

# Opuscula Ugaritico-Accadico-Hebraica: Relative particles, pa‘am, and Amraphel

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# OPUSCULA UGARITICO-ACCADICO- HEBRAICA: RELATIVE PARTICLES, PA<sup>ʿ</sup>AM, AND AMRAPHEL

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This paper is devoted to a few thorny issues relating to Hebrew, Ugaritic, and Akkadian. First, I tackle the vexing and multifarious question of the relationship between the Hebrew relative particles and their East Semitic counterparts, building on work by John Huehnergard. In the latter part of the paper, I turn my attention to the relationship between the Ugaritic word *pamt* (“time,” as in “three times”) and Hebrew *paʿam* (“time” or, rarely, “foot”), which also seems to have a similar counterpart in the Ugaritic *pʿn*, meaning “foot”—and its possible phonological connection with the background of the biblical name Amraphel. In both of these matters, issues of possible conflation between different roots and inner-Semitic transdialectal borrowing arise, making the discussions methodologically related. However, as will be argued in the article, different processes may actually have been involved in the two cases.

## 1. A MARRIAGE AMONG RELATIVES: ON THE PARTICLES *šEC-* AND *šA* IN BIBLICAL HEBREW AND AKKADIAN

The purpose of this first part of the article is to discuss some aspects of the problems inherent in the background of the Hebrew relative particle *šEC-/šA* and its etymology—as well as its possible relationships with or to the more common Biblical Hebrew relative particle *ʾāšer* and the Akkadian relative pronoun/particle *šA*, which is sometimes associated with *šEC-/šA*.

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due to its similarity in both form and meaning. It is certainly rather strange that Biblical Hebrew seems to have possessed two relative particles (three, if one also counts the more uncommon  $\text{שָׁן}$ ). Much ink has been previously spent on these questions, yielding a number of different positions; not least influential has been the trajectory laid out already by Bergsträsser (1909),<sup>2</sup> that  $\text{’āšer}$  was originally a word meaning “place,” which was subsequently expanded into meaning something like “the place where” and thence took the final step towards becoming a general-purpose relative particle, whereas  $\text{šeC-}/\text{šaC-}$  was the original Hebrew relative morpheme, itself etymologically related to Akkadian  $\text{ša}$ . At least the first part of this model—that the word  $\text{’āšer}$  developed out of a word meaning “place” (cf. Ugaritic  $\text{atr}$  and Akkadian  $\text{ašru}$ )—can be regarded as more or less a standard view today.<sup>3</sup> The other half—that concerning  $\text{šeC-}/\text{šaC-}$ —is, however, much less clear-cut.

***THE SUGGESTIONS OF JOHN HUEHNERGARD: ŠE C- / ŠA C- AS SHORTENED FORMS OF ’ĀŠER***

In a 2006 article, John Huehnergard published a revised scenario concerning the background of these words, which stood some of Bergsträsser’s views on their heads. According to Huehnergard, the seeming correspondence between Hebrew  $\text{šeC-}/\text{šaC-}$  and Akkadian  $\text{ša}$  is wholly coincidental; basing himself on classical, Neogrammarian-style comparative linguistic methodology, he shows that the two words can hardly be related, given what else is known about their background. He posits, instead, that Hebrew  $\text{šeC-}/\text{šaC-}$  is actually a shortened (unaccented) version of  $\text{’āšer}$ , and that the actual cognate of Akkadian  $\text{ša}$  is the rather infrequent Hebrew relative  $\text{שָׁן}$ .<sup>4</sup>

In the main, I quite agree with Huehnergard’s arguments and conclusions. In this article, I would like to add some ideas

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<sup>2</sup> G. Bergsträsser, “Das hebräische Prefix  $\text{שֵׁ}$ ,” *ZAW* 29 (1909), 40–56.

<sup>3</sup> There is textual material from Emar that shows that this type of development of the “place” word could appear sporadically in Akkadian-language contexts as well (even though it never attained “standard language” status): see B. Faist and J.-P. Vita, “Der Gebrauch von  $\text{ašar}$  in den Akkadischen Texten aus Emar,” *WO* 38 (2008), 53–60. The authors regard this use as not necessarily representing West Semitic substrate influence but as being the result of “der lokalen, sog. Syrischen Schreibtradition . . .” On the wider matter of relative clauses in the Semitic languages generally, I may mention (in addition to the specific references appearing below) the anthology volume J.C.E. Watson and J. Retsö (eds.), *Relative Clauses and Genitive Constructions in Semitic* (JSSSup 25; Oxford: Oxford University Press on behalf of the University of Manchester, 2009).

<sup>4</sup> J. Huehnergard, “On the Etymology of the Hebrew Relative  $\text{שֵׁ}$ ,” in S.E. Fassberg and A. Hurvitz (eds.), *Biblical Hebrew in its Northwest Semitic Setting: Typological and Historical Perspectives* (Publication of the Institute for Advanced Studies 1; Jerusalem: The Hebrew University Magnes Press, 2006), 103–25.

of my own to the structure that he has built, agreeing on the central points yet disagreeing in some details.

I believe that Huehnergard is quite right in arguing that the attempts to connect Hebrew *šeC-/šaC-* and Akkadian *ša* merely based on phonetic similarity do not stand up to scrutiny. In his 2006 article, he argues instead that the Akkadian word, derived from an earlier form *\*θa*, probably represents an initially devoiced variant of the Proto-Semitic relative pronoun series in *\*ð-* (nominative *\*ðū* etc.), which is also reflected in Ugaritic *d*, Aramaic *dī/dē-*, Arabic *ðu* and others (see further on for later, and somewhat revised views of Huehnergard's, published in collaboration with Na'ama Pat-El).

This line of reasoning leaves no place for Hebrew *šeC-/šaC-* as a cognate, unless one reckons with a spontaneous devoicing of *\*ð* to *\*θ-* in pre-Hebrew as well (as *\*θ* does indeed regularly yield *š* in attested Hebrew). Such an identical and spontaneous process in both Hebrew and Akkadian is hard to swallow and is made even less probable by the fact that the regular reflex of the nominative *\*ðū* actually does appear in Hebrew as *šū*, its “mechanical” descendant. This, then, would mean that the similarity between the Hebrew and Akkadian words would be due to chance. As mentioned earlier, Huehnergard views Hebrew *šeC-/šaC-* as a shortened and prefixed form of *'āšer*. The beauty of his proposal lies in the fact that all of the Hebrew relative particles are explainable in terms of inner-Hebrew processes and that they would all have convincing etymologies.

#### **SOME DIFFICULTIES WITH HUEHNERGARD'S PROPOSAL**

The problem inherent in this otherwise very compelling argument of Huehnergard's is the fact that it itself requires two rather unexpected changes in the lexical material involved—one in Northwest Semitic and one in Akkadian. It is these questions that I here intend to tackle with some ideas of my own.

If, as Huehnergard surmises, Hebrew *šeC-/šaC-* is simply a reduced variant of *'āšer* (from *\*'aθar*, originally meaning approximately “[the] place [where]”), one has to wonder why both forms exist in the Hebrew language—and sometimes in the same text (even though such a change in pronunciation would at first be in a state of flux, one would rather have expected the forces of textual normativity to edit one of the variants out). Why was *'āšer* sometimes shortened in practice, or rather: why did the shortened version (*šeC-/šaC-*) sometimes gain ground at the expense of the longer one? And, on a related note, why did the Canaanite languages introduce this word *\*'aθar* as a relative particle in the first place, into a system that also had reflexes of *\*ðū* readily available?

As concerns the Akkadian word, Huehnergard (2006) posits a devoicing of the initial consonant of the original Proto-Semitic pronoun *\*ðū* (with genitive *\*ðī* and accusative *\*ðā*), which would have produced forms in *\*θ-*, which in turn would subsequently yield Akkadian *š-* by regular sound laws—or possibly the opposite development, the original form then having shown *\*θ-* and West Semitic having voiced it. Regardless of which of these

two was actually involved, the question here is: what would be the reason for that spontaneous devoicing or voicing? This type of spontaneous phonological change is irksome.<sup>5</sup>

A detailed critique of Huehnergard's hypotheses was offered in 2007 by Holmstedt,<sup>6</sup> who rejected the idea of Hebrew *šeC-/šaC-* and *'āšer* being connected, and rather opted for explaining the former as being linked etymologically to the Akkadian relative, perhaps representing the result of language contact, as an actual borrowing from Akkadian *ša*.<sup>7</sup> Huehnergard's ideas were subsequently defended by Pat-El,<sup>8</sup> and in the main, I find her arguments convincing. The idea of one single piece of the pronominal system being spontaneously borrowed from Akkadian into Canaanite does not quite inspire confidence; of course, such things do happen, but they are certainly not common. However, even though I find Huehnergard's analysis generally to be very well-argued, I believe that it may not be quite necessary to ascribe the similarity between the Hebrew and Akkadian particles to pure chance; there are other possibilities than pure borrowing that may elucidate the matter, other models that may be posited. This is what I shall attempt to do here. First, we will discuss the Hebrew developments (presupposing that the original relative pronoun was indeed *\*šin*); we will return to the Akkadian development later on.

