

HISTORICAL LINGUISTICS AND BIBLICAL HEBREW: AN INDO-EUROPEANIST'S VIEW

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ABSTRACT

Rezetko and Young's *Historical linguistics and Biblical Hebrew: steps toward an integrated approach* brings variation analysis to bear on the question of the periodisation of Biblical Hebrew. However, this methodology is at best microdiachronic, dealing with variation in synchronic terms. In order to answer the question they pose, a language with a history as long as Biblical Hebrew requires macrodiachronic techniques which look at real linguistic processes. Several such processes are discussed in this paper, and though they collectively converge in pointing to a late date for Qoheleth, they are insufficient to establish a linguistically-based entity "Late Biblical Hebrew". At the present time, one can at best apply this term in a non-linguistic sense to the Hebrew of those books known on extra-linguistic grounds to have been chronologically late.

INTRODUCTION

In March 2015 I received an invitation from Prof. Jacobus Naudé to present a paper at the 2015 meeting of the Society for Biblical Literature in a session dealing with the appropriation of historical linguistics methodology to the study of Biblical Hebrew (BH) in the recent volume *Historical linguistics and Biblical Hebrew: steps toward an integrated approach*, by Robert Rezetko and Ian Young (henceforth Rezetko and Young 2014). As an Indo-Europeanist whose research has focused on Vedic Sanskrit, Homeric Greek, Gothic, and Classical Armenian,¹ I am an outsider to BH studies; and although I have been studying BH since I was six years old, I have no knowledge of the literature on its periodisation, and the only thing I know about the dating of the constituent texts of the Hebrew Bible is what may be inferred by comparing their content or setting with external events in the history of Israel. Finally, it goes without

¹ Representative publications: Klein (1978, 1988, 1996); Klein and Condon (1993).

saying that I have heretofore carried out no research of my own on BH and its possible periodisation.²

STARTING ASSUMPTIONS

I do start, however, with several basic assumptions: all languages have diachronic, diatopic, and diaphasic dimensions.³ This means that they change over time, are spoken in different places, and may be articulated in different settings leading to what we might call variation in style or register. Consequently, we can be certain that the spoken form of the language that has been handed down to us in its literary form as the Hebrew Bible changed over the period of eight centuries or so during which this corpus was composed. This change, like all linguistic change, would have proceeded in situ, but in case the individual texts were composed in different places, this would have contributed a second dimension to this change, insofar as the different dialects represented in the text, constituting their own independent linguistic systems, would themselves have changed over time. However, what we have in the Hebrew Bible is not a thesaurus of spoken Hebrew but rather a text, indeed a library and a literature which, while composed by different people over the better part of a millennium, certainly gives the impression of being a single language. That is to say, as I read, for example, Esther or Lamentations, I am not cognizant of reading a different language, of processing a different linguistic system, from what I read when I am perusing Genesis, Numbers, or Deuteronomy,⁴ although to be sure I am exceedingly aware of

² Following the conference, Professor Naudé was kind enough to send me a copy of the volume *Diachrony in Biblical Hebrew*, edited by Cynthia Miller-Naudé and Ziony Zevit, which deals extensively with the problem of linguistic change within the corpus. Fairly intense perusal of this book has led me to modify in a number of ways the conclusions I presented at the conference.

³ Cf., *ex multis*, Anttila (1972:21). On diaphasic aspects of language, focused on English, cf. the classic treatment of Joos (1967).

⁴ An anonymous referee has questioned whether the judgment of a contemporary reader of Hebrew on this issue is identical to that of a speaker/reader of Hebrew during biblical times. My assumption is that what is true of someone who has learned the language academically is a fortiori true of someone who knows the language natively. After all, the goal of successful non-native-language instruction is to help the learner come as close as possible to acquiring the largely unattainable native language intuition.

stylistic differences, relating in large part to genre, subject matter, authorship etc., separating these.

