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ON THE ORIGIN OF THE ḤÉĐɛR ~ ḤĂĐÁR *ALTERNATION IN HEBREW* [1]

by

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The *ḥéðɛr ~ ḥăðár* alternation is limited to nouns with final resonant, because resonants are particularly prone to acquisition of syllabicity, which, in turn, often leads to epenthesis. The construct forms of such nouns underwent epenthesis earlier than their absolute co-allomorphs — early enough, in fact, to be affected by the well-known Hebrew stress-shift — because they lost their case-endings earlier. The original epenthetic vowel was lowered to *a* by Philippi's law. The retention of *e* in *hăvêl* is due to overlapping of Philippi's law and stress-shift.

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[1] I am greatly indebted to Professors Zeev Ben Hayyim, Joshua Blau, Daniel Boyarin, Henry Hoenigswald, and Saul Levin for their comments on an earlier version of this paper. I would particularly like to thank Professor Levin for spending a great deal of time answering questions which I sent him concerning the accentuation of Hebrew *qotol* forms in the Septuagint. His answers forced me to revise my thinking about these forms, and, in the end, to omit my discussion of them entirely.

1. THE *ḤÉƉƐR ~ ḤĂƉÁR* SUBCLASS OF SEGOLATE NOUNS

Scholars have long been intrigued by a small group of exceptions to one of the fundamental rules of Hebrew morphology, viz., the rule which states that the construct-state allomorph of segolate nouns is identical to the absolute-state allomorph. Both the rule and the exceptions were discussed already in the 11th century by Yonah ibn Janāḥ (1886:205-6, my translation):

> "Know that that which is of the form CĕCeC, with six points [ː ː] or with five [ː ·], usually does not change when put in construct with a non-pronominal substantive, for example, *ʾéreṣ miṣrayim, dĕrɛx yam suf, sēfer hat-torɔ(h)* ... — most of this class follows in the same path. But some of them do change when put in the construct, for example, *ḥéƉer* in *u-va-ḥăƉār miškɔvăxɔ; šéger* in *šăgār ʾălɔfɛxɔ* although it is unchanged in *wă-xɔl peṯer šéger bĕhemɔ(h); zéraʿ* in *ki-zrāʿ gaō hu(ʾ)* although it is unchanged in *kă-zéraʿ gaō lɔvɔn; néṭaʿ* (as in *wă-ʿɔśɔ(h) qɔṣir kă-mo néṭaʿ*) in *năṭāʿ šaʿăšuʿɔw; [ḥéśer]* ([as in] *bă-ḥéśer u-vă-xɔfɔn*) in *wɛ-ʾĕwîlim ba-ḥăsār lev yamuθu; yéreq* (as in *wă-lo(ʾ) noθar kɔl yéreq bɔ-ʿeṣ*) in *wi-(y)râq dɛšɛ(ʾ)*. . . But it is also possible that *wi-(y)râq dɛšɛ(ʾ)* is the construct of *ṭov ʾăruḥaθ yɔrōq* ... although in that case there would not be any evidence for us in it, since it would not be a member of the *ʾéreṣ* class. And I wonder at Abu Zakariyya [Ḥayyuj] when he says that none of this class change except *hével* in *hăvēl hăvɔlim* . . .[2]

If we eliminate the one uncertain example here (*yéreq*), we are left with a group of nouns all of which end in a liquid or ʿ:

ḥéƉer	~	*ḥăƉār*	'room'
šéger	~	*šăgār*	'offspring'
zéraʿ	~	*zărāʿ*	'seed'
néṭaʿ	~	*năṭāʿ*	'plant(ation)'
ḥéser	~	*ḥăsār*	'want, poverty'
hével	~	*hăvēl*	'vanity'

Three of the new examples discovered by Ewald (1835:250, 1855:472) fit in perfectly here,

śāḵar	~	*śăḵār*	'trade, profits'
šévaʿ	~	*šăvāʿ*	'seven'
téšaʿ	~	*tăšāʿ*	'nine'

