Dating the Hebrew Bible: Can Linguistics Help?
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1. Introduction

It might be a surprise to learn that we don't know when the books of the Hebrew Bible were written. They are not exactly obscure books; on the contrary, they are among the most studied books in the world. However, the dates and circumstances of their production are not easy to pin down. As we will see, reconstructing the history of Hebrew is not exactly business as usual for a historical linguist — many of the usual techniques we use to establish a historical sequence are not available, for reasons I will go into. It has been recently argued that we cannot in fact establish any historical sequence based on the language of the books. But I will try to show that this conclusion is too pessimistic, and that linguistics *can* help in dating the books, eventually.

2. How Biblical Hebrew is different

So why is Biblical Hebrew such a challenge? A good place to start is by looking at the timeline on the page before the last of this paper. This is just to give a general sense of the span of time covered by various stages of Hebrew. On the right side, for comparison, I have put some landmarks in the history of English. If we assume that the books of the Bible were written at about the time of the events they describe — that is, if we assume that Exodus dates from around 1200 B.C.E., and that the story of Kind David was written shortly after 1000, and so on down to the post-exilic books — we arrive at a time span of almost 1,000 years from the earliest parts of the Bible to the latest. That is equal to the distance between Old English — say, *Beowulf* — and ourselves.

Even a smaller span of time can produce a lot of change in a language. Consider how English changed in the 600 years from *Beowulf* to the time of Shakespeare. On the last page of this paper are reproduced the first few lines of *Beowulf*. We could not confuse that with any kind of English spoken today. A modern speaker of English would have to study it as if it were a foreign language, though if we look closely we can find the ancestors of words that are still in use. The every last phrase sounds like English: that was (a) good king.

The Norman Conquest in 1066 can be said to mark the end of the Old English period. The next excerpt is from Chaucer's *Canterbury Tales*, written in around 1400. The language is very different from *Beowulf*, but still quite different from the English we speak. Following that are some famous lines by Shakespeare, written around 200 years later. This is almost the kind of English we speak, though the spelling conceals changes in pronunciation.

Why are we looking at English if our interest is the Hebrew Bible? Because we might have expected, if the books of the Bible were as widely separated in time as suggested above, that the language of the oldest books would be as different from the

newest ones as Beowulf is from Shakespeare. But that is not the case. Though there is variation in the language of the Biblical books, it is not nearly as great as what we have seen in English.

Now, it is not the case that all languages change at the same rate. Various factors might cause language change to be accelerated or retarded. But all languages change, and in 600 to 1000 years we would expect more changes than we find in Biblical Hebrew. Therefore, the books of the Bible must have reached their final form in a smaller time frame.

A second problem is that the books appear to have been heavily revised, so that the language of original composition of early books may have been updated. Conversely, there is also evidence that later authors sometimes attempted to sound more ancient by using archaisms. Both tendencies make dating a tricky proposition.

A third problem is that sound change, which is a mainstay of historical reconstruction, is not available to us. The reason is that Hebrew writing was originally consonantal, with no indication of vowels. The first texts we have with vowels and other prosodic and phonological marks are the Masoretic codices that date from around 900, that is, around 1,000 years after the fixing of the consonantal text. The phonology indicated by the Masoretic texts is largely uniform; therefore, phonological changes are generally not accessible to us through the Biblical books. There is still morphology, syntax, and semantics, so all is not lost, but a significant source of historical evidence is not accessible to us.

A fourth problem is that in the Biblical period we do not have much evidence for the state of the language outside the Bible. This is not to say there is none, but there is not a great deal. Therefore, the chief source of evidence for what is early Biblical Hebrew and what is late Biblical Hebrew comes from the Bible itself. Thus, features characteristic of early books are considered early Biblical Hebrew, whereas features characteristic of later books are attributed to later Biblical Hebrew.

(1)   Special problems with dating Biblical Hebrew

    a.   The variation in the texts is not as great as between Old English, Middle English, and Modern English.

    b.   The books of the Bible have been revised, so that early language may have been updated; conversely, later language is disguised by archaisms.

    c.   Sound change is not available: indications for vowels, stress, and other prosodic markers date only from around 900 C.E., and is fairly uniform across all the biblical books.

    d.   There is not much extra-biblical evidence for the state of the language in the biblical period; the main evidence comes from the Bible itself.

One might detect some circularity here, and this is what is charged in a recent book called *Linguistic Dating of Biblical Texts* (henceforth *LDBT*), by Young, Rezetko and Ehrensvärd (2008). How do we know that a certain feature is early? Because it occurs in an early book. And how do we know that that book is early? Because its language has early features. To some extent this critique is valid: our current models of the development of Hebrew are not particularly sophisticated, and some particulars are

open to the charge of circularity. But *LDBT* goes too far, in my view, in discounting the possibility of any sort of diachronic account of the variation found in the texts. This is because the methodology they use to argue against particular diachronic interpretations of variation is overly rigid, and would, if applied to other languages, fail to identify even well attested diachronic variation in texts. Further, their central arguments against diachronic accounts rest on flawed reasoning and unrealistic assumptions about dialects and language change. So here is the first place that linguistics can help: by showing that the methodology of Young et al. is incorrect.