PARALLEL CONSIDERATIONS AMONG OLD INDO-EUROPEAN LANGUAGES

But even if BH is a literary language, does that mean that it must have been completely static over the period of its use? Clearly, no; but such change could have been relatively minor. Perhaps the best parallel from my own experience is that of Classical Armenian. The language of fifth century C.E. Classical Armenian, known as the *grabar* or simply “literary language”, continued to be used for written Armenian until at least the seventeenth century. Within this vast period the only major linguistic change was the monophthongisation of the diphthong *au* to *o*, requiring an additional letter to represent this new vowel. Other changes, including the introduction of a new letter to represent the [f] of foreign words and a redactional change in the representation of *e* before another vowel were orthographic only. Consequently, there is really no such thing as a diachronic linguistics of Classical Armenian in these centuries. According to Kouwenberg (2012:438), the same is true of Standard Babylonian, which lasted for at least 1300 years. Might this also have been the case with BH? Indeed, Rezetko and Young (2014) have illustrated in great detail the kinds of variation, both lexical and grammatical, that one finds in our BH texts. But variation is one thing, diachrony another. Note that the Hebrew Bible shares with texts such as Homer and the Rigveda what I have termed “immanent diachrony” (Klein and Condon 1993:36). In this way it differs from a text such as the Gothic Bible, which tradition says was translated by a single individual at a particular point in time. But even in the case of Homer, aside from the fact that the *Odyssey* is thought to have been composed at a somewhat later time than the *Iliad*, people have argued back and forth about which parts of the *Iliad* are old and which more recent.⁵ In the case of the Rigveda, arguments for internal chronology are made less on linguistic grounds than based on the structure and to a certain extent genre of its constituent parts.⁶ This text

⁵ Cf., *ex multis*, Kirk (1960) and Tichy (2010).

⁶ Thus, the most thoroughly elaborated attempt to formulate an internal chronology is that of

consists of ten books or “Maṇḍalas”. Seven of these are composed by distinct bardic families, an eighth is composed completely of hymns to Soma, which are only sparsely represented in the rest of the collection, and two are composed by multiple small groupings of bards. Traditionally, the “family books” (II–VIII) are taken to represent the archaic core of the collection, and the soma hymns of the ninth Maṇḍala are thought, quite reasonably, to represent an inchoate and subsequently abandoned attempt to rearrange the collection according to deity. The tenth Maṇḍala stands in close proximity to the second oldest text in India: the Atharvaveda, a parallel corpus to the Rigveda stemming from a lower stratum of society practicing a form of popular religion, whereas the Rigveda represented the higher brahmanic religion. Many of the hymns of the tenth Maṇḍala are from the perspective of genre Atharvavedic in character, and many linguistic forms found in this Maṇḍala do not appear elsewhere in the Rigveda, while appearing frequently in the Atharvaveda and later. Consequently, here alone do we find on the surface what looks like real diachrony relative to the rest of the collection.

EVIDENCE FOR PERIODISATION

Considerations of this sort suggest that it might be a priori difficult to ferret out diachronic strands in the language of the Hebrew Bible. Let me suggest, as a linguist, the kinds of evidence I would look for in order to be able to tease out such layers: 1) phonological change; 2) grammatical change (both morphological and syntactic).⁷ The appearance at a certain point of constructions known to occur in the next-attested stage

Arnold (1905), who bases his results (which are not universally accepted) on metrical structure. Arnold was also very careful to separate the “popular Rigveda” (largely but not exclusively hymns from the tenth book) from the rest of the collection.

⁷ Other changes, including lexical innovations, semantic development of existing lexical items, and the evidence provided by Aramaic loan words and semantic loans are not as important for establishing a linguistic period, which for me relates primarily to systemic diachronic change. Note that a loanword may be late, if it is found first in a demonstrably late book such as Esther, and under the right circumstances it may be considered a BH word (i.e., if it is not used specifically as a foreign word, as is *’āḥašdarpān**, Est 3:12), but, paradoxically, that does not mean that it is an item of “Late Biblical Hebrew”, since I take this to designate a linguistically-determined *état de langue*, not a simple chronological point at which Hebrew was used.

of the language, viz. Mishnaic Hebrew (MH), is potentially also significant but must be treated on a case-by-case basis. Of these, phonological change does not seem to be a significant factor in our BH corpus. In this way the Hebrew Bible differs from, say, the Avesta, the sacred text of Zoroastrianism, where the two strands of Old and Young Avestan, separated temporally by perhaps half a millennium, are recognisable by significant phonological differences (Hoffmann and Forssman 2004:32–34).

