
ANCIENT HEBREW WITHOUT AUTHORS¹

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ABSTRACT

Current scholarship on the history of the Hebrew Bible text sees the composition of biblical literature as a long, drawn-out scribal process of rewriting, to which many individuals contributed. This approach is in harmony with the evidence for variability in the scribal transmission of distinctive (less common) linguistic features in non-MT biblical manuscripts and parallel passages in the MT. The Text-Critical paradigm contrasts with the MT-Only paradigm which presupposes the composition of biblical books or identifiable parts of them by single authors at specific dates. This article focuses on the unusually well-attested text MT 2 Kings 25:1–12// LXX 2 Kings 25:1–12// MT Jeremiah 39:1–10// LXX Jeremiah 39:1–10// MT Jeremiah 52:4–16// LXX Jeremiah 52:4–16 where it is discovered that not a single distinctive linguistic feature is shared by all texts. It concludes with suggestions as to how the application of this approach can help reformulate some of the questions scholars ask in their study of ancient Hebrew.

INTRODUCTION: TWO PARADIGMS

One of the major challenges facing scholarship on ancient Hebrew is how to re-integrate it with mainstream scholarship on the Hebrew Bible. Despite the ongoing discovery of Hebrew inscriptions, and the preservation of the non-biblical Dead Sea Scrolls, albeit only a few of them preserving a significant proportion of text, over 80% of the evidence for ancient Hebrew comes from manuscripts of the Hebrew Bible (Rezetko and Young 2014:63). It should be evident, therefore, that taking seriously scholarship on the nature of these manuscripts is the essential first step in the study of the linguistic evidence they present.

¹ I would like to thank Professor Jacobus Naudé for his kind invitation to present the paper, on which this article is based, at the Society of Biblical Literature Annual Meeting in Atlanta in November 2015. I would also like to offer my thanks, as always, to Robert Rezetko and Martin Ehrensverd, for their help in writing this article, which represents our current thoughts on these issues.

In *Historical linguistics and Biblical Hebrew*, Robert Rezetko and I pointed out some areas in which scholarship on the Hebrew language has not kept pace with developments in wider biblical studies (Rezetko and Young 2014). In that book, we told a parable to illustrate why it is not possible to undertake historical linguistic study of Hebrew without first understanding the nature of the sources, which I reproduce here.

A linguist decides to investigate the language of the prophet Micah, which he naturally assumes is evidenced by the biblical book of Micah. He knows that the prophet Micah lived ca.700 B.C.E., and so considers that his analysis will provide valuable insight into the language used in Micah's time. The linguist does his analysis, using the finest linguistic methods available, and presents his results. But the linguist has made a fundamental error: he has analysed the English language of the King James Bible's book of Micah. Because he was unaware of the nature of the linguistic evidence he was analysing, he made the false assumption that that evidence would give him an insight into the language of the prophet Micah, when in fact the analysis is worthless, despite its methodological brilliance (Rezetko and Young 2014:59).²

This article bears the title "Ancient Hebrew without authors". As we documented, again in *Historical linguistics and Biblical Hebrew*, traditionally scholarship on ancient Hebrew has proceeded on the assumption that the manuscripts of the Hebrew Bible provide us with detailed evidence of the language of individual authors, writing in specific chronological periods.³ This is a natural common-sense assumption, based on the way we experience books in our modern world. It seems obvious that if a biblical book was written early, it is an example of early language. If a book was written late, it reflects the language of the late period in which it was written. If the prophet Micah (to use Micah again) wrote ca.700 B.C.E., then the language of the book of Micah is the Hebrew of 700 B.C.E. These obvious statements have been presupposed by almost all scholars of the Hebrew language. More particularly the

² This "somewhat ridiculous story" (Rezetko and Young 2014:59) is intended solely to make the point about the importance of understanding the nature of the sources being analysed, not as a story about a mistake any linguist has actually made.

³ Rezetko and Young (2014:68–71, 83–110) provides a detailed discussion of the views of the major scholars, with quotes from their publications.

Masoretic Text (MT) of the Hebrew Bible is taken as reflecting in detail the language that left the pens of the original authors of the biblical texts. Thus I label this approach as “the MT-Only paradigm”. However, these obvious presuppositions of language scholars are diametrically opposed to the current consensus of mainstream Bible scholars as to the production history of the Hebrew Bible.

There is substantial agreement between experts on the main points of a model of the emergence of the biblical text. Rather than provide an almost endless list of quotes from scholars to illustrate this well-known consensus, in case anyone is not aware of it, I will focus on the work of two of the leading experts, Emanuel Tov and Eugene Ulrich, who have provided particularly authoritative articulations of this consensus.⁴

Emanuel Tov’s *Textual criticism of the Hebrew Bible* is widely considered to be the standard handbook on the text of the Hebrew Bible. In the third edition of his work, published in 2012, Tov says, for example, that “one thing is clear, it should not be postulated that **MT** [the MT] better or more frequently reflects the original text of the biblical books than any other text” (Tov 2012:11–12); “Most of the biblical books were not written by one person nor at one particular time, but rather over many generations” (Tov 2012:166); “The assumption of textual plurality in that period [i.e., of the Qumran scrolls] is now accepted among scholars” (Tov 2012:186 n. 79); “The textual diversity visible in the Qumran evidence from the 3rd century BCE onwards is probably not representative of the textual situation in earlier periods, when the text must have been much more fluid” (Tov 2012:166 n. 24); “The textual evidence does

⁴ The fact that Tov and Ulrich are well chosen as representatives, since they are currently the leading scholars representing the consensus on the history of the text of the Hebrew Bible, and that they both share similar views, despite inevitable differences in detail and emphasis, is acknowledged by Tov and Ulrich themselves. For example, Ulrich has recently stated: “Currently, Emanuel Tov and Eugene Ulrich, both of whom were students of both Cross and Talmon, continue to explore ways of envisioning the history of the biblical text in light of the complete publication of all the biblical scrolls. Tov’s wide-ranging and detailed analyses of the Masoretic and Septuagint textual traditions have justifiably achieved the current position as the most comprehensive explanation of the state of the art” (Ulrich 2015:312–313). For Tov’s endorsement of Ulrich’s work and adoption of his terminology, see Tov (2012:174, 182 etc.). There are of course many other fine works by scholars representing the consensus view on the history of the text of the Hebrew Bible, but they commonly acknowledge the seminal nature of Tov and Ulrich’s work for their own.

not point to a single ‘original’ text, but a series of subsequent authoritative texts produced by the same or different authors ... the original texts(s) remain(s) an evasive entity that cannot be reconstructed ... Some biblical books, such as Jeremiah, reached a final state more than once ... the original text is far removed and can never be reconstructed” (Tov 2012:167–169); “When creating new copies, scribes altered the transmitted text, first as authors / editors-scribes, and later as copyists-scribes. Editorial freedom ... is also reflected in changes in orthography and morphology” (Tov 2012:184).

Eugene Ulrich has very recently published an authoritative statement of his current views in *The Dead Sea Scrolls and the developmental composition of the Bible*. In agreement with what we have heard from Tov, Ulrich says: “Each book is the product not of a single author, such as Plato or Shakespeare, but of multiple, anonymous bards, sages, religious leaders, compilers, or tradents. Unlike much classical and modern literature, produced by a single, named individual at a single point in time, the biblical books are constituted by earlier traditions being repeated, augmented, and reshaped by later authors, editors, or tradents, over the course of many centuries. Thus the text of each of the books is organic and developmental, a composition-by-multiple-stages, sometimes described as a rolling corpus” (Ulrich 2015:2). He states that it is an “undisputed fact” that “virtually all the books now recognized as the Hebrew Scriptures ... are the late literary results of a complex evolutionary process of composition” (Ulrich 2015:201) and he states, “with a few possible minor exceptions, there is no non-rewritten Scripture” (Ulrich 2015:207).

