no doubt that the historical linguistic study of biblical Hebrew (BH), which until recently was a relatively quiet and stable niche for a few dedicated scholars, is no longer quiet or stable. The tenacious challenges of Ian Young, Robert Rezetko, and Martin Ehrensverd (along with a few other ‘young turks’), have just this Spring culminated in the publication of a two-volume opus, *The Linguistic Dating of Biblical Texts* (hereafter, *LDBT*). Although the title suggests that they might actually date texts, in fact they argue that linguistic dating of BH texts is a hopeless enterprise and that a better model is that of non-chronologically related dialects, that is, that Classical Biblical Hebrew (CHB) and Late Biblical Hebrew (LBH) co-existed. This is not the proverbial shot across the bow, but a full broadside against both the three-stage model of BH diachrony and the current application of loanwords from Aramaic and other near eastern languages and comparison with epigraphic, Qumran, and Mishnaic Hebrew as diagnostic tools in dating texts. The question, of course, is will the ship sink? And, if so, will the skiff they provide carry us to safety or founder in place?

In this paper I cannot address all of the challenges leveled in *LDBT*. Instead, I will use the book of Ruth, for which I have recently finished a grammatical commentary (Holmstedt

¹ This essay is a revision of a paper, “Dating the Language of the Book of Ruth,” presented at the annual meeting of the Canadian Society of Biblical Studies (Ottawa, May 23, 2009). I am indebted to John A. Cook, B. Elan Dresher, Bernard Levinson for feedback that significantly improved this essay. All errors remain my responsibility.

forthcoming), to explore how we can trace the development of grammatical issues and begin to build a linguistic profile of a biblical book that is chronologically informative. The diversity of opinions on the book of Ruth's language -- variously dated from the early monarchic period to the post-exilic period² -- indicates just how complicated linguistic dating is and thus the book will provide an interesting test of Young, Rezetko, and Ehrensverd's challenges.

Let us begin by acknowledging a few rules of the game, as it were. First, pre-modern linguistic evidence rarely allows one to determine an absolute date; instead, historical linguistics typically aims for relative dating, that is, situating features with regard to each other on a temporal cline. Young, Rezetko, and Ehrensverd also acknowledge this, but then twist it into an absurd knot:

...if we were unable to date biblical texts absolutely, but somehow we still managed to establish a relative chronology for these texts, we might still be incapable of deciding whether the texts in question spanned the tenth to eighth century BCE, or the eighth to the sixth, or the sixth to the fourth, and so on. In other words, we would be left with a moveable preexilic/exilic/postexilic span of time for the dating of these texts. (2009, I: 58; cf. pp. 57, 61-62)

The fallacy of this position is the notion that somehow the linguistic evidence must provide an absolute date apart from other historical evidence. Their complaints of circularity notwithstanding (pp. 65-68), this a baffling limitation of what data we may apply in an

² The language of Ruth has been variously dated to the early monarchic period (Campbell suggests the Solomon's reign for the basic story, which was finally fixed by writing in the ninth century, "tinged with archaic" features [Campbell 1975:26-28]; cf. Wolfenson 1911; Myers 1955; Hubbard 1988:23-35) to the linguistically "transitional" late pre-exilic or early post-exilic periods (Bush 1996:20-30), and at least one commentator despairs of identifying the book's date by linguistic or any other means (Sasson 1979:240-52).

historical investigation -- and historical linguistics is patently 'historical'. Moreover, it simply does not reflect how historical linguistics is practiced (for example, separating off 'linguistic' data from 'historical' data would turn the entire volume of Joseph and Janda 2003 into nonsense). Whether the topic is Old and Middle English or archaic Chinese, the relative temporal cline is anchored to history by non-linguistic evidence, such as explicit dates or the mention of externally datable events or people.³

Furthermore, the distinction between 'historical' and 'linguistic' is artificial: since all our data for ancient Hebrew comes from some variety of written sources and each of these written sources is grounded in some historical context (whether we can confidently identify and reconstruct it or not is irrelevant at this point), all our data are both linguistic and historical in nature. To ignore that the language data in the book of Nahum is associated with the period close to the fall of Assyria, that is around 612. B.C.E., is senseless. Or more specifically, is there any logic to disallowing our relative dating conclusions to benefit from the explicit mention of Darius I's second year as the context for the book of Haggai and thus also the context of the language data contained in that book?⁴ I think not.

³ Young, Rezetko, and Ehrensward elaborate on their objections to performing this kind of analysis of biblical texts by asserting that none of the "pre-conditions" available in other languages are available in Hebrew, e.g., clear innovations, datable loanwords, or an adequate control corpus (2009, I: 61-62, 90-91). In their attempt to exclude all dating evidence, they set their preconditions unreasonably high and contrary to the real practices in historical linguistics. Moreover, they confuse non-linguistic 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 6th-3rd centuries B.C.E., they conclude that there is no "control corpus" (90). In making this assertion, they glide right past the obvious non-linguistic evidence that is relevant for dating at least some of the biblical books (e.g., many of the prophetic books, or sections thereof). 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 non-linguistic evidence is illegitimate. If so, then most of what we do in historical linguistics in general is illegitimate. It is also important to point out that consistently throughout the work, Young, Rezetko, and Ehrensward resort to asserting that even if all their preconditions are met, dating could still not happen since the evidence could be explained as "different contemporary styles of BH in the preexilic and postexilic periods" (91). In other words, they have set up an unfalsifiable theory.