#### ***A NEW PROPOSED SOLUTION: MORPHOLOGICAL CONFLATION***

What I would argue here about the relationship between Hebrew *'āšer* and *šeC-/šaC-* is the following. The Northwest Semitic (proto-Canaanite?) word *\*'aθar* first started to acquire the meaning "the place where," a meaning quite syntactically logical in itself, given that the base noun was probably in the construct state.<sup>9</sup> This, if we follow Huehnergard's idea about a shortened

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<sup>5</sup> See below for further developments of Huehnergard's analysis (in collaboration with Na'ama Pat-El, an article from 2018) suggesting some possible answers to these questions, which, however, differ from those I will argue here.

<sup>6</sup> R.D. Holmstedt, "The Etymologies of Hebrew *'āšer* and *šeC-*," *JNES* 66 (2007), 177–91 (esp. 183).

<sup>7</sup> His ideas are further elaborated in R.D. Holmstedt, *The Relative Clause in Biblical Hebrew* (Linguistic Studies in Ancient West Semitic 10; Winona Lake: Eisenbrauns, 2016), 98–101, where he states that early occurrences of *šeC-/šaC-* reflect "Phoenician or Assyrian influence on Northern Israel" and that later instances have to do with Akkadian pressure in the Judahite exile communities in Babylonia. The former is somewhat problematic, however, as Neo-Assyrian pronounced the /š/ phoneme as [s], which would fit rather poorly with the Hebrew pronoun. However, it must also be granted that the early Hebrew pronunciation of /š/ is not necessarily a clear-cut question.

<sup>8</sup> N. Pat-El, "The Syntax of *'āšer* and *šeC* – Yet Again," in R. Haselbach and N. Pat-El (eds.), *Language and Nature: Papers Presented to John Huehnergard on the Occasion of his 60<sup>th</sup> Birthday* (SAOC 67; Chicago: Oriental Institute of the University of Chicago, 2012), 319–27.

<sup>9</sup> Note that even the Hebrew Bible itself gives testament to the type

or unstressed/proclitic version of the word appearing, would have produced a reduced or quickly pronounced form perhaps looking something like \* $\theta ar$ , or \* $\theta a$ - followed by gemination of the initial consonant of the following word.<sup>10</sup>

When this first relativizing and grammaticalizing step was taken in the case of \* $\theta ar$ —or \* $\theta a(r)$ —I would propose that a further process took hold (and now we come to a part of my suggested answer to the questions above). To discuss this, we have to remember that the changes and developments here delineated must have taken place at an early point—not Proto-Northwest Semitic itself (as neither Aramaic nor Ugaritic shows any other relative marker than the inherited \* $\partial u/ \partial i/ \partial \bar{a}$ )<sup>11</sup> but at quite an early point in the history of what would become Canaanite.<sup>12</sup> Such an early date could well imply that  $\theta$  was still a separate phoneme in pre-Canaanite at that time, not yet having coalesced with  $\partial$ . If this was so, and the original relative pronoun in \* $\partial$ - was also pronounced with an interdental fricative (voiced, in that case), the following possibility presents itself: the nascent relative particle \* $\theta a(r)$  may have been synchronically identified or conflated with the inherited relative pronoun \* $\partial$ -, especially with the accusative form of the latter, \* $\partial \bar{a}$ .<sup>13</sup> The formal similarity between \* $\theta a$  (+ gemination) and \* $\partial \bar{a}$  is rather striking, especially if one reckons with the possibility of the voiced sound \* $\partial$  and the preglottalized sequence \* $\theta$  having been to some extent phonetically identified with each other. The end of the respective morphemes both include an  $a$  sound and a heavy syllable (a long  $\bar{a}$  in one case and an implicit gemination in the other). My proposal is, therefore, that these two relative morphemes were conflated by speakers of early Canaanite and partly identified.

This would provide a solution to a number of questions partly posed earlier: (1) Why does the shortened version of \* $\theta ar$  appearing as  $\partial C-$ / $\partial \bar{a} C-$  even exist? (2) why does it look so much

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of construction that probably served as template for this grammaticalization. See, e.g., 1 Kgs 21:19, which includes the words *bimqôm*  $\partial \bar{a} \partial$ , literally “in the place where,” with the word *maqôm* inflected as though in a construct relationship with  $\partial \bar{a} \partial$  itself.

<sup>10</sup> For the assimilation of the  $-r$ , Huehnergard, (“Etymology of the Hebrew Relative  $\partial$ ,” 121–22) compares with the Babylonian Aramaic word  $\partial \bar{m} \bar{a}$ , “to say,” which is derived from an original  $\partial \bar{m} \bar{a} r$ .

<sup>11</sup> As pointed out by Huehnergard (“Etymology of the Hebrew Relative  $\partial$ ,” 105), there are not many examples of \* $\theta ar$  in non-Hebrew Canaanite languages either: one example in Moabite and one in Edomite.

<sup>12</sup> Huehnergard states that the particle appearing in Hebrew as  $\partial C-$ / $\partial \bar{a} C-$  must have been in existence in the 12<sup>th</sup> century BCE; this rests upon the dating of the Song of Deborah to that period. This is, of course, a rather classical dating, although one must acknowledge that it would be very tough to swallow for exegetes trained in the *literarkritische* traditions of continental Europe. I personally find the dating at least possible, however.

<sup>13</sup> Note that the Akkadian relative  $\partial \bar{a}$ , too, continues an accusative form (a fact actually mentioned in Huehnergard, “Etymology of the Hebrew Relative  $\partial$ ,” 115).

like the Akkadian relative pronoun *ša* (derived from the *\*ð*-pronouns)? and (3) how did a word originally meaning “[the] place [where]” acquire the full grammaticalized properties of a complete and functioning relative particle applicable to all relevant uses?

If the Hebrew short particles actually represent a conflation between *\*’aθar* and the inherited relative pronoun in *\*ð*-, questions (1) and (3) are easily answered. The conflation carried the relative force of a general relative pronoun into a nascent local-relative particle. From the perspective of the original relative pronoun, it could be said that it lost the voicing of its initial consonant due to the identification of that consonant with the sequence *\*’θ*, which yielded *\*θaC-* (with *C* standing for the gemination of the following consonant) and subsequently Hebrew *šaC-/šeC-*. The similarity with the Akkadian relative pronoun (question 2) is not accidental, but is due to a partly common origin in the *\*ð*-series, the initial consonant of which was separately devoiced in both subphyla (we will soon return to the development of the Akkadian word; note that the devoicing in Canaanite would not be spontaneous under this analysis, but conditioned by a concrete morphophonological process). Also, the conflation probably provided an additional impetus for the “shortening” of the word and the loss of the final *\*-r* as an autonomous consonant. Of course, one may argue (as Huehnergard does) that these shortening changes could be due only to a general tendency for unstressed particles to be reduced, but I believe such an explanation to be somewhat *ad hoc* or at least unnecessary: the idea of a conflation with the old relative pronoun carries explanatory power both for the form of *šaC-/šeC-* and for the development of its grammaticalized meaning.

If the form of the inherited relative pronoun that was conflated with the shortened form of *’aθar* was indeed the accusative, *\*ðā*, this would provide answers to yet more problems. The use of the accusative specifically would fit very well with an identification with an originally local noun (and subsequently particle), given the prominent use of the accusative case as a general adverbial form in Semitic languages.<sup>14</sup> It is not at all hard to imagine a relativized frozen word meaning “[the] place [where]” coalescing with a form of a relative pronoun that, among other uses, could answer the question “where?”

Analogical pressure from the (short) *a*-vowel of the nominal accusative (which was probably still present at the early stage of Proto-Canaanite at which these developments must have taken place) may, in fact, have helped in the conflation between *\*’aθar* (> *’āšer*) and old *\*ðā*. At the time when the Canaanite *\*a>ō* shift was probably starting to make itself heard,<sup>15</sup> just keeping

<sup>14</sup> For an overview of uses of the accusative in various Semitic languages, see R. Hasselbach, *Case in Semitic: Roles, Relation, and Reconstruction* (Oxford Studies in Diachronic and Historical Linguistics 3; Oxford: Oxford University Press, 2013), 260–313 (esp. the table on p. 292).