LINGUISTIC EVIDENCE FOR THE TEXT-CRITICAL PARADIGM

Such a view of the nature of the biblical manuscripts leads to the clear expectation that minor details of the texts, such as linguistic features, were not copied carefully and exactly in the scribal transmission and reshaping of the biblical compositions. In contrast to the MT-Only paradigm that is still common among Hebrew language scholars, this can be labelled “the Text-Critical paradigm”. Like any good theory, the

predictions which arise from it are completely fulfilled when we examine the actual evidence. The linguistic evidence is found to be extremely fluid, specifically the more distinctive, less common standard linguistic forms that scholars have previously used to create a profile of the language of individual authors.⁵

The first body of evidence which indicates the linguistic fluidity that characterises the transmission of the text of the Hebrew Bible is the variant linguistic profiles of non-MT manuscripts of biblical compositions. By the very late date of the Qumran manuscripts, the last three centuries B.C.E., a significant minority of the manuscripts are quite close to the MT (depending on one's criteria, anywhere from ca.16%–ca.40%; see Young 2005a:124–125; cf. Lange 2009:45–47). However, there are also plenty of manuscripts with variant linguistic profiles among the Qumran scrolls. In addition, like the MT, the Samaritan Pentateuch represents medieval manuscripts that preserve a textual tradition already attested among the Qumran scrolls, one which preserves a variant linguistic profile to the MT. Rezetko and I have at various times written studies of the variant language in these texts for the Pentateuch poetry (Young 1998), Judges (Rezetko 2013), Samuel (Rezetko and Young 2014:171–210; 453–591), Song of Songs (Young 2001a), and Isaiah (Young 2013a), for which we already long ago had the study of Kutscher (1974). Kutscher of course set the tone for the early reception of the variant linguistic forms at Qumran by starting from the now indefensible presupposition that the MT was essentially the original text of the Hebrew Bible, and that therefore any non-MT text must be some sort of vulgar corruption of the true text (see Young 2013a:103–104; Rezetko and Young 2014:69–70). This suggestion is countered, however, by the similar evidence of high linguistic variability within the MT itself, as evidenced by the parallel passages, i.e., passages found in two different places in the MT (Rezetko and Young 2014:145–169, 413–452).

⁵ For the definition of “less common” linguistic features, see below in the section “Less common linguistic features in the triple parallel”.

THE 2 KINGS 25// JEREMIAH 39// JEREMIAH 52 PARALLEL

The main focus in this article is one of these parallel passages, one that is unusual in the MT for being a triple parallel. 2 Kings 25 verses 1–12 are not only parallel with Jeremiah 52:4–16, but also with Jeremiah 39:1–10. These passages concern the events of the end of the kingdom of Judah at the hands of the Babylonians under Nebuchadnezzar, the triple parallel particularly covering the capture of King Zedekiah of Judah and the destruction of the Temple and other parts of Jerusalem. Rezetko and I have already dealt with the evidence for less common linguistic forms in the MT of the parallel texts, including the Kings-Jeremiah parallels (Rezetko and Young 2014:145–169; 413–452). What is different here is, first, that the study focuses only on the verses covered by the triple parallel, whereas in the previous study the evidence from Jer 39 was only introduced in a supplementary way to that from the Kings-Jer 52 parallel. No statistics were presented in the earlier study that just focused on the triple parallel. Second, this study includes all text in the triple parallel, including verses, such as Jer 39:3, that are restricted to only one part of the material. This is more in line with the usual understanding of “parallel passages” which takes into account all the material in parallel sections. Third, this study introduces further data from the Greek (LXX) texts of the parallel passages.⁶

What do scholars mean when they say that these are “parallel passages”? On the one hand it is obvious to all commentators that the texts agree with each other to such a high degree that there must be a literary relationship between them. They are too similar for it to be a coincidence.⁷ On the other hand, though, it is clear that the texts disagree with each other on many details. In fact, as is typical with the text of the Hebrew Bible in the B.C.E. period, the more texts, the more variants. The Greek

⁶ Note that due to the placement of the oracles against the foreign nations in the middle of LXX Jeremiah, rather than at the end, as in MT Jeremiah, LXX Jer 39 is actually numbered as Jer 46. However, to avoid confusion and to make the connection with MT Jer 39 transparent, I will continue to talk of LXX Jer 39, meaning the LXX version of MT Jer 39.

⁷ “The agreement among these four texts [of Kings and Jer 52] certainly argues for some literary dependence. However, it also seems apparent that all of the texts have undergone further development independently of one another” (Person 1997:95). “Despite some significant differences among the accounts, the word-for-word parallels imply, beyond any question, that all of the passages are literarily dependent” (Müller et al. 2014:109).

translations of each of the passages are quite literal renditions of yet further variant versions of the parallel, so rather than having one text in six forms, it is more correct that we have six different, albeit closely related, texts. As Tov and Ulrich have said, biblical texts were commonly not copied exactly, but were the subject of constant revision, so that it can even be suggested that each manuscript of a composition was to a greater or lesser degree an independent rendition of it. There is no space available in this context to go through the many variations in the triple parallel even in a cursory manner.⁸ However, as an illustration of how even very simple things can be said in different ways, in 2 Kgs 25:4// Jer 39:4// Jer 52:7 the texts tell us how Zedekiah fled Jerusalem by night. MT Kings simply says “by night”. LXX Kings says “they went out by night”, and LXX Jer 52 agrees. MT Jer 52 says “and they went out from the city by night”, using a different grammatical form of “by night” than MT Kings. MT Jer 39 agrees with MT Jer 52 but says “and they went out by night from the city”, with the words in a different order and using a different Hebrew grammatical form for “from the city”, while LXX Jer 39 presents a minus at this point.

No definite solution can be presented as to the relationship between the three texts and how the triple parallel arose, even though this must be a problem that has a solution, since the texts are definitely related in some way. The most common idea is that Kings is the source for Jer 52 and quite likely Jer 39. This is based especially on the appearance of characteristic features of Kings’ style in the Jer 52 passage. Statements such as that King Zedekiah “did what was evil in the sight of the LORD” are exactly the sort of formulaic statements that are repeated throughout Kings, so it looks to be clear that the material was first composed for its context in Kings, then brought into Jeremiah as an appendix to the book showing how Jeremiah’s prophecies about Jerusalem’s doom were fulfilled (see, e.g., Cogan and Tadmor 1988:320). However, none of these characteristic statements is found in the Greek text of Jer 52,

⁸ A full six column presentation of the Hebrew and Greek witnesses to each text, with variants noted, can be accessed on my academia.edu site, titled “Synopsis of variants in 2 Kings 25:1–12// Jeremiah 52:4–16// Jeremiah 39:1–10”. I also discuss the more general text-critical issues in more depth in my forthcoming article. In the present article I will concentrate on the linguistic evidence and only give the basic information necessary on the broader text-critical issues relating to the triple parallel.

so we are back to square one, and it is even conceivable that a minority view, that both Kings and Jer 52 were drawing on a common source, could be correct (see, e.g., Hyatt 1956:1137, cf. 790). In any case, there seems to be evidence that at some later stage MT Jer 52 was brought closer to MT Kings (Person 1997:95–99).

The evidence seems to indicate that the parallel in MT Jer 39 was quite a late development. The critical Greek text presented in the Göttingen Septuagint does not contain verses 4–13 (Ziegler 1957:411–412). This does not seem to be accidental, but is evidence of an earlier text with very little of the parallel material in Jer 39 present (see, e.g., Jones 1992:452–453; McKane 1996:976–978; Müller et al. 2014:116). This is confirmed by further evidence of an even earlier Greek text which did not have verses 1–2 either, in other words, it had none of the parallel material in this chapter (Bogaert 2003:52–60). Thus there is evidence that could back up the idea that the parallel in MT Jer 39 developed late. Despite this possibly late development, MT Jer 39 goes its own way against MT Kings and MT Jer 52 in many details.

LESS COMMON LINGUISTIC FEATURES IN THE TRIPLE PARALLEL

Among the many details where each text goes its own way are the less common linguistic features. My study makes a distinction between those features of BH that are standard or at least the most common form in those cases where there is an option,⁹ and those which are less common. To use a clear example, the morpheme *-îm* (ִּי־) is by an overwhelming proportion the standard form to mark the masculine plural noun in BH. In contrast, the use of a *nûn* rather than a *mêm* is found only some 25 times in the MT Bible (Young 2001a:124–125). The form with *nûn* is therefore a less common linguistic feature in BH. The appearance of masculine plural nouns with *mêm* is not something that would ever be remarked on when talking about the linguistic profile of a biblical composition, whereas features such as the masculine plural with *nûn* are distinctive. I also make a distinction in the study between those linguistic forms which

⁹ “Large-scale and basic features of Classical Hebrew only rarely show variation” (Rezetko and Young 2014:168, cf. 180–181, etc.).

can be demonstrated to be the less common form in linguistic opposition to another, more common form, and those forms which are rare but which cannot be conclusively demonstrated to be less common than another form in linguistic opposition to it.