3.   How Biblical Hebrew is the same

Above, I discussed a number of ways that Biblical Hebrew poses a different problem form the reconstruction of, say, of Old English. Nevertheless, this does not mean that Biblical Hebrew is impervious to linguistic investigation. Hebrew is a language like other languages, and therefore we may assume that basic assumptions about language in general apply to Hebrew as well. Some basic assumptions that most linguists agree on are listed in (2).

(2)   Some basic premises

   a.   All natural languages change.
   b.   All languages have dialects (regional, social, etc.).
   c.   Diachronic change begins with synchronic variation.
   d.   We must distinguish between a language and its reflection in texts.
   e.   To use linguistic criteria as an aid in dating texts we must have a model of the history of the language, that is, of both diachronic and synchronic variation.

All natural languages change. Although the rate of change is not necessarily constant, and the direction of changes may not be predictable, it appears to be part of the nature of things for languages to keep changing.

The second assumption follows from the first. If languages are always changing, and if the directions of change are not predictable, it follows that a language will change in different ways in different sub-groups of speakers, giving rise to dialects. These dialects may be regional, or social, or even age-based.

The third premise, that diachronic change begins with synchronic variation, follows from the observation that many linguistic changes begin as variation within the grammars of individual speakers. Added to this is the fact that speakers (or writers, in our case) with different grammars co-exist at the same time, so that learners (or philologists) may be receiving input from speakers with slightly different grammars, creating both intra- and inter-grammatical variation.

A fourth premise appears to be obvious, but it is worth stating at the outset: we must distinguish between a language and its reflection in texts. Historical linguists are mainly interested in trying to reconstruct the history of a language. This is never a simple task, even in the case of languages that are well-documented with texts whose authors and dates of composition are known. As William Labov has famously remarked (1994:11), historical linguistics is "the art of making the best use of bad data." In the case of Biblical Hebrew, we have to make the best use of very bad data. Nevertheless, we

still aim to arrive at the most plausible scenario we can, using all the evidence available to us. In the case of early Hebrew, this means relying heavily, though not exclusively, on the biblical texts.

Given the doubts about the circumstances in which these texts were created, linguistic arguments have played, and will continue to play, an important role in establishing their provenance. But this puts us in the somewhat uncomfortable position, as *LDBT* remind us, of using the language to date the texts, and then using the texts as evidence for the history of the language.

In this situation, it follows that dating the texts cannot be our primary goal: rather, establishing a plausible history of the language is a prerequisite to dating texts. This is because dating a text using linguistic evidence is a more difficult problem than establishing a diachronic sequence for a language. If we have some notion of the history of the language, we can say, for example, that a given form in a given text comes from an earlier or later stage of the language (or alternatively, from this or that synchronic dialect). But making such a determination still leaves many unanswered questions about how the text as a whole came to have this form in it. It could represent the date the text was composed; or it could be a later insertion into an older text; or a borrowing from another dialect, and so on. If the editorial history of a text is particularly complex, there may not be a well-defined answer to the question, "To what date should this text be assigned?" Thus, *LDBT* may well be correct in asserting (II, 100) that "the outward form of the biblical texts was in constant flux. In this context, the question of the 'original date' when a biblical book was composed is anachronistic and irrelevant."

My disagreements with *LDBT* concern its model of linguistic change in general and the history of Hebrew in particular.

4. An example of variation: *mamlākâ* and *malkût*

Let us take as an example the much discussed variation between several forms for 'kingdom', in particular *mamlākâ* and *malkût*. According to *LDBT* (I, 21 n. 21), the distribution of these forms is considered a "classic illustration" of a diachronic shift, with *mamlākâ* being the older form and *malkût* the newer form. A table showing the number of occurrences of each form in each book is given in (3).

If we sort the books in terms of rising percentage of *malk͟ūt̲*, we can display them on a chart as in (4): books with less than three examples in both columns have been omitted. The books on the left side of the chart are, for the most part, those that are conventionally considered to be early, and the books on the right side are considered to be late. This is a very rough first approximation: we are assuming that all the books can be treated as uniform wholes, which is not the case.

(3)   Number of occurrences of *mamlākâ* and *malkût* in biblical texts

Book	mamlākâ	malkût	% malkût	Book	mamlākâ	malkût	% malkût
Genesis	2	0	0	Micah	1	0	0
Exodus	1	0	0	Nahum	1	0	0
Numb	2	1	33	Zephan	1	0	0
Deuter	7	0	0	Haggai	2	0	0
Joshua	2	0	0	Psalms	6	6	50
1 Sam	6	1	14	Lament	1	0	0
2 Sam	6	0	0	Eccles	0	1	100
1 Kings	12	1	8	Esther	0	26	100
2 Kings	5	0	0	Daniel	0	16	100
Isaiah	14	0	0	Ezra	1	6	86
Jerem	17	3	15	Nehem	1	2	67
Ezekiel	4	0	0	1 Chron	3	11	79
Amos	3	0	0	2 Chron	19	17	47
				Totals	*117*	*91*	*44*