<sup>15</sup> That the Canaanite shift had appeared during the Late Bronze Age can be seen in the form *a-nu-ki* for “I” appearing in the Amarna

the accusative  $*\delta\bar{a}$  would have resulted in  $**\delta\bar{o} > **z\bar{o}$ , the latter actually appearing very rarely as a variant form of the feminine demonstrative  $z\bar{o}'t$  (twice in the Hebrew Bible, in Ps 132:12 and Hos 7:16). By conflating  $*\delta\bar{a}$  with  $*'a\theta ar$ , the accusative morphology of the relative pronoun could in a sense be “saved” from the workings of the  $*\bar{a} > \bar{o}$  shift and thus retain the formal characteristics of an accusative in  $-a$  (cf. how the gemination in the *wayyiqtol* form—whatever the origin of that gemination—helped preserve the vocalism of the ancient narrative construction).

A conflation having taken place between  $*'a\theta ar$  and  $*\delta\bar{a}$  could provide an example of a process similar (though not identical) to the type of word creation termed “phono-semantic matching” by Ghil'ad Zuckermann.<sup>16</sup> This term refers to cases in which a word is borrowed from one language into another in an attempt to create a match not only in the meanings of the words but in their phonological shape as well. Of course, in this case the question would not be one of borrowing of etymologically foreign lexical material but of two inherited words being both phonologically and semantically identified with each other, but the process of reinterpretation and phonological matching of two semantically close words would be similar.

The proposed conflation would also provide a clue to the reason for the shorter relative particle appearing as both  $\text{\textit{\$}a}C-$  and  $\text{\textit{\$}e}C-$ . The vacillating vocalic quality could then be due to the forms representing a reception of either the  $a$ -timbre of  $*\delta\bar{a}$  or the  $e$ -vowel of the developing form  $'\bar{a}\text{\textit{\$}er}$ . In this context, it is highly interesting to note that in what is in all probability the oldest attestation of the conflated particle, the Song of Deborah (Judg 5:7), the form with  $a$  is used (according to the Masoretic tradition), indicating an early stage in the development of the particle, at which the original vowel of the accusative relative pronoun was still visible and had not yet been obscured by contamination with the  $e$  of  $'\bar{a}\text{\textit{\$}er}$ . The vocalization  $\text{\textit{\$}a}C-$  also occurs in Cant 1:7 (together with  $\text{\textit{\$}e}C-$  in the same verse!). The possible attestation of  $\text{\textit{\$}a}C-$  in Job 19:29 is probably illusory, however; the word written  $\text{\textit{\$}dyn}$  is to be read  $\text{\textit{\$}adday\bar{m}}$ , a reference to the class of deities known from the Deir 'Alla inscription and from the Hebrew divine name  $\text{\textit{\$}adday}$ .<sup>17</sup> Given the early attestation of  $\text{\textit{\$}a}C-$

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literature (probably representing /'anōki/), as opposed to the Ugaritic  $a-na-ku$  (for /'anāku/) from roughly the same time period; on this phenomenon, see A. Rainey, *Canaanite in the Amarna Tablets: A Linguistic Analysis of a Mixed Dialect Used by the Scribes from Canaan, Vol. 1: Orthography, Phonology: Morphosyntactic Analysis of the Pronouns, Nouns, Numerals* (HdO, 1/25; Leiden: Brill, 1996), 47–48. One may note that the relative scarcity of relative uses of  $*'a\theta ar$  in Canaanite languages other than Hebrew rather points to a larger proliferation of the word in this grammaticalized use at a date later than the Proto-Canaanite linguistic unity at which the vowel shift started.

<sup>16</sup> For an introduction to this linguistic concept, see, e.g., G. Zuckermann, *Language Contact and Lexical Enrichment in Israeli Hebrew* (Palgrave Studies in Language History and Language Change; Houndmills/New York: Palgrave Macmillan, 2003), 34–37.

<sup>17</sup> The analysis of  $\text{\textit{\$}dyn}$  in Job 19:29 as having to do with Shaddai

(on which the occurrence in Canticles may well be modeled), we may regard that form as representing a transitional stage in the history of the word.

The model presented here presupposes that the appearance of the ancestor of *šeC-/šaC-* was early enough that the interdental *š* was still separate phonemes (and had not yet shifted to sibilants), which means a point in time not too far removed from the grammaticalization of *ʾaθar* itself (as it only occurs in Canaanite and thus probably post-dates Proto-Northwest Semitic<sup>18</sup>). It may be objected that this close temporal relationship between the appearance of the forms makes their co-occurrence unlikely.<sup>19</sup> Why would a rather recently grammaticalized particle so quickly develop a co-occurring and competing variant? The answer may be that, for a time, the ancestor of *šeC-/šaC-* was still thought of as “really being” the *\*ð*-pronoun (which had long been handed down through normal linguistic inheritance, and thus was not exceptional), though “tainted” with the word-final *-r*. The word *\*ʾθa(r)* may thus not have been thought of by the speakers of this early stage of the language as a version of *\*ʾaθar*, but rather as a somewhat altered version of (what had been) *\*ð*. Note that the assimilation of the *\*r* to the following word would in most cases have obscured the relationship with (or rather, influence from) *\*ʾaθar*, reinforcing the feeling that this was just the same old *\*ð*-pronoun living on. Thus, there is no need to presuppose that the speakers of this early form of the language necessarily regarded *\*ʾaθar* and *\*ʾθa(r)* as variants of the same synchronic lexeme. This would substantially lessen this chronological problem.

The prehistory suggested here would also suggest a possible partial explanation for *šeC-/šaC-* being in the main restricted to poetry and late prose in Biblical Hebrew (the classical prose works generally using *ʾāšer*). It is sometimes argued that this is due to linguistic “Northernism,”<sup>20</sup> yet one should not discount the possibility that the shorter relative particle was part of an archaic, poetic stratum of the Hebrew language—which would line up very well with the distribution of *zū*, the reflex of the nominative form of the ancient relative pronoun. Both the reflexes of the nominative and the (transformed) accusative of the relative *\*ð*-pronoun would then be part of a specifically poetic diction, whereas the fully relativized *ʾāšer* would be indicative of classical prose and more at home there, by virtue of being less archaic. In fact, such a division could actually provide some modest support for the idea of *šeC-/šaC-*, as well as *zū*, representing linguistic

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goes back to Loren R. Fisher, who regarded the word as a singular form—L.R. Fisher, “*šū* in Job XIX 29,” *VT* 11 (1961), 342–43.

<sup>18</sup> Unless, of course, one wants to posit wide-scale loss of the *ʾaθar* pronoun in Aramaic and Ugaritic—but that solution is extremely non-parsimonious and best avoided.

<sup>19</sup> This point, indeed, is raised by an anonymous reviewer.

<sup>20</sup> One should note that the concept of Northern/Israelian Hebrew is quite an uncertain proposition: see N. Pat-El, “Israelian Hebrew: A Re-Evaluation,” *VT* 67 (2017), 227–63, for some critical arguments on the issue.

“Northernism” (or at least dialectalisms): those two words represent different fossilized case forms of the inherited relative pronoun from various dialectal strata, and Aramaic (northern in relationship to Canaanite languages) preserves the third one (the genitive, *dh*).

Again, this analysis implies that, at least synchronically, the early forms of *šeC-/šaC-* was viewed not as a variant of the “place” relative but as a continuant of the old *\*ð*-pronoun. It also provides a rationale for the processes going forward in the history of the (textually attested versions of) the language. The form of Hebrew that grew into “standard biblical prose” let the grammaticalization of the new relative particle become full-blown, whereas the poetic (Northernizing?) stratum tried as best it could to preserve a remnant of the old relative particle—albeit only by means of a synchronic conflation with the prose-coded particle. The conflation into *šeC-/šaC-* would thus constitute a sort of semi-deliberate conservatism in the face of inexorable grammaticalizing change. It appears, then, to have been a matter of “saving what could be saved” from an older Northwest Semitic poetic diction by “hiding” the old particle within the new, so to speak. Of course, I do not mean this in the sense of conscious language planning (at least that was probably not the case, though one can of course never know for sure) but of spontaneous association—at least in some dialectal contexts—of a word occurring in traditionally transmitted (oral?) poetry with another that came into use in prose settings.