⁴ Historical linguists in other fields may often have more absolute dates by which to situate their diachronic

The second rule is that some linguistic features will provide more reliable dating information than others. In Hebrew studies not only have the general principles and techniques been mostly ‘home-grown’, which puts the results at risk of being inaccurate, but in the last few decades this field has been myopic in its nearly exclusive focus on lexical items -- words.⁵ In fact, the strongest argument in *LDBT* is the use of Avi Hurvitz’s criteria of *distribution, opposition, extra-biblical attestation, and accumulation* (see, e.g., Hurvitz 2000, 2006) to turn Hurvitz’s own techniques against him. To simplify Young, Rezetko, and Ehrensverd’s arguments somewhat (although fairly), for just about every lexical item or morphological feature typically identified with CBH or LBH, they point to at least one attested example in the ‘other’ corpus and draw the conclusion that if a so-called early item exists in late texts or a so-called late item exists in early texts, the chronological scheme fails. Ironically, Young, Rezetko, and Ehrensverd’s acceptance of the essential validity of Hurvitz’s criteria keeps their own arguments mired in the same methodological and theoretical inadequacies they criticize. But more on this later -- for now let us consider the nature of linguistic change in more depth before moving to an analysis of the language in the book of Ruth.

2. Linguistic Change

It is axiomatic that natural human languages change and historical linguists study the how and why of the changes. The challenge is how to sort through the extant data to identify

cline of features, but in essence they carry out their task the same way that we do in Hebrew studies, by taking full advantage of the non-linguistic historical information in their linguistic analysis. Moreover, we cannot lose sight of the basic nature of historical linguistic work -- that it is ‘historical’ and thus part and parcel of general historical studies.

⁵ Contrast, for example, the lexical heavy works of Avi Hurvitz with Kropat 1909, Polzin 1976, and Guenther 1977, all of whom acknowledged the importance of tracking syntactic changes and attempted to do so.

what is relevant and what is not:⁶ this means allowing for complexity of textual composition and transmission, including distortion in the transmission process, dialectal variation, discontinuities in the written record, different rates of change, and severe limits to our ability to reconstruct the socio-linguistic contexts (e.g., multi-lingualism, diglossia, standard-with-dialects). But these challenges are not new to historical linguistics and, while they are significant, should not be exaggerated to the extreme degree that Young, Rezetko, and Ehrensverd do.

For instance, Young, Rezetko, and Ehrensverd begin their discussion of textual criticism (chp. 13) by striking a balanced pose: “[the] study of language change in BH is inseparable from research on the literary and textual facets of biblical texts” (2009: 341). So, on the one hand, they suggest that textual criticism, compositional analysis, and historical linguistics can work together constructively by helping each other to discern the copying, compositional, and linguistic layers.⁷ This is reasonable if it is carried out carefully, which has not been the general case, and Young, Rezetko, and Ehrensverd are right to criticize current practice. On the other hand, they quickly move to the assertion 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” (344). Thus Young, Rezetko, and Ehrensverd

⁶ Note the insightful comment by Waltke and O’Connor:

The Hebrew of the Bible is sufficiently homogeneous that differences must be tracked on a statistical basis. The sophistication of such study is not in the statistics; advanced statistical methodologies are generally designed to deal with bodies of evidence quite different from what the Bible presents. *The sophistication is rather in the linguistic discrimination of what is counted and in the formulation of ensuing arguments.* (WO §1.4.2f; emphasis added)

⁷ I recognize that the line between the activities of copying and editing is very blurry, but it remains helpful to distinguish the two in this discussion.

have slammed the door shut on linguistic dating altogether by asserting that modified texts obscure the linguistic layers.

The full implication of their arguments does not seem to have dawned on the authors of *LDBT*. They spend ten pages discussing examples where differences among the textual evidence shows “that the language was subject to constant revision at the hands of editors and scribes who passed down the biblical tradition through many generations” (2009: 359; also see p. 351). This is either a case of rhetorical sleight of hand or confused thinking. If such chronological evidence can be discerned by textual critics, why is it not available to historical linguists? If differences between texts can be determined to reflect textual layers (which are inherently chronological), that information is just as available to those working on issues of language change. In contrast, if the differences are not layers, they can only represent different forms of the same composition, which necessarily implies that the author/editor made more than one version at the same time and thus there is no chronological relationship. If this is so, then all the text critical arguments made by Young, Rezetko, and Ehrensverd to show editorial updating are baseless, since not only would it be impossible to trace linguistic development, it would also be impossible to trace textual development.