Grammatical change presents a greater challenge. This is one of the areas that a priori might be thought to show the greatest promise for diachronic development, and indeed Rezetko and Young (2014) do discuss grammatical features, but they do so for the most part focusing on variation rather than change. And it is their treatment of grammatical features more than anything else which leads me to conclude that their book contains very little of what Indo-Europeanists would call diachronic linguistics. It will be useful here to distinguish two aspects of what is traditionally labelled “historical linguistics”: microdiachronic linguistics and macrodiachronic linguistics (Klein 1999:92). The first deals with language change in the interstitial time span of generation to generation; the latter deals with time intervals measurable not in generations but in centuries. The former yields the basic material for change reflected in competing variants; the latter yields linguistic saltations in which the victorious variants are on display, but not the internecine competitions which have produced them. The former has its counterpart in sociolinguistics, the latter in language history. The two are complementary, their dichotomy paralleling the great nineteenth century argument between the adherents of the *Wellentheorie* and the *Stammbaumtheorie* as models of language change. The former yields facts on the ground, the latter ultimate outcomes. Rezetko and Young (2014) is really about microdiachronic variation, not about macrodiachronic change. The latter involves linguistic processes.

DIACHRONIC LINGUISTIC PROCESS IN BIBLICAL HEBREW

Let us consider one example of a real diachronic change which, however, had been completed well before the period of BH. The Hebrew relative pronoun *'āšer* originally meant “place”, cf. Aramaic *'ātar* “idem”. Its usage must therefore represent ultimately

a generalisation beginning with the specific grammaticalisation of place-relativisation. It is not hard to see that this was fostered by the tendency in the Semitic languages generally to use anaphoric pronouns following relatives. Cf. Gen 21:17 *kī šāma' 'ēlohīm 'el qōl han-na'ar ba-'āšer hū' šām*, which must originally have meant “For Elohim has heard the voice of the child in the place he was there”, whence, with desemanticisation of *'āšer*, “in the [unspecified antecedent] he was there”, ultimately being understood as “... where he was”.⁸ From here, *'āšer* must have become generalised as a full-service relative pronoun. While this process is well outside our ken, another, also involving a relative pronoun, is in fact within our sights. As is well known, in addition to *'āšer*, BH possesses a relative particle of more limited distribution, *šeC-*, manifestly present in at least some dialects of Hebrew in the late second millennium B.C.E.⁹ This particle was in all likelihood widespread in the spoken language in those dialects where it occurred and thus represents both a diatopic and diaphasic variant of *'āšer*.¹⁰ Even if we might wish to eschew a characterisation of

⁸ In generative terms this might be said to represent *in situ* relativisation, hence without movement and without leaving a trace. The surface structure here is in fact what classical transformational grammar would posit as a deep structure prior to any movement rules. The retention of *hū' šām* in the surface structure must be related to the twin facts that Biblical Hebrew has a strong preference for nominal clauses and, unlike English, prefers not to leave a substantive verb dangling at the end of the sentence (“where he was”). Consequently, *hū' šām* stands for *hū' hāyā šām*, thereby facilitating the grammaticalisation of *'āšer* as a relative pronoun.

⁹ Cf. Judges 5:7 (Song of Deborah) *'ad šaq-qamtī* (2x). On the *a*-vocalism see Huehnergard (2006), who notes that the comparative method assures that the reconstruction of the “place”-word is **'aθar*, which is actually attested, *mutatis mutandis*, in its Babylonian vocalism as *'əšar/'ašar*. One may therefore assume that one or more (probably Northern) dialects retained this form as such and reduced it to *šaC-*.

