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USING LINGUISTIC DIFFERENCE IN RELATIVE TEXT DATING: INSIGHTS FROM OTHER HISTORICAL LINGUISTIC CASE STUDIES*

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The article addresses the question as to how optimistic we can be about the possibility and accuracy in converting linguistic typology into linguistic chronology. Applying the insights from standard text books on historical linguistic theory and the implementation of these insights in two particular case studies—namely the dating of Old English and Eddic (Old Norse) Poetry—the article argues for a balanced view on the possibility of determining the chronological stage(s) of the Hebrew language represented by biblical texts embedded and preserved in late copies from the second century B.C.E. or later. As historical linguists in general use a very restrained language in describing the possibility of dating texts linguistically, we should expect historical *Hebrew* linguists to be equally cautious. However, since the case studies reveal at least *some* reasons for describing linguistic criteria for dating autographs or earlier copies of texts in late, extant manuscripts, we should also expect enough evidence to establish *some* kind of diachronic grid in the development of the Hebrew language that can be used for at least a relative dating of texts that cannot be dated on other grounds. Especially Hurvitz's focus on lexicographical change and Eskhult's on loanwords seem promising areas in this regard.

1. INTRODUCTION

When you are a Bear of Very Little Brain, and you Think of Things, you sometimes find that a Thing which seemed very Thingish inside you is quite different when it gets out into the open and has other people looking at it.

Winnie the Pooh

It is relatively easy to write a consensus list of syntactical, orthographical, and lexicographical differences between the various books of the Hebrew Bible. Consensus soon comes to an end, however, when these differences are to be explained. Some scholars argue that the various types of Hebrew are traces of a diachronic development within the Hebrew language that can be dated with great accuracy, while others contend that these differences are to be explained solely—or at least primarily—along synchronic lines. The argument is thus not over the factual linguistic difference between the various bib-

* This is an expanded version of a paper read at the National Association of Professors of Hebrew session at the Society of Biblical Literature's Annual Meeting in Philadelphia, November 2005. I am much indebted to Shelly Harrison, University of Western Australia, for critical comments and suggestions for improvement. The actual wording, however, remains solely my responsibility.

lical texts, but over the interpretation—or, to be more precise, *criteria* for interpretation—of these differences. As Philip R. Davies phrases it:

The difference between Hurvitz's approach and mine lies, in fact, not over the basic facts, but over the way in which they can be explained. There is no disagreement over the facts that two kinds of Hebrew can be distinguished by vocabulary and syntax ... The difference between Hurvitz and me does not lie in accepting these facts, but how they are to be best interpreted.¹

How optimistic, scholarly speaking, can we be about the possibility and accuracy in converting linguistic typology into linguistic chronology? Can the arguments for diachronic variation in Biblical Hebrew put forward by Eskhult, Hurvitz, Polak, Rendsburg, and others be methodologically substantiated, or must we surrender to the more cautious or skeptical approach voiced by others?

I believe strongly in the complementary relationship between inductive and deductive research. Inductive linguistic arguments may easily lead to wrong conclusions without the correction from deductive or holistic considerations of hypotheses, theory, and method (or, as Philip Davies dubs it, "common sense"). Deductive linguistic arguments, on the other hand, are only valid as long as they are taken to be working hypotheses capable of constant adjustment in order to be the best explanation of the available data. The inductive and deductive methods are, in other words, mutually supplying each other's lack and therefore equally indispensable in interpreting linguistic variation in Biblical Hebrew. Though the remainder of this paper focuses on historical linguistic *method*, it is in due acknowledgment of the fact that it is the available linguistic data that set the perimeters for possible interpretations, and that linguistic analyses like Eskhult's on loanwords, Hurvitz's on Aramaisms, Polak's on verbal style, etc. are of paramount importance in defining the boundaries for *possible* interpretations.

That being said, I believe it is of equal importance to bring the methodological presuppositions of historical Hebrew linguistics "out into the open and has other people looking at it" as Pooh phrases it. Biblical scholars too often use methods that are either dated or taken out of context in comparison with the way they are used in their "domestic" environments of historiography, anthropology, sociology, linguistics, etc., and bringing them out in the open often reveals that "a Thing which seemed very Thingish" inside the perime-

¹ P. R. Davies, "Biblical Hebrew and the History of Ancient Judah: Typology, Chronology and Common Sense," in *Biblical Hebrew: Studies in Chronology and Typology*, ed. I. Young (JSOTSup 369; London: T & T Clark International, 2003), p. 152.

ters of biblical studies, is less straightforward than you expected when it is brought to test in the broader society of scholars.

What the remainder of this article will do, therefore, is not to go into linguistic argument with Eskhult, Hurvitz, and others, but to take up the thread from Davies's and Naudé's articles in Ian Young's volume on Biblical Hebrew² and focus on methodological issues in general historical linguistic theory. I am not a historical linguist, so what I have done is to read through a number of standard text books on historical linguistic theory and to acquaint myself with the methodological problems involved in two particular case studies, namely the dating of Old English and Eddic (Old Norse) Poetry.³ The choice of these relatively new branches of the Indo-European language tree may at first sight seem a bit odd, but there are—as we shall see—good arguments for choosing precisely these examples for comparison. Applying insights from these studies, it is the aim to argue for a balanced view on the possibility of determining the chronological stage of language represented by biblical texts embedded and preserved in late copies from the second century B.C.E. or later.

2. HISTORICAL LINGUISTIC METHODOLOGY

Pooh looked at his two paws. He knew that one of them was the right, and he knew that when you had decided which one of them was the right, then the other was the left, but he never could remember how to begin.

Winnie the Pooh

As historical linguistics aims to identify instances of genetic relatedness amongst languages, to explore the history of individual languages, and to develop a theory of linguistic change, the comparative method is a natural prerequisite, the *sine qua non*, for the historical linguist. Or as Rankin phrases it:

[T]he comparative method is arguably the most stable and successful of all linguistic methodologies. It has remained essentially unchanged for over a century. This is not because comparative linguistics has faded from view or is less im-

² I. Young, ed., *Biblical Hebrew: Studies in Chronology and Typology* (JSOTSup 369; London: T & T Clark International, 2003).