Finally, Young, Rezetko, and Ehrensverd are ‘textually’ confused. They conclude that “assigning dates to biblical *texts* on the basis of linguistic analysis stands at odds with text-critical perspectives on those *texts*” (359, emphasis theirs). If by ‘texts’ we mean the concrete historical evidence, then we must only deal with the extant manuscripts (e.g., B19a, Vaticanus, the various DSS) and while we can certainly date these using various non-

linguistic techniques, we can go no further. If, however, by ‘texts’ we mean the compositions we assume to exist (or reconstruct to determine the ‘archetype’ or ‘Ur-text’) behind the extant manuscript evidence, then whatever reality we assign this ‘pre-text’ should benefit the historical linguist too. In a nutshell, while Young, Rezetko, and Ehrensverd appear to stack the deck in favor of the text critic, it is only logical that what holds for the historical linguist also holds for the text critic. Their criticism may be valid -- that BH historical linguistics may have ignored or played down the textual complexity of the text, but the only conclusion that can reasonably be drawn from this is that historical linguists and text critics should work more closely together, not that historical linguistics is effectively impossible to carry out.

On scribes and the transmission process in particular -- scribal errors and updating certainly may complicate the data, but if the Hebrew scribes had changed everything to match their own grammar, the Bible would have been written in late Rabbinic Hebrew! And if they did not change everything, then the potential exists for us to sort through what was changed and what was not. In fact, Young takes a much more positive stance vis-à-vis the textual tradition in an earlier study. In a reconstruction of Edomite phonology (the monophthongization of *ay < e), Young cites biblical texts that he argues have an Edomite origin or accurately reflect the Edomite language and then admits that his argument “rests upon assuming the conservatism of the Hebrew reading tradition over a thousand years” (1992: 29). He goes on to defend this assumption by making three points, two of which are relevant to the current discussion:⁸

⁸ Young’s third point is relevant but as a more general methodological point:

Thirdly, until some better explanation of the coincidence that these two forms are only found

Firstly, the validity of the Masoretic vocalization as historical evidence of a period well before the Masoretic period has been demonstrated in recent scholarship. Secondly, it is the tendency of both reading traditions, and of language in general, to level anomalous forms. Therefore the retention of such forms can be taken with caution as survivals of earlier or divergent linguistic systems. (1992: 30)

If the Masoretic textual tradition has preserved linguistic elements that are no longer part of whatever Hebrew the Masoretic scribes knew, as Young 1992 suggests, 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 moreso dating the discernible layers and of establishing a linguistic chronology.

As for dialectal variety, yes, the biblical texts may reflect different but closely related dialects of Hebrew, but again this is no different than the study of many pre-modern languages. Consider the odd situation of the record of English, which has a significant

in passages dealing with Moab and Edom is proposed, the hypothesis presented above is the best means to explain the features of the biblical text. (1992: 29-30)

This is simply a variation on the principle of parsimony (often referred to as Occam's Razor): given the same data and conditions, the simplest or most economical hypothesis that accounts for all the data is preferred. Where this point intersects with the recent arguments in *LDBT* is that Young, Rezetko, and Ehrensverd believe that the non-chronological, co-existing dialects/styles model is a better means of explaining the data that they present. The problems with their arguments are manifold, as I am demonstrating in the current essay. Here I will make two smaller points that I have not bothered to work into the body of the argument. First, it is notable that Young, Rezetko, and Ehrensverd do not marshal any new data in support of their case. That is, by re-hashing well-known evidence, they are not able to change the playing field, so to speak. Second, they commit a basic inductive fallacy: they argue (with some success, I agree) that current formulations of the chronological argument have deficiencies, and therefore they assert the chronologically-oriented approach to be invalid. The answer is, of course, a methodologically sound chronological argument not the diachronic nihilism they espouse.

discontinuity -- most of the evidence for Old English is written in the Wessex (West Saxon) dialect whereas Modern Standard English is descended from Mercian (for which we have very few records), not Wessex. Commenting on this, Janda and Joseph write:

... despite this lack of direct continuity in our records of English, it is common for linguists to make comparisons across the different periods of the language as if they were truly meaningful; this is a graphic instance of Labov's characterization of historical linguistics [as the art of making the best use of bad data], since in doing so, one is simply making the most of the imperfect situation that the accidents of the attestation of English have provided, and letting an indirect ancestor stand in for the unattested direct ancestor. Although dialects can differ radically from one another, this step is based on the reasonable assumption that a given non-ancestral dialect is likely to be linguistically closer to the unattested ancestor than any other available point of comparison. (Janda and Joseph 2003: 31, n. 35)⁹

Thus, even if CBH and LBH were different dialects,¹⁰ it would still be possible to place them

⁹ Consider also the following comment from Janda and Joseph:

...no matter how carefully we deal with documentary evidence from the past, we will always be left with lacunae in coverage, with a record that remains imperfect and so confronts us with major chasms in our understanding that must somehow be bridged. And "chasm(s)" is sometimes a charitable characterization of the impediments that bedevil the pursuits of diachronic linguists.... Surprisingly often, the discontinuities posed by apparent gaps are compounded many times over when it turns out that what we actually face is not an interruption of a single linguistic tradition, but the end of one line of language transmission and the beginning or recommencement of a related but distinct line. (Janda and Joseph 2003: 30-31)