(4)   *mamlākâ* and *malkût* (at least 3 of either form)
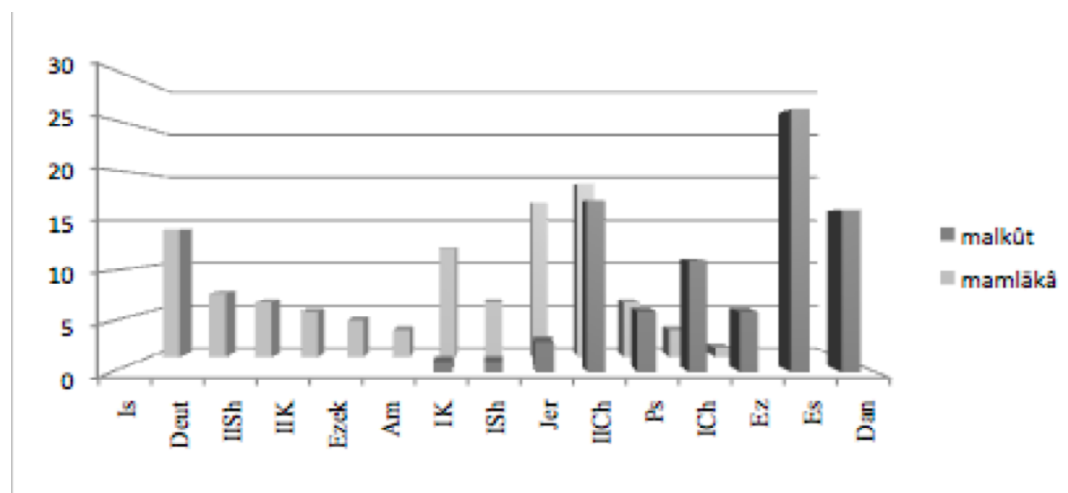
A diachronic interpretation of this distribution appears to be supported by extra-biblical attestation, as *LDBT* points out. In (5) is a listing of the distribution of these forms in Ben Sira, Qumran documents, and the Mishna (numbers provided by Robert Holmstedt). They also fit in on the right side of the chart, as we might expect if they are late books.

(5)   Extra-biblical occurrences of *mamlākâ* and *malkût*

Book	mamlākâ	malkût	% malkût
Ben Sira	3	2	40
Qumran	36	52	59
Mishna	0	20	100
Totals	*39*	*74*	*65*

5.   ***LDBT*'s central argument against a diachronic interpretation of variation**

LDBT do not accept the conventional diachronic interpretation of the distribution of these forms. Their arguments with respect to this example are fairly typical of their general position, so it is worth considering them in some detail.

Their first argument runs as follows: if *malkût* is a late form, then its appearance in a text indicates that the text is late. That would make Numbers, Samuel, and Kings late books, a conclusion that would be unacceptable to almost all writers who support a distinction between Early Biblical Hebrew (EBH) and Late Biblical Hebrew (LBH). Anticipating the obvious reply to this line of reasoning, *LDBT* presents perhaps its central argument against the entire diachronic project (I, 86; cf. II, 84):

> If against this is it argued that the LBH linguistic feature found in the EBH text is not actually 'late' but was also available in an early period, then its value for dating texts 'late' is negated…Therefore, if EBH texts are early, and most LBH features are attested in EBH texts, then LBH features already existed in an early period, and were available to early authors, and thus their use is a matter of style, not chronology.

As stated, this argument is untenable. It is a well-attested fact in many languages that competing forms may coexist over a period of time, so that a late form may occur sporadically in early texts, and an early form may survive in late texts. It is an empirical question, in any given case, whether the distribution of forms has a diachronic dimension or not; there is no basis for ruling out chronology as part of the story. Thus, it does not follow from the mere fact of co-existence that all the variation in the distribution of these forms must be stylistic and not diachronic.

Let us consider more closely the claim that if a late feature existed at an earlier period that it was therefore "available" to early authors. This notion of "availability" is contrary to findings in historical linguistics that much synchronic variation has a diachronic trajectory. Contrary to *LDBT*'s assertion that the co-existence of competing forms "negates" their value for dating texts, it can be shown that the proportion in which such forms occurs has a characteristic signature in a given time and place, and can have considerable predictive value in dating a text.

6.   The rise of English periphrastic *do*

In this connection I would like to look at the rise of periphrastic *do* in English. In Present Day English, an auxiliary verb *do* must appear in a variety of contexts, as shown in (6).

(6)   Present Day English contexts requiring periphrastic *do*
      i.   Negative declarative sentences
          a.   She does not deserve it.
          b.   *She deserves it not.

      ii.   Negative imperatives
          a.   Do not look at the answers.
          b.   *Look not at the answers.

iii. Yes-no questions
 a. Do you know the answer?
 b. *Know you the answer?

iv. *wh*-adverbial questions
 a. Why does she deserve a reward?
 b. *Why deserves she a reward?

In each type of sentence in (6), the (a) sentence with *do* is grammatical, and the (b) sentence, in which the main verb moves to the left of the negative marker or the subject, is ungrammatical.

In Old and Middle English, the equivalents of the (b) sentences were all grammatical, and *do* was not used in these constructions. Beginning around 1400, verbs, with the exception of *be, have,* and modals (*shall, will, may, can*, etc.), began to lose the ability to move to the left, and periphrastic *do* began to be used in sentence types that require the tensed verb to be to the left of a subject or *not*. This change began slowly and took hundreds of years to complete. A graph showing the percentage of *do* in different types of sentences is shown in (7).