³ The following text books on historical linguistics have been consulted: L. Campbell, *Historical Linguistics: An Introduction* (Cambridge, Mass.: MIT Press, 1998); B. D. Joseph and R. D. Janda, eds., *The Handbook of Historical Linguistics* (Oxford: Blackwell, 2003); H. H. Hock, *Principles of Historical Linguistics* (2d ed.; Berlin: Mouton de Gruyter, 1991); W. P. Lehman, *Workbook for Historical Linguistics* (Publications in Linguistics 71; Dallas, Tex.: Summer Institute of Linguistics and the University of Texas at Arlington, 1984).

portant than it was a hundred years ago. Quite the opposite: its principles have withstood the tests of time and the onslaughts of its critics.⁴

The comparative method, described as the strength of historical linguistics, is, however, also its weakness, since the famous *circulus vitiosus* lies near at hand if the comparative method is used without methodological rigor. For the same reason standard textbooks on historical linguistics are full of *caveats* regarding the possibilities and limitations of the comparative method, and as it is highly relevant for the methodological discussion in historical *Hebrew* linguistics, a few remarks on these methodological observations are in place.⁵

On the more general level we find a call for more methodological awareness and—especially pertinent to our case—a caution against being too optimistic regarding the possibility of absolute dating on the basis of mere linguistic difference. As far as methodological awareness is concerned, Tarpent notes (somewhat in contrast to Rankin) that

[r]ather than *major* and *thriving*, historical linguistics today is largely *marginal*, and has been for several decades. As a result, with a few exceptions in the long-established specialties such as Indo-European and its best-known subfamilies, most contemporary linguists are only skimpily trained in historical work, they do not get much opportunity to sharpen their skills in this area through exposure to the work and opinion of their peers, nor to the questioning of their students, and very few of those students are receiving the thorough professional training needed if the work is to continue in the future. Our discipline still exists, and does have many distinguished full-time practitioners, but it survives mostly in the shadow of more prestigious, synchronically-oriented specialties.⁶

Tarpent thus argues that

the diminished diachronic community has tended to close ranks and to rest on its past glory, continuing in the footsteps of its revered predecessors, but rejecting most attempts to go beyond the paradigm established by them. Again, it is not that this paradigm has become outdated, or is inherently incapable of further development, but that it is less and less well mastered.⁷

⁴ R. L. Rankin, "The Comparative Method," in *The Handbook of Historical Linguistics*, ed. B. D. Joseph and R. D. Janda (Oxford: Blackwell, 2003), p. 208.

⁵ S. P. Harrison, "On the Limits of the Comparative Method," in *The Handbook of Historical Linguistics*, ed. B. D. Joseph and R. D. Janda (Oxford: Blackwell, 2003), pp. 213–243.

⁶ M.-L. Tarpent, "On the Eve of a New Paradigm," in *Historical Linguistics 1999: Selected papers from the 14th International Conference on Historical Linguistics, Vancouver, 9–13 August 1999*, ed. L. J. Brinton (Current Issues in Linguistic Theory 215; Amsterdam: John Benjamins, 2001), p. 310.

⁷ M.-L. Tarpent, "On the Eve," pp. 314–315.

Tarpen's call for methodological rearmament is backed by Harrison, who wonders why

so many historical linguists feel drawn towards absolute dating. Sure, it would be nice to know when, but the comparative historical enterprise doesn't stop because that question can't be answered. It seems to me that the obsession with dates, like the obsession with family trees, is at least partly the result of 'prehistorian envy'. Too many comparative historical linguistics want to dig up Troy, linguistically speaking. They consider it more important that comparative historical linguistics shed light on prehistoric migrations than that it shed light on the nature of language change.⁸

The first lesson that can be learned from current historical linguistic theory is that the primary task of historical linguistics is to identify instances of genetic relatedness amongst languages, to explore the history of individual languages, and to develop a theory of linguistic change, and that the results of these investigations more often than not are too meagre to provide even relative dates for texts that cannot be dated on other grounds.

On a more specific level, the *caveats* form two clusters of methodological problems, namely problems related to theory on linguistic change and diffusion, and problems related to insufficient control material, that is, lack of linguistic controls external to the biblical texts.

As far as language change is concerned, current linguistic theory holds that language change starts in an individual, diffuses in this individual's speech community and—if it is viable—further into society at large. Language change on the individual level occurs when the grammar of an individual differs from that of the input source (e.g., child versus parents). If an individual's differing grammar gains acceptance by his local speech community and later by society at large, language change on a greater scale occurs. As this by nature is a continuous process, multiple grammars will always exist side by side in a speech community, let alone a society, and a given language will always be in transition. Multiple grammars do not only exist between individuals, however, since most individuals have multiple grammars themselves in order to generate different linguistic styles (idiolects, sociolects, civil service style, etc.). Naudé is therefore fully in line with current linguistic theory when he argues that

the shortcomings in conceptualization and method which have given rise to misconceptions of language change lie in the failure to utilize a coherent conception

⁸ S. P. Harrison, "On the Limits," p. 231.

of the nature of language and the lack of clarity surrounding the notion of change,⁹

and when he defines three dimensions by which linguistic variation comes about and within which it persists:

[T]he dimension of time, society, and the individual along the time dimension languages are constantly, pervasively, and systematically changing. The result is linguistic variation, different ways of saying the same thing. Language change starts in an individual and diffuses in a speech community, a group or network of people whose language is more or less the same because they learn and influence one another in all sorts of behaviour including language. Speech communities are based on factors including geography, religion, age and gender, and may be more or less cohesive (depending on the level of standardization), and recognizable by their characteristics of language use. Speech is also influenced by the speech-situation, which determines the style and register of utterances (e.g. intimate, casual, consultative, formal, and frozen). Another factor is the difference between speech and writing. Writing is secondary to speech and employs special forms of language for its unique purposes (e.g. the utilization of devices for the organization of discourse).¹⁰

Naudé concludes that in the light of this understanding of language variation and change,

no variety of BH can be viewed exclusively as a Hebrew in transition. Each variety is a continuation of earlier grammars of Hebrew, but is in certain respects distinct from other *contemporary* [italics mine] grammars. The language variation as reflected in a specific variety is rather due to a situation where different grammars exist next to each other in the author's/speaker's mind. ○

reference?