¹⁰ I am not yet convinced that the written Hebrew record presents us with such discontinuities; two facts must first be demonstrated convincingly: first, that the written records that we have received actually reflect different dialects in an historically plausible setting, and second, that the dialects diverged to the point that the kinds of changes we see cannot still be placed on a temporal cline. Even if CBH and LBH represented the 'high/written' and 'low/spoken' registers in a diglossic ancient Israel (a la Rendsburg 1990) and the spoken vernacular displaced the high register as the medium of writing at some point (a shift that has been well-documented for other languages), LBH as a *system for writing* would then necessarily be later than CBH, and thus the two registers as they are represented by the written texts would stand in a relative chronological relationship.

on a temporal cline if the combination of linguistic and non-linguistic data reasonably suggests so (and I believe it does). Thus, the chronological model, though admittedly in need of refinement, remains the best of the competing hypotheses.¹¹

3. Linguistic Changed Exhibited in the Book of Ruth

Now we turn to the book of Ruth. Using the language in the book as a test case for Young, Rezetko, and Ehrensward's approach, the first question is what kind of data in the book might shed light on these issues? If I were to follow the typical pattern in Hebrew studies, the remainder of my argument would focus on lexical items and simple collocations (e.g., the use of $\text{וַיְהִי} + \text{preposition} + \text{infinitive construct}$) -- most of which do not exhibit probative diachronic patterns. The table below lists the features most often treated in commentaries on Ruth.

¹¹Beade reminds us that, according to Popper, a statement or theory is falsifiable "if and only if there exists at least one potential falsifier – at least one possible basic statement that conflicts with it logically" (1989: 176). As he concludes, "the best falsification is any science is a better hypothesis" (180). For historical linguistics, then, a given reconstruction is scientific as long as the elements accord to known (typological) patterns and make better sense of the data than any competing reconstruction. Any reconstruction that adheres to these two basic principles is also falsifiable in principle in that newly discovered data could show it to be inaccurate. The question for Young, Rezetko, and Ehrensward's theory is simple: does the proposal of two co-existing dialects with the types of differences attested in the biblical text make better sense than a chronological proposal? As of yet, they have not provided convincing evidence that their proposal makes better sense (or much at all).

<p>Orthographic Features</p> <p>Spelling of Qal participle/agentive noun: with ך in 2:4-7, 15; 3:11; w/o ך in 1:1; 2:3, 20; 3:2, 8, 9, 12; 4:1, 3, 4, 6, 8, 14, 16</p> <p>Forms of the fpl verb: short (מִצָּאן in 1:9, לָכֵן in 1:12, and קָרָאן in 1:20); with נה- in 1:9-14, 19-21; 4:14, 17</p> <p>Loss of א: וּתְשִׁנֶּה in 1:14 and צַמַּת in 2:9</p> <p>א is written for expected ה in the name מֵרָא</p> <p>Alternation of שָׁדִי and שָׁדָה</p> <p>Spelling of the name דָּוִד w/o י in 4:17, 22</p> <p>Phonological Features</p> <p>Consistent assimilation of the ך in מן to noun w/o article in Ruth</p> <p>Morphological Features</p> <p>The paragogic ך in 2:8, 9, 21; 3:4, 18</p> <p>The 2fs <i>qatal</i> verb ending in תי- in 3:3, 4</p> <p>The ׁ- pronouns used for feminine antecedents in 1:8, 9, 11, 13, 19, 22; 4:11</p> <p>Syntactic Features</p> <p>וְיָהִי + preposition (+ infinitive construct): וְיָהִי בְּשָׂכְבוֹ in 1:19; וְיָהִי בְּחֻצֵי הַלְּיָלָה in 3:8, and וְיָהִי בְּבִקְרָה in 3:13</p> <p>כי introducing a verbal complement clause with the שמע in 1:6, ראה in 1:18, ידע in 3:11, 14, and a noun complement with טוב in 2:22 and עדים in 4:9</p> <p>Predicate-Subject order within the null-copula complement clauses in 1:18 and 3:11.</p> <p>The use of Y-בין ... X-בין, instead of Y-ל ... X-בין in 1:17</p> <p>Object suffixes on verb / no examples of א+suffix: 1:21; 2:4, 13, 15; 3:6, 13 [2x]; 4:15 [2x], 16)</p> <p>Semantic Features</p> <p>Abundant use of <i>wayyiqtol</i> and modal <i>qatal</i> (for latter, see 1:11, 12; 2:7, 9, 14, 16; 3:3, 4, 9, 13, 18; 4:5, and perhaps 4:15)</p> <p>Some use of non-modal <i>qatal</i> clauses in narrative (1:14, 22; 4:1, 18-22) in non-reported speech/narrative main clauses</p> <p>Lexical Features</p> <p>שָׁדִי (1:20-21)</p> <p>אֲנִי (2:10, 13; 3:9, 12, 13; 4:4 [2x]) versus אָנִי (1:21; 4:4)</p> <p>לָקַח [אִשָּׁה] (4:13) versus נָשָׂא אִשָּׁה 'to take a wife' (1:4)</p> <p>לָקַח (4:5, 10) versus לָקַח (4:7)</p> <p>שָׁלַף נַעַל 'to remove a sandal' (4:7)</p> <p>שָׁבַר 'to hope, wait for' (1:13)</p> <p>עָגַן 'to hinder' (1:13)</p>