Building on work by Kroch (Kroch 1989, Han and Kroch 2000), Warner (2006) demonstrates that the changes in the percentage of *do* in the different sentence types advance in lockstep. The reason, according to Kroch and Warner, is that a single basic change in the grammar affects all these sentence types, and in each period the old grammar and the new grammar co-exist in a proportion that manifests itself in each type of sentence.

The graph also illustrates another characteristic of language change, namely the S-shaped curve of an innovation. Thus, periphrastic *do* advances relatively slowly at first until just before 1500, when it takes off and rises at an increasing rate (with some local dips) until it reaches about 90%, at which point the rate of change necessarily slows as the change moves to completion.

Warner (2006) also argues for a stylistic influence on the development of *do* in negative declarative sentences (the dotted line in the middle of the graph). Starting in about 1575 the percentage of periphrastic *do* in this type of sentence dipped, and fell far behind the affirmative sentences (solid line just above it). This deviation is an apparent counterexample to the claim that periphrastic *do* increased at a constant rate across sentence types.

Warner argues that a more detailed analysis shows that the dip was not universal, but occurred mainly in texts of what he calls "high lexical complexity," that is, texts that use longer words, and a greater variety of words. Such texts tend to be more literary and sophisticated than texts with low lexical complexity, which tend to be more colloquial and closer to speech. Warner proposes that the drop in *do not* after 1575 in texts of higher lexical complexity was due to a stylistic avoidance of the sequence *do not*. This stylistic dispreference did not extend to texts of lower lexical complexity, meaning that in the spoken language *do not* continued to advance, and over the long run this stylistic tic did not significantly impede the rise of periphrastic *do* in negative declaratives.

(7) Percentage of *do* in different types of sentences (Ellegård 1953: 162, cited by Warner 2006: 48)
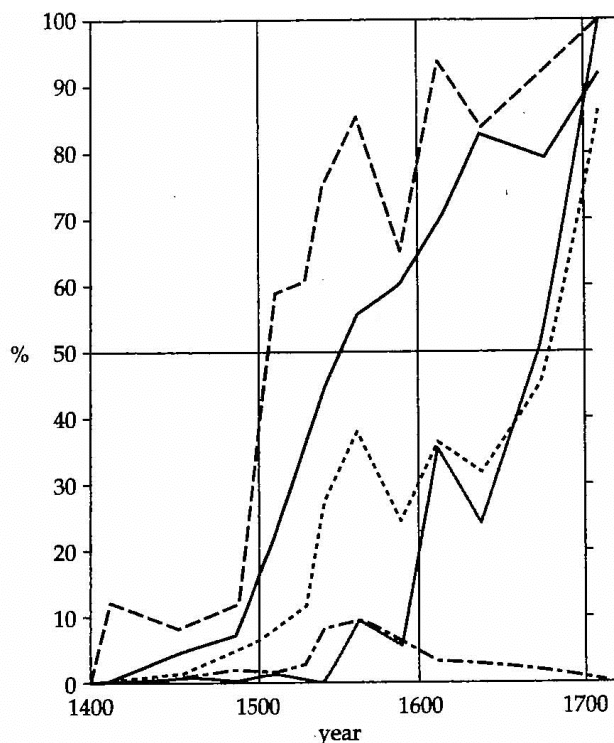
Key

negative yes–no and *wh*-adverbial questions	(– – –)
affirmative yes–no and *wh*-adverbial questions	(———)
negative declaratives	(·········)
negative imperatives	(———)
affirmative declaratives	(–·–·–)

The conclusions I want to draw from this example are summed up in (8).

(8) Conclusions from the rise of periphrastic *do*
   a. Old and new forms can co-exist over a long period.
   b. This co-existence is not static, but changes systematically over time.
   c. The proportions of old and new forms are highly significant, and can be used to estimate the date of texts.
   d. There is a place for stylistic variation, but the stylistic influences are specific and occur in the context of ongoing diachronic change.

Applying these conclusions to the Hebrew example of the words for 'kingdom', it follows that the differing proportions of the two forms in different texts could well point to a diachronic difference in the texts. *LDBT* ignores differences in proportions, and considers only presence versus absence of forms, a criterion that would miss the entire diachronic development of periphrastic *do*. Thus, they observe (I, 88) that the EBH books Numbers, Samuel and Kings have both *mamlākâ* and *malkût*, "but so do Jeremiah, Ezra, Nehemiah and Chronicles." The suggestion is that these books are all

the same in having both forms. The table in (9) reprises the numbers of forms in these books.

(9)   Distribution of *mamlākâ* and *malkût* in selected books

Book	mamlākâ	malkût	% malkût
Numbers	2	1	33
Samuel	12	1	8
Kings	17	1	6
Jeremiah	17	3	15
Ezra	1	6	86
Nehemiah	1	2	67
Chronicles	22	28	56

If we exclude Numbers and Nehemiah, which have only 3 forms each, it is apparent that the two groups of books (actually, three, since Jeremiah occupies an intermediate position) are quite different with respect to the distribution of the two forms. In fact, they fit quite well the conventional division of books into periods reviewed by *LDBT* (I, 11), as shown in (10).