One could of course ask why *šeC-* was subsequently admitted into the standard prose language as part of Late Biblical Hebrew and subsequently in Mishnaic/Rabbinic Hebrew (where it becomes the normal relative particle, eventually ousting *’āšer* almost completely),<sup>21</sup> whereas *zū* was not. The simple reason may be that, even in the poetic language of earlier periods, the “old accusative” *šaC-/šeC-* was more common than *zū* and thus more amenable to transfer into a prose setting, and possibly also the fact that *šaC-/šeC-* shared part of its genesis and (more importantly from a synchronic point of view) its phonology with *’āšer*, the common prose relative particle, in a way which *zū* never

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<sup>21</sup> A recent study of the transitional stage between *’āšer* and *šeC-* style Hebrew prose can be found in W.R. Garr, *אשר and -ש in the Book of Ecclesiastes: A Linguistic Experiment* (American Oriental Series Essay 14; New Haven, CT: Pennsylvania State University Press, 2017). Garr argues (see esp. pp. 65–67) that the combined use of both pronouns in Ecclesiastes is indeed the result of historical transition, but that the two have acquired somewhat diverging (though overlapping) uses at that stage of linguistic evolution (*’āšer* being a stronger nominalizer). In the matter of the different Hebrew relatives, one could perhaps compare with the Phoenician use of *’š* and *š*—as well as *zū*—however, as pointed out in C.R. Krahmalkov, *A Phoenician-Punic Grammar* (HdO, 1/54; Leiden: Brill, 2001), 94–95, the *š* version does not seem to be a true relative pronoun in Phoenician or Punic but rather a “determinative” one, serving to introduce genitive relationships. However, the co-occurrence of the three variants may well have a similar background to the one delineated above for Hebrew.

did (the latter would, of course, imply that the earlier synchronic identification with the \**ð*-pronoun that I suggested above was waning with time, or at least that the phonetic similarity between *šeC-* and *ʾāšer* was apparent to the speakers and played a role in elevating the former to a status at first coequal with and finally supplanting the latter—and, thus, that the association between the two was, in a sense, “re-discovered”). Also, as *zū* continues the old nominative of the \**ð*-relative and *šaC-/šeC-* under this analysis represents an altered continuant of the accusative, this difference in frequency would be quite logical, as one often finds typologically that accusatives survive on at the expense of nominatives when the cases collapse—cf. Romance forms such as *nazione* from Latin *natione(m)*, and (within Hebrew itself) with the generalization of the oblique plural masculine *-im* from historical *-ima* at the expense of the historical nominative *-ima* (and the parallel developments in colloquial varieties of Arabic). The use of the old nominative *zū* in some poetic texts would then perhaps represent an even more archaizing type of poetic diction that actually kept not only a continuant of the \**ð*-pronoun but specifically its nominative form. This would create a system of three levels of diction: (a) one ousting the \**ð*-pronoun completely in classical prose settings (replacing it with what became *ʾāšer*), (b) one conservative and/or dialectal one, “saving” the old accusative of the \**ð*-pronoun—probably once a statistically common form—by conflating it with the local particle, forming *šeC-/šaC-*, and (c) one uncommon and probably deliberately archaizing one, actually keeping the old nominative *zū* as a poetic flourish.

Theoretically, one could toy with the possibility of the existence of Akkadian *ša* providing a sort of template or impetus for the increased use of *šaC-/šeC-* in later prose texts, but this sits oddly with the chronology: when the shorter particle starts gaining ground in Hebrew prose (the later part of the 1<sup>st</sup> millennium BCE), Akkadian was already deep in the process of being supplanted by Aramaic. If any such “pulling” influence was ever in action, it is more likely to have taken place at an earlier period, perhaps reflected in the use of *šaC-/šeC-* in early poetic texts. It should be clearly pointed out, however, that I do *not* believe that the *existence* of *šaC-/šeC-* is due to language contact with Akkadian, as does Holmstedt. If anything, this could be another instance of something akin to Zuckermann’s “phono-semantic matching”: the existence of *ša* as a relative pronoun in a prestige language could have increased the likelihood of a Hebrew speaker in the earlier periods using a phonetically and semantically similar word already created by language-internal processes.<sup>22</sup>

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<sup>22</sup> This type of increase in the use of a word due to the existence of a phonetically similar word with the same meaning in a well-known language is actually discussed by Zuckermann himself, who gives examples of this process in Modern Israeli Hebrew: see Zuckermann, *Language Contact and Lexical Enrichment*, 42, esp. n. 48. It should be noted that, as mentioned in footnote 7, there are phonological obstacles to

For the transition from the *ʾāšer*-style classical prose language into the *šC*-style language of Late Biblical Hebrew and Mishnaic Hebrew, one could perhaps compare with the sociological suggestions made for the rather sudden appearance of the classical Old Irish language from the very different Primitive Irish of the Ogham inscriptions. Koch suggested that the changes between Primitive Irish and Old Irish were due to the former having been the standard language of the pre-Christian Druid priesthood, whereas the latter represented popular speech that appeared in writing when the religious intelligentsia changed with the coming of Christianity, the older prestige variety thereby losing its standing.<sup>23</sup> Similar developments can be seen in the transition from Ancient Egyptian to Coptic.<sup>24</sup>

### ON THE AKKADIAN PRONOUN

It remains to provide a suggestion for why the apparent devoicing of *\*ð-* in Akkadian took place (to produce *ša* by way of a pronoun with initial *\*θ-*). From a methodological standpoint, it is necessary to provide some reason for this irregular development. In a footnote to his 2006 article, Huehnergard draws analogies to similar devoicing processes concerning relative particles in Punic and certain Neo-Aramaic dialects, and these analogies are of course possibly illustrative, though hardly probative.<sup>25</sup> One would like for there to be a possible inner-Akkadian process that could account for such a change.

I would argue that there is such a possibility. While Old Akkadian did, indeed, keep the old *\*θ* as a separate sound from *\*š* (with which it later became identified), it is not totally clear how that separate sound was actually pronounced during the Old Akkadian period. Given that it subsequently became completely identified with the sibilant phoneme /š/, it is quite conceivable that its actual phonetics were already moving in a similar direction (although not having gone all the way, so to speak).<sup>26</sup> If this

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the idea of a direct borrowing from Neo-Assyrian into Hebrew. From that perspective, the Babylonian period actually would fit better.

<sup>23</sup> J.T. Koch, “The Conversion and the Transition from Primitive to Old Irish *c.* 367–*c.* 637,” *Emania* 13 (1995), 39–50.

<sup>24</sup> One could also compare with the part of Latin vocabulary that appears in early (pre-classical) literature, whereupon it was relegated to substandard language during the classical period, only to resurface in post-Golden Age Latin literature: one such example is the use of the verb *manducare* for “eating, chewing.”

<sup>25</sup> Huehnergard, “Etymology of the Hebrew Relative *šē*,” 118, n. 81.

<sup>26</sup> An analysis similar to this (though definitely not identical), can be found in V. Meyer-Laurin, “Zur phonologischen Rekonstruktion von ‘Schin’ (<š>) im früheren Akkadisch (sargonische bis altbabylonische Zeit),” *AoF* 43 (2016), 77–146. Meyer-Laurin arrives at the conclusion that historical *\*š* was originally an affricate [tʃ], which was deaffricated into [ʃ] in Sargonic Akkadian (the latter being written with *š*-signs). While this, of course, entails an analysis in which the interdental pronunciation of the phoneme was still there, such deaffrication would provide an example of the type of “moving in a similar direction” that I propose in the main text. And the existence of an *actual*

is so, a possible source of analogical pressure may have been present in the form of the personal/demonstrative pronouns, (classical) masculine *šū* and feminine *šī*, which did indeed begin with unvoiced sibilants. The ancient Central Semitic languages have such pronouns in *b-* instead (cf. Hebrew *hūʾ*, *hīʾ*, Ugaritic *hw*, *hy* and Arabic *humā*, *hiyā*), thus providing no such template.<sup>27</sup> It is quite probable that the Central Semitic *b-*pronouns are derived by regular sound law from the ones with sibilants,<sup>28</sup> but this phonological development would have occurred much earlier than the periods that are relevant concerning the Northwest Semitic relatives discussed above. In Akkadian, however, the presence of a series of demonstratives beginning with an unvoiced fricative may have provided a template for the initial consonant of the relative pronoun also becoming unvoiced.<sup>29</sup>

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\*š-series in the demonstrative pronouns would have provided excellent analogical pressure for this tendency to go further in case of the relative particle. Note also that Meyer-Laurin argues (pp. 77, 116, 137) that the corresponding voiced sound (i.e. what I have referred to here as \*ð) not only was affricated originally but that Sargonic Akkadian did *not* totally deaffricate this phoneme like it did [tθ] (even though she states in footnote 178 that the process may have been “*bereits begonnen*”); this asymmetric state of affairs would have left the \*ð-relative as even more of an outlier compared to the other pronouns and would have facilitated a stepwise convergence towards a nonaffricated fricative, perhaps first as [θ] and later as an actual sibilant.

<sup>27</sup> The Sayhadic (Old South Arabian) languages, which are sometimes regarded as part of Central Semitic and sometimes as South Semitic, are on the fence, showing demonstrative pronouns both in *b* and in a sibilant.

<sup>28</sup> See, e.g., A. Al-Jallad, “Yusap‘il or Yuhap‘il, that is the Question: Two Solutions to Sound Change \*s<sup>1</sup> > h in West Semitic,” *ZDMG* 165 (2015), 27–39.