Table 1: Linguistic Features often used in Dating the Book of Ruth

As a brief aside, I should explain why I treat lexical data as circumstantial but not primary evidence. It is a fact that lexical items may continue to be used through long periods of linguistic history even though near synonyms are introduced and the overall grammatical system changes around them.¹² For instance, in the example from Chaucer

¹² A questionable syntactic pattern that is often pointed to as an example of change is the use of אָשַׁר to introduce complement clauses in LBH instead of the SBH use of כִּי (Sáenz-Badillos 1993:127; Young, Rezetko, Ehrensverd 2009, vol 1: 258; cf. Polzin 1976:128; JM §157c). In Ruth כִּי presents a verbal

given below, there are a number of words that a twenty-first century reader not only can recognize but regularly uses (spelling differences aside), e.g., *there, dwelling, rich, guests, craft, carpenter, poor, scholar, learned, art, fantasy, turned, astrology, drought, showers, befall, reckon.*

Geoffrey Chaucer, Canterbury Tales, The Miller's Tale (14th c. C.E.)

<p>Whilom ther was dwellynge at Oxenford A riche gnof, that gestes heeld to bord, And of his craft he was a carpenter. With hym ther was dwellynge a poure scoler, Hadde lerned art, but al his fantasye Was turned for to lerne astrologye, And koude a certeyn of conclusiouns, To demen by interrogaciouns, If that men asked hym, in certein houres Whan that men sholde have droghte or elles shoures, Or if men asked hym what sholde bifalle Of every thyng; I may nat rekene hem alle.</p>	<p>A while ago there dwelt at Oxford a rich churl fellow, who took guests as boarders. He was a carpenter by trade. With him dwelt a poor scholar who had studied the liberal arts, but all his delight was turned to learning astrology, He knew how to work out certain problems; for instance, if men asked him at certain celestial hours when there should be drought or rain, or what should happen in any matter; I cannot count every one.</p>
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Text and translation from eChaucer (<http://www.umm.maine.edu/faculty/necastro/chaucer>)

While it is true that some of the words witness semantic shift, so that, for instance, ‘reckon’, which likely derives from Germanic *rechnen* ‘to count up’, still means ‘to count’ in Chaucer while its primary modern usage is ‘to conclude’, the point is that the word lasts.¹³ Moreover,

complement clause with the *שׁמַע* in 1:6, *רָאָה* in 1:18, *יָדַע* in 3:11, 14, and a noun complement with *נָטַח* in 2:22 and *עָדִים* in 4:9. Thus, Ruth would seem to pattern in this case with SBH rather than the books of LBH. However, an observant author who was aware of older writings could have easily avoided such a simple lexical substitution. To wit, when I want to communicate the ‘highly educated professor’ persona, it is not difficult to lecture consistently with pied-piped prepositions in relative clauses, e.g., ‘to whom’, ‘in which’, rather than leaving the preposition stranded inside the relative, e.g., ‘who I spoke to’ - the usage that has become the standard in all but the most highfalutin registers.

¹³ Identifying semantic change in an ancient, modestly-attested language like Hebrew, is a precarious matter and does not often serve us well for dating (consider, for example, the difference of opinions on the meaning of *שׁלַח* and its value in dating Ecclesiastes; compare the four different views in Whitley 1979: 29; Fredericks 1988: 239; Seow 1996:653-54, 1997:13-14; and Fox 1999:187). Consider also the two idioms for ‘taking a wife’ used in Ruth: the phrase typical in SBH texts, *לָקַח [אִשָּׁה]* (4:13), as well as the phrase typical in LBH texts, *נָשָׂא אִשָּׁה* ‘to take a wife’ (1:4). While the phrase *נָשָׂא אִשָּׁה* appears once in a SBH text (Judg. 21:23), it is mostly found in LBH texts (2 Chr. 11:21; 13:21; 24:3; Ezra 9:2, 12; 10:44; Neh. 13:25). It is also found in Ben Sira 7.23b and the Mishna, to the exclusion of *לָקַח אִשָּׁה*. Thus, the use of *נָשָׂא אִשָּׁה* in Ruth 1:4 appears to be a clear case of LBH language (admitted so even by Young, Rezetko, and Ehrensverd 2009, 1:265). Yet, this does not explain the use of the alternate phrase, *לָקַח [אִשָּׁה]*, in 4:13. It is possible that the story-teller simply has at his command (i.e., within his ‘mental lexicon’) two synonymous phrases for expressing ‘taking a wife’. It is possible that Guenther 2005 is correct, that the explanation is a socio-linguistic distinction rather than a diachronic distinction, at least for the book of Ruth. Guenther

it is often difficult to identify precisely when a neologism is introduced since it may be a simple historical accident that the item has not been found in earlier texts (Alinei 2004: 216-18).¹⁴ Finally, it is much easier for an author to mine earlier sources (assuming at least an incipient archival culture) for a choice word that is no longer generally used than it is to revert back to earlier syntactic and semantic patterns. For these reasons, I will leave behind the matter of lexical change, and focus instead on a syntactic change.