(10)   Conventional division into periods
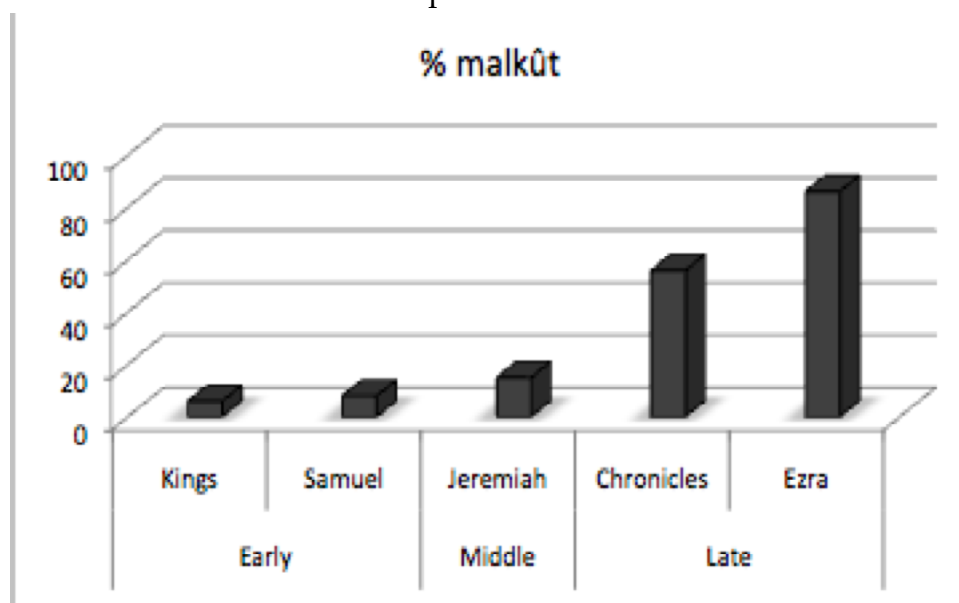
7.   External attestation and dialect variation

We observed above that the diachronic interpretation of this distribution is supported by extra-biblical attestations in Ben Sira, Qumran Hebrew, and the Mishna. *LDBT* is not impressed by these facts, however, arguing that Mishnaic Hebrew is not in fact later than Biblical Hebrew. Again, their reasoning is flawed and rests on unrealistic assumptions about dialects and language change.

LDBT argues that the "nineteenth-century model of a steady development from EBH to LBH to MH is in conflict with the evidence." This appears to be true, but is of

dubious relevance, because a diachronic account of differences between MH and BH does not depend on this model. Scholars such as Kutscher (1982) and Sáenz-Badillos (1993) also reject the nineteenth-century model, but still accept that late biblical texts would be expected to show more MH elements than early ones.

The general consensus is that MH developed from a vernacular dialect of Hebrew, whereas BH was a literary language that coexisted with vernacular dialects (Bar Asher 1999). *LDBT* adopts a similar position, but with a significant twist (II, 2.1.3): "MH is an independent Hebrew dialect of great antiquity. Both 'Aramaisms' and Mishnaisms', far from being markers of a late date, were available in all periods of Hebrew."

The above quote appears to suggest that there was no diachronic development in the MH dialect. It is one thing to say that Mishnaic Hebrew develops from vernacular dialects that can be traced back to pre-exilic times (cf. Bar Asher 1999). It does not follow from this that pre-exilic Mishnaic Hebrew forms all persisted unchanged for hundreds of years and could appear in any proportion in any text written in this period. If this is the case, then, as Delitzsch (1877: 190) remarked in a similar context, there is no history of the Hebrew language!

8.   Diachronic discontinuities

Maybe *LDBT* does not intend this radical interpretation. Perhaps they allow that MH changed over the course of hundreds of years. They could still argue that the fact that Biblical Hebrew did not become Mishnaic Hebrew poses problems for diachronic interpretations of variation, because "MH is simply a different dialect of Hebrew" (I, 227).

In this respect, Hebrew is not so different from other languages whose history is better documented. The earliest attested examples of Old English, for example, tend to come from the Northumbrian dialect in the north. Starting around 715 the Mercian kingdom in the midlands became ascendant and the Mercian dialect became the standard. In 825 the West Saxons in the south defeated the Mercians and West Saxon became the standard until the end of the Old English period.

Therefore, in studying the history of Old English, as one moves back in time, one also moves further north. For example, a Mercian form from 700 is both older and from a different dialect than a West Saxon form from 1000. But even though early Mercian is not the ancestor of late West Saxon, for many purposes one can pretend that it is. The reason is that in many respects these dialects were similar, and underwent many of the same diachronic changes. So the Mercian form might reveal to us the original vowels that appear in reduced form in later West Saxon. The main point is that dialect differences do not negate diachrony, but must be considered together with diachrony.

Toon (1983) discusses the problem of variation in Old English texts. He writes (1983: 106–7), "It is important to students of the language that variable data need not preclude, as it has for some, meaningful analysis." One problem he considers is the spelling of the vowel in the Old English ancestor of the word 'man'. We observe the distribution of spellings shown in (11).