In the table below I summarise the 24 less common linguistic features in these parallel texts. In addition, I identified another five rare forms without linguistic oppositions, making a total of 29 linguistic forms. If the language of the biblical compositions was transmitted with great accuracy, as is presupposed by the MT-Only paradigm, the parallel texts should share a high proportion of these distinctive linguistic features, which might then be reasonably claimed to go back to the earliest stage of composition. In other words, the linguistic peculiarities of the MT could be argued to preserve the linguistic peculiarities of the authors of the biblical compositions. Against this, the model derived from the text-critical consensus would predict a very high rate of variation in minor textual details such as distinctive linguistic forms, and, consequentially, there should be a significant proportion of cases where the less common linguistic form is not shared by all texts. This table summarises the data:

Features with linguistic oppositions

	Verse reference	Details ¹⁰	Summary ¹¹
1.	2 Kgs 25:1// Jer 39:1// Jer 52:4	All texts except MT Jer 39 use or reflect ויהי in the date formula “in the ninth year of his reign”. While ויהי is regular in some temporal clauses such as with the infinitive construct plus <i>bêt</i> or <i>kāp</i> , with “in the Xth year” it is less common than the form without in Kings, Isaiah and Jeremiah 27–14. MT Jer 39 has no ויהי in a temporal clause, whereas LXX Jer 39 agrees with the MT and LXX of both Kings and Jer 52 with its <i>καὶ ἐγένετο</i> .	MT, LXX Kgs, LXX Jer 39, MT, LXX Jer 52 ≠ MT Jer 39
2.	2 Kgs 25:1// Jer 39:1// Jer 52:4	MT Kings’ use of the construct בשנת “in the year (of)” with an ordinal number in the date formula “in the ninth year of his reign” is less	MT Kgs ≠ MT Jer 39, MTJer 52

¹⁰ In many linguistic details the Greek translations cannot give evidence for the Hebrew *Vorlage*, and hence they are only mentioned when they have relevant information.

¹¹ The book with the rare form is listed first.

		common than the use in both MT Jer 52 and MT Jer 39 of the absolute form of “year”: בשנה.	
3.	2 Kgs 25:1// Jer 39:1// Jer 52:4	NebuchadRezzar in MT Jer 39 and Jer 52 is less common than Kings’ NebuchadNezzar. The LXX texts of Jer 39 and 52 (as well as LXX Kings) agree with MT Kings in having the form NebuchadNezzar (Ναβουχοδονοσορ).	MT Jer 39, MT Jer 52 ≠ MT, LXX Kgs, LXX Jer 39, LXX Jer 52
4.	2 Kgs 25:1// Jer 39:1// Jer 52:4	In “Nebuchadnezzar ... came against Jerusalem” we expect על as in MT Kings// MT Jer 52, whereas MT Jer 39 has אל. It is very likely that all three LXX texts agree with the regular form, since ἐπί is a standard equivalent for על.	MT Jer 39 ≠ MT, LXX Kgs, LXX Jer 39, MT, LXX Jer 52
5.	2 Kgs 25:4// Jer 39:2// Jer 52:6	In MT Jer 39 הבקעה represents the only <i>hop^cal</i> of this root; MT Kings// MT Jer 52 use the <i>nip^cal</i> which is common.	MT Jer 39 ≠ MT Kgs, MT Jer 52
6.	2 Kgs 25:4// Jer 39:4// Jer 52:7	MT Jer 52 has יברחו for ויברחו. The use of a <i>yiqtol</i> is unexpected in the sentence: “The city was breached and all the men of war <i>would flee</i> (?) and they went out from the city by night.” The parallel in MT Kings is itself defective “and all the men of war <...> by night,” but does not read the unusual <i>yiqtol</i> . Scholars commonly restore the expected <i>wayyiqtol</i> form in Kings with reference to the parallel in MT Jer 39:4 (Cogan and Tadmor 1988:317). LXX Kings and LXX Jer 52 agree on a different sentence structure, which like MT Kings does not contain a verb “to flee”, but unlike MT Kings contains the verb “to go out”, hence “and all the men of war went out by night”.	MT Jer 52 ≠ MT, LXX Kgs, MT Jer 39, LXX Jer 39 (minus), ¹² LXX Jer 52.
7.	2 Kgs 25:4// Jer 39:4// Jer 52:7	The assimilated form מהעיר for “from the city” is attested in MT Jer 52. The non-assimilated form מן ה- is by far the most common, 635–94; in Jeremiah 31–3 (Young 2013b:387–389). The phrase “they went out from the city” is absent from Kings (see the previous point). “From the city” is absent in both LXX Kings and LXX Jer 52, although they both have the verb “they went out”. The parallel in MT Jer 39:4 has the regular form מן העיר.	MT Jer 52 ≠ MT Jer 39 (minus in MT, LXX Kgs, LXX Jer 52)
8.	2 Kgs 25:4// Jer 39:4// Jer 52:7	MT Kings’ הלילה for “by night” only appears elsewhere in Zech 1:8, and Neh 4:16, whereas	MT Kgs≠ MT Jer 39, MT Jer

¹² LXX Jer 39 has a minus from verse 4 until the end of the triple parallel. It will not be cited any more.

		MT Jer 39 and 52's לילה is a more common way of saying "by night" according to BDB (539a).	52
9.	2 Kgs 25:5// Jer 39:5// Jer 52:8	Kings' use of the form אחר "after" in "and the army of the Chaldeans pursued after the king" is much less common than Jeremiah's אחרי (Rezetko and Young 2014:565). The parallel in MT Jer 39:5 has, different to both of them (and to LXX Kings and LXX Jer 52): אחריהם "after them."	MT Kgs ≠ MT Jer 39, MT Jer 52
10.	2 Kgs 25:5// Jer 39:5// Jer 52:8	MT Kings uses את plus suffix, which is a rarer linguistic form than verbal suffixes, in the phrase: "and they overtook him (אתו)". MT Jer 39 and MT Jer 52 instead read: "and they overtook Zedekiah". Both LXX Kings and LXX Jer 52 agree with MT Kings in having "and they overtook him", although it is not possible to know if their <i>Vorlagen</i> expressed this by the use of את plus suffix or whether they used a verbal suffix.	MT Kgs ≠ MT Jer 39, MT Jer 52
11.	2 Kgs 25:6// Jer 39:5// Jer 52:9	While MT and LXX Kings and MT and LXX Jer 52 have "and they captured (ויתפשו) the king", MT Jer 39:5 has "and they took him" (ויקחו אתו) with an unforced use of את plus suffix.	MT Jer 39 ≠ MT, LXX Kgs, MT, LXX Jer 52
12.	2 Kgs 25:6// Jer 39:5// Jer 52:9	Both MT Kings and MT Jer 52 use את plus suffix in: ויעלו אתו "and they brought him up". In contrast, for "and they brought him up" MT Jer 39:5 uses the more common verbal suffix: ויעלהו.	MT Kgs, MT Jer 52 ≠ MT Jer 39
13.	2 Kgs 25:6// Jer 39:5// Jer 52:9	MT Jer 39 has NebuchadRezzar which is less common than NebuchadNezzar, in the phrase "to Nebuchadrezzar, the king of Babylon". MT, LXX Kings and MT, LXX Jer 52 have simply "to the king of Babylon".	MT Jer 39 ≠ MT, LXX Kgs, MT, LXX Jer 52
14.	2 Kgs 25:6// Jer 39:5// Jer 52:9	MT Kings has the singular משפט while MT Jer 39 and 52 have the plural משפטים in the idiom "speak judgements with (את)" which is found only in Jeremiah (5 times, including the parallel in Jer 39:5) and in this parallel in Kings, where the unusual singular is labelled as an error by Holladay (1986:40). ¹³ The word "judgement" is in the singular also in LXX Kings and LXX Jer 52, but we cannot rule out that this is simply a translation choice, cf. NRSV "passed sentence", reflecting English idiom.	MT Kgs (cf. LXX Kgs, LXX Jer 52) ≠ MT Jer 39, MT Jer 52

¹³ Holladay further discusses whether the את should be understood as the *nota accusativa* or "with", since both are used in the MT (Holladay 1986:22, 40).