In his classic study of the article in biblical Hebrew, James Barr mentions the use of the article as a relative marker (1989: 322-25). He notes that, while grammars often list examples of the type illustrated in (1), they neither provide an accurate grammatical analysis nor

speculates that השן לקל is the basic phrase for marriage, while השן נשן implies, among other things, a marriage without a dowry, bride-price, or any other exchange of wealth due to poverty and low status. For Ruth specifically, he says:

In Ruth 1.4 Mahlon and Chilion are described as taking (נשן) Moabite women for themselves after the death of their father, Elimelech. Elimelech and his family had left Bethlehem in Judah for Moab because of famine. That they had mortgaged their land and used up their capital is borne out by the need for a kinsman to redeem their property when Naomi returned to Bethlehem. In their poverty, they could not afford bride-prices for Ruth and Orpah. Thus they married poverty-stricken or low status women who brought no dowry into the marriage. When all three husbands had died, Naomi, Orpah, and Ruth were financially destitute; the marriages added nothing to the family's ability to survive. Neither bride-price nor dowry were exchanged in this (נשן , 'seizure') marriage. (2005:400)

Although Guenther does not address the use of לקל in 4:13, presumably he would suggest that in the case of Boaz the notion of a low-status marriage was intentionally avoided, even if there was no official bride-price paid by Boaz. However, Guenther's argument rests on assuming an ancient setting for Ruth and that the description accurately reflects economic distress -- neither assumption can be granted without great hesitation. However, in light of this possible socio-linguistic and literary explanation and lacking any discernible diachronic clues, it is doubtful that we can place any weight on the use of השן נשן in Ruth 1:4 as evidence in dating the book's language (contra Bush 1996:26).

¹⁴ Similarly, Janda and Joseph state,

Moreover, despite all the philological care in the world, even something as seemingly fixed as date of first attestation is not always a reliable indication of age. For instance, the word *éor* is attested very late in the Ancient Greek tradition, occurring only in glosses from the 5th century AD attributed to the lexicographer Hesychius, but it clearly must be an "old" word, inherited from Proto-Indo-European, since it seems to refer to female kin of some sort and thus appears to be the Greek continuation of PIE **swés(o)r* 'sister', altered by the action of perfectly regular sound changes. The complete absence of this word from the substantial documentary record of Greek prior to the 5th century AD, which covers thousands and thousands of pages of text, is thus simply an accidental gap in attestation. Further, oral transmission clearly can preserve archaic forms, as the evidence of the *Rig Veda* in Sanskrit shows, even though there is no (easy) way to assign a "first attestation" to an orally transmitted text. (Janda and Joseph 2003: 23)

recognize the possible diachronic evidence such relatives provide.

(1) וּבְכָל-תַּיָּה הַרְמֵשֶׁת עַל-הָאָרֶץ

‘and over every creature that creeps on the ground’ (Gen 1:28)

Barr concludes this section of his argument by saying,

...the relative article has a main function other than that of normal determination; it is frequent in some poetic texts in which the usual article is rare; and it may possibly suggest a path which leads from an older state of the language, in which determination by the article was unusual, to the classical state, in which such determination was central. (1989: 325)

It is in the potential diachronic information where this issue of the relative article intersects with the language of Ruth. Consider the three examples given in (2)-(4):

(2) וּתָשָׁב נְעָמִי וְרוּת הַמּוֹאֲבִיָּה כְּלָתָהּ עִמָּה הַשְּׂבָה מִשְׂדֵי מוֹאָב

‘and No‘omi returned, Ruth the Moabite, her daughter-in-law, with her, who (also) returned from the territory of Moab’ (Ruth 1:22)

(3) וַיֹּאמֶר נְעֵרָה מוֹאֲבִיָּה הִיא הַשְּׂבָה עִם-נְעָמִי מִשְׂדֵה מוֹאָב

‘and he said: she is a Moabite girl who returned with No‘omi from the territory of Moab’ (Ruth 2:6)

(4) חֶלֶקֶת הַשָּׂדֶה אֲשֶׁר לְאַחֵינוּ לְאֵלִימֶלֶךְ מְכָרָהּ נְעָמִי הַשְּׂבָה מִשְׂדֵה מוֹאָב

‘the portion of the field that belongs to our kinsman, to Elimelek, No‘omi, who returned from the territory of Moab, is now selling’ (Ruth 4:3)

In each case the underlined verb is accented by the Masoretes as a 3fs *qatal* preceded by the relative article.¹⁵ I will use these three examples and the larger issue of the development of the relative article to situate Ruth's language.