[2] Wa-ʿlam ʾanna-hu mā kāna ʿalā miṯāli peʿel bi-sittati nuqatin ʾay [l.c. Ibn Tibbon ʾaw] bi-xamsatin fa-ʾinna ʾakṯara-hā lā yataġayyaru ʿinda ʾiḍāfati-hā ʾilā l-ʾasmāʾi ẓ-ẓāhirati miṯla ʾɛreṣ miṣrayim dɛrɛx yam suf sefɛr hat-torɔ(h) ... ʿalā hāḏā yaṯṯaridu ʾakṯaru l-bāb. Wa-qad yataġayyaru baʿḍu-hu ʿinda l-ʾiḍāfati ka-taġayyuri ḥɛśɛr fī qawli-hi u-va-ḥăƉar miškɔvăxɔ wa-ka-taġayyuri šɛgɛr fī šăgar ʾălɔfɛxɔ wa-ʾin kāna ġayra mutaġayyirin fī qawli-hi wă-xɔl peṯɛr šɛgɛr bʾhemɔ(h) wa-ka-taġayyuri zɛraʿ fī qawli-hi ki-zraʿ ṣaō hu(ʾ) wa-ʾin kāna ġayra mutaġayyirin fī qawli-hi kă-zɛraʿ gaō lɔvɔn wa-ka-taġayyuri nɛṭaʿ ʾaʿnī wă-ʿɔśɔ(h) qɔṣir kă-mo nɔṭaʿ fī qawli-hi nĕtaʿ šaʿăsuʿɔw wa-ka-taġayyuri bă-ḥɛsɛr u-vă-xɔfɔn fī qawli-hi wɛ-ʾĕwîlim ba-ḥasar lev yamuθu. Wa-ka-taġayyuri wălo(ʾ) noθar kɔl yɛrɛq bɔ-ʿeṣ fī qawli-hi wi-(y)raq dɛšɛ(ʾ) ... Wa-rubba-mā kāna wi-(y)raq dɛšɛ(ʾ) muḍāfa ṭov ʾăruḥaθ yɔrɔq ... ʾillā ʾanna-hu laysa yakūnu ḫīna-ʾiḍin lanā fī-hi šahādatan ʾiḏ laysa min bābi ʾɛreṣ. Wa-ʾinnī la-ʾaʿjabu min ʾabī Zakariyyā fī qawli-hi ʾanna-hu lam yataġayyar min hāḏā l-bābi ʿinda l-ʾiḍāfati ġayra hăvel hăvɔlim.

but Ewald's fourth new example

qāḥaθ	~	*qəḥáθ*	'taking, to take'

does not. It should be grouped instead with medial pharyngal forms like:

bá^{ɛ}aḏ	~	*bə̆^{ɛ}áḏ*	'behind'
**?mṓ^{ɛ}aṯ*[3]	~	*mə̆^{ɛ}ṓṯ*	'fewness'

An additional example was discovered not long ago by Berggrün (1950:7) in the Kaufmann manuscript of the Mishnah — he could have cited Codex Parma A (= De Rossi 138), as well — and Elijah Levita's *Seɸer Ha-Tišbí* (s.v.):

ʂéγɛn	~	*ʂəγán*	'superior priest'

It should be noted that, although neither of these singular forms occurs in the Bible, the plural form does, and it is therefore likely that the alternation attested in the Mishnah is much older than that source. This example ends in a nasal rather than a liquid, but since the liquids and nasals form a natural class (commonly called resonants), it fits in well with the other examples.

Three other construct forms deserve mention here, although it is not certain that they are actually derived from segolates: *gə̆ḏṓl* (Exodus 15:16), *gə̆vṓah* (I Samuel 16:7), and *qə̆ṣṓṣ* (Psalms 46:5, 65:5). The idea that *gə̆ḏṓl* and *qə̆ḏṓṣ* are in some way equivalent to *gṓɛl* and *qṓɛṣ* (the Hexapla actually READS κοδο in Psalms 46:5) goes back at least as far as Samuel ben Meir (Exodus 15:16), but the suggestion that *gə̆ḏṓl*, *gə̆vṓah*, and *qə̆ḏṓṣ* have something in common with *ḥăḏár*, *hăvél*, etc. seems to be original with S.D. Luzzatto ([1860] 1970:7-8, [1871] 1965:290; cf. also Lambert 1931:108, Kogut 1969:23-4, Blau 1971:318). There is, however, another school of thought which maintains that *gə̆ḏṓl*, *gə̆vṓah*, and *qə̆ḏṓṣ* are the construct forms of the adjectives *gɔḏṓl*, *gɔvṓah*,[4] and *qɔḏṓṣ* (Brockelmann 1908:II,48, Rabin 1967:7, Koehler-Baumgartner 1967, s.v. *qɔvṓah*). Brockelmann (op. cit.) adduces these forms as evidence that adjectives can serve as abstract nouns in Hebrew, a proposition which becomes much more attractive when limited to adjectives in the construct state (cf. also *mar* in I Samuel 15:32, Psalms 38:15, *yə̆ɸaθ* in Deuteronomy 21:11, and perhaps *yə̆ɸe(h)*[5] in I Samuel 16:12, 17:42). This syntactic explanation obviously implies that *gə̆ḏṓl*, *gə̆vṓah*, and *qə̆ḏṓṣ* have nothing to do with *ḥăḏár*, *hăvél*, etc., and we shall therefore take the prudent (and convenient) course of omitting these forms from the discussion which follows.