15.	2 Kgs 25:7// Jer 39:6// Jer 52:10	The Hebrew texts seem to indicate a distinction between רבלה “Riblah” and רבלתה “to Riblah” with the directional <i>hê</i> . However, in Jer 52:10 there is the form ברבלתה for “in Riblah” with no directive sense. The use of the directive <i>hê</i> in reference to location in a place rather than direction towards is a less common usage of the suffix. ¹⁴	MT Jer 52 (cf. LXX Jer 52) ≠ MT Kgs, MT Jer 39 (minus)
16.	2 Kgs 25:7// Jer 39:7// Jer 52:11	MT Jer 39 reads: “and he bound him with fetters to bring him (לביא אתו) to Babylon”. The use of the <i>hip'il</i> infinitive construct with the syncope of the <i>hê</i> (להביא for לביא) is not paralleled in MT Kings// MT Jer 52, which agree on the use of a <i>wayyiqtol</i> verb in the phrase “and he bound him with fetters and he (Jer 52: the king of Babylon) brought him (ויבאהו) to Babylon”. LXX Kings and LXX Jer 52 agree with this phrasing against MT Jer 39.	MT Jer 39 ≠ MT, LXX Kgs, MT, LXX Jer 52
17.	2 Kgs 25:7// Jer 39:7// Jer 52:11	When, as in the last point, MT Jer 39 reads: “and he bound him with fetters to bring him (לביא אתו) to Babylon,” it also uses את plus suffix instead of the more common verbal suffix. This too is not paralleled in MT Kings// MT Jer 52, which agree on the use of a verbal suffix in the phrase “he brought him (ויבאהו) to Babylon.” As noted, LXX Kings and LXX Jer 52 agree with this phrasing against MT Jer 39, but it is not possible to know whether their <i>Vorlagen</i> also used a verbal suffix.	MT Jer 39 ≠ MT, LXX Kgs, MT, LXX Jer 52
18.	2 Kgs 25:7// Jer 39:7// Jer 52:11	In the phrase “and he brought him to Babylon”, the form in MT Kings, בבל is absent the locative <i>hê</i> , present in MT Jer 39 and MT Jer 52 בבלה “to Babylon”.	MT Kgs ≠ MT Jer 39, MT Jer 52

¹⁴ See, e.g., GKC (§90c–i, pp. 249–251); JM (§93c–f, pp. 256–258); WO (§10.5, pp. 185–186); Hoftijzer (1981). It is also worth noting in this connection the LXX texts of Kings and Jer 52 in the previous verse. The fact that they render a preposition which could reflect “in” attached to the transliterated form of the place name (in the form Deblatha) with directive *hê* (εις Δεβλαθα), coupled with the fact that this form appears in the Hebrew of Jer 52:10, as we have noted, might lead to the suggestion that the form that was in the *Vorlage* of these texts was the same form as in MT Jer 52:10, and therefore this would be another less common form. On the other hand, the fact that the MT of Kings, Jer 39 and Jer 52 have רבלתה without a preposition, that there is evidence of a tradition that took the whole form “Deblathah” as the place name (Person 1997:91 notes this as a consistent transliteration in the LXX), and the difference in the preposition in Jer 52:10 (ἐν) when the MT form is ברבלתה with the preposition, mean that we must view this suggestion with caution.

19.	2 Kgs 25:8// Jer 39:7// Jer 52:12	MT Jer 52's NebuchadRezzar is less common than MT and LXX Kings' NebuchadNezzar. LXX Jer 52 and MT Jer 39 are minus this phrase.	MT Jer 52 ≠ MT, LXX Kgs (MT Jer 39, LXX Jer 52 minus)
20.	2 Kgs 25:9// Jer 39:7// Jer 52:13	MT Kings is minus a definite article on “great” in the phrase <i>וְאֵת כָּל בַּיִת גָּדוֹל</i> “and every great house” and hence uses <i>אֵת</i> before an indefinite phrase. Jer 52 has the same phrase with the expected definite article. LXX Kings just has “and every house,” although it is not possible to see if this phrase had the object marker or not. The phrase is missing in MT Jer 39.	MT Kgs ≠ MT Jer 52 (MT Jer 39 minus)
21.	2 Kgs 25:10// Jer 39:8// Jer 52:14	MT Jer 52 (cf. LXX Jer 52) says that the walls of Jerusalem were broken down by “the army of the Chaldeans who were with (<i>אִתּוֹ</i>) the chief of the bodyguards”. For “with” <i>אִתּוֹ</i> is less common than <i>עִמּוֹ</i> in general (Young, Rezetko and Ehrensverd 2008/2:112), in particular in the sense of accompaniment. MT Kings does not have <i>אִתּוֹ</i> here. ¹⁵ The whole phrase is missing from MT Jer 39.	MT Jer 52 ≠ MT Kgs (MT Jer 39 minus)
22.	2 Kgs 25:11// Jer 39:9// Jer 52:15	With <i>נִפַּל</i> in the sense of “to defect to”, BDB indicates that MT Kings' use of <i>עַל</i> is the more common form, while MT Jer 52's <i>אֶל</i> is the less common (BDB 657b, §4b). The parallel in MT Jer 39:9 agrees with Kings (<i>עָלִיז</i>), while LXX Jer 52 is minus this phrase.	MT Jer 52 ≠ MT Kgs, MT Jer 39, (LXX Jer 52 minus)
23.	2 Kgs 25:11// Jer 39:9// Jer 52:15	Kings expresses the phrase “the king of Babylon” as <i>הַמֶּלֶךְ בַּבֶּל</i> , with an unexpected definite article on the first element. The definite article is not present in MT Jer 52, and MT Jer 39 has “him” (<i>עָלִיז</i>) instead.	MT Kgs ≠ MT Jer 39, MT Jer 52
24.	2 Kgs 25:11// Jer 39:9// Jer 52:15	While MT and LXX Kings and MT Jer 52 (LXX Jer 52 has a minus) agree on “[the preceding groups] Nebuzaradan the chief of the bodyguards exiled”, MT Jer 39 adds “to Babylon” (<i>בַּבֶּל</i>). The use of “exile (<i>hip^cil</i> of <i>גִּלְהָ</i>) to Babylon” without a locative (or preposition) is unusual. ¹⁶	MT Jer 39 ≠ MT, LXX Kgs, MT, LXX Jer 52 (all these are minus)

¹⁵ Cogan and Tadmor (1988:316) recommend reading it in line with some manuscripts of Kings and of the parallel in Jeremiah.

¹⁶ For the locative see 2 Kgs 24:15; Jer 20:4; 29:1; 29:4. For a preposition see Ezra 2:1; 2 Chron 36:20. For another form without locative see Jer 43:3.

Rare features without linguistic oppositions

	Verse reference	Details	Summary
1.	2 Kgs 25:8// Jer 39:7// Jer 52:12	MT Jer 52 has בא...בירושלם “came (in)to Jerusalem” while MT Kings has בא...ירושלם “came to Jerusalem.” MT Kings reflects the normal practice in the MT of leaving “Jerusalem” unmarked in the phrase “came to Jerusalem”. MT Jer 52’s use of the preposition <i>bêt</i> is rare, cf. Jer 36:9; Ezek 21:25 (?). However, the two uses do not seem to be in linguistic opposition if we understand Jeremiah to have the specific nuance of “come into”. There is no parallel to this phrase in MT Jer 39.	MT Jer 52 ≠ MT Kgs (MT Jer 39 minus)
2.	2 Kgs 25:11// Jer 39:8// Jer 52:15	MT Jer 52 has a plus at the beginning of verse 15 which includes the unusual feminine plural of the collective דלות “poor”, in the phrase “and some of the poor of the people.” Holladay notes that a plural of a collective is “puzzling” (Holladay 1989:437).	MT Jer 52 ≠ (MT Jer 39, MT Kgs minus)
3.	2 Kgs 25:12// Jer 39:10// Jer 52:16	MT Jer 52 has ומדלות הארץ “and some of the poor of the land”. Holladay notes for this case also that a plural of a collective is “puzzling” (Holladay 1989:437). MT Kings has the expected singular form (ומדלת הארץ), while MT Jer 39 has an alternative phrasing (ומן העם הדלים).	MT Jer 52 ≠ MT Kgs, MT Jer 39
4.	2 Kgs 25:12// Jer 39:10// Jer 52:16	MT Kings <i>Ketib</i> ולגבים (root גוב “to dig”). See the following. LXX Kings transliterates γαβιν, clearly related to the <i>Ketib</i> form here. A point of interest is whether the Greek letter <i>nu</i> indicates that the word in the <i>Vorlage</i> of Greek Kings had a masculine plural ending with <i>nûn</i> , a form attested only some 25 times in the MT Bible (Young 2001a:125), rather than the regular form with <i>mêm</i> . However, we treat this form with caution since the phenomenon of final <i>mêm-nûn</i> interchanges is well attested in the transliterations of the LXX and other sources from the period (Kutscher 1974:61; Young 2001a:124–125).	MT Kgs ≠ MT Jer 39, MT Jer 52 (LXX Kgs?)
5.	2 Kgs 25:12// Jer 39:10// Jer 52:16	MT Jer 52 (= MT Kgs Qere) וליגבים (root יגב “to till, be husbandman”). Both this and the previous example are unique forms; no other verbs are formed from these two roots.	MT Jer 52 ≠ MT, LXX Kgs, MT Jer 39