<sup>29</sup> An anonymous reviewer objects that this type of explanation for the similarity between the Akkadian relative particle and the Northwest Semitic ones simply pushes the “chance similarity” (so to speak) back in time, as the processes leading to the similar words would still have been entirely independent in Northwest Semitic and Akkadian. That, in itself, is quite true, but I would argue that both of the proposed developments are, in themselves, parsimonious in terms of their own linguistic contexts, and that the processes are, in fact, more parallel and historically interrelated than they seem: both result from etymologically identical relative particles including historical interdental fricatives being “saved” by reinterpretation as morphologically relevant sibilant sounds while the interdentals became obsolescent in the individual languages. This can, of course, be called a strange coincidence, but the actual, underlying coincidence is rather the fact that both Northwest Semitic and Akkadian started merging the interdental fricatives into sibilant phonemes—the reidentifications of the relative particles are partly reactions to these parallel phonological developments. Also, as mentioned in the main text, I believe it quite possible that the popularity of Hebrew *šC-*/*šāC-* in some early texts may have been bolstered by the existence of the phonologically similar *šā* in the prestige language that was Akkadian, providing yet another piece of commonality. But, again, this was not the *reason* for the creation of the Hebrew word.

A very different type of explanation of the difference in voicing between the East Semitic and West Semitic relative markers is offered in a recent article by Huehnergard and Pat-El (2018)<sup>30</sup>—though it represents, in a sense, a sort of inverse of the type of interpretation offered here. They argue in that article that the unvoiced version in *\*θ-* (i.e. the one underlying the Akkadian forms) is the original one, and suggest that the West Semitic relative in *\*ð-* came about due to voicing influence from a mimation ending *-m* putatively added to the West Semitic *demonstrative* pronouns in *\*ð-* (i.e. the ancestor of Hebrew *zēb*, etc.; they point to an example in Sabaic, which shows nunation of the *demonstrative* particle, *ðn*). As Huehnergard and Pat-El argue that the demonstrative and relative particles/pronouns in interdental are etymologically related and closely intertwined—the relative marker being primary—they then posit the possibility that this mimation-induced voicing spread by analogy to the closely related relative particles of West Semitic.<sup>31</sup>

This explanation is certainly possible, but it is weakened by the fact that none of the attested “classical” Central Semitic languages shows mimation of the demonstrative particle—and it also demands a very complex set of processes, involving many successive stages. First, Huehnergard and Pat-El argue, the relative particle starting with a (voiceless) interdental gave birth to a related demonstrative pronoun, and then that demonstrative pronoun acquired mimation. The mimation subsequently induced voicing of the interdental in the demonstrative pronoun, which then spread “back” by analogy to the relative particle from which the demonstrative is argued to have originated. And, finally, almost all languages completely lost the mimation/nunation of the demonstrative particle itself. This, to me, seems like an unnecessarily complex set of changes to explain the data. It should be noted, however, that both the explanation argued in this article and that proposed by Huehnergard and Pat-El do involve some sort of interference between the relative and demonstrative systems (albeit in different subphyla and involving different specific demonstrative roots).

### CONCLUSION

To sum up: a model involving neither borrowing nor chance resemblances but *conflations* of similar pieces of inherited etymological material provides a parsimonious explanation both for the shape of the Hebrew relative particles and for their distribution. It also helps explain why the Hebrew and Akkadian

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<sup>30</sup> J. Huehnergard and N. Pat-El, “The Origin of the Semitic Relative Marker,” *BSOAS* 81 (2018), 191–204.

<sup>31</sup> The suggestion that the relative particle is primary is found in Huehnergard and Pat-El, “The Origin of the Semitic Relative Marker,” 195. The argument concerning mimations and spread of voicing can be found on p. 198. The opposite development of the semantic domains of the pronouns (demonstrative > relative) is argued e.g. in R. Hasselbach, “Demonstratives in Semitic,” *JAOIS* 127 (2007), 1–27 (18, 20).

words involved appear to be so similar, without having to postulate *ad hoc* sound laws or complex schemes of language contact. In this part of the article, I have argued:

- A. That the Hebrew relative particles *ʾāšer* and *šeC-/šuC-* are indeed variants of the same morpheme, as Huehnergard surmises (and that the latter was not itself borrowed from Akkadian *šā*—even though its use may perhaps have been bolstered by it),
- B. but that the latter was also conflated with the accusative form *\*dā* from the old relative particle *\*dū* and thus carries on the etymological tradition of that pronoun also (as the latter was “integrated” into *šeC-/šuC-*, which helps explain its distribution).
- C. That the Akkadian relative pronoun may have had its initial consonant devoiced and turned into a sibilant due to analogical pressure from demonstrative particles.

## 2. ONE STEP AT A TIME: HEBREW *PAʿAM* AND UGARITIC *PAMT*—*AMRAPHEL* AND SCRIBAL KOINÉ

We now turn to our second object of discussion. One of the more etymologically abstruse words of the Ugaritic language is the term *pamt*, meaning “time” (as in “number of times”). This word is often explained as being a relative of the Hebrew word *paʿam*,<sup>32</sup> which has this exact meaning, in addition to a more concrete meaning of “foot” or “leg” (even though the latter meaning is uncommon in Hebrew). There is also a Phoenician word *pʿm* (“foot”), as well Akkadian *pēmu* or *pēnu*, meaning “upper leg,” “thigh” or perhaps “hip.”<sup>33</sup>

The semantic development from the concrete “foot” or “leg” (and thereby “step”) to the abstract “time” has a typological parallel in French *pas* (used in negative sentences) from Latin *passus* (“step”) and perhaps an even stronger one in the Swedish *gång* (literally “going”). So, the generally seen explanation of the Hebrew word is that the root originally meant “foot, step” and then acquired the abstract meaning by such a development, which would be supported by the similar Ugaritic word also having the same abstract meaning.

### **SOUND-LAW PROBLEMS**

However, there are two glaring problems with this etymological connection, persuasive though it may appear from a semantic

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<sup>32</sup> So, e.g., G. del Olmo Lete and J. Sanmartín, *A Dictionary of the Ugaritic Language in the Alphabetic Tradition*, 3rd ed. (tr. by W.G.E. Watson; HdO, 1/112; Leiden: Brill, 2015), 648 (s.v. *pamt*).

<sup>33</sup> On these words for body parts and the possibility of their representing a Proto-Semitic word for “foot,” see L. Kogan, “8. Proto-Semitic Lexicon,” in S. Weninger (ed.), *The Semitic Languages: An International Handbook* (Handbücher zur Sprach- und Kommunikationswissenschaft 36; Berlin: de Gruyter, 2011), 179–258 (224).

and “general look of the word” perspective. The first of these is the fact that, from the standpoint of formal historical linguistics, the root-equation  $pa'am = pamt$  just does not work. There is no reason for the historically secure  $'ayin$  of the Hebrew and Phoenician forms<sup>34</sup> “magically” to have turned into an  $'aleph$  (written with the  $a$ -sign) in Ugaritic. In the latter language,  $'ayins$  normally remain  $'ayins$ , and sound laws are, after all, sound laws. Spontaneous exceptions like this one should ideally not be allowed, as they undermine the *Ausnahmslosigkeit* on which the entire comparative method of historical linguistics rests. Serving as an illustration of this principle is the second problem with the equation: the existence of the separate Ugaritic word  $p'n$ , “leg, foot,” with the  $'ayin$  in the expected place. The two forms should not exist together in the same language.

Even though it is clear that all these words are somehow related originally,<sup>35</sup> it appears that major restructurings and changes have taken place. The Ugaritic word which is historically “logical” ( $p'n$ ) only appears to have the concrete sense of a body part, whereas the historically “erroneous” form  $pamt$  has the abstract meaning (“time”) associated with most instances of the Hebrew  $pa'am$ . These historical discrepancies must be explained.

One suggestion that has been made is that the Hebrew word should be interpreted as a kind of mixed form only partly descended from the “foot” word. This would in itself not be impossible (it would, in fact, be rather similar to the explanation for the Hebrew  $\text{šC-}$  given earlier in this article). However, I see a rather glaring problem with such an explanation in this case. If Hebrew really inherited a word (with an  $'aleph$  instead of an  $'ayin$ ) that was etymologically connected with Ugaritic  $pamt$  and subsequently became conflated with  $pa'am$ ,<sup>36</sup> one would have liked to see a good etymology for such a word, explaining its meaning of “time” without reference to the “foot” word (with which it would only secondarily have been identified). Such a clear etymology for  $pamt$  is, however, lacking—the suggestions presented do not inspire confidence (see, for example, footnote 35). But a semantic development “foot/leg > time” is typologically probable and understandable, making it preferable to regard the connection between Hebrew  $pa'am$  and Ugaritic  $pamt$  as somehow original. But the irregular sound-correspondence has to be explained somehow, and so does the co-occurrence of  $pamt$  and  $p'n$  in Ugaritic.