One last alternation which may belong here is the alternation of the infinitive construct, *qə̆ṭṓl*, with its co-allomorph (before suffixes), *qɔṭl-*, but since the original shape of the

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[3]This form is reconstructed on the analogy of forms like *kṓvɛḏ* 'heaviness', *ḥṓzɛq* 'strongness', etc. The reconstruction depends on the assumption that *mə̆^{ɛ}oṯ* is an abstract noun rather than an infinitive construct, a distinction which is perhaps dubious from a historical point of view but seems to have synchronic validity.

[4]One problem for this theory might be the fact that we expect *gə̆vāh* as the construct of *gɔvṓah* (on the analogy of alternations like *mizbéaḥ* (abs.) ~ *mizbaḥ* (const.)), and this form actually occurs several times in the Bible. On the other hand, the retention of *o* in *gə̆vṓah* would seem to be just as much of a problem for those who take it as the construct of *gṓvah*.

[5]The appropriateness of this example hinges on the meaning of *^{ɛ}im* in the two verses cited. It is generally taken to mean 'and' in these verses, but it might, like English 'with', have the meaning 'possessing (a quality)'. This meaning would fit nicely in I Samuel 25:25 and Psalms 89:14 as well.

latter is uncertain due to fluctuation in the Massoretic pointing (between forms like *šɔxbŭxɔ* and *ʿɔṣbi*, which point to an original *CuCC, and forms like *šɔxvo* and *kɔθvo*, which point to an original *CuCuC), we cannot be sure. Moreover, if the vocalization of *qăṭol* owes anything to epenthesis, it owes at least as much to contamination of the infinitive construct with the infinitive absolute (Joüon [1923] 1965:109-10) and the imperfect (cf. Bauer-Leander [1922] 1965:316-7). The original form of the alternation and its conditioning may accordingly be beyond recovery.

A form which does not belong here, contrary to what I once thought, is σφαρθελλειμ, the rendering of *sfr tlym* "Book of Psalms" in Origen's list of the books of the Bible, reproduced by Eusebius in the *Ecclesiastical History* (Schwartz 1908:574). The form σφαρ is open to several interpretations. It could be a scribal error for *σαφρ which crept in before the time of Eusebius (σφαρ is definitely the form which Eusebius had; cf. the critical apparatus, loc. cit.), but *σαφρ would be anomalous in its own right as a rendering of Hebrew **sifr* (one would expect *σεφρ[6]).

A second possibility would be to take σφαρ as a rendering of the Aramaic construct form[7] *sfar*. It is true that θελλειμ has the HEBREW plural suffix and that all of the other names[8] on Origen's list are Hebrew (e.g. αμμεσφεκοδειμ[9], δαβρηϊαμειν[10]), but, as Kutscher has shown (1959:15-6,46-7), none of this is incompatible with an Aramaizing vocalization. Indeed, Origen's vocalization of the Biblical text itself is not free of Aramaisms (loc. cit.), so there is no reason to exclude the possibility of an Aramaism in his vocalization of the Biblical book-titles. On the other hand, it should be noted that, out of the many segolates in the extant fragments of Origen's *Hexapla* (Mercati 1958), there is not one on the pattern of σφαρ (Brønno 1943:136). Even *gbr* in Psalms 18:26, vocalized *gĕvâr* by the Massoretes, is vocalized γαβρ by Origen (loc. cit.). In any case, it is obvious that both of the above interpretations obviate the need to deal with σφαρ within the framework of this article.