		<p>However, they are not necessarily in opposition to each other, since the vocabulary items may be interpreted to mean slightly different things. Note further that the parallel in MT Jer 39:10 is ויגבים and is pointed in the MT as a noun “fields”, in the differently formulated statement involving a unique plus (plus material is underlined): “And some of the poor people, <u>who had nothing</u>, Nebuzaradan, the chief of the bodyguards, left <u>in the land of Judah, and he gave to them</u> vineyards and fields in that day.”</p>	
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Summary and consideration of the data

This table shows that of the 29 less common or rare linguistic features that I identified in the texts, not one of them – not one! – is shared by all three parallel passages in the MT. This is an even more remarkable result when we recall the evidence presented earlier that MT Jer 52 has been brought, at a later stage, into line with MT Kings, and that Jer 39 is a secondary development out of the parallel texts. To be honest, I would have expected that just by sheer coincidence, if nothing else,¹⁷ there would have been a few shared forms. I note, however, that our earlier study of just the double parallel of the poetic sections of MT 2 Sam 22// MT Ps 18 found only two shared forms out of 30, or just 6.67%, which is approaching zero shared forms (Rezetko and Young 2014:158). This emphatic result means, in any case,¹⁸ that under no circumstances can this evidence be taken any other way than that the theory that distinctive linguistic features were copied with a degree of accuracy is absolutely wrong.¹⁹

We have very strong evidence, therefore, both within the MT and in non-MT biblical scrolls, that distinctive linguistic features were not normally considered important enough to be copied with any degree of exactitude. Instead, they could be added and subtracted at will from the texts as they were transmitted. A third line of

¹⁷ Some of the less common forms are still quite common, such as תא plus suffix.

¹⁸ Even in the quite possible case that I missed a shared form or two.

¹⁹ As already mentioned, in contrast to these distinctive, less common linguistic features: “Large-scale and basic features of Classical Hebrew only rarely show variation” (Rezetko and Young 2014:168, cf. 180–181, etc.).

evidence adds to the picture already outlined from non-MT manuscripts and parallel passages. One of the major challenges in the study of ancient Hebrew is the absence of significant dated and localised evidence, as is available in the study of much better attested languages such as Akkadian (Young 2013b:398–400; Rezetko and Young 2014:21–45, 61–68). Instead, as I stated at the outset, the vast majority of our evidence for ancient Hebrew consists of late copies of literary texts, exactly the sort of evidence scholars in other fields more blessed with documentary sources try to avoid. Our only dated and localised evidence for early Hebrew is the inscriptions from the monarchic era. These are too few to provide much evidence, but they do throw up a few cases that fit with the suggestion that our manuscripts are heavily linguistically modified. For example, we have a substantial body of evidence that a significant minority of theophoric names, especially in the northern part of the country, ended with –YW rather than –YHW in the monarchic era. Why is not a single one of them preserved in any biblical manuscript? What about other peculiarities such as the regular use of תש for “year” in the Samaria Ostraca? The logical deduction is that our late copies of literary texts have undergone strong editing that has removed some unusual linguistic forms entirely (Rezetko and Young 2014:112).²⁰

Every book of the Hebrew Bible, in whatever manuscript we have it, is therefore a linguistically composite text reflecting language from different layers of composition, redaction and transmission. No distinctive detail, or collection of distinctive linguistic details, in our biblical manuscripts is likely to represent the language of the “original author” or earliest stage of composition, except in very large-scale and/or exceptional circumstances (see below).

OBJECTIONS

Objection 1: Diachronically significant linguistic patterns in the MT

Surely, however, the followers of the MT-Only paradigm will object, even allowing

²⁰ See the section “New directions for research in the Text-Critical paradigm” below for further possible examples.

some interference in the textual transmission of the linguistic forms,²¹ the clear attestation of diachronically significant patterns of linguistic forms in the MT testifies unequivocally to the fact that the Text-Critical paradigm is overstating the evidence for textual diversity. The MT-Only scholars could conclude that the preservation of these diachronically significant patterns is clear proof of the reliability of the MT as the premiere witness to the language of the biblical authors. The topic of “diachronically significant” linguistic patterns, however, raises some of the major challenges to the methodology of the MT-Only system made by the Text-Critical paradigm.

A first point that needs to be made is that there are many patterns of linguistic forms attested in the MT. As can be seen already by the table of less common linguistic forms above, only some of them are mentioned by the literature of the MT-Only paradigm, the ones that can be made to fit the presupposed chronological framework. Thus, for example, כָּנַס “to gather” is attested in the core “Late Biblical Hebrew” (LBH) books, Esther, Daniel, Ezra, Nehemiah, and Chronicles, and never in “early” books like Samuel or Kings. Or, even though מַלְכוּת “kingdom” is found a few times in texts like Samuel and Kings, most of its occurrences are concentrated in those five LBH books. Going beyond the focus of much of the scholarship from the MT-Only paradigm on lexicon, the fact that the writing of מִן “from” before a noun without the definite article is only found as a significant minority form in Chronicles, has been seen as diachronically significant (Young 2013b:385–387). However, there are many other patterns of usage in the MT that are simply not mentioned since they do not easily fit with the presupposed chronology. In lexicon, for example, we have studied the distribution of לֵב/לֵבָב “heart” (Young, Rezetko and Ehrensverd 2008/2:108–111). In morphology we have studied, for example, מִן before a noun with the definite article (Young 2013b). The selection of which patterns in the MT to discuss is dictated by the presupposed chronology.²² A different starting corpus would end up with a different

²¹ Hurvitz, for example, has always allowed for the possibility of rare scribal changes to the language of the MT, e.g., Hurvitz (1982:19; see the survey of his text-critical views in Rezetko and Young 2014:84–89).

²² Furthermore, quite often the presupposed chronology leads to a skewed reading of patterns as fitting with the chronology. We discuss many of these in Young, Rezetko and

set of patterns which could in their turn be considered significant, depending on the presupposition about the corpus in question.

The idea that the distinctive patterns of linguistic forms in Esther–Chronicles are “diachronically significant” is derived from the most basic assumptions of the MT-Only method. The method proceeds from the valid observation that the five books Esther, Daniel, Ezra, Nehemiah, and Chronicles, are confirmed to be post-exilic since they refer to events and people from post-exilic times. The MT-Only paradigm’s next (unargued) assumption, while logical, has turned out to be false. It not only assumes, with irreproachable logic, that the language of these five books must be evidence of post-exilic Hebrew, but it proceeds on the further assumption that therefore these five books are the measure of the nature of post-exilic Hebrew. The MT-Only method proceeds on the basic assumption that all post-exilic books will inevitably display links with the language of the five core “LBH” books, and that these links are diachronically significant, markers that other works, not so obviously post-exilic, are from the same late linguistic milieu.²³

A good deal of our research (see especially Young, Rezetko and Ehrensverd 2008), has argued that even within the MT-Only paradigm, and accepting many of its working hypotheses (which we do not necessarily accept), the logic of that paradigm itself leads to opposite conclusions to what is claimed by MT-Only scholars.²⁴ In

Ehrensverd (2008), and Rezetko and Young (2014), e.g., קעצ/קער “cry out” (Rezetko and Young 2014:278–282).