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<sup>34</sup> Also historically implied by the  $\epsilon$  vowel in the Akkadian forms.

<sup>35</sup> Alternative etymologies that have been suggested for the Ugaritic word are not convincing. One example can be found in C.L. Seow, *Job 1-21: Interpretation and Commentary* (Illuminations; Grand Rapids, MI: Eerdmans, 2013), 722, where it is related to the Arabic word  $fa'ama$ , “to fill, add to,” the semantics of which can hardly be said to fit (Seow explains Ugaritic  $pamt$  as originally meaning “addition, increase”).

<sup>36</sup> This is suggested in C.H. Gordon, *Ugaritic Textbook: Grammar* (Revised reprint; Roma: Pontifical Biblical Institute, 1998), 45, n. 3.

*INTERDIALECTAL BORROWING*

When one finds two apparent forms of the same word or root in a language, one showing a phonetically regular development and one not (often with some semantic difference between the two), there is one possibility that clearly suggests itself: that of interdialectal borrowing, i.e., one of the words having been borrowed into the language from a (probably closely) related dialect with somewhat differing sound developments.

This, I suggest, would be a highly probable explanation for the seeming phonological irregularity of Ugaritic *pamt* and its co-occurrence with *p'n*. Such goings-on are certainly not unheard of (and, as we shall see, it has been suggested earlier by Josef Tropper for the present word as well, albeit in a somewhat different form than I do here). One of the most famous examples of such interdialectal borrowing in historical linguistics generally is the Latin word *bās* (“ox, cow”) which was apparently borrowed from another Italic dialect (probably Oscan or something akin to it), as the regular Latin descendant of Proto-Indo-European *\*g<sup>w</sup>ōus* or *\*g<sup>w</sup>eb<sub>3</sub>us* (the ultimate, reconstructed “parent word” of Sanskrit *go-*, Greek *βοῦς*, Old High German *chou*, English “cow” etc.) would have been the nonexistent *\*vōs*. As I have argued before,<sup>37</sup> one should not be too hasty in positing such interdialectal borrowings if there are easier solutions at hand. There are, however, cases in which it is necessary to assume such “sound-law-breaking” interdialectal borrowings. There are known instances of this process in Hebrew (as related to Ugaritic). One probable example can be found in the word for “serpent/snake” represented in Ugaritic as *bt<sub>n</sub>* (and, on a wider linguistic scale, in Akkadian *bašmu*). This word appears at a quick glance to have a natural cognate in Hebrew *peten*, but on closer inspection, that word will not work as a cognate, as the regular sound laws would not produce such a reflex in Hebrew—the consonants in the Hebrew word should be *b-š-n*, and, indeed, this regular reflex of the root consonants does seem to appear in a few places under the guise of the word *bāšān*, which is normally the name of a region but in some cases probably represents the old “snake” word. The more common form *peten* would then represent a borrowing from a neighboring Semitic dialect that kept the (inter)dental articulation of the historical *t̪* phoneme and did not change it into *š* (as did Hebrew).<sup>38</sup>

This is exactly the type of scenario that I find probable for the “time/foot” words. In essence: there may have been a related but distinct Semitic dialect in the Syro-Levantine area that shifted

<sup>37</sup> O. Wikander, “Job 3,8 – Cosmological Snake Charming and Leviathanic Panic in an Ancient Near Eastern Setting,” *ZAW* 122 (2010), 265–71 (265–66, n. 1).

<sup>38</sup> The difference in voicing between *b* and *p* would, of course, be explained in the same way. For my arguments concerning these words (and references to earlier relevant literature), see O. Wikander, *Unburning Fame: Horses, Dragons, Beings of Smoke, and Other Indo-European Motifs in Ugarit and the Hebrew Bible* (ConBOT 62; Winona Lake, IN: Eisenbrauns, 2017), 64 (with n. 129).

'*ayin* to '*aleph*, a dialect from which Ugaritic *pamt* could be borrowed.

As mentioned earlier, a model involving interdialectal borrowing as a solution to this problem has been suggested before by Josef Tropper.<sup>39</sup> He argued that the loan-giver was Phoenician, and that a genre-based specificity in usage was a pointer in this direction: Tropper notes that *pamt* only occurs in the ritual corpus (as opposed to epic or epistolary texts) and compares this with the use of *pa'am* in ritual texts from Leviticus. This, Tropper argues, would suggest that the word has its origin as a Canaanite technical term from the sphere of ritual practice. However, a Phoenician origin does not explain the '*ayin*/*'aleph* difference, as there are no indications that Phoenician was in the process of losing the pharyngeal fricative in the Late Bronze Age. Tropper suggests the possibility of a Hurrianized pronunciation as the middle stage in this process. The genre-specific usage of the word is certainly quite interesting, but the phonological difficulty needs further and more concrete explaining. One needs supporting evidence for a loan-giving language or dialect which turns historical '*ayins* into '*alephs*, and one that is not an *ad hoc* proposition for the present purposes. We shall keep Tropper's suggestion of a Hurrianized pronunciation of Canaanite in mind as we continue our argument.

#### ***A PARALLEL CASE: AMRAPHEL—AND AN INTERNATIONAL SCRIBAL KOINÉ***

No chronologically appropriate language fitting the above-mentioned phonological parameter is textually attested. Therefore, using such a model—an interdialectal borrowing—to explain Ugaritic *pamt* could perhaps be regarded as a rather unpersuasive hypothesis (and as a form of special pleading) if it were the only example of the phonological correspondence involved. However, I believe that there may be at least one other known instance of the process—and probably more, as we shall see later.

A rather astonishing possible parallel to the idea of *pamt* being borrowed from an otherwise undocumented Semitic dialect shifting '*aleph* to '*ayin* appears to be in evidence in another well-known word—or, rather, a name—in a Northwest Semitic language that shows this nonstandard correspondence. I am referring to the much-discussed name Amraphel ('*amrāpel*), occurring in the list of ancient kings in the battle in which Abram becomes involved in Gen 14:1. In early scholarship (going back to Eberhard Schrader in 1888), this name was often identified with Ḫammurāpi of Babylon, especially since Amraphel is said to be king of Šin'ār, a region commonly identified with Mesopotamia, Babylon, or Sumer. There are, after all, other names in the list that appear at least to represent some kind of muddled reception of Bronze Age rulers from the Ancient Near East: *Tid'al* is probably

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<sup>39</sup> J. Tropper, "Kanaanäische Lehnwörter im Ugaritischen: Neue Überlegungen zu *bnš* = *bu-nu-šu*, *pamt* und *mḫqtm*," *UF* 35 (2003-2004), 663-72 (666-70).

a form of the name Tudḥaliya,<sup>40</sup> borne by a number of kings of the Hittite Empire, and *Kēdorlā'omer* probably includes actual Elamite linguistic material (cf. the first part of name of the Elamite ruler Kutir-Nahḥunte and the name of the Elamite goddess Lagamar).<sup>41</sup>

However, this identification of Amraphel was subsequently subjected to severe criticism, and today, it is often dismissed out of hand due to the nonstandard appearance of the first letter in relation to the languages in which the name Ḥammurāpi appears. This name, which was not Akkadian but Amorite in origin, really began not with a *ḥ* but with an *'ayin*; the *ḥ* only appears due to Akkadian spelling, as that language did not have an *'ayin* in its phonetic inventory during its classical periods and had to resort to *ḥ*-signs in order to express the pharyngeal sound. The “real” shape of the name is quite clear; the same name was borne by one of the kings of Ugarit, and his name is spelt *'mḥpi* (the name probably means “the ancestor/kinsman is a healer” or something similar). This spelling, with the initial *'ayin*, would not fit well with the *'amrāpel* of the Vale of Siddim story, as that name is written with an initial *'aleph*, and Hebrew has no difficulty in separating these sounds before the Qumran and the Talmudic era (and even then only dialectally). This discrepancy has, according to many modern scholars, made the interpretation of Amraphel as Ḥammurāpi impossible.<sup>42</sup>

However, as our discussion of Ugaritic *paḥm* in relation to Hebrew *pa'am* has indicated, it is possible that there actually was a dialect (or at least form of diction) in the Syro-Levantine area that shifted *'ayin* to *'aleph*. Given that an identification between the names Ḥammurāpi and Amraphel would make great sense in the context (as there are other actual earlier Ancient Near Eastern names mentioned in the context, and Amraphel is explicitly

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<sup>40</sup> For one example of this view (identifying *Tid'al* with Tudḥaliya III), see O. Margalith, “The Riddle of Genesis 14 and Melchizedek,” *ZAW* 112 (2000), 501–8, 501. Margalith does accept that Amraphel represents a version of the name *'ammurāpi*, but opts not for the Babylonian king but rather his Ugaritic namesake (or one of them, as Margalith believes there were several). My own views on the phonological history of *Tid'al* in the MT and LXX can be found in O. Wikander (with contributions by A. Šorgo), “Stop Being Fricative! The Hebrew *Šēwā' Medium*, Syllabic Consonants, *Tid'al* and the Aesthetics of Linguistic and Exegetical Models,” *SEĀ* 84 (2019), 125–66 (160–65).