The third, and most likely, hypothesis is that σφαρ is to be connected with the Samaritan Hebrew form *asfår* 'book' (the initial vowel is prothetic and probably late). This form is not a segolate since it occurs (Ben Hayyim 1961) in the absolute state (e.g. Deuteronomy 31:24,26) as well as in the construct (e.g. Deuteronomy 24:1,3). It is, rather, a noun on the

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[6]Cf. forms like ζεχρ (= Massoretic *zixr-*) and ρεσθ (= Massoretic *riš̌t-*) in the second column of Origen's *Hexapla* (Mercati 1958). Other forms with ε correspond to Massoretic *a*-stems: νεφσι (= Massoretic *naf̌ši*), δερχω (= Massoretic *darko*).

[7]The construct still existed at this period (Kaddari 1969:104) even though *d*-periphrasis had become, in Kaddari's words, "the regular way of nominal subordination" (ibid., 102).

[8]It should be noted, however, that many of these titles "consist of either (1) the first word or words of the book ... or (2) the name of the hero or supposed author..." (Swete [1914] 1968:214), and that such titles cannot be adduced as evidence concerning σφαρθελλειμ which is "a description of the contents" (loc. cit.). Consequently, only titles which fall into this latter category are adduced here.

[9]This is close to the Mishnaic title of the Book of Numbers, but the vocalization differs. Codex Kaufmann has *ḥummaš hap-piqquḏim* in *Soṭah* VII,7 and in *Menaḥot* IV,3 and *ḥumaš hap-pɔquḏim* (the *ketiv* is *hpyqwdym*) in *Yoma* VII,1. Codex Parma A (De Rossi 138) has *ḥomeš hap-piqquḏim* in the Yoma passage.

[10]Hebrew *davre* (= Tiberian *divre*) *yamin* "Chronicles" (lit. 'things of days'). Note that this title cannot be Aramaic because the Aramaic plural of *yom* 'day' is *yomin* with an *o*, and because the Aramaic word for 'things of' is *mille*. The plural suffix *-in* is, of course, almost as common in Mishnaic Hebrew as it is in Aramaic.

qŏṭŏl pattern, virtually identical to the Late Biblical (II Chronicles 2:16)[11] and Mishnaic (*Nazir* VII.3, *Kelim* I.1, *Zavim* V.10) word *sŏfŏr* meaning 'counting'. It follows that, even according to this interpretation, σφαρ has little in common with the Hebrew segolate construct forms which are the subject of this article.[12]

2. FOUR QUESTIONS ABOUT THE ORIGIN OF THE *ḤĒḎER ~ ḤĂḎÁR* ALTERNATION

We are left, then, with a highly coherent set of segolate construct forms, all of which end in a resonant or ᶜ (and it is not unreasonable to theorize, as does Levin (1966:4), that the latter was also a resonant, i.e. an *a*-vowel with pharyngeal constriction but no audible friction, in ancient Hebrew, at least in the environment of C_#) and are stressed on their epenthetic vowel. It is obvious that any attempt to explain how these forms originated will have to account for these facts by answering the following questions:

1) Why did the stress shift, in these forms, to the epenthetic vowel?
2) Why didn't this change take place in the absolute state of *ḥăšār, hăvēl,* etc. as well as in the construct?
3) Why was the change favored by a final resonant?

One final question is needed to round off this list:

4) Why does the epenthetic ɛ of the absolute alternate with other vowels in the construct, viz.
   a) *e* in *hăvēl*
   b) *a* in *ḥăšār, sŏfár,* etc.

3. PREVIOUS ATTEMPTS TO EXPLAIN THE *ḤĒḎER~ḤĂḎÁR* ALTERNATION

3.1. Sievers' Theory

Previous attempts to explain the *ḥéšɛr ~ ḥăšār* alternation have tended to focus on only two or, at most, three of these questions, question 3 being the one most consistently ignored. Sievers, for example, gives the following explanation (1901b:279) for the alternation:

> "Beim Status constr. ist ja auch der Accentwechsel ganz verständlich. Je stärker der Status constr. enttont wird, um so mehr verliert er seine eigene (d.h. historisch berechtigte) Tonsilbe und ordnet sich lediglich dem allgemeinen rhythmischen Gefüge unter, in dem er auftritt . . ."

In other words, the weakened accent of a noun in the construct state was more easily influenced by external rhythmic factors, e.g., the accent of the following noun, requirements

[11] I owe this reference to Professor Ben Hayyim. In fact, it was he who first called my attention to the form *sŏfŏr* and to the possibility of linking it with σφαρ.