Starting with the origin of the definite article in both Phoenician and Hebrew, it has been argued convincingly that -ה began its grammatical life as a subordinator, i.e., a relative marker (see, most recently, Gzella 2006; contra Pat-El 2009). This accounts for examples like (1) above, in which the head of the relative does not exhibit 'agreement in definiteness'; it also accounts for the similar construction with adjectives, as in (5):

(5) וַיְבֵא הַלֵּךְ לְאִישׁ הָעֵשִׂיר

'a traveller came to a man who was rich' (2 Sam 12:4)

And finally, it account for examples such as (6) -- cases where the original relativizing function of the ה was extended to finite verbs:

(6) וַיִּקְרָא אַבְרָהָם אֶת־שְׁם־בְּנוֹ הַנּוֹלָד־לוֹ אֶשֶׁר־יָלְדָהּ־לוֹ שָׂרָה יִצְחָק

'and Abraham called the name of his son who was born to him, who Sarah bore for him "Isaac"' (Gen 21:3)¹⁶

¹⁵ Although grammars and commentaries typically advise the reader to ignore the Masoretic accents these examples, such advise is illogical. The Masoretes had no need to indicate the accent on this penultima if the reading tradition had not preserved this placement of the word stress and since, as we will see, this syntactic pattern had long fallen into disuse by the period of the Masoretes, there was every reason for the reading tradition to adjust the word stress to its expected placement. But it did not, which suggests that the reading tradition preserved a grammatical feature that was much older. So, I suggest we ignore such injudicious philology and respect what concrete historical evidence we have (see Lass 1997: 100).

¹⁶ This last example illustrates the fundamental difference between ה relatives and אשר relatives: the ה relative was limited to constructions in which the head of the relative was also the subject within the relative; the אשר relative had no such constraint. However, the ה relative also forced a restrictive relative clause reading, whereas the אשר relative did not. Thus, there were good uses for the ה relative.

Interestingly, for whatever reason this use with finite verbs did not survive;¹⁷ we have only nineteen examples in the Hebrew Bible (and none outside), listed in (7):

- (7) ן-relative with finite verb: Gen 18:21; 21:3; 46:27; Josh 10:24; 1 Kgs 11:9; Isa 51:10; 56:3; Ezek 26:17; Job 2:11; Ruth 1:22; 2:6; 4:3; Ezra 8:25; 10:14, 17; 1 Chr 26:28; 29:17; 2 Chr 1:4; 29:36.

A simple study of the verses listed in (7) does not produce an obvious chronological pattern, but further investigation provides the key. When we turn to the examples in which the relative clause with ן modifies a noun phrase not marked with ן, i.e., asymmetric agreement examples like those in (1), (3), and (5), we may reconstruct a change in which Ruth figures prominently. Consider the group of data in (8):

(8) ן-relatives modifying anarthrous NP head

(a) Finite Verbs: Ruth 2:6; Ezra 10:14, 17; 1 Chr 26:28

(b) Participles: Gen 1:28; 7:21; 49:17; Exod 26:12; 38:26; Lev 11:46; 16:16; Judg 21:19; 1 Sam 25:10; Isa 65:2; Jer 27:3 46:16; 50:16; Ezek 2:3; 21:19; 28:16; 32:22, 24; 47:2; Song 4:5; Dan 9:26; Ezra 10:17; 1 Chr 26:28; 2 Chr 31:6.

(c) Adjectives: Judg 16:27; 1 Sam 12:23; 16:23; 2 Sam 12:4; 1 Kgs 7:8, 12; 2 Kgs 20:13; 24:4; Jer 6:20; 17:2; 22:17; 38:14; Ezek 21:19; 40:28, 31; 42:9; Ps 62:4; Eccl 11:5; Neh 9:35; 2 Chr 4:10.

Notice within the first sub-group, Ruth is one of three examples with a finite verb and the

¹⁷ I think that it is likely this restriction to non-finite verbs and adjectives had to do with the confusion that the prefixed ן would cause with verbs that also has prefixed ן due to the *binyan*, e.g., Hifil and Hitpael.

other two are from late biblical books. When the participial and adjectival relatives are added, the modest trend is towards a greater use of this construction in texts associated with 'late' biblical Hebrew. Now consider the extra-biblical evidence in (9):

(9) ה-relatives modifying NP head without ה

(a) Ben Sira:¹⁸ 14.21; 16.7, 9, 10; 36.31a, b; 49.12; 50.9, 26.

(b) Qumran:¹⁹ 1QS 8:11; 4Q167 f2:3, 252 1:9; 373 f1a+b:4, 394 f1_2ii:8; 11Q19 21:12, 46:5.

(c) Mishna:²⁰ Shev 2:7, 9; Ter 1:8, 9; Maaser2 1:5; Eruv 8:2; Yoma 7:3; Sheqal 2:1; 4:2; 6:5; Sukk 3:1, 2, 3; 5:4; Meg 1:9; Hagig 3:4; Ketub 8:6; 9:7; Nazir 7:3; Sota 6:3; Qidd 2:9; BabaQ 1:4; 2:5; Shevu 1:7; Ed 6:3; AvodaZ 2:3; 5:9; Avot 1:3; Hor 3:4; Zevah 4:4; 5:2; 8:1; 12:5; 14:1; Menah 9:7; Hul 5:3; Arak 3:1; 4:4; Ker 3:8; Meil 2:3; Mid 1:9; Tamid 1:1; Kelim 2:7; 14:5; Ohol 1:5; 8:2, 5; 18:1, 2; Neg 1:2; 13:7, 9, 12; Para 1:1; 8:3; 11:6; Tohar 4:5; 7:7; Miqw 7:1; Maksh 3:5; 6:7.