If there is ever any truly sharp division between two historical stages of a language over a relatively short time period, then it is an accident. Catastrophic change in language is not the norm. Current theory rebuts, therefore, the argument (often stated *ex silentio*) that only one kind of Hebrew was being used at any one time, and Davies is therefore right in arguing that one cannot automatically convert linguistic typology into linguistic chronology.¹¹ A range of synchronic factors must be taken into consideration before a diachronic explanation can be settled: dialect, colloquial language, idiolect, sociolect, archaizing language, etc. This is also true for periods where such

⁹ J. A. Naudé, "The Transitions of Biblical Hebrew in the Perspective of Language Change and Diffusion," in *Biblical Hebrew: Studies in Chronology and Typology*, ed. I. Young (JSOTSup 369; London: T & T Clark International, 2003), p. 189.

¹⁰ J. A. Naudé, "The Transitions," p. 213.

¹¹ P. R. Davies, "Biblical Hebrew," p. 153.

differing grammars are unattested in the written sources. Since writing is secondary to speech, vernaculars and dialects must by necessity have existed alongside the written *Hochsprache*. Before jumping to diachronic explanations of linguistic difference one must acknowledge, therefore, that the dark side of the moon is just as real as the visible, and that the existence of additional *contemporary* grammars may account better for the linguistic differences than diachronic ones.

Furthermore, since language change is influenced by a number of unpredictable factors (time, society, and the individual) no linear development can be ascribed *automatically* to any language. Modern linguistic theory has, for the same reason, dismissed the idea that language change is governed by an internal “biological clock” that makes it possible for the historical linguist to reconstruct prior stages and to predict future developments of a given language.¹² This touches on the next cluster of methodological problems in the comparative method, namely the problems of gaps in the linguistic record. Using an example of a geologist studying the erosion of a hillside, Joseph and Janda argue that

studying a diachronic correspondence like the relation between the starting-point and the endpoint of a hillside’s erosion could rarely, if ever, provide as much insight into that long-term phenomenon as detailed research on the actual series of innovations which make up the overall change-process of erosion itself. However, in doing historical linguistics, we are generally closer to being in the position of a geologist who has only two before-versus-after snapshots—or, perhaps more fittingly, only a pair of hand-drawn sketches based on two such photographs. Nearly all historians, in fact, confront (to varying degrees) this kind of yawning chasm amidst fragments of documentary evidence, a predicament which led the American scholar Charles Beard to say that, in doing history, “We hold a damn dim candle over a damn dark abyss”. In our own field, too, Labov has noted that “[h]istorical linguistics can ... be thought of as the art of making the best use of bad data,” though we would prefer to characterize the data in question as “imperfect”.¹³

Considering the gap in Hebrew inscriptions from the sixth to the third centuries, and the relatively sparse record from the period before the sixth century, the situation in historical Hebrew linguistics is close to the two before-versus-after snapshots mentioned by Joseph and Janda above. At least as far

¹² Compare, for example, D. Ringe, “Internal Reconstruction,” in *The Handbook of Historical Linguistics*, ed. B. D. Joseph and R. D. Janda (Oxford: Blackwell, 2003), p. 244; D. J. Birnbaum, “Historical Linguistics” (paper presented at the Workshop on the Future of Slavic Linguistics in America, February 18, 2000), p. 2. Cited September 1, 2005. Online: <http://www.indiana.edu/~slavconf/SLING2K/pospapers/birnbaum.pdf>.

¹³ B. D. Joseph and R. D. Janda, eds., *The Handbook*, pp. 13–14.

as the Persian period is concerned. Consequently, if current theory on linguistic change is taken into consideration, any attempt at describing a linear change in the gap-periods of the Hebrew language by way of statistics must be rendered suspicious conjectures. This is true however sophisticated the statistical methods one uses, since it is impossible to imagine a method that can allow satisfactorily for such unpredictable factors as time, society, and individuals. One example is William J. Adams's use of statistics to account for the development of Hebrew in the Persian period. Though Adams is well aware of the problems related to sparse (pre-Persian period) or nonexistent (Persian period) inscriptional material, it is highly questionable whether his sophisticated statistical methods of determining (a) the level of confidence, (b) the degree of relationship between variables, and (c) the linear regression analysis, justifies—linguistically speaking—a division of Hebrew into no less than eight diachronic dialects from Archaic Biblical Hebrew (ca. 925–900 B.C.E.) to Mishnaic Hebrew (ca. 100 C.E.).¹⁴

Another example is the attempt to date texts by way of psycholinguistics by Quentin D. Atkinson and Russell D. Gray who argues that

[m]aximum-likelihood models, Bayesian inference of phylogeny, and rate-smoothing algorithms are powerful investigative tools for research in historical linguistics, just as they are in evolutionary biology. Using these methods, we have been able to focus the age of Indo-European to within 1,200 years of 8,700 B.P.—a meaningful range that certainly helps us answer the question of when PIE [Proto Indo-European] was spoken.¹⁵

Even if it is possible to demonstrate certain diachronic changes in grammar, morphology, and vocabulary, we simply have too few “snapshots” of diachronic Hebrew to determine precisely *when* the changes occurred. The best we can hope for in these cases is a relative chronology. In other cases, especially as far as the Hellenistic and Roman period is concerned, but to some extent also the pre-Persian period, we have considerably more “snap-

¹⁴ This is where I probably have to revise my own thesis in J. B. Kofoed, *Text and History: Historiography and the Biblical Text* (Winona Lake, Ind.: Eisenbrauns, 2005), p. 150. The bibliographic reference to Adams's work is W. J. Adams, Jr., “An Investigation into the Diachronic Distribution of Morphological Forms and Semantic Features of Extra-Biblical Hebrew Sources” (Ph.D. diss.; The University of Utah, 1987). See also the dismissal of Swadesh's glottochronology in M. Alinei, “The Problem of Dating in Linguistics,” *Quaderni di Semantica. Lingua, Storia e Antropologia culturale* 25:2 (December 2004): 1. Cited September 1, 2005. Online: http://www.continuitas.com/problem_dating.pdf.

¹⁵ Q. D. Atkinson and R. D. Gray, “Are Accurate Dates an Intractable Problem for Historical Linguistics?” in *Mapping our Ancestry: Phylogenetic Methods in Anthropology and Prehistory*, ed. C. Lipo, M. O'Brien, S. Shennan, and M. Collard (Chicago: Aldine, forthcoming). Draft version: May 19, 2004. Cited December 1, 2005. Online: <http://www.psych.auckland.ac.nz/psych/research/Evolution/AtkinsonandGrayrev3wfigs.pdf>.

shots,” and thus much firmer ground for describing linguistic change and diffusion. But again it must be taken into consideration that multiple grammars existed side by side, so that only changes that are broadly diffused into society at large (especially lexical change) and changes in the syntactical structures of Hebrew can be described as genuine *diachronic* changes and used for dating texts on mere linguistic grounds.¹⁶ Davies’s protest against Hurvitz’s claim that linguistic evidence forces us to view classical Hebrew as pre-Persian seems in this light to be substantiated: “There are too many factors involved, and our ignorance of the Persian period is too profound.”¹⁷ Whether Davies’s own suggestion that classical Hebrew can be dated to the Persian period is better supported by the linguistic evidence is another issue to which we will return in due course.