<sup>41</sup> On the Elamite onomastic elements Kutir- and Kutur-, see R. Zadok, *The Elamite Onomasticon* (Supplemento n. 40 agli Annali, 44, fasc. 3; Naples: Istituto Universitario Orientale, 1984), 24–25. On the divine name Lagamar, see B. Becking in *DDD*, 498–99 (s.v. “Lagamar”), with references.

<sup>42</sup> For a skeptical (and negative) assessment, see, e.g., J.A. Emerton, “Some False Clues in the Study of Genesis XIV,” *VT* 21 (1971), 24–47, 30–32 (with many references to earlier literature), seeing the *-el* at the end of the Hebrew word as a major stumbling block. I personally do not see this as that serious an objection, as Hebrew has created other names with this very ending (*Karmel*, possibly *šē'ól* and *'arāpel* [cf. Ugaritic *ḡrpl*]). For this possible suffix in the first two mentioned cases, see *HALOT*, 1368 (s.v. *šē'ól*).

associated with Mesopotamia), I would argue that both these words—Ugaritic *pamt* and the name *'amraphel*—may indicate the existence of a loan-giving (perhaps Northwest Semitic) dialect with this phonological characteristic, not otherwise preserved in writing. Another variation of the same possibility is the proposition that the *'ayin*-less Amraphel name was borrowed from speakers of Akkadian, who tried to reproduce the actual Northwest Semitic onomastic form (with the pharyngeal) but failed to do so (rather than just producing a “spelling pronunciation” with *h*). A similar explanation could be given for *pamt*: as there were demonstrably Akkadian speakers in the area of Ugarit, one could imagine that the *'ayin*-less dialect appeared due to phonological adstrate influence from that language on the dialect from which *pamt* was borrowed into standard Ugaritic.

It is certainly not a self-evident proposition to argue for the existence of a hitherto unknown Semitic dialect (indeed, that is something of an understatement); again, the possibility that the putative dialect that I am here referring to was not a separate language unto itself, but rather a phonologically Akkadian-influenced variety of local (Northwest Semitic?) languages should be seriously considered. Tropper’s suggestion of Hurrian influences is not impossible either; however, one may note that Hurrian does not appear to have had a glottal stop (*'aleph*) as a phoneme,<sup>43</sup> whereas Akkadian did—which would somewhat lessen the likelihood of Hurrian being the sole transmitting or influencing actor. Given the role of Hurrian as a religious language at Ugarit, as well as the presence of the language over large parts of the Levantine area, one could perhaps think of a type of Hurrianized-Akkadianized scribal koiné or prestige language as the original locus of these *'ayin*-less words. It would then not so much be a question of a specific unrecorded dialect, but rather of a superstrate-influenced sociolect, or perhaps even one representing the local language as pronounced by speakers of Akkadian (or possibly Hurrian). Such a “Levantine Semitic as (mis)pronounced because of foreign élite influence” would actually provide a good type of source both for borrowing a simple word as *pamt* and a (Mesopotamian) name such as Amraphel—the loan-giving “dialect” would then represent a sort of prestige pronunciation because of its association with the prestige language that Akkadian was.<sup>44</sup> Possible Hurrian influence could have had a similar background given the role of that language as *lingua sacra*. This would mean that we need not posit a real “unknown Semitic dialect” but rather a prestige-influenced way of pronouncing known Semitic languages.

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<sup>43</sup> Reconstructions of the Hurrian phonological system can be found in I. Wegner, *Hurritisch: eine Einführung*, 2nd rev. ed. (Wiesbaden: Harrassowitz, 2007), 43-49 and in G. Wilhelm, “Hurrian,” in R.D. Woodard (ed.), *The Ancient Languages of Asia Minor* (Cambridge: Cambridge University Press, 2008), 81–104 (84–85).

<sup>44</sup> Here, one may compare typologically with the associations of the Europeanized/Ashkenazi-ized *'ayin*-less pronunciation of Modern Hebrew, which holds higher societal prestige than the Mizrahi variants that do pronounce the phoneme.

A “mispronounced scribal elite language” of this sort would also fit well with Tropper’s point of *pamt* only being used in the technical, ritual corpus as opposed to in literary or epistolary writing. Scribes using such a prestigious yet phonologically “wrong” type of language would be a good fit for such textual endeavor, and could well have imparted this variant to actual Ugaritic-speaking colleagues. The appearance of a name mediated through such a dialect/sociolect would also be quite fitting for the Vale of Siddim story of Genesis 14, as it involves a mythologized pseudo-historical situation involving various personages from the greater Levantine, Asia Minor or Syrian-Mesopotamian area (Hittites, Mesopotamians and even an Elamite!). The roots of such a transcultural story could very well have been transmitted by people using an international “not-quite-at-home” Semitic scribal koiné, as suggested here.

It must, however, be emphasized that the loan-giver in the case of *pamt* cannot have been Akkadian itself, as the actual cognate in that language (*pemū*) shows neither the *’āleph* nor the semantic drift to “time.” Also, there appear to be certain interesting phonological characteristics involved in the “*’ayin*-less dialect” that suggest something more than simply an Akkadianized pronunciation. To these we now turn.

**FURTHER EVIDENCE FOR THE “DIALECT”/SCRIBAL  
KOINÉ—AND THE PHONOLOGICAL PECULIARITIES  
THEREOF**

There are more pieces of possible phonological evidence for the “dialect” which is proposed here. A third word which could possibly be relevant in this context is the etymologically highly unclear Ugaritic noun *palt*, which occurs in the Aqhat Epic as a description of Danel’s parched land. Among the explanations suggested for the etymological background of this word, the most promising one is a connection to the root of Arabic *fallun*, which does mean “cracked, parched land” (the Arabic root being *fl*).<sup>45</sup> This root certainly occurs in the Baal Cycle, in the expression *pl’nt šdm* (“The furrows of the fields are parched!”), but there, the extra *’āleph* in the middle is missing. This throws doubt on the derivation of *palt* from the same root, just as the discrepancy between *p’n* and *pamt* needs to be explained. Even though the difference does not involve an *’ayin* in this case, it is still the matter of an extraneous *’āleph* in a parallel, *Nebenform*-like root. Possibly, this could also be an import from the above-delineated dialect or sociolect.

One may note with some interest that all three words mentioned thus far—*pamt*, *palt* and *’amrāpel*—have similar phonological shapes in certain respects. All include a voiceless labial (*p*) together with resonants (labial as well in two of the cases, and *liquidae* in two). Maybe this was a type of phonological context that helped attract the *’āleph* change in the hypothetical “dialect.” For example, if we suppose that the speakers of the “dialect” had a general difficulty in pronouncing pharyngeal fricatives, it is not

<sup>45</sup> See del Olmo Lete and Sanmartín, *Dictionary*, 648 (s.v. *palt*).

hard to imagine that this difficulty was more pronounced in words that included labials (which have a maximum articulatory distance from the pharynx) and resonants (which provide a voiced consonantal environment that could devour, as it were, the voicedness of the *'ayin* phoneme, which would have been intrinsically alien and “difficult” for the speakers to produce). One may note with some interest—as a point of “phonetic typology”—that a study by Proctor, Shadle and Iskarous<sup>46</sup> suggests the production of sustained voiced fricatives (in English) involves pharyngeal expansion and lowering of the larynx. To be sure, the present case involves not fricatives but resonants, but the two categories are quite close in the sonority hierarchy, and one could imagine that a similar scenario is at work here, influencing de-pharyngealization of the guttural phoneme. This would provide a plausible phonetic explanation of the phenomenon in the loan-giving dialect (combined with the probable Akkadian influence mentioned earlier).

There are further examples. As a fourth argument, more possible examples of phonological interference from this possible scribal koiné can be found in a number of highly interesting personal names from the Ugaritic corpus: the most important examples are *abdhr* and *bdhmn*, which clearly represent “degraded” variants of the attested names *'bdhr* and *'bdhmn*, which means that they show the same development.