¹⁰ I am inclined to follow Huehnergard (2006) in seeing what I write *šeC-* as a very early reduction of *'āšer*. For me the decisive factor is the synchronic morphological gemination that *šeC-* effects on a following word. Such gemination can only be owing to two factors: 1) the bound, proclitic status of *šeC-* and 2) assimilation of an original final consonant of this word to the initial consonant of its host. If *šeC-* is in fact a prosodic reduction of *'āšer*, then this assimilation, hence gemination, is immediately explained. The second aspect of this proclitic reduction, aphaeresis of *'ā*, is also in line with what one finds in grammaticalisation processes generally and is paralleled elsewhere in Hebrew in the form *naḥnū* “we” beside *'ānaḥnū* “idem”. The critical point in Huehnergard’s resuscitation of this, the oldest etymology of *šeC-*, is his demonstration, based on the application of the comparative method, that *šeC-* cannot economically be equated with Akkadian *ša*. The

šeC- as late,¹¹ it may be said, at the very least, that it is a very serious competitor of *'āšer* in Qoheleth (68x *šeC-* vs. 89x *'āšer*) and is used to the near-exclusion of *'āšer* in Song of Songs (32x vs. 1x *'āšer*), books that Rezetko and Young (2014) characterise as “peripheral” BH. As is implicit in its reduction to clitic status, *šeC-* from its inception became capable of proclitic univerbation with *l* + pronominal endings, resulting in such collocations as *šel-lī*, *šel-ləkā*, *šel-lāk*, *šel-lō*, etc. Originally these structures meant “which (is) to me, to you, etc.”, whence “of me, of you, my, your, etc.”. Cf. Song of Songs 1:6 *karmī šel-lī* lit. “my vineyard, my own” (with pleonastic first person possessive marker signalling here strong contrasting force, since the larger context is adversative: *sāmunī noṭērā 'et hak-kērāmīm/karmī šel-lī lo' nāṭārtī* “They made me keeper of the vineyards/My own vineyard I did not keep”). At a certain point, however, within the period of BH, these structures became metanalysed from **šel-lī*, etc., to *šell-ī*, with extraction of a new lexical item *šel* (with degemination in absolute final position), which was to remain prominent as a *nota genitivi* throughout the entire ulterior history of the language. In fact, we can trace the steps which led to the metanalysed form within our texts.

In Jonah 1:7–8 we find a set of parallel passages in which the preposition *bē-* followed by a relative pronoun forms a subordinating causal conjunction. In the first clause the relative pronoun is *šeC-*, in the second clause *'āšer*:

standard etymology of *šeC-* does relate these two directly but is at a loss to explain the morphological gemination. In passing, the reduction of *'āšer* to *šeC-* has all the earmarks of an allegro pronunciation in colloquial speech, and therefore supports the frequently expressed idea that the former belongs to the more formal, written style, which is clearly what is on display in the overwhelming bulk of the Hebrew Bible, while the latter, as a colloquialism, appears to have “broken through”, aside from scattered usages here and there throughout the *Tanak*, particularly in those books, such as Song of Songs and Ecclesiastes, which appear to reflect a more colloquial style or at any rate a form of expression at odds with that of the *Tanak* as a whole.

¹¹ It occurs in Genesis, Judges, and Kings (books consensually thought to be early), although not in early inscriptions. At the other end of the spectrum, it is attested to the exclusion of *'āšer* in the Bar Kochba texts from Naḥal Hever and Wadi Murabba'at. In the non-biblical Qumran texts 3Q15 (Copper Scroll) and 4QMMT (394–399) it occurs to the near-exclusion of *'āšer*, and in the Mishnah it is the regular, indeed exclusive relative pronoun except for biblical quotations and allusions (see Holmstedt 2012:113–119).

Jonah 1:7–8

lěkū wě-nappīlā gōrālōt wě-nēdě‘ā bě-šel-lě-mī hā-rā‘ā haz-zo‘t ... (8)

... haggīdā-nnā‘ lānū ba-‘āšer lě-mī hā-rā‘ā haz-zo‘t

“Come, let us cast lots that we may know on whose account is this evil ...