[12] The form σφερ(τελειμ/τελιμ) which occurs in Epiphanius' list of the Biblical books (Audet [1950]1974:55) and in the genealogically related list published by Audet (ibid., 53) is, of course, even further removed from the forms treated in this article. It is also much more difficult to interpret.

of meter (in poetry), etc. This theory would lead one to expect that proper nouns, which occur only rarely (if ever) in the construct state, would be immune to shift of stress from etymological vowel to epenthetic vowel, and the fact that this is not the case (see §4.2 below) casts a heavy cloud of suspicion over the entire theory. Furthermore, it is apparent that Sievers' theory does not address itself to questions 3 and 4 at all, a defect pointed out, in part, already by Bauer and Leander ([1922]1965:574):

> "Die Umfärbung des Ultimavokals bleibt hierbei ... unerklärt."

3.2. Bauer-Leander's Theory

Bauer and Leander's own explanation (loc. cit.) is designed to provide a partial remedy for the above-mentioned defect:

> "Wahrscheinlich sind sie einfach nach Analogie der 4. Klasse entstanden:
> $d^{e}\underline{b}\breve{a}r\bar{i}m : d^{e}\underline{b}\grave{a}r = g^{e}\underline{b}\breve{a}r\bar{i}m : x;\quad x = g^{e}\underline{b}\grave{a}r.$"

The same type of analogy would presumably explain the Umfärbung of *ḥăðâr*, *ḥăsâr*, *šəγâr*, *səḥâr*, and *səγân*, so even if we do not agree with Bauer and Leander (ibid., 573) that *gəvâr* (Psalms 18:26) is a construct form (see fn.31 below), we are still obliged to consider whether their explanation is capable of accounting for the other forms. Consider, then, the analogy which supposedly produced the singular construct form *ḥăðâr*. Should not the same analogy have produced a plural construct form **ḥɛðrê* (< **ḥiðrê* on the analogy of *divrê*),[13] a singular absolute form **ḥɔðɔ́r* (on the analogy of *dɔvɔ́r*), and a suffixed singular form **ḥăðɔrɔ́* (on the analogy of *dăvɔrɔ́*), especially since the plural construct and the singular absolute are semantically closer to the plural absolute (which stands to the left of the equals sign in the proportion) than is the singular construct.[14]

This explanation, then, leaves us in a worse position *vis à vis* question 2 than we were to start with: not only does it fail to answer the question, it actually strengthens it. In addition, it fails to account for the special role played by final resonants in the shift (question 3), or the ɛ ~ e alternation (question 4a).

3.3. Malone's Answers to Questions 1 and 4a

A third attempt to solve the riddle of the *ḥéðɛr* ~ *ḥăðâr* alternation was recently made by Malone (1971). Malone answers question 1 (ibid., 53-4) by assuming that epenthesis in the construct forms under discussion (unlike epenthesis in the corresponding absolute forms) took place early enough to feed the general stress-shift which Semitists have long posited for ancient Hebrew (Bergsträsser 1918:114-5, Bauer and Leander [1922]1965:177ff, Cantineau 1931:97, Harris 1939:50, Goetze 1939, Blau 1972:81). Descriptions of this stress-shift vary from scholar to scholar, but all agree that nouns with two etymologically short stem-vowels (e.g. *kɔvéð*, *dɔvɔ́r*, *ʿenɔ́v*) were originally stressed on the first of these vowels, and that the position of the accent in Massoretic Hebrew (on the second stem-vowel) is a product of the stress-shift (Bergsträsser 1918:114, Bauer and Leander [1922]1965:178, Cantineau 1931:97,

[13] It must be admitted, however, that it is possible to claim that Tiberian *divrê* comes from **davrê*; see above, fn. 10.

[14] One might also ask why analogies of this sort did not change nouns of the form CeCɔ́C (plural CɔCɔC-*im*) to CɔCɔ́C.

Goetze 1939:442, Blau 1972:81).[15] A segolate noun which underwent epenthesis before this stress-shift would clearly have been part of this larger class of nouns, and would, therefore, have participated in the stress-shift along with the other members of this class.[16] The same conclusion was reached independently by the author of these lines.[17]

Malone's answer to question 4a is also new. He assumes (op. cit., 46) that the original epenthetic vowel was *e*, and that this *e* was later lowered to ɛ in unstressed position. Malone does not motivate this solution, apparently expecting his readers to realize that the latter assumption is necessary in any case to account for such alternations as: *šēm ~ šɛm-, ʦošēm ~ ʾal-ʦošɛm, haggēō ~ haggɛō-nō, yešēv ~ yēšɛv-nō*.