A final methodological problem worth mentioning is the problem of using written evidence. While historical linguists seek to date linguistic *phenomena*, there is no necessary connection between the *attestation* of a linguistic phenomenon in a certain manuscript and the *date* of the autograph since the text may have been copied for centuries. There may be, in other words, a considerable temporal gap between the genesis of a linguistic phenomenon and its earliest written expression. Written evidence does not, therefore, represent a term *a quo* (‘from which’), that is, the real ‘beginning’ of a certain lexeme. An example mentioned by Alinei is worth quoting in full:

Leaving aside the cases in which written evidence permits the precise and reliable dating of a new word (for instance, *gas* and generally all modern words of educated origin), we can say that as a rule written evidence does not and cannot represent a term [sic] *a quo* (‘from which’), i.e. the real ‘beginning’. At most, it represents, as already Schuchardt recognized, a term *ante quem* (‘before which’), which has quite a different value. It certainly cannot be argued that the earliest written evidence of a language, dated to moment X, proves that it existed only, for example, half a century, or 1, or 3, or 5, or 10 centuries before X, in compliance with some statistical law or the supposed molecular clock of organic change. It may be even supposed that it existed whole millennia before its first attestation ... What should be said, for example, about Finnish, which, according to specialists, was one of the first to establish itself in its historical territory in Mesolithic times, and which nevertheless appears for the first time in a written record only in 1524, at least *ten millennia* after its separation from the common

¹⁶ For lexicographical and syntactical change as the most promising possibilities for dating see M. Alinei, “The Problem of Dating,” and M. Eskhult, “The Importance of Loanwords for Dating Biblical Hebrew Texts,” in *Biblical Hebrew: Studies in Chronology and Typology*, ed. I. Young (JSOTSup 369; London: T & T Clark International, 2003), pp. 8–23.

¹⁷ P. R. Davies, “Biblical Hebrew,” p. 163. Compare the same cautious view by I. Young, “Late Biblical Hebrew and Hebrew Inscriptions,” in *Biblical Hebrew: Studies in Chronology and Typology*, ed. I. Young (JSOTSup 369; London: T & T Clark International, 2003), p. 310.

stem? It is hard to understand, then, why one should think, when confronted with the first rough texts of mediaeval dialects, that they had come into being only a few centuries earlier. This chronological distance between the linguistic phenomenon and its earliest written expression is also observable in the present day. Written and generalized documentation concerning modern dialects, for example, begins almost everywhere in Europe only in the 19th century, many centuries after that concerning national languages. And yet most national languages derive from one of those local dialects of uncertain origin which has been promoted to language by the area dominant group. Whence this chronological difference between the two phenomena?¹⁸

Concluding these deliberations on methodology it is obvious that the cautious approach taken by Ehrensverd, Davies, Naudé, and Rezetko seems to be in agreement with methodological concerns in current historical linguistic theory. "An accurate and absolute dating of texts by their language alone is," as Søren Holst phrases it,

a far from usual procedure in historical and literary studies (cf. the statement on Sanskrit literature in Winternitz 1909, where only a vague relative dating is attempted: 'Vor allem ist die Chronologie der indischen Litteraturgeschichte in ein geradezu beängstigendes Dunkel gehüllt ... in diesem Meer von Unsicherheit gibt es nur wenige feste Punkte.... Da ist vor allem das Zeugnis der Sprache, welches beweist, das die Lieder und Gesänge, Gebete und Zauberformeln der Veda unanstreitig das Älteste sind, was wir vom indischen Literatur besitzen), and practically unheard of in linguistics: Cf. e.g., Lass 1997: 242: 'Absolute chronology (if it can be established at all) is an archival, not reconstructive matter.' Just about the closest one gets is a short article by Mario Alinei (1991).¹⁹

It is important, however, not to over interpret this conclusion. While it does justify Davies's assertion that Hurvitz "is incorrect to *insist* [italics added] that he must be right," it does not follow that he or other proponents of a more positive view on the possibilities of discerning between a Classical Biblical Hebrew and a Late Biblical Hebrew is incorrect. It only demonstrates that the problem is more complicated than often allowed, and that insight in current historical theory forces us to be more cautious and nuanced in our interpretation of the evidence. We will now turn to two comparable case

¹⁸ M. Alinei, "The Problem of Dating," p. 5. The first grammar and lexicon of "my own" dialect (spoken on the island of Bornholm in the Baltic Sea) was published as late as 1908: J. C. S. Espersen, *Bornholmsk Ordbog* (København: Det Kgl. Danske Videnskabernes Selskab, 1908).

¹⁹ S. Holst, "Verbs and War Scroll: Studies in the Hebrew Verbal System and the Qumran War Scroll" (Ph.D. diss., University of Copenhagen, 2003), n. 84. The bibliographical data for the texts referred to are: M. Winternitz, *Geschichte indischen Litteratur* (Die Litteraturen des Ostens 9; Leipzig, 1909); R. Lass, *Historical Linguistics and Language Change* (Cambridge Studies in Linguistics 81; Cambridge: Cambridge University Press, 1997); and M. Alinei, "The Problem of Dating in Historical Linguistics," *Folia Linguistica Historica* 12 (1991): 107–126.

studies in the hope that they will throw some light on just *how* optimistic we can be in dating the late extant texts of the Hebrew Bible on linguistic grounds.

3. COMPARATIVE CASE STUDIES

Always watch where you are going. Otherwise, you may step on a piece of the Forest that was left out by mistake.

Before beginning a Hunt, it is wise to ask someone what you are looking for before you begin looking for it.