²³ As I mentioned, this is an unargued assumption, and therefore I would emphasise that this is my formulation, based on my understanding of the MT-Only paradigm as first of all, a participant, and later as a critic. Classic formulations of the methodology are not precise on exactly which books are to be included in defining late Hebrew. They often include books that are not obviously late, such as Qoheleth/ Ecclesiastes (dated primarily on the basis of its language, hence circularly included among the post-exilic books, and a work whose initial composition I have argued to be pre-exilic; see Young (1993:140–157; 2005b:347–348) and Ezekiel (a work whose internal dates place it mostly at the end of the pre-exilic period). In addition, with more justification, they will include works such as Zechariah. However, since definitely post-exilic works outside the five core books have only a sparse representation of the classic LBH forms (see below), it is the language of the five that sets the standard of “late” Hebrew.

²⁴ This has unfortunately been a source of confusion. Reviews of Young, Rezetko and Ehrensverd (2008) from the MT-Only camp did not seem to be aware that when they

relation to this topic, for example, our research has indicated that, following the methodology of the MT-Only paradigm, far from being the measure of post-exilic Hebrew, the language of the five core “LBH” books seems to be an aberration, since we have not found any other post-exilic books that match their linguistic profile.²⁵ Books that are definitely post-exilic and do not share the same linguistic profile include Haggai,²⁶ Zechariah, Ben Sira, and a range of Qumran texts such as Peshet Habakkuk,²⁷ the Community Rule, and the War Scroll.²⁸ In addition, scholars outside

declared our methodology deficient, they were actually attacking their own side. See, e.g., Rezetko and Young (2014:597–598).

²⁵ Cf. Young (2013c:20–24). For the data, see Young, Rezetko and Ehrensverd (2008/1:129–139, 271–279). There is, of course, no reason why such books or texts that match the linguistic profile of LBH should not be found, and it would not materially affect the current argument about the peripheral nature of this type of profile were this to occur.

²⁶ Rendsburg (2012) has claimed, on the contrary, to find a great many “LBH” features in Haggai. He concedes, however, that his “aforecited figures derive from a comparison of apples and oranges” (Rendsburg 2012:340 n.44) since he follows a different definition of LBH features and a different methodology for identifying them to ours (cf. his further statement: “Of the various discriminants between SBH and LBH recognised by scholars [Avi Hurvitz *super omnes alios*], I have identified only three of the former in Haggai” [Rendsburg 2012:341]; a preliminary survey in accordance with our methodology would indicate there are a few more, but not many, so that Haggai seems to be very far away from the profile of the core LBH books). Rendsburg never defines what a “LBH” feature is. He does not put forth any justification for presenting his results as comparable to our work, and we are not aware of any. Further, it seems that he is arguing against our supposed suggestion that Haggai “is devoid of LBH features” (Rendsburg 2012:330). This unfortunately is a straw-man claim since it is contrary to one of the basic and often-cited conclusions of our work: “None of the sample passages are free from LBH features” (Young, Rezetko and Ehrensverd 2008/1:136); “every sample we have done so far includes LBH features” (Young, Rezetko and Ehrensverd 2008, 2:86; italics in original); etc. Admittedly we did not include a sample for Haggai, but given the repeated emphasis that all biblical texts have LBH features, our citation of an earlier work by Ehrensverd, which talks generally of the lack of characteristic LBH features, cannot be taken as a basis for attributing to us a current view that Haggai (as opposed to every other biblical text) lacks all LBH features (see Rendsburg 2012:330, 340 n.44, citing Young, Rezetko and Ehrensverd 2008/1:56). If we followed Rendsburg’s variant methodology, the volume of “LBH” forms would expand greatly in all of our samples, “early” or “late”. To Rendsburg’s credit, however, he has realised how damaging our data are to the MT-Only paradigm if not countered, even if he has not succeeded in coming to grips with our approach, which is a strict application of Hurvitz’s method (as explained in Young, Rezetko and Ehrensverd 2008/1:130–131). For more on Rendsburg’s treatment of Haggai, see Rezetko (2016).

²⁷ Rendsburg has recently produced a study of the language of Peshet Habakkuk which shows the same divergent methodology and misunderstanding of our argument as his earlier work

the MT-Only linguistic paradigm not only take books like Joel as post-exilic, but substantial parts of other biblical literature, such as late layers in the Pentateuch, notably the P material, or late layers in the Deuteronomistic History.²⁹ This linguistic isolation is the reason we prefer to label Esther–Chronicles not, misleadingly, Late Biblical Hebrew (LBH), as if they represent the only or the main type of Hebrew in the late period, but instead, Peripheral Classical Hebrew (PCH).

At this point the fuzziness of the core methodology of the MT-Only paradigm becomes crucial. Scholars working in that paradigm would counter the previous paragraph by pointing to the various “diachronically significant” linguistic forms in the other post-exilic texts mentioned (but not P or DtrH). For example, the Community Rule (1QS 1:25) from Qumran uses the *hip^cil* form of *פשע* in the sense of “to do wickedness,” rather than “to condemn as guilty,” the former being a feature of the PCH books (Young 2008:14–15). This shows, they would say, that even though the late author attempted to write in (early) Classical Hebrew, and mostly succeeded, he could not completely avoid using a few diachronically significant linguistic forms, i.e., those characteristic of the PCH books. This argument, however, mixes different

on Haggai, mentioned in the previous footnote (Rendsburg 2015). The point of my article, part of which was taken up into Young, Rezetko and Ehrensverd (2008/1:255–262, 271–276), was to show that “PHab does not exhibit a concentration of LBH linguistic features comparable to or exceeding the core LBH books of the MT Bible. In fact, the number of LBH features is no higher than in core EBH [Early Biblical Hebrew] texts” (Young 2008:36). I noted the differences between the language of Peshier Habakkuk and other types of non-LBH Hebrew (Young 2008:35–36), and hence did not recommend it simply be called “Early Biblical Hebrew” (EBH), but suggested names like “Qumran EBH” (Young 2008:38). Rendsburg’s article is not responding to my argument, therefore, by claiming that I consider Peshier Habakkuk’s language to be identical to pre-exilic Hebrew (Rendsburg 2015:135; what he means by “SBH”) and by concentrating most of his attention on links between the language of Peshier Habakkuk and other Qumran texts (we both note many of the same forms, but by this part of his article, having set up his supposed target, Rendsburg does not actually discuss my work). As with his work on Haggai, he does not seriously engage with our (Hurvitz’s) strict methodology for defining “LBH” forms, so unfortunately he does not contribute to the subject I was discussing, which was the fact that Peshier Habbakuk has very few links with the special linguistic forms of Esther–Chronicles.

²⁸ See the data referenced in note 25.

²⁹ This was pointed out in the discussion at the SBL session in Atlanta by Ehud Ben Zvi.

categories of evidence, due to the fuzziness of the application by the MT-Only paradigm of Hurvitz's famous and generally excellent method at this point.

Hurvitz acknowledges that what he considers diachronically significant linguistic forms, i.e., those characteristic of the PCH books, also sometimes occur in what he considers indisputably early books (e.g., Hurvitz 1972:24–26). We have documented this in detail, following Hurvitz's own methodology (Young, Rezetko and Ehrensverd 2008/1:132–136). For example, both מלכות and the *hip^cil* form of רשע occur in MT Samuel. It was to cope with these specific occurrences that Hurvitz insisted in his methodology that what demonstrated that a text was really late was a significant accumulation of these features.³⁰ Hurvitz has, however, never specified what a significant accumulation is, nor how to measure it. This has allowed his method to be applied with a great deal of flexibility and circularity by proponents of the MT-Only paradigm. When a few PCH features turn up in Samuel or Kings, clearly this is not a sign for them that these books are late (the MT-Only scholars know that these books are early). However, when a few PCH features turn up in Peshar Habbakuk or the Community Rule, this is taken by MT-Only scholars as clear evidence that they are late³¹ (because they know they must be, and that late books are characterised by these PCH features).

When we attempted to systematically apply Hurvitz's system and actually quantify "accumulation" we found that Hurvitz is correct that the five PCH books are marked by an extremely high accumulation of the PCH features.³² However, we found that the much smaller accumulation of PCH features, identified using Hurvitz's criteria, in "early" books like Samuel and Kings is not distinguishable from that in non-PCH late books. For example, the Qumran Peshar Habakkuk has, following a strict application of Hurvitz's methodology, six PCH forms, exactly the same as several samples of

³⁰ For an extensively documented discussion of Hurvitz's method, see Young, Rezetko and Ehrensverd (2008/1:12–23). Key publications include Hurvitz (1972; 1982).