(11) Spellings of the vowel in Mercian Old English 'man'

Text	Date	a	o
Epinal Glossary	c. 700	58	1
Erfurt Glossary	c. 750	32	33
Corpus Glossary	c. 800	38	95
Vespasian Psalter	c. 830	none	all

Toon observes that the mixed spellings in the glossaries might lead one to suppose that they are the result of dialect mixture, or idiosyncratic stylistic choice. But he argues that one can make sense of the variation in terms of diachrony. Like other Old English dialects, Early Mercian originally had the vowel /a/ in the word *mann* 'man'. A sound change then occurred in Mercian whereby /a/ became *o* before a nasal consonant. We can see the very beginning of this change in the Epinal Glossary. The later texts reflect later stages in which the change was either becoming more established in the spoken language, or alternatively was becoming more acceptable to be written. By the time of the Old English gloss of the *Vespasian Psalter*, *o* was the only option.

It would be misleading and unproductive, in this case, to argue that both *a* and *o* spellings were "available" to Mercian scribes in the entire period 700–830, and that therefore the choice of one over the other was a matter of style, not chronology. Both spellings overlapped for a time, but they were not equally "available;" their distribution has a chronological as well as synchronic dimension.

As Toon (1983) shows, there is also a political dimension to the variation in spellings. The table in (12) is a summary of spellings in Kentish charters.

(12) Spellings of the vowel of Old English 'man' in Kentish charters

Period	Dates	a	o
Before Mercian influence	679–741	5	0
Mercian ascendancy	803–824	0	64
End of Mercian influence	833–870	23	65
After Mercian exodus	859–868	13	3
Late Kentish	958–1044	25	0

In Toon's interpretation, the Kentish vernacular dialect never underwent the change of *a* to *o*. The *o* spellings reflect the Mercian standard; once Mercian influence was gone, the *a* spellings return.

This example shows that we cannot simply label *a* spellings as "early" and *o* spellings as "late". This equation does hold within Mercian, but it is only part of the story. If we include other dialects, we see that it is also true that *o* spellings are "northern" and *a* spellings are "southern ." In the Kentish documents, where *a* forms are both early and late, it can be said that *o* spellings reflect the official standard spelling, and *a* spellings the vernacular. All these dimensions play a role in fashioning a coherent account of the variation in spellings.

It is also relevant to note that the dialects of the texts are not necessarily different stages of a single dialect. I have argued (Dresher 1985), following Kuhn (1939), that the dialect of the *Corpus Glossary* is not the direct descendant of that of the *Epinal Glossary*,

but more like a younger sister. Thus, it can be shown that two sound changes that were active in the same period reached these dialects in different orders, as displayed in (13). The fact that the *Epinal Glossary* dialect co-existed with the *Corpus Glossary* dialect does not preclude us from assigning a diachronic dimension to the variation in the documents.

(13) Sound changes in different orders

    a. *Epinal Glossary*: Second Fronting of *a* before Back Mutation

Earlier forms	fatu	weras
Second Fronting	fætu	
Back Mutation	fæatu	weoras

    b. *Corpus Glossary*: Back Mutation before Second Fronting of *a*

Earlier forms	fatu	weras
Back Mutation	—	weoras
Second Fronting	fæatu	

9. Accounting for variation in the biblical texts

Returning to Hebrew, the central empirical problem we are dealing with is: what is the best way to account for the variation in the texts? In the particular example we have been looking at, how can we account for the distribution of *mamlākâ* and *malkût* in the biblical texts? In (14) I have summarized the two proposals in front of us. In (14a) is the conventional theory, what LDBT calls the "chronological model" (II, 95). It accounts for much of the distribution by diachrony, but does not attribute all variation to chronology. The difference in (10) between Chronicles and Ezra, for example, may be partly explained in terms of style, in that the former was more concerned to imitate certain elements of the earlier grammar. In other cases dialect differences have also been invoked, as well as genre differences between prose and poetry.

    LDBT proposes to replace this model by (14b), "a model of multiple contemporary styles of literary Hebrew." They designate EBH as a "conservative" style whereas "LBH authors/editors/scribes are more open to using a variety of linguistic forms" (I, 141). They hasten to stress (I, 141 n. 91) that they "use 'conservative' here in the sense of 'moderate, cautious, avoiding extremes' rather than conservatism in the sense of favouring an older style…both the conservative and non-conservative styles co-existed throughout the period of the composition of the biblical literature."

(14) Two theories of the variation of *mamlākâ* and *malkût*

    a. The "chronological" model
      i. *Diachronic:*
        mamlākâ is the earlier form and *malkût* is a later form. Books with mixed forms show different stages in the rise of *malkût*.
      ii. *Stylistic:*
        Chronicles and Ezra are both late, but the former was more concerned to imitate elements of the earlier grammar.

b. *LDBT*'s model: "Multiple contemporary styles of literary Hebrew"
    i. *Stylistic:*
       mamlākâ predominates in books written in the "conservative" (EBH) style (='moderate, cautious, avoiding extremes', not older); *malkût* is preferred in the style (LBH) that is "more open to using a variety of linguistic forms."

Let us now consider the empirical status of the two theories, summed up in (15).