(8) ... Tell us, please, on whose account is this evil.”

In each case the subordinator is followed by the possessive interrogative *lě-mī* “to whom?, whose?”, and the difference in usage, appearing so close together in the text, must represent stylistic variation. Indeed, in verse 12 Jonah acknowledges in reply that the great storm that has arisen is *bě-šel-lī* “on my account”. In none of these instances is there reason to assume anything other than a banal case of *lě-*, in one instance followed by a suffixal pronoun (*lī*) and in the other proclitic to an interrogative pronoun (*lě-mī*). However, in Qoheleth 8:17 we find *bě-šel ‘āšer* in which *šel* is used independently, without any possessive suffix, as part of a historically pleonastic causal conjunction: *bě-šel ‘āšer ya‘āmol hā-‘ādām lě-baqqēš wě-lo‘ yimšā‘* “because (even if) man takes the trouble to seek (it out), he does/will not find (it)”. This can only mean, however, that the metanalysis is complete in the language of the author of this book. Since this is the only place in the *Tanak* where independent *šel* occurs, it must be the case that we are witnessing the very beginning of the establishment of this word as an independent lexical item. This is indeed full-blown diachronic change in BH, and on this basis alone one is justified in assigning this book to a late stage of BH.

These are the kinds of processes which, for me, signal real diachrony. The more evidence of such processes we can find within the attested time span of BH, the firmer will be our confidence in linguistic change. Such processes have the nature of unidirectional vectors: they point outward from a starting point to an end point. What is more, they represent internal linguistic change completely independent of the dating of texts. It is, on the contrary, the dating of texts that emerges or at least may emerge from these changes. And it is precisely these kinds of changes that we, as historical linguists, should be looking for in the continuum that is BH.

SYNCHRONIC VARIATION IN BIBLICAL HEBREW

As opposed to these types of changes, the phenomena that Rezetko and Young (2014) investigate fall rather under the rubric of synchronic variation. Thus, to take just one example, the third person masculine plural possessive endings *-ōtām* and *-ōteyhem* have the appearance of variant patterns that coexisted throughout the period of BH. Instructive in this regard is Deut 7:5, the command to the children of Israel, once they enter the land of Canaan, to remove all physical manifestations of the *'avodā zārā* of the nations they encounter there: *mizbēḥoteyhem tittošū umaššēbotām tēšabbērū* “You shall uproot their altars and smash their sacred pillars”, which to my eye and ear has all the hallmarks of stylistic variation.

As noted in footnote 2 above, I assign lesser weight to lexical variation and semantic variation in existing lexical items as markers of linguistic change and periodisation. Much of Rezetko and Young (2014) deals with this material. And in fact, using the techniques of variational analysis, they find the patterns of attestation to be not sufficiently clear to allow an unambiguous periodisation of the biblical language. Aramaic loan words (not studied by the authors in the book under consideration) may perhaps be better indices, at least as chronological markers, but in part these are redundant anyways, because the largest concentration of them occurs in texts which are already ascertainable as late on non-linguistic grounds. It is apparent that Aramaic and Hebrew were spoken in contact from an early period but that this contact must have intensified over time, reaching its zenith during the period of Achaemenid hegemony. This variation in degree of contact over time may be actually reflected in our text, in sociolinguistic terms. Thus, when Laban and Jacob make a pact in Gen 31:47, the author tells us that Laban called the sacred stone heap set up to confirm the agreement *yēgar śāhādūtā*, while Jacob called it *gal'ēd*. Now, if we are entitled to impute verisimilitude to this story, then it is clear that Jacob and Laban could communicate perfectly well. After all, the former lived with the latter for more than fourteen years, he asked Laban for Rachel as well as for cattle for his livelihood. What is more important, however, is that the author feels obliged to present the different designations as if they were in two mutually unintelligible languages. This