Winnie the Pooh

The obvious choice of a comparative case study would of course be to pick a well-documented contemporary linguistic case in the same literary genre and from the same cultural stream. The closest match in that regard is probably the Babylonian “literary” language or “Standard Babylonian,” which remained so stable that even distinguished scholars erroneously dated compositions late that later were proved to stem from Old Babylonian times.²⁰ Such a choice is, however, not as obvious as it seems, since Babylonian is almost *too* well-documented to be compared with the relatively sparse Hebrew linguistic evidence. Choosing a case from another time, from another literary genre and from a different cultural context is certainly not without problems. Certain cases share so many basic problems with historical Hebrew linguistics, however, that they, in spite of their temporal, cultural, and generic (poetry as opposed to prose) distance, seem useful as comparative case studies. Linguists dating Old Slavonic Chronicles,²¹ Old English Poetry, and Eddic

²⁰ Von Soden, for example, in his analysis of the Neo-Babylonian copy of “The Great Prayer to Istar” written in this dialect, dated it to Neo-Babylonian times, that is, after 1,000 B.C.E. (A. Falkenstein and W. von Soden, *Sumerische und Akkadische Hymnen und Gebete* [Zurich: Artemis, 1953], p. 401), while the discovery of an exact duplicate of the late Neo-Babylonian copy of the hymn in the Bogazköy archives invalidated his “guess” and indicated a much earlier date of composition, around the middle of the second millennium—that is, the late Old Babylonian period (E. Reiner and H. G. Güterbock, “The Great Prayer to Ishtar and Its Two Versions from Bogazköy,” *JCS* 21 [1967]: 256).

²¹ The lost original Radziwil Chronicle, dated 1214–1216 C.E. is believed to have been faithfully copied by one scribe and from two to five illuminators at the end of the fifteenth century. It was written in small Cyrillic half uncials on ‘rag flax’ paper, thought to be of Polish manufacture, with watermarks dated by N. P. Likhachev by traditional methods using Briquet, Piccard, and other basic references. The manuscript is unique in that it chronicles historical events in Russia from the ninth to the thirteenth centuries, its 618 illustrations in particular providing a window on detailed events in those early centuries. It records not only the ancient history of Russia and other Slavic nations but also their eventual interactions with the peoples of Western Europe. The chronicle is of considerable interest for linguistic research and for paleography. The text has almost completely retained the language of its protograph and, along with many archaisms, it contains a great deal of linguistic corrections. The language of the chronicle was first examined by V. M.

Poetry are thus struggling with precisely the same problems as is the case in the dating of biblical texts in that the extant texts are late copies of assumedly much earlier but no longer existing autographs, and the control material for linguistic comparison is relatively sparse.

Before discussing the two latter examples in some length, a few words on the generally high methodological awareness and interpretational cautiousness of scholars involved in these cases is in place. Jan Ragnar Hagland, for example, writes on the possibility of linguistic dating of Runic inscriptions that cannot be dated on archaeological or other grounds that

when I naively and expectantly ask my colleagues in archaeology, numismatics, art history, etc. about the dating of particular artifacts with Runic inscriptions, they return the question: What does the Runic inscription of this and that artifact tell about its date? And to make it clear from the beginning: The answer to these questions are usually discouraging imprecise.²²

Hagland continues to discuss the methodological problems of language change and the sparse material.²³ Bjarne Fidjestøl, writing on Eddic Poetry, is also aware of these problems and mentions, in addition, the problem of textual history. Referring to the Russian scholar Mikhail Steblin-Kamenskij, who said of the Eddic poems that “any attempt to penetrate beyond their fixation in writing or to separate the ‘late’ from the ‘ancient’ or the ‘original’ in them is Sisyphean toil,”²⁴ Fidjestøl quotes another scholar with consent, saying that

[t]he Elder Edda, which has reached us in manuscripts from the 13th century, represents, as is well known, the last phase of an extremely long existence as an epos throughout the centuries, and this fact raises for us the question of its complicated stratification.²⁵

Gontsov in 1907, at the time of Academician Shakhmatov (Online: “Memory of the World,” <http://www.unesco.org/webworld/memory/cd-rom.htm>).

²² My translation of “Like ofte som eg naívt og forventningsfullt spør mine arkeologkollegaer og venner, numismatikarar, kunsthistorikarar osv. om datering av einskildgjenstandar med runeinnskrifter, så får eg tilsvarande spørsmål tilbake: Kva datering kan språk og runeinnskrift gi i det og det tilfellet? Og for a ha gjort det klart med det same: Svaret på slike spørsmål bliver gjerne nedslående upresise.” J. R. Hagland “Innleiing. Litt om datering på runologisk og språkleg grunnlag,” in *Innskrifter og datering* [Dating inscriptions], ed. A. Dybdahl and J. R. Hagland (Senter for middelalderstudier skrifter 8; Trondheim: Tapir, 1998), p. 9.

²³ J. R. Hagland, “Innleiing,” pp. 10–12.

²⁴ B. Fidjestøl, *The Dating of Eddic Poetry* (Edited by O. E. Haugen; *Bibliotheca Arnarnagana. A Jón Helgason Condita* 41; Copenhagen: C. A. Reitzels Forlag, 1999), p. 187. Though we are dealing with poetry and not—as is generally the case in Hebrew historical linguistics—non-poetic texts, the problem of scribal transmission in the dating of linguistic phenomena in copied texts is the same whether in Old English Poetry or Hebrew narrative.

²⁵ B. Fidjestøl, *The Dating*, p. 191 (referring to the Russian scholar Aron Ya. Gurevich).

Fidjestøl himself, nevertheless, takes a slightly more optimistic stance towards the possibility of tracing the text diachronically, linguistically speaking. Warning against too sceptical approaches he maintains that “[f]ortunately, practitioners of higher criticism have a tendency to cancel each other out. Their results very seldom coincide, and this fact greatly contributes to weakening faith in them.”²⁶

Full awareness of these methodological problems does not, therefore, force these scholars to abandon their efforts in converting Old English and Old Norse linguistic typology into linguistic chronology. Although the easiest solution would be to isolate the content from the linguistic form and read the poems as cultural reflexes of the time of the *extant* manuscripts, this is not what the majority of scholars have chosen to do. To focus on the content, Bjarne Fidjestøl maintains, “is a perfectly sound and scholarly position.”²⁷ It places, however, the investigator of Eddic Poetry in an unsatisfactory dilemma: “[E]ither to concentrate on the content of the poems without any regard for their status as literary texts, or to stick to the study of the Edda as 13th- and 14th-century written texts.” Fidjestøl proceeds to mention a number of reasons why he finds “it difficult to rest content with this dilemma.” It is important for the interpretation of the texts and it is important for the understanding of literary history.²⁸