Other onomastic cases—they too representing the *'ayin* of *'bd*, “slave,” with an *'aleph*—appear in *abdadt* (for *'bdadt*), *abd'n* (for *'bd'n*), and *abdb'l* (for *'bdb'l*).<sup>47</sup> In these cases as well, *'ayins* have been turned into *'alephs*—and once again in the vicinity of labials. Note also that the name *'bd'nt* (“Slave of Anat”) sometimes appears with the second *'ayin* elided, which would possibly be a part of the same phenomenon. In the cases other than *'bdhr* and *'bdhmn*, dissimilation from another *'ayin* or assimilation to another *'aleph* (in the case of *abdadt*) could also be involved, which makes those cases somewhat less probative, but they are still suggestive, and in the case of *'bdhr* and *'bdhmn*, no such language-internal process is at hand as an explanation. The fact that we are talking about personal names in texts from a multi-cultural city also increases the likelihood for the mutated variants representing a sort of international scribal pronunciation.

Admittedly, there are a few (three) isolated examples of Ugaritic *'alephs* that “should” have been *'ayins* but do not fit into the above-delineated phonological pattern; however, the examples of the principle are so many as to suggest at least a clear tendency for the phenomenon to have been especially prominent in the vicinity of labials.<sup>48</sup>

<sup>46</sup> M.I. Proctor, C.H. Shadle and K. Iskarous, “Pharyngeal Articulation in the Production of Voiced and Voiceless Fricatives,” *The Journal of the Acoustical Society of America* 127 (2010), 1507–18.

<sup>47</sup> See, e.g., del Olmo Lete and Sanmartín, *Dictionary (sub vocibus)*, noting the connections to the “correct” forms with *'bd*. The name *abdadt* for some reason has no entry of its own, but it is mentioned on p. 137 (s.v. *'bd*).

<sup>48</sup> The words in question are the verb *s'd* (“help, support,” which

A fifth argument for such a dialectal phonology may perhaps be represented in evidence from Hieroglyphic Luwian. As has been pointed out by Alwin Kloekhorst and (following him) Zsolt Simon, there is an interesting variation to be noted in the way in which Hieroglyphic Luwian renders the Northwest Semitic divine name *Baʿl* (Baal) and its derivatives.<sup>49</sup> One of the ways in which this element is transcribed in Luwian texts uses a specific spelling pattern, which was used by Kloekhorst and Simon as parts of their arguments for the Hieroglyphic Luwian sign *á* (sign number \*19) representing a glottal stop followed by an optional vowel. This spelling is *pa-á-li-ma-li*, which is a rendering of the Semitic name *baʿli-malik*. But there is also another spelling of a Baal-derived divine name in Cuneiform Luwian using a *b*-sign, namely *pa-ba-la-ti-* (for the female divine name *Baʿalat*). From this discrepancy, Kloekhorst drew the conclusion that the former represented a (borrowing from a) dialectal variant in which the etymological *ʿayin* had shifted to an *ʿaleph*. In this, I believe, he may possibly be correct,<sup>50</sup> and if he is, these variant forms provide additional circumstantial evidence for the

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according to etymological considerations ought to have been *sʿd*, the form *tar* (from the root *tʿr*, “to set up, arrange”), and the personal name *yadm* (for *yʿdrn*). For all of these, see J. Tropper, *Ugaritische Grammatik*, 2nd rev. ed. (AOAT 273; Münster: Ugarit-Verlag, 2012), 153—as well as the relevant entries in del Olmo Lete and Sanmartín, *Dictionary*. It is not at all impossible that these words, too, originate in the same scribal dialect, even though the phonological environment is different.

<sup>49</sup> A. Kloekhorst, “The Preservation of \**b*<sub>1</sub> in Hieroglyphic Luwian: Two Separate *a*-Signs,” *Historische Sprachforschung* 117 (2004), 26–49 (esp. 36–37); Zs. Simon, “Once Again on the Hieroglyphic Luwian Sign \*19 <á>,” *Indogermanische Forschungen* 118 (2013), 1–22 (esp. 9–11).

<sup>50</sup> However, Kloekhorst’s argument (p. 36) that the Ugaritic syllabic spelling *ba-a-lu-ma* must also reflect such a dialect is incorrect; this is probably merely an Akkadian way of rendering a word with *ʿayin*—or, at least, this is quite as probable a possibility, invalidating the argument. This is also noted in Simon, “Once Again” (11), where, however, the possibility of two dialects is tacitly denied and the difference in spelling is interpreted as simply representing differing attempts to spell a voiced pharyngeal fricative alien to the Luwian phonological system (the same problem as the Akkadian spelling shows, as Akkadian transcriptions sometimes use *h*-signs for the purpose as well). However, as both Kloekhorst and Simon adduce *other* arguments for sign \*19 as a glottal stop as well, I find it quite possible that an articulatory difference was actually present. As shown by examples enumerated by Simon on p. 13, it cannot be denied that there are at least two other cases in which etymological Semitic *ʿayin* is rendered by \*19/*á* in seals from Emar—but the occurrence two different spelling methods for the *same word stem* is certainly suggestive of a phonetic difference as well. It may of course be objected that this dual spelling of *ʿayin* occurs also in transcriptions into Akkadian (and would thus be devoid of merit as an argument for an actual phonetic difference underlying the Luwian transcriptions); the dual cuneiform spelling in Akkadian, however, has a prehistory in a tradition of rendering Amorite *ʿayins* with *h*-signs. The Luwian spellings have no such background to justify them historically, and it is therefore more likely (though not certain) that they reflect an actual difference in pronunciation.

existence of an *'ayin-to-'āleph* form of speech in the Bronze Age Northwest Semitic speaking area. Again, the words in question interestingly involve a labial plosive and a liquid resonant.

To sum up, *ḫamt*, *'amrāpel*, possibly *palt*, the Ugaritic personal names *abdḫr* and *abdḫmn* (and possibly others)—as well as the Luwian transcription *pa-á-li-ma-lí*—all appear to attest to this type of non-native (Hurrianized/Akkadianized?) scribal “dialect” of Northwest Semitic, and all of them attest to similar phonological contexts for the unexpected glottal stop, a fact that strengthens the argument.

#### **SEMANTIC DEVELOPMENT**

One of the reasons for squarely situating Ugaritic *ḫamt* and (most cases of) Hebrew *ḫa'am* in the abstract semantic realm of “time, number of times” (rather than “foot”) may have been the associations between the postpositive element *-m* with various adverbial functions (cf. words like *ḫinnām* or *réqām*). In these cases, of course, this must originally have been a misinterpretation of the *m* that is apparently part of the root, but it may have served to ensconce the adverbial meaning more clearly in the language—and, in the Ugaritic case, to separate *ḫamt* securely from the actual native form *ḫn*, which is only used to refer to a physical body part. The separation of identical etymological material (inherited as opposed to borrowed) into specific semantic realms has many typological parallels. One example is the Swedish words *dräpa* and *träffa*, which have the same etymological background but have come into the language in different ways. The former is an inherited Norse word, whereas the latter was borrowed from German. Both are used in modern Swedish, but with rather different meanings (“slay, kill” and “hit [as in ‘hit a target’]” respectively—and in the latter case, by extension, “meet”). A similar example from English is the inherited word “fire” and its borrowed (from Greek) counterpart “pyre,” both reflecting Proto-Indo-European *\*peh<sub>2</sub>ur* (“fire”).

### CONCLUSIONS

In this latter part of the article, I have argued:

- A. That Ugaritic *pamt* (“time”) and the Hebrew name *’amrāpel* of Genesis 14 both derive from and attest to a Levantine scribal koiné that tended to conflate *’ayin* with *’āleph*,
- B. That *’amrāpel* really *is* connected with the name *’ammurāpi* / Ḥammurāpi,
- C. That this koiné was probably influenced by Akkadian and possibly by Hurrian,
- D. That the conflation of sounds was especially common in the presence of labial consonants, and
- E. That there are other examples of attested words from this dialect, both at Ugarit and in Hieroglyphic Luwian.

In both the case of Amraphel and that of *pamt* (given the distributional data adduced by Tropper concerning the latter)—as well as in the case of the phonologically aberrant Luwian terminology for Baal-worship and onomastic material—a social locus in a transcultural scribal koiné based in Northwest Semitic but influenced by Akkadian (and possibly Hurrian) may be detected. The words certainly fit well with such a milieu. In using these words to explain each other—in combination with the above-mentioned *palt* and the onomastic evidence showing *’ayins* turning into glottal stops—we may get a view not only of the etymological background of certain lexemes, but also into a piece of socio-linguistic reality. With the possible exception of *palt*, the “dialect words” fit a social milieu of multilingual, technical scribal activity (cult, onomastics). And both in this case and in that concerning the relative pronouns and particles, processes involving meetings between different types of diction—and the interactions between them—make themselves heard. Analyzing the confluents, splits and phonological overlays grants us a window into the complex and multifaceted linguistic milieu of early (especially Northwest) Semitic.