can only mean that for him they did indeed have this status. Over time, however, we have every reason to believe that when Judea was drawn into the sphere of the Achaemenid empire and Aramaic became the administrative language, it acquired the prestige that comes with political power and must have exerted a lasting influence upon Hebrew. Even if a certain “loanword fatigue” is detectable in discussions about the significance of this influence for the diachrony of BH, the same cannot be said for demonstrable syntactic calques, which presuppose the most intense degree of contact. Thus, the demonstration by Pat-El (2012:254–259) that *bě-šel ’āšer*, which we have accorded such importance in the creation of an independent item *šel* in Hebrew, must be a calque on Aramaic *b-dyl d(y)* is evidence of the strongest type for intensive contact between the two languages and yet one more character¹² that would lead us to suppose a post-exilic dating of this text.

MISHNAIC HEBREW AS A TOUCHSTONE

Also suggestive (but only on a case-by-case basis) would be the loss in some part of the corpus of features common elsewhere in the Bible but non-existent in MH or, conversely, the presence of features widespread in MH in certain books of the Bible. Thus, for example, if it were the case that the narrative *waw*-consecutive construction did not occur in some book(s) we know on non-linguistic grounds to be late or the periphrasis of *hāyā* with a participle to indicate consuetudinal action in the past (type *hū’ hāyā ’ōmer*) did occur in some such book(s), then we could be pretty certain that these would represent a late stage of BH; for we know that narrative *waw*-consecutive pervades BH generally and that the periphrastic consuetudinal construction is not a feature of BH (its occurrence in I Kgs 22:35 and 2 Chr 18:34 is durative but not consuetudinal). Note that the prehistory of MH is irrelevant to these considerations. There can be no doubt that this stage of the language is a continuation of one or more spoken dialects tracing their roots to the very origin of Hebrew itself. But any feature of MH not itself a characteristic of BH generally that penetrates the literary language

¹² By “character” I mean a linguistic feature at any level that may serve as a datum in a discussion of linguistic change or periodisation.

only in books of the *Tanak* that can justifiably be considered late on non-linguistic grounds must be said to be late relative to the time-stream of BH.

PROBLEMS OF COMPOSITION AND TRUSTWORTHINESS OF SOURCES

However, as Rezetko and Young (2014) repeatedly state, when it comes to the actual dating of texts, these considerations apply, strictly speaking, only to a text that has been handed down in pristine form. If, for example, the language of Judges were the *ipsissima verba* of the author of this book, then we could view its language as, by definition, representing his particular idiolect or more generally the genre-specific literary language of his generation. But few if any scholars of BH would make such a claim. Ever since at least the nineteenth century it has been clear that there are multiple strands within the Bible: the two stories of creation; the contradiction in the flood tale whereby Noah was told by the Deity under a more generic name (E) to bring specifically two (male and female) of each species into the ark, contradicted four verses later by his being commanded by the deity under a more particularistic name (J) to bring seven pairs of clean animals but only two of the unclean; the differences in the two forms of the Ten Commandments, particularly the Sabbath commandment, in Exodus and Deuteronomy, which certainly looks like someone tried to invest an ancient cultural institution rooted in the economic principle that one should not work one's slaves and beasts of burden to death (Deuteronomy) with a theological underpinning (Exodus); the inconsistencies in the Bil'am story in Numbers, which cannot seem to decide whether it wants to present Bil'am as a sage, humble servant of the Most High, or a fool, inferior in insight to his donkey. All of these make it abundantly clear that a text once circulated within a community was reworked over and over again by different editors, and copied by different scribes, with no attempt to produce a cohesive text, almost certainly over a considerable period of time and most likely in different places.