As is well known, poems like *Lokasenna*, *Prymskviða* and *Rígsþula* are read by some scholars as Christian and archaizing persiflages of old lore, while others read them as genuine heathen mythological poems. If we know nothing of their ultimate preliterary age, neither interpretation can be discarded, even if neither can be their *original* meaning. Even if we choose to limit our perspective to what is known without any doubt, namely the written poems as they were ‘fixed’ in the 13th century, and to give them an interpretation in terms of their Medieval reception, it is still very important whether the poem, in a given historical context, is perceived as being brand new or age old. Also the study of ‘Edda medii aevi’ in its synchronic perception will have to allow for a certain amount of diachronic awareness. It is generally accepted by students of literature that the meaning of a literary work can never be confined to one exhaustive interpretation and that each new reader as well as each new period will perceive the work in different ways. It is thus obvious that the full original meaning of an Eddic poem lies outside the immediate grasp of any modern reader.²⁹

By distinguishing between the content of the Eddic poems and their form Meulengracht Sørensen succeeded in rescuing them as sources to the history of Old Norse religion, but at the same time they are lost as sources to the history of

²⁶ B. Fidjestøl, *The Dating*, p. 198.

²⁷ B. Fidjestøl, *The Dating*, p. 191.

²⁸ B. Fidjestøl, *The Dating*, pp. 192–194.

²⁹ B. Fidjestøl, *The Dating*, p. 194.

Old Norse poetry anterior to the period when they were written down. If epistemological considerations compel us to accept this loss, we must do so of course, but I am not quite convinced that this is the case. The logical consequence of this sceptical position would be to reduce the history of Old Norse poetry before the advent of writing to the history of skaldic poetry, a genre which is not only eminently datable, but which also, thanks to its rigid form, gains a sort of intermediate state of being halfway between orality and literacy.³⁰

In addition to these reasons, both Fidjestøl and Amos find *linguistic* possibilities for dating the texts in question. To that we now turn.

4. OLD ENGLISH POETRY

Old English literature survives first and foremost in manuscripts from the late tenth, eleventh, and twelfth centuries. The earliest poetry, like *Cædmon's Hymn*, was written as early as the late seventh century, whereas *The Death of Edgar*, for example, was written as late as the mid-eleventh century. Other poems may well be earlier or later. Much of the poetry cannot, however, be dated on the basis of external evidence like authorship or historical references, and a *terminus ad quem* for these texts can only be provided by the paleographic characteristics of the manuscripts in which they survive. This date often leaves a period of two or three hundred years, however, during which they may have been composed. For the same reason scholars have become resigned to this uncertainty in dating, but the implications, as Amos notes, "that this ignorance has for Old English literary history should not be overlooked."³¹

Amos divides his historical linguistic analysis into four broad categories, namely phonological-metrical tests, syntactic and grammatical tests, lexical tests, and stylistic tests, and his conclusions are as follows.

4.1 Phonological-Metrical Tests

"The syncope and apocope of unaccented vowels in Old English, like *u*-apocope, are useful only in offering a very general *terminus a quo* for Old English poetry and cannot serve as a chronological test,"³² "the test of vowel length after loss of post-consonantal *h*, although it does not lead to particu-

³⁰ B. Fidjestøl, *The Dating*, pp. 194–195.

³¹ A. C. Amos, *Linguistic Means of Determining the Dates of Old English Literary Texts* (Cambridge, Mass.: The Medieval Academy of America, 1980), p. 1.

³² A. C. Amos, "Linguistic Means," p. 29.

larly fine-structured datings, has not been invalidated,”³³ “[t]he test of contraction [after loss of intervocalic *h*] does not provide reliable evidence for dating Old English texts,”³⁴ “in general Sarrazin’s test of contraction in negated verbs and in compounds seems of little value,”³⁵ “[t]here is simply not a sufficient spread to the results of the syllabic consonant test to allow of any confidence in its dating value.”³⁶

A poet’s metrical practice with respect to the alliteration of palatal and velar *g* is probably a valid chronological criterion, as long as it is recognized that metrical practice is in part a function of personal poetic style, and there is no *a priori* reason that a late poet might not adopt an earlier, traditional style. Nevertheless, it is difficult to imagine a late poet depending for functional alliteration on sounds that no longer alliterated in most of his hearer’s ears ... Therefore, although the change in alliterative practice may have occurred at different times in different dialects, in general we can consider those poems in which *g* and *ġ* can be proved to alliterate earlier than those in which *g* and *ġ* can be proved not to alliterate.³⁷

“Nevertheless, phonological methods of dating, although they cannot be applied to all Old English literature as a chronological ‘test,’ are valid means of establishing dates and can be of great value in dating individual works.”³⁸

4.2 Syntactic and Grammatical Tests

“[T]he ‘Lichenheld test’ of the definite article and the weak adjective are not reliable for either prose or verse.”³⁹

Lichenheld, Groth, and others are right to point out the reduced use of the instrumental case in later Old English. But the case was always available to a writer who wished to use it. Neither a preference for nor the avoidance of the instrumental case provides sufficient grounds for dating a text, especially since scribal alterations will tend to disguise authorial practice.⁴⁰

“His [Mürkens] criterion of the relative pronoun must be judged as unproved in general, and unusable because of scribal variants,”⁴¹ “[t]here is no reason to trust that the unexpressed subject—or its replacement by a personal

³³ A. C. Amos, *Linguistic Means*, p. 35.

³⁴ A. C. Amos, *Linguistic Means*, p. 49.

³⁵ A. C. Amos, *Linguistic Means*, p. 66.

³⁶ A. C. Amos, *Linguistic Means*, p. 76.

³⁷ A. C. Amos, *Linguistic Means*, p. 102.

³⁸ A. C. Amos, *Linguistic Means*, p. 208.

³⁹ A. C. Amos, *Linguistic Means*, p. 124.

⁴⁰ A. C. Amos, *Linguistic Means*, p. 128.