(15) Empirical status of the two theories
    a. The "chronological" model
      i. *LDBT* presents no compelling argument against this model.
      ii. The variation profile is entirely consistent with what we would expect to find, and is in keeping with the English cases we have looked at.

    b. *LDBT*'s model
      LDBT's model has no testable empirical consequences, therefore it has no explanation for why *mamlākâ* and *malkût* occur in the attested proportions.

Looking first at the chronological theory in (15a), I have argued that *LDBT* presents no compelling argument against this model. Moreover, the variation profile is entirely consistent with what we would expect to find, and is in keeping with the English cases we have looked at. Of course, this does not prove that the diachronic account is correct, only that it is plausible and consistent with the evidence we have reviewed.

Let us turn to *LDBT*'s alternative. Does this model explain the variation in the forms *mamlākâ* and *malkût* as we find them in the texts? I don't see how it does. Why was *mamlākâ* considered a conservative form and *malkût* not? We can no longer say it is because *mamlākâ* was an older form, or belonged to a more prestigious dialect. While rejecting these hypotheses, LDBT does not replace them with anything which can explain why EBH and LBH have the properties that they do. How do we account for the variation in books that contain both these forms, and why in the proportions that they do? The chronological hypothesis suggests an answer — perhaps a wrong answer, but something we can try to support further or disconfirm. But *LDBT* suggests in the end that all variation is due to "stylistic choices of authors and scribes" (II, 95). As these choices are "unpredictable", the proposed model has no testable empirical consequences.

I mentioned at the outset that some model of how Hebrew developed, some notion of chronological stages and dialects, is a prerequisite to being able to date texts, because we have to have some sense of where the forms in the texts come from. By removing time and space from consideration, *LDBT* make it impossible to arrive at a coherent model of the history of Hebrew.

10.   An analogy: co-existing Achaemenid-period Aramaic styles in Elephantine

What is lost is nicely illustrated by an analogy that *LDBT* (I, 294; II, 99) draws between their own proposal and co-existing styles of Achaemenid-period Aramaic in Elephantine as portrayed by Kutscher (1970: 362) and Folmer (1995: 709–10). There were two dialects of Aramaic, Eastern and Western, co-existing at the same time. Note here the introduction of geography. Writers in Elephantine, which is in the west, used the Western dialect in their ordinary writings, as we might expect. The Western dialect is in greater continuity with Old Aramaic than the Eastern; note here the introduction of a diachronic dimension. When writing legal documents the Elephantine writers wrote, as we do, in a more conservative style that has older elements of the language (more diachrony). The Eastern dialect is more innovative, and has more Persian loanwords (geography again). In letters directed to the Persian authorities, Elephantine scribes tried hard "to write in the official style of the royal chancelleries" (Folmer 1995: 727), that is, in the Eastern dialect; here is a political dimension.

(16)   Elements of the Kutscher-Folmer account (*LDBT* I, 294; II, 99)

    a. *Geographic*
       i.   Two co-existing dialects of Aramaic, Eastern and Western. Eastern Aramaic (in Persia) has more Persian and Akkadian loanwords.
      ii.   Elephantine writers use their native Western dialect in private letters.

    b. *Diachronic*
       i.   The Western dialect is closer to Old Aramaic than the Eastern.
      ii.   Elephantine legal documents are in a more conservative (= older) style.

    c. *Political*
      In letters directed to the Persian authorities, Elephantine scribes tried hard "to write in the official style of the royal chancelleries" (Folmer 1995: 727), that is, in the Eastern dialect.

I think this is a very plausible and convincing analysis. Here is what *LDBT* says about it. They write (II, 99): "It shows us that there is no need to posit chronological or geographical distance to explain the use of different styles of language." But we have seen that both, in addition to politics, are crucial in explaining *why* the various styles are the way they are. *LDBT* has in mind that the same community in the same time and place could produce two different styles of writing; but without a diachronic and synchronic account of Aramaic we would not be able to make sense of these two different styles.

LDBT suggests that its account of EBH and LBH is a lot like the account of the two types of Aramaic produced in Elephantine. LBH writers, they propose, were trying to "distance this style of literature from literature produced in the EBH style. Rather than geographical or chronological distance, we would have intellectual or ideological distance." However, without history or geography, or even a clear idea of who the two groups were, we have none of the elements that make the Elephantine analysis so

compelling. Rather, juxtaposing that account with *LDBT*'s only serves to highlight the elements that *LDBT* is lacking.

11. A methodology for Biblical Hebrew linguistics

To return again to our example of *mamlākâ* and *malkût*, I have argued that *LDBT* does not provide a real alternative to the "chronological" model in (14a). That does not mean that this model is correct. There will always be a number of ways to account for the variation in any one feature studied in isolation. The real challenge is to arrive at a consistent model that can account for all the variation in the biblical texts, or as much of it as is feasible. This model should make use of any internal or external evidence available, and should incorporate contemporary theories of linguistic change and typology.

Thus, we can consider the chart in (4) to give us a profile of the variation between *mamlākâ* and *malkût*. We can similarly plot the profiles of other variable features. As De Caën (2001) has argued, the traditional division into EBH and LBH is too simplistic: language change does not present us with early features and late features. Rather, every linguistic change follows its own route. As we saw with the Mercian glosses, changes start at different times in different places and move at different rates. Therefore, we do not expect every variable feature to give us the same profile as *mamlākâ* and *malkût*. The grid we need to construct is not one-dimensionally diachronic, but multi-dimensional, including time and space as well as genre, politics, and style.