Over and above these considerations stands the issue of trustworthiness of sources. There can be no doubt about the care with which the Masoretic text was treated from a certain point forward; however, its origin is centuries removed from the composition

of its constituents, bearing witness to a time of consolidation of religious practice far removed from the days when *'iš hay-yāšār bē- 'eynāyw ya āšeh*. The variant ancient versions of the Septuagint are based on Hebrew texts current in Alexandria in the third century B.C.E., some of which may have been early versions of the Masoretic text. The same may perhaps be true of the Samaritan Pentateuch. The Dead Sea Scrolls constitute collectively the one pristine witness that we have, tied, at least pragmatically, to a particular place and, roughly, time, and their value is immense; but they provide only limited portions of the *Tanak*. Finally, inscriptions from the monarchic period are invaluable, but their corpus size is very limited. Other possible witnesses to the text not considered by Rezetko and Young (2014), such as *Targum Onqelos* and the many citations from the entire *Tanak* found in both the Babylonian and Jerusalem Talmud, are palpably late and, at least in the latter cases, presuppose an already fixed text.

TEMPERING OUR CONCLUSIONS

So where does all of this leave us? Is it possible to produce a scientifically demonstrable, as opposed to an intuitional, periodisation of BH that is based on more than just one or two features? As an outsider, my view is that we may have to be satisfied with something less than this. There can be no doubt that the underlying linguistic system of which BH is a stylistically more or less rigid reflection changed over eight hundred years or so. It is also clear to me that such books as Song of Songs and Qoheleth, irrespective of when they may have been composed, have the special status they do¹³ precisely because in them some of the barriers elsewhere erected between the literary language and the living language have broken down. This can be

¹³ Although the case of Song of Songs is equivocal, it is hard not to suppose that the author of Qoheleth was working within the intellectual ambiance created by the great philosophical movements of fourth century B.C.E. Greece. However we understand its aberrant style relative to other books of the *Tanak*, this would mean that Qoheleth is a late book, and as I hope to have shown here, with regard to at least one of its features (the employment of a metanalysed independent *šel*), presupposes the kind of processual linguistic change that is so often associated with language periodisation and therefore can be assigned to Late Biblical Hebrew.

imputed to any number of factors, beginning with the societal strata from which they emerged and extending to dialect, place of composition, time of composition, and genre. Otherwise, the more or less closed nature of the BH literary language affords us few opportunities to see real systemic diachronic change at work. As far as I can see, the only kinds of authoritative statements we can make are statements about the history of Hebrew in general. Here what can be said relates first and foremost to “drift”, Sapir’s term for the most general features characterising a language in a teleological sense over the long course of its history (1921:147–170). On this level much can be said. Thus, Hebrew shows a clear movement from a more synthetic type of structure to a more analytic type. This is seen in the loss of the Proto-Semitic case endings, the loss of the allative *hē* over time (despite the difficulty of demonstrating this loss within the period of BH), the development of an analytic genitive involving *šel*, the shift from a pro-drop type where the pronoun is implicit in the verb and not needed unless the subject is topicalised or focalised to a type with routinely expressed first and second person pronouns (at least in the present tense), the ultimate replacement of verbal object suffixes with *’et* + object suffix, the development of a present tense form of the verb from the old participle, the development of a more completely specified set of subordinators, replacing the polysemous *wě-* of the biblical text, and the development of a periphrastic consuetudinal past tense utilising the verb *hāyā*. Of a different nature is the development of a tense system out of an aspect system, a phenomenon no doubt in part the result of the assimilation of the Hebrew verbal system to those of the languages of the diaspora where Jews have lived over the ages.

IN THE VOID: BIBLICAL HEBREW, MISHNAIC HEBREW, AND TRAJECTORY

However, there is another factor in language change which may be relevant to the question of the dating of linguistic features in BH, although it is, cross-linguistically speaking, probably not infallible. In addition to “drift”, which refers generally to very long-term linguistic change, I would like to introduce the parallel but distinct notion of “trajectory”, by which I mean the linguistic change between successive stages of a