⁴¹ A. C. Amos, *Linguistic Means*, p. 131.

pronoun or *hit*—can validly indicate the relative dating of Old English literature,”⁴² “[a]n author’s sentence structure does not provide a reliable indication of the date of his work.”⁴³

4.3 Lexical Tests

In his conclusion on the lexical tests, Amos is generally optimistic: “within the limits sketched in this discussion, studies of the vocabulary of Old English can be of great value in assigning relative and absolute dates to Old English texts.”⁴⁴

4.4 Stylistic Tests

Concluding his stylistic tests, Amos writes that these tests are less objective than the other tests, but that some of them are helpful in determining “the sequence of an individual author’s works and the use of a broad cultural criterion in dating.”⁴⁵

4.5 Summary

Amos’s overall conclusion is that though a number of tests are promising in terms of providing valid criteria for dating texts, most of the tests are of no or limited value: “Most of the linguistic tests, however, are so limited by qualifications that they do not provide clear unambiguous, objective evidence with respect to date.”⁴⁶ The most important lesson to be learnt from Amos’s analysis is therefore his methodological rigor in discerning between linguistic typology and chronology on the one hand, but also his demonstration of certain linguistic tests—especially regarding vocabulary—as valid criteria for linguistic dating.

5. EDDIC POETRY

The Poetic Edda—as opposed to *The Prose Edda*—is the older of the two Eddas and therefore sometimes called the *Elder Edda*. It consists of fif-

⁴² A. C. Amos, *Linguistic Means*, p. 135.

⁴³ A. C. Amos, *Linguistic Means*, p. 140.

⁴⁴ A. C. Amos, *Linguistic Means*, p. 156.

⁴⁵ A. C. Amos, *Linguistic Means*, p. 164.

⁴⁶ A. C. Amos, *Linguistic Means*, p. 167.

teen mythical and twenty-three heroic poems written down in the eleventh to thirteenth centuries and eventually put together by an anonymous person at the end of the thirteenth century as *The Poetic Edda*. The date of origin of the various poems has long been under discussion, but the poems are, nevertheless, a primary source for our knowledge of ancient Norse paganism. These Icelandic poems or *eddas* have had a great influence on western culture. Wagner's operas are largely based on incidents from the Edda, via the *Nibelungenlied*. J. R. R. Tolkien also plundered the Eddas for atmosphere, plot material, and the names of many characters in *The Hobbit*, and *The Lord of the Rings*.⁴⁷

Fidjestøl, in his historical linguistic analysis, considers three types of criteria for dating the poems, namely linguistic criteria, stylistic criteria and influential hypotheses about different types of influence on the poem. Fidjestøl begins his research—as noted above—with the disheartening remark that “[n]ot very much is to be expected from dating Eddic poetry on linguistic grounds. The most obvious reasons for this is the fact that bringing the language more or less up to date will be a normal part of the oral transmission of a poem.”⁴⁸ Instead of backing away from the task Fidjestøl finds, however, that there are certain traces of linguistic stratification in the poems. The control material for the historical linguistic analysis of Eddic poetry is the more fixed and thus datable text of *skaldic poetry*.⁴⁹ These are the conclusions of Fidjestøl.

5.1 The Expletive Article

Fidjestøl mentions as his first example the expletive article *of/um* that, in Old Norse, “had no definite semantic meaning content any longer, but merely served as a reminder that the word in question had once been derived with the help of a prefix.”⁵⁰ Referring to the control material, Fidjestøl continues that

Kuhn's well-known observation that the overall use of the particle in skaldic poetry decreases as time passes. On the basic assumption that Eddic poetry be-

⁴⁷ See <http://www.sacred-texts.com/neu/poe> for H. A. Bellows's translation of *The Poetic Edda*. Links to other online translations can be found at <http://www.ugcs.caltech.edu/~cherryne/edda.html> and <http://runeberg.org/eddan>.

⁴⁸ B. Fidjestøl, *The Dating*, p. 201. For a helpful and most recent discussion on methodological problems and linguistic development in Old Norse see the various essays in R. McTurk, ed., *A Companion to Old Norse-Icelandic Literature and Culture* (Oxford: Blackwell Publishers, 2005).

⁴⁹ See <http://www.vikinganswerlady.com/ONLiteratureBibliography.htm> for an excellent introduction to Viking literature with a helpful bibliography.

⁵⁰ B. Fidjestøl, *The Dating*, p. 207.

haves in this respect in the same manner as skaldic poetry, this decrease may, according to Kuhn, be used as a means of dating Eddic poetry.⁵¹

Fidjestøl concludes:

I think they [statistics on the expletive particle] may be used as valuable and positive criteria in a rather restricted and abstract manner. Provided that the Eddic poems in Codex Regius received their original linguistic form in the same period as the skaldic poems from Bragi to *Nóregs konungatal*, and that this form is rendered by the manuscript in a reasonably faithful manner, it is difficult to see any reason why the language of Eddic poems should not reflect *grosso modo* the same linguistic development as skaldic poetry as far as the use of the particle is concerned. If so, the criterion may in my opinion serve as a kind of control for a more comprehensive hypothesis bearing on the chronology of the totality or a larger part of Eddic poetry, even if it is of little use as a means of dating any particular poems.⁵²

Fidjestøl warns, however, that

it should be noted that the use of the particle in Eddic poetry or parts of Eddic poetry might be an aspect of style related to particular authors or particular subject matters rather than related to chronology. According to Ingerid Dal, "In Eddic poetry *of* has mainly become a stylistic device," and the particle is above all used "in lively narrative." It may also be tempting to surmise that some poets used it as a deliberate means of archaizing the style.⁵³

5.2 Words Beginning with *vr-*

In preliterate Old (West) Norse, words beginning in *vr-* lost their initial *v-*, thus creating a distinct isogloss between the West Norse and the East Norse languages. In Old Norse manuscripts therefore no word beginning in *vr-* normally occurs. In a number of cases this linguistic change has affected the alliteration, however, so that the forms in *vr-* must be restored in order to get normal metric forms. Very early in the history of Eddic scholarship it was observed that this might serve as a criterion for dating, in the sense that poems containing such cases were supposed to antedate the shift.⁵⁴

Fidjestøl concludes:

According to this test, *Vafþrúðnismál*, and probably *Atlakviða* and *Lokasenna*, possibly also *Hávámál* I, *Baldur's draumar* and *Sigrðífumál*, have a higher relative age than *Grípisspá*, *Atlamál*, and probably *Helgakviða Hjörvarðssonar*,

⁵¹ B. Fidjestøl, *The Dating*, p. 208.

⁵² B. Fidjestøl, *The Dating*, p. 205.

⁵³ B. Fidjestøl, *The Dating*, p. 228.

⁵⁴ B. Fidjestøl, *The Dating*, p. 231.