Looking at these data, Young, Rezetko, and Ehrensverd would no doubt assert that any perceived difference between texts commonly taken as earlier and those taken as later, biblical or extra-biblical, are simply a matter of dialectal variation and not chronological change. But, let me suggest an alternate story.

¹⁸ The only examples in Ben Sira are participial. For whatever reason, the book contains only four cases of the adjective with the article (Sir 12:13; 45:25; 48:22; 50:25) regardless of the syntax of its head. Of these four examples, only 12:13 might be included in our list. However, since the preceding noun is כל 'all', which complicates the syntax: is the quantifier the head or does it modify a null head?, and also, is the modifier הַקָּרֵב an adjective or participle?

¹⁹ The list includes both participles and adjectives. The examples cited were not fragmentary, many more were suggested by the fragmentary evidence. However, since these could not be determined with great confidence, they were excluded.

²⁰ This list includes only participles. There are many more examples (approximately 200) of the relative article with adjectives modifying a NP lacking ה that I have not listed. In fact, this pattern for adjectival modification is considered to be the 'norm' in Mishnaic Hebrew (see Pérez Fernández 1997).

The article in Central Semitic was a relatively late innovation in each of the languages. For Northwest Semitic, the lack of clear evidence for an article in Ugaritic suggests that it was an early first millennium innovation. If it began within relative clauses, it was a feature competing with an already established relative particle; in fact, Hebrew already had at least two: \aleph and the \beth -series (if not also ψ). The biblical examples suggest that the change to include the relative article began by replacing the other relative words slowly at first and in a restricted environment: non-verbal modification in which the head of the relative was also the subject within the relative. Then the change increased -- and it expanded to include finite verbs in some isolects -- before finally tapering off with an established but constrained dominance in its original context -- non-verbal modification.

Notably, the use of the other relative words to introduce participial relatives exhibits a corresponding decrease, so that by the Mishna, \beth -participial relatives are favored 11-to-1 over ψ -participial relatives (ψ replacing \aleph by this point).²¹ What I have described here is an example of an S-curve of linguistic change:

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 tail 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)

If my reconstruction for \beth -relatives is accurate, it places Ruth's use of the relative article in

²¹ \aleph + participle in the Hebrew Bible: Gen 7:8; 39:22; Num 21:34; Deut 1:4; 3:2; 4:46; 1 Kgs 5:13; Isa 11:10; 49:7; Jer 38:16; Ezek 9:2; 13:3; 43:1; Zech 11:5; Pss 115:8; 135:18; Eccl 4:1; 8:12, 14; Esth 8:8; Neh 5:2, 3, 4; 2 Chr 34:10. In the DSS: 1QS 11:6; 4Q274 f1i:8; 4Q410 fl:3; 4Q419 fl:9; 4Q504 fl_2Rvi:5. In Ben Sira: 13.2; 38.5. And in the Mishna it is ubiquitous; note also that many of the ψ -participial relatives are constructions that never occur with \beth , such as ψ - \aleph .

that strong middle surge in which it expanded to include even finite verbs, and this middle surge is concentrated in texts often categorized as ‘late’ biblical Hebrew. Given the typologically-grounded support of the f-curve pattern, even if CBH and LBH were slightly different dialects, in this feature they were operating together as a larger dialect and thus we can still legitimately trace linguistic change in the data as a whole.²²

4. Conclusion

What I have suggested in this study is that there are linguistic changes that we can track diachronically within the biblical Hebrew corpus. For Ruth, I used the issue of the ׀-relative to illustrate how I think the process *ought to be carried out*. The use of the ׀-relative with finite verbs and with indefinite heads is the kind of change that would be hard to suppress -- a subtle syntactic change. Thus, the book sits on the relative dating cline between books like Gen-Deut, Josh-Kings on the one side and Ezra-Neh, Chronicles, and Qohelet on the other. Greater nuance will come with further study of the well-known evidence as well as new observations, such as I have presented here. It is true that tacking the relative cline, regardless of the nuance, to the historical timeline of ancient Israel relies on non-linguistic evidence, but that does not invalidate the cline itself or the process by which it was constructed.

My arguments in this study are not against the likelihood that there were multiple

²² The pattern exhibited by the ׀-relative is similar to 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 co-existed with the newer construction for over 300 years, until finally being replaced entirely by the ‘do’-support construction.

dialects of Hebrew -- perhaps even written forms -- in ancient Israel at any given period. However, Young, Rezetko, and Ehrensverd have taken this idea to an absurd extreme by asserting that neither the kind of linguistic change I have described in this essay nor linguistic borrowing (for which Ruth provides no clear cases) contributes to the dating of biblical books. Although we must admit the gaps in our knowledge of ancient Hebrew of all sorts due to evidential lacunae, this is no different than any historical endeavor, linguistic or otherwise. Charles Beard has famously quipped that, in doing history, "We hold a damn dim candle over a damn dark abyss" (cited in Janda and Joseph 2003: 19). So be it, since we cannot change the facts of the damn dark abyss, but Young, Rezetko, and Ehrensverd are effectively trying to blow our candle out and at the same time tell us the abyss is horizontal rather than vertical. I'll keep my own candle, thank you very much.

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