language. Most relevant here is the relation between BH and MH. Thus, Rezetko and Young (2014) study a number of characters that have often been thought by Hebrew Bible scholars to be late, either in and of themselves or in opposition to another form or set of forms. In nearly all such cases Rezetko and Young (2014) argue, based on variation analysis, that the lateness of the character in question is not clearly demonstrable within the BH corpus. However, in a number of such instances the item generally assumed to be late shows in MH a much more robust rate of occurrence relative to its variant(s) than it does in BH, in one instance (the employment of the *qal* of 'md in the sense "arise") even essentially driving its BH competitor in this value (the *qal* of *qwm*) out of business.¹⁴ In such cases it is clear to me that we are dealing with a trajectory, which may be interpreted to mean that the "late form" was indeed in the early stages of competition with its variants within the BH corpus and by the time of MH had either clearly won out over its competitors or was in a much more advanced stage of acceptance than in the earlier period. This perhaps highlights the limitations of variation analysis with regard to the later books of the *Tanak*. That is, if we consider the latest books of the corpus to have been produced in the fifth to mid-fourth centuries B.C.E. and to have been completed in their essential features by about 400 B.C.E., this may give us only 100 to 150 years at most until the Dead Sea Scrolls, which play an important role in Rezetko and Young's (2014) study. It is quite possible that this time period is simply not sufficiently long to provide us with a clear picture of the competition among variants. On the other hand, if we date the Mishnah to about 200 C.E., the period of nearly a half-millennium between the two corpora would have been sufficient to make clear what the winners and losers in this competition were, or at least to show us a more advanced stage of the competition. We might think of the temporal hiatus between the latest chronological stages of BH and the Mishnah text as

¹⁴ The Mishnaic data for allative *hē* <-āh> is equivocal in this regard, because this item has become lexicalised in so many words. Rezetko and Young (2014) state that according to Accordance, allative *hē* occurs 83x in the Mishnah. When I pulled down my copy of this text and started reading from the beginning of *Kēlīm*, I found nine occurrences of *lēma 'ālā mē-/min* "exceeded by" (lit. to the above from) in the first four chapters. The lexicalisation of this form (identifiable by the hypercharacterisation of the allative meaning by *lē-* as well as the preposition which follows it) had already occurred in BH. I presume that forms such as this are not included in the figure of 83 given in Accordance.

being similar to (albeit longer than) the time period between about 1460 C.E. and 1620 C.E. in the history of English, during which the systemic replacement of 3rd pers. sg. -*th* by -*s* occurred (Rezetko and Young 2014:236, fig. 7.3). That is, many of the cases studied by Rezetko and Young (2014) showing no decisive pattern in BH must reflect the beginnings of diachronic change (cf. the well-known doctrine of synchronic variation as a prerequisite to diachronic change¹⁵). By incorporating the notions of trajectory (between BH and MH) into the overall picture of diachrony in BH, we lessen the gap between the findings of biblical scholars such as Hurvitz and those of Rezetko and Young.¹⁶ Given the tenuous nature of terms such as “early”, “middle”, and “late” in the periodisation of languages,¹⁷ I would not want to assert that these characters are indicative of a linguistically defined entity “Late Biblical Hebrew”, but I feel comfortable in saying that they are chronologically late within the literary language.¹⁸

¹⁵ Cf. again, *ex multis*, Anttila (1972:47).

¹⁶ Rezetko and Young (2014) freely admit in the case of many characters that there is some tendency toward late usage. What they deny is not the fact of late usage but rather the inverse conclusion that books not showing such usage are necessarily early, a point with which I wholeheartedly concur. Variation analysis is very helpful in making sense out of characters that are of fluctuating occurrence within a textual tradition, but it has nothing to say about evidence that is not there, particularly in a corpus where editorial and scribal intervention have produced massive fluidity in the form of individual books. Note that I have said virtually nothing about the dating of texts in this paper. That is because diachronic linguistics and textual dating are separate enterprises, even though the results of the first can be useful to the second.

¹⁷ For example, often the designation “late” refers to nothing more than a chronological period in the history of the attestation of a language, without regard to linguistic features. By this nonlinguistic conception, “Late Biblical Hebrew” is simply the Biblical Hebrew of the latest books of the *Tanakh*.

¹⁸ This is by no means to deny the possibility that intensive work with books which are known to be late may ultimately succeed in assembling a range of morphosyntactic constructions characteristic of these books that will provide a solid linguistic basis for characterising their language as “Late Biblical Hebrew”.

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