Such a project was proposed by De Caën (2001: 23): "One form or one contrast yields precious little, but *all possible variants* statistically correlated should yield much." Though I have taken issue with *LDBT*'s methodology and some of their conclusions, their detailed discussion and compilation of many such variants will be a great assist in carrying this project forward.

12. A parting bit of advice from the field of Old English

I started by showing how the problem of dating Biblical Hebrew texts is different from the parallel problem in Old English. But in some cases Old English presents similar difficulties. This is the case in trying to date the language of Old English poems, such as *Beowulf*, which exists in a manuscript from the end of the tenth century, but was probably composed much earlier. The language shows a mixture of forms suggesting a complex history. Friedrich Klaeber, editor of the authoritative edition of *Beowulf*, had this to say about linguistic tests for dating Old English poems (1950: cviii–cix); I think it holds equally well for Biblical Hebrew:

> Investigations have been carried on with a view to ascertaining the relative dates of Old English poems by means of syntactical and phonetic-metrical tests… It must be admitted that these criteria are liable to lead to untrustworthy results when applied in a one-sided and mechanical manner and without careful consideration of all the factors involved. Allowance should be made for individual and dialectal variations, archaizing tendencies, and…scribal alterations…. Yet it cannot be gainsaid that these tests, which are based on undoubted facts of linguistic development, hold good in a general way.

Acknowledgements

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1500 B.C.E		
1400		
1300	Beginnings of Hebrew as	
1200	a separate language	
1100		
1000	King David	
900	Earliest Biblical Hebrew	
800		
700		
600	Babylonian Exile, 598–538	
500		
400		
300	Late Biblical Hebrew	
200	*Septuagint*	
100		
0 C.E.	Dead Sea Scrolls	
100	Rabbinic Hebrew	
200	*Mishnah*	
300		
400		
500		Angles and Saxons arrive in England
600		Beginnings of Old English
700		Northumbrian Old English
800		Mercian Old English
900	Masoretic Text (*Aleppo Codex*)	
1000		*Beowulf*: West-Saxon Old English
1100		Norman Conquest, 1066
1200		Early Middle English
1300		
1400		Chaucer, *Canterbury Tales*: Middle English
1500		
1600		Shakespeare: Early Modern English
1700		
1800		
1900	Israeli Hebrew	
2000		

Anon., *Beowulf* (Old English, c. 1000), lines 1–11

Hwæt! Wé Gárdena in géardagum	Listen! We of the Spear-Danes in the days of yore,
þéodcyninga þrym gefrúnon	of those clan-kings-- heard of their glory.
hú ðá æþelingas ellen fremedon	how the worthy princes performed courageous deeds.

Oft Scyld Scéfing sceaþena þréatum	Often Scyld, Scef's son, with bands of warriors
monegum maégþum meodosetla oftéah	from many peoples seized mead-benches;
egsode Eorle syððan aérest wearð	and terrorised the fearsome Heruli after first he was
féasceaft funden hé þæs frófre gebád	found helpless and destitute, he then knew
	recompense for that:-

wéox under wolcnum weorðmyndum þáh	he waxed under the skies, throve in honours,
oð þæt him aéghwylc þára ymbsittendra	until to him each of the bordering tribes
ofer hronráde hýran scolde,	beyond the whale-road had to submit,
gomban gyldan þæt wæs gód cyning.	and yield tribute:- that was a good king!

Chaucer, *The Canterbury Tales* (Middle English, c. 1400), General Prologue, lines 1–12

Whan that Aprill, with his shoures soote	When in April the sweet showers fall
The droghte of March hath perced to the roote	That pierce March's drought to the root and all
And bathed every veyne in swich licour,	And bathed every vein in liquor that has power
Of which vertu engendred is the flour;	To generate therein and sire the flower;

Whan Zephirus eek with his sweete breeth	When Zephyr also has with his sweet breath,
Inspired hath in every holt and heeth	Filled again, in every holt and heath,
The tendre croppes, and the yonge sonne	The tender shoots and leaves, and the young sun
Hath in the Ram his halfe cours yronne,	His half-course in the sign of the Ram has run,

And smale foweles maken melodye,	And many little birds make melody
That slepen al the nyght with open eye-	That sleep through all the night with open eye
(So priketh hem Nature in hir corages);	(So Nature pricks them on to ramp and rage)
Thanne longen folk to goon on pilgrimages	Then folk do long to go on pilgrimage

Shakespeare, *King Richard II* (Early Modern English, c. 1600), Act 2, scene 1

John of Gaunt: …
>   This royal throne of kings, this sceptred isle,
>   This earth of majesty, this seat of Mars,
>   This other Eden, demi-paradise,
>   This fortress built by Nature for herself
>   Against infection and the hand of war,
>   This happy breed of men, this little world,
>   This precious stone set in the silver sea,
>   Which serves it in the office of a wall
>   Or as a moat defensive to a house,
>   Against the envy of less happier lands,--
>   This blessed plot, this earth, this realm, this England.