³¹ And that therefore Qumran Hebrew is a continuation or development of "LBH".

³² 17 to 25 in a 500 graphic unit sample. There is some circularity in this, since PCH features were defined by being found in these five books, however, the degree of accumulation of these features, which have linguistic oppositions to features more common elsewhere in BH, seems to be a genuine fact.

similar length from MT Samuel (Young 2008; Young, Rezetko and Ehrensverd 2008/1:134–135, 273–274).³³ What a systematic application of Hurvitz’s method and the resulting accumulation shows is, therefore, the isolated nature of the PCH books. What marks them off is not the individual linguistic features, which are generally found in other books, whether early or late, but the high accumulation of these specific less-common linguistic features.

The result of this clearer investigation, using Hurvitz’s own method in a precise manner, is that it is not the appearance of individual features that is potentially “diachronically significant”, but rather it is the high accumulations of these PCH features that mark the PCH books off. However, the PCH books are marked off, not only from “early” books, but also all other “late” books. If the linguistic profiles of the PCH books are diachronically significant, it does not tell us much, since they are isolated.³⁴ More likely, however, given the evidence that other late books do not share their style but are more in line with “early” books, the reason for their isolation is that they are simply unusual. Therefore, mapping the relationship of other texts to the PCH books, as the MT-Only paradigm’s method does, is not establishing anything that is necessarily diachronically significant. Instead, it is simply mapping what links other books have to these five unusual books. We could usefully utilise Hurvitz’s excellent methodology of distribution and opposition to map how the language of the Qumran Peshet Habakkuk links with say, patterns in the book of Samuel, or Psalms, or Proverbs, but this, while interesting, would not be diachronically significant, just as the links with the PCH books are not diachronically significant. They are simply patterns. And given the current lack of external anchors in the form of dated and localised texts, we do not know whether these patterns came from an early compositional stage or a later one. At the moment, I say again, they are just patterns.

³³ For Rendsburg’s unsuccessful attempt to dispute this, see note 27 above.

³⁴ However, it might lead to interesting speculations about the (relatively) high accumulations in the Temple Scroll. There is also the case of Qoheleth, however, this link would tend to argue against the PCH books being diachronically significant if I am right that the initial composition of the book goes back to the pre-exilic period (see note 23 above). For the peripheral nature of the characteristic PCH linguistic forms, even in the late books, see Rezetko and Naaijer (2016).

Non-MT manuscripts of biblical books have different patterns (see the references in the section “Linguistic evidence for the Text-Critical paradigm” above). Which patterns are early and which are late? At present, with the limitations of our current evidence, we just do not know.

It is thus premature to speak of “diachronically significant” linguistic features. To use such features in an argument against the very clear text-critical evidence is therefore to put the cart before the horse, arguing from what is not known, against data that are known and well documented.

Objection 2: Only a few “diachronically significant” linguistic forms are involved

It has been objected by followers of the MT-Only paradigm that only a small percentage of the data for variation of less common linguistic forms involves these PCH features. This is actually a sign of how selective the data used by the MT-Only paradigm are. There are indeed very many linguistic variations in the MT and other biblical texts, only a relatively small number of which are noticed by these scholars, as we have discussed above. Therefore it is inevitable that only a percentage of the many linguistic variants involve the specific forms that have caught the interest of these scholars due, as I have pointed out, to their fitting the presupposed chronological model. Nevertheless, as a sub-set of the larger data pool, these “LBH” features are just as fluid as the rest of the less common linguistic forms.

I have shown that the evidence indicates that almost every distinctive linguistic feature³⁵ was subject to change over the course of the transmission of the texts of the Hebrew Bible. This includes that narrower category of linguistic forms considered “diachronically significant” or marks of “Late Biblical Hebrew” by scholars in the MT-Only paradigm. We have seen a number of examples in the linguistic variants discussed in this article from the Kings// Jeremiah parallels, such as allegedly “non-classical” uses of the directive *hê*, or interchanges between לֹא and לָע. In an earlier study, I pointed to data from other parallel texts. For example, while 2 Sam 22 and Ps

³⁵ As opposed to large-scale and basic features, cf. note 9 above.

18 both have six PCH features, not one of them is precisely shared between both texts. Or, “early” 1 Kgs 22:6–35 has eight PCH features, while its parallel in “late” 2 Chron 18:5–34 has seven, but less than half of them, only three, are shared by both texts.³⁶ Parallel passages, as well as non-MT biblical manuscripts, show the variability of the allegedly “diachronically significant” linguistic features, along with numerous other linguistic features which the MT-Only paradigm overlooks.

Objection 3: The concentration of PCH forms in the “late” books

Proponents of the MT-Only paradigm might object that if late items were due to the vicissitudes of transmission, not composition/authors, they would be distributed more evenly through BH and not concentrated in LBH writings. This is a very important point, with which I partially agree. First, however, it must be pointed out that such a distribution is in fact generally the case. Our work has provided evidence that all biblical texts contain some PCH features, and the vast majority of them have them in similar concentrations. Of the 28 samples we conducted, 19 are in the range of having 1–9 PCH features with eight of them having six features (Young, Rezetko and Ehrensverd 2008/1:132–136). This indicates a fairly even distribution throughout most of the biblical manuscripts in our possession.³⁷ Nevertheless, there are exceptions to this even distribution, and other pervasive linguistic peculiarities that must be taken into consideration.

In *Historical linguistics and Biblical Hebrew* and further below, we have suggested that in exceptional cases an argument can be made that deviant or less common language might go back to the earliest stage of composition in a biblical book or passage. The first example we suggested was the possibility of arguing that the pervasively different language of Qoheleth and Song of Songs in general (not in specifics) can be traced back to their earliest composition layer (Rezetko and Young

³⁶ Young (2013a:110–111), and see the rest of the article for variations between MT Isaiah, 1QIsa^a, and MT Kings.

³⁷ However, the surprising number of supposedly post-exilic PCH features in pre-exilic inscriptions (Young 2003a:292–299) is one of the strong arguments against the easy conclusion that all PCH features were necessarily simply the result of later scribal updating.

2014:112–113). We then discussed whether such a case could be made for the quantifiably higher accumulations of less common linguistic features in the PCH books. In brief, we noted cautions to such a conclusion from (a) the fact that we have evidence of texts adding a significant accumulation of PCH forms to a composition, and from (b) remembering that we are still talking about relatively small accumulations of individual details,³⁸ each of which is highly fluid in textual transmission. We concluded that it is possible to argue that the general openness to linguistic variety was a characteristic already in the earliest stages of composition of these books, but that the evidence does not allow us to be sure that the current linguistic profiles do not exhibit a higher degree of linguistic variety due to the way these (canonically marginal?) books were treated in scribal transmission.³⁹ Even granting that the unusual linguistic profiles of these five books are “original”, however, we have shown above that the logic of the MT-Only paradigm itself, when the methodology is applied strictly, does not mean that the language of these five books is diachronically significant. In particular, the question of why these five books and no others preserve such a high rate of linguistic variability is actually a problem for the chronological interpretation, since other late or even later books do not share this linguistic profile.

The next point to be made is that the objection being discussed only makes sense if one accepts the unexamined presuppositions of the MT-Only approach. To ask why the PCH items are concentrated in the PCH books and not evenly through all the biblical manuscripts presupposes a teleology where all of Hebrew is developing towards the form of Hebrew represented by the PCH books.⁴⁰ This again is the presupposition that not only are the PCH books demonstrably late, but they are also the measure of what “late” Hebrew was. We have critically examined this

³⁸ Compared to the full number of words in the passages in question, the proportion of PCH linguistic features is almost always small.

³⁹ For the full discussion, see Rezetko and Young (2014:113–114).