Alvíssmál, and *Grógaldur*, possibly also *Grímnismál*. The absolute age can only be suggested very broadly. Allowing for a period of overlap of about a century, one might suggest a dating of the early group before the year 1000, and the late group after 900.⁵⁵

5.3 Contracted Forms

The next analysis concerns the contraction test.

The contraction test concerns the development of hiatus, which in Old Norse word-forms can be found only where a long vowel is followed by a short vowel. In the course of the Old Norse period there was a tendency to replace these by shorter forms without hiatus, with the loss of one syllable, and the occurrence of such forms may thus reveal something of the age of the text.⁵⁶

Fidjestøl concludes: "The main result of the examination of this test, however, is that it is extremely inconclusive."⁵⁷

5.4 Kennings

Fidjestøl refers next to Jan de Vries's assumption that

a brief mention of some aspect of mythology in a kenning⁵⁸ must presuppose a more general and widespread knowledge of the underlying myth, without which the kenning would not be comprehensible. ... In collecting his material, de Vries observed that the number of mythological kennings in skaldic poetry was very unevenly distributed throughout the centuries, and this fact may have some bearing on the general history of religion in Scandinavia, he surmised. ... De Vries's most conspicuous finding is a veritable boom of mythological kennings in the latter half of the 10th century, followed by an abrupt slump after the introduction of Christianity in the first half of the 11th century. This decrease is in close accordance with what might be expected, of course, but more remarkable is a lesser, but clearly recognizable increase in the latter half of the twelfth century, which de Vries interpreted as evidence for a revival of heathen antiquity, a sort of 12th century Renaissance.⁵⁹

Fidjestøl, referring to Hans Kuhn, criticizes Vries's methodology by calling for a distinction between poetry directed to the Christian kings of Norway,

⁵⁵ B. Fidjestøl, *The Dating*, p. 245.

⁵⁶ B. Fidjestøl, *The Dating*, p. 246.

⁵⁷ B. Fidjestøl, *The Dating*, p. 259.

⁵⁸ A figurative, usually compound expression used in place of a name or noun, especially in Old English and Old Norse poetry; for example, *storm of swords* is a kenning for *battle*. It could—and probably should—be argued that kennings should be classified as literary devices rather than linguistic evidence.

⁵⁹ B. Fidjestøl, *The Dating*, pp. 270–271.

whose opposition to the old religion was notorious, and the poetry composed back in Iceland. In Iceland, skaldic traditions may well have continued more or less as before, but it so happens that with few exceptions, purely Icelandic poetry from the latter half of the 11th century is lacking, because this period is rather inadequately covered by the family sagas. The decline demonstrated by de Vries's statistics, Kuhn therefore explained by the fact that very little poetry is known from this period, apart from poems made for the Norwegian court. As Eddic poetry is supposed to have been composed mostly in Iceland, de Vries's conclusion concerning the impossibility of Eddic mythological poetry in the period between 1000 and 1150 is completely untenable.⁶⁰

And Fidjestøl concludes: "This study thus reinforces the criticism of de Vries's hypothesis, which in my view seems to lack any solid foundation."⁶¹

As was the case with Amos's analysis of Old English poetry, Fidjestøl is also fully aware of the methodological problems involved in interpreting the linguistic evidence, and again the lesson must be to exercise great caution in using linguistic criteria for accurate dating. Fidjestøl also finds, however, that certain tests do provide promising results for at least a relative dating of Eddic texts, and that there is no need for the historical linguist to back away from the task as long as the necessary methodological rigor is observed.

6. HEBREW HISTORICAL LINGUISTICS IN THE OPEN

We may now return to the questions raised in the introductory remarks. Just how optimistic can we be—in the light of current historical linguistic theory and the mentioned case studies—about the possibility of and accuracy in converting linguistic typology into linguistic chronology? What happens when historical Hebrew linguistics is brought "out in the open"? Can the arguments for diachronic variation in Biblical Hebrew put forward by Eskhult, Hurvitz, Polak, Rendsburg and others be methodologically substantiated, or must we surrender to the more cautious or sceptical approach voiced by others?

First, since both standard text books on historical linguistic method and the historical linguists involved in the two case studies mentioned above seem to use a very restrained language in describing the possibility of dating texts linguistically, we should expect historical *Hebrew* linguists to be equally cautious unless there are *exceptionally* good grounds for the opposite. There are, for example, too many unknown factors involved, to *insist* that the differ-

⁶⁰ B. Fidjestøl, *The Dating*, pp. 273–274.

⁶¹ B. Fidjestøl, *The Dating*, p. 293.

ences between texts in the Hebrew Bible known to be late and allegedly older texts are evidence of genuine diachronic development or language change.

Second, however many methodological *caveats* there may be (not least the problem of comparing Hebrew *prose* with Old English and Old Norse *poetry*), there are at least *some* literary, historical, and linguistic reasons for describing linguistic criteria useful for dating autographs or earlier copies of texts in late, extant manuscripts. As this is the case in historical linguistic case studies where similar methodological problems with text history and control material exist, there should be enough evidence to establish *some* kind of diachronic grid in the development of the Hebrew language that can be used for at least a relative dating of texts that cannot be dated on other grounds.

Third, although we have only looked at two comparative case studies, syntactical and lexical tests seem to be the most promising criteria for dating texts on mere linguistic grounds, especially when current theory on linguistic change in the syntactical structures of language is taken into consideration. This means that, while Davies is right in asserting that Hurvitz “is incorrect to *insist* [*italics added*] that he must be right,” Hurvitz nevertheless seems to have got the upper hand by focusing on lexicographical change. And Eskhult is likewise convincing in his demonstration that

loanwords that could not possibly have entered Hebrew before a certain time in history present themselves as stronger criteria when judging the date of certain texts than loanwords that may have passed into Hebrew at an earlier period.

By focusing on the—theoretically speaking—most likely areas of valid diachronic linguistic criteria, Hurvitz, Eskhult, and others may not have provided conclusive evidence for the absolute (pre-Persian) age of texts attesting the allegedly older lexeme *sefer* or texts void of Persian loanwords, for the Persian-period gap in Hebrew inscriptions still makes it impossible to assess precisely *when* the allegedly younger lexeme *ʿiggeret* or certain Persian loanwords entered Hebrew. But even when the abovementioned *caveats* on multiple *contemporary* grammars and lexemes that may have existed centuries prior to their first attestation must be taken into consideration, it must be acknowledged that the lexical focus is right and that the evidence provided does seem to make at least a *relative* dating possible that makes a pre-Persian dating more likely than a Persian one.