⁴⁰ In older times, a similar objection would have asked, if there was significant change of language in transmission, why the language of the biblical texts is not the same as rabbinic Hebrew? This was also based on a false idea of the nature and direction of linguistic variation in the manuscripts. For recent views on rabbinic Hebrew see Young, Rezetko and Ehrensverd (2008/1:223–249).

presupposition above. If the PCH books' language does not represent normal or standard "late" Hebrew, there is no reason to expect that even where other books have undergone linguistic change, it will inevitably involve the replacement of one set of linguistic forms with PCH forms. This is, in fact, demonstrably not the case. Although the narrow group of PCH forms is seriously affected, they are just part of a larger set of possibly variant linguistic forms. The fact that the majority of linguistic changes do not involve PCH forms is further evidence of how "peripheral" they are in this period.⁴¹

Finally, this objection does not save "linguistic dating" as it has been conducted so far in the MT-Only paradigm. Since other works do not share the distinctive high accumulation of PCH forms with the five PCH books, the attempt to argue that other books can be dated late on linguistic grounds has been done on the basis of a very small number of linguistic forms in other texts. In *Historical linguistics and Biblical Hebrew* we mentioned classic works from the MT-Only paradigm, where (among other examples discussed) two linguistic forms in 40 words of Psalm 133 or seven in 749 words in Prose Job are used as evidence to date the original composition of the work (Rezetko and Young 2014:86–87). Other scholars have, for example, argued for the late date of the Song of Songs on the basis of one Persian loanword in the 1 250 words of the MT book (cf. Young, Rezetko and Ehrensverd 2008/1:310). Conclusions drawn from such small amounts of linguistic material must be viewed with extreme caution in view of the strong evidence for textual variability presented by the Text-Critical paradigm.

NEW DIRECTIONS FOR RESEARCH IN THE TEXT-CRITICAL PARADIGM

In the light of this better understanding of the nature of our sources for ancient Hebrew, what new directions might research take? Here are a few ideas that occurred to me.

⁴¹ For a detailed discussion demonstrating just how rare and idiosyncratic the conventional PCH variants are even in late writings, see Rezetko and Naaijer (2016).

An obvious first point is that straightforward diachronic explanations of linguistic variation cannot be treated as the normal go-to strategy in linguistic studies. For example, it is interesting that some books have a noticeably higher number of Persian loans than others (Young, Rezetko and Ehrensverd 2008/1:291–295; Wilson-Wright 2015:155–157; Noonan forthcoming). Typically this is simply explained according to chronology: later books have Persian words because they date from the Persian or later periods. But not all “late” books have them, and even more important, we now know that all biblical books are mixtures from various chronological periods. All biblical manuscripts had the opportunity of picking up a selection of Persian words. The really interesting question is therefore: Why do only some of them have a significant number of Persian words? I am not saying that in the new framework the answer to such questions may not involve chronology as one of the factors,⁴² but it cannot be the only answer, and it cannot be an easy answer, arrived at without detailed argumentation.

The choice that we have offered has never been “chronology or no chronology”.⁴³ However, we have always stressed the extreme difficulties that the nature of the

⁴² I chose Persian loanwords, since it is a (probably rare) case where a reasonable argument can be made for identifiable chronological factors. “Openness to Persian linguistic elements is a stylistic feature of some BH texts. One might speculate that it is probable that the strong influence of the Persian language on BH was confined to the Achaemenid period” (Young, Rezetko and Ehrensverd 2008/1:295). The presence of large numbers of Persian loanwords in the five PCH books may be another example to add to those discussed above where a case can be made that a feature of the linguistic profile of a text might be traceable to an early compositional stage. These are the only books with a significant number of Persian loanwords, however, and the presence of smaller numbers of them cannot be considered reliable evidence for dating the earliest compositional stages, quite apart from the fact that Hebrew was exposed to Iranian languages before the Persian period and therefore sporadic loans could be early (Young, Rezetko and Ehrensverd 2008/1:296–298; Wilson-Wright 2015:158–159; Noonan forthcoming).

⁴³ We have tried to make this plain in Rezetko and Young 2014:594–596; cf., Young, Rezetko and Ehrensverd 2016). We must bear a good deal of the blame for this misunderstanding which was due, I believe, to both Young and Rezetko coming from a scholarly background heavy in textual criticism and assuming that other scholars understood the history of the biblical text in a similar way to us. It was only when we saw the misunderstandings of our work that we became aware to what extent Hebrew language scholarship was based on a MT-Only approach. As an example of fundamental misunderstanding of our earlier work, the main point made in Joosten’s extensive (8 page) review of *Linguistic dating of biblical texts* is that “the authors appear to be happy to do away with the entire idea that the Hebrew language developed over the biblical period” (Joosten 2012:540). He does not engage with

sources and the evidence they provide cause for attempts to make valid chronological statements about ancient Hebrew.

Second, scholars of textual criticism have long been talking about the decentralisation of the MT. This should happen in language study too. The MT does not preserve the linguistic details used by the “original authors” any more than do other non-MT biblical texts. It is quite possible that the linguistic profiles of various MT books are simply odd. For example, some scholars see the writing of the 3rd person feminine singular independent pronoun with *waw* so that it looks identical to the masculine form וָה as representing significant linguistic evidence, but others see it as merely a scribal quirk of the MT tradition in the Pentateuch (cf. Rezetko and Young 2014:107).⁴⁴

Third, text-critical scholars have moved away from believing that we are in touch with original forms of biblical texts, if such a thing ever existed. Similarly, it seems likely that if we discovered more Hebrew inscriptions, we would discover more cases where all the biblical manuscripts in our possession reflect major linguistic developments away from Hebrew of the monarchic era. For example, we presented evidence in *Historical linguistics and Biblical Hebrew* that while in inscriptions the directive *hê* is always (100%) used with the motion verbs בּוֹא and עָלָה to express movement to a destination, MT Samuel uses it in similar contexts only once, i.e., 3% of the time (Rezetko and Young 2014:393–394). An analogy for the low retention of pre-exilic forms has long been known in orthography. While the third person

our actual arguments or the great amount of data we presented to back them up at any point, confining his critical comments to general assertions that earlier scholarship must be correct, and using five specific points of detail where he either disagrees with our interpretation, or misses the point, to make the general condemnation: “Inaccuracies [sic!] like these do not inspire confidence in Young and Rezetko’s ability to deal seriously with the linguistic data” (Joosten 2012:542).

⁴⁴ For discussion of another possible scribal quirk see the unusual treatment of מִן “from” before a word with the definite article in MT Samuel in comparison with 4QSam^a (Young 2013b). Without a MT focus we can also return with new eyes to questions such as the history of the second person feminine suffixes on nouns and *qatal* verbs, where the non-MT evidence of the typologically older forms כִּי- and תִּי- is judged later than the MT even though the MT has the typologically later forms without the final vowel (see the arguments of Kutscher 1974:25–27, 188–190, 209–213, who ascribes the non-MT forms to Aramaic influence).

masculine singular suffix on singular nouns was regularly spelled with *hê* in pre-exilic inscriptions, only some 55 cases are preserved in the MT (Young 2001b).⁴⁵

Fourth, it is possible that for some reason an unusual linguistic feature got stuck in the tradition, and was retained, since it had become significant. It would be interesting to look for any of these, although without sufficient dated and localised evidence to provide anchors, it would be difficult at present to argue that a linguistic form was, for example, actually early, and not just, say, late and peculiar.

FINAL WORDS

In conclusion, let me say: there is no going back. Unless the MT-Only paradigm can take on and overturn the consensus of biblical scholars about the composition of the Bible, the only choice open to language scholars is to take seriously the implications of mainstream biblical scholarship, as reflected in the Text-Critical paradigm, for their research. Language scholarship must strive to achieve what Eugene Ulrich calls a “revised post-Qumran mindset” (Ulrich 2015:310). One of the major messages of *Historical linguistics and Biblical Hebrew* is that historical linguistics of BH should be handled by those trained in (historical) linguistics, scholars such as John Cook, Robert Holmstedt, Cynthia Miller-Naudé, Jacobus Naudé, Na‘ama Pat-El, and Elitzur Bar-Asher Siegal, to name but a few. And a second major message of our book was to those scholars who will hopefully carry research forward, to take seriously the nature of the sources for ancient Hebrew, including the many, many anonymous authors who contributed to the current composite language of the biblical text.

⁴⁵ I have already mentioned the significant fact that not one example of the possibly “northern” theophoric suffix *h-* occurs in any biblical manuscript (see the section “Summary and consideration of the data”, above). The idea of a considerable disconnect between aspects of the language of the inscriptions and that witnessed in our biblical manuscripts fits in with the observation of the very high number of linguistic items found in the inscriptions but unattested or rare in the MT (see Young 2003a:299–308).

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