Malone's answers to questions 1 and 4a are very appealing for the simple reason that they require very few assumptions beyond those required to account for other Hebrew phenomena. In dealing with questions 2 and 4b, on the other hand, Malone (op. cit., 58) offers no solutions beyond those set forth by Sievers (question 2) and Bauer and Leander (question 4b), which, as we have seen, are far from economical. The answers which we are about to give to these questions — and to question 3 — are, the author believes, more in harmony with the multiply motivated solutions to questions 1 and 4a which Malone put forth.

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[15]Most of these scholars believe that the stress-assignment rule of Classical Arabic (the so-called *Dreisilbengesetz*) originally held for Hebrew as well. This rule assigns the stress in this word-class to the first stem-vowel. Goetze agrees with this result for his own reasons. His position (1939:442-3) is that the first stem-vowel would have been syncopated if it had not been stressed — and the contrast between the treatment of short *i* in *zŏrōaʿ* < **ðiraʿ*, *ḥămōr* < **ḥimār, and ʦŏḥōl* < **ʦiḥāl*, on the one hand, and the treatment of that same vowel in *ʿenōv* < **ʿinab* and *ṣelōʿ* < **ṣilaʿ*, on the other (ibid., 444) would certainly seem to bear him out. The fact that the Akkadian reflexes of this class are also stressed on the first syllable is further proof, according to Goetze (ibid., 444-5). This latter argument is developed and refined in Steiner 1975 (cf. esp. p. 8).

[16]That early epenthesis (in the vicinity of a final resonant!) shows up as a shift of form-class in Tigre as well is clear from Gragg's description (1974:3):

> "Te does not tolerate final clusters either, but uses various processes to get rid of them. The most frequent is epenthesis (*ø* > *ə*/C_C# : *kalb* > *kaləb*) ...; but shift in form class (*bäql* > *bäqäl*, so frequently for CäCR, where R is a resonant) and deletion of homorganic consonants (*qärn* > *qär*) are also attested."

The synchronic repercussion of early SYNCOPE is also a shift of form-class, cf. *kɔvēō ~ kēvɛō, kɔθēɸ ~ kēθɛɸ, yɔrēx ~ yērɛx, gɔðēr ~ gēðɛr, ʿɔrēl ~ ʿērɛl, ṣelōʿ ~ ṣēlaʿ, šɛʿōr ~ šāʿar, ʿɔšōn ~ ʿēšɛn, šɔxōr ~ šēxɛr, ʾɔrōx ~ ʾērɛx, ʾɔšēō ~ ʾēšɛō* (Steiner 1975:3-4).

[17]As Malone points out (1971:60fn58), this same rule-ordering (or "change-ordering") device was employed by Bauer and Leander ([1922]1965:213) to explain the shift of the accent in other classes of nouns (cf. also Goetze 1939:447). It is not entirely clear why these scholars did not extend this solution to the class of construct forms under discussion. It is possible that they were put off by the fact that it leaves the *Umfärbung* of the epenthetic vowel (in forms like *ḥăðār*, etc.) unexplained. But then we may legitimately ask why they didn't use the analogy theory IN CONJUNCTION WITH the rule-ordering theory as does Malone. By doing so they would have strengthened their analogy theory, since the probability of any given analogy taking place would appear to rise in direct proportion to the similarity between the analogandum and the analogans. This consideration clearly outweighs the "economy" of allowing one theory (viz. the analogy theory) to account for both *Umfärbung* and stress-shift.

4. NEW ANSWERS TO QUESTIONS 2, 3, AND 4b

4.1. Question 2 (The Stress Shift)

It has often been noted that alternations like *zɔqḗn ~ zɔ̆qán*, internally reconstructed to obey Philippi's law (which is, in turn, based on an internally reconstructed version of *zɔqḗn ~ zɔqánti*, and states that short, stressed, checked, non-low, front vowels become low) prove that final vowels (case endings) were apocopated earlier in the construct state than in the absolute (Bergsträsser 1918:115,149, Bauer and Leander [1922]1965:523, Harris 1939:41, Blau 1972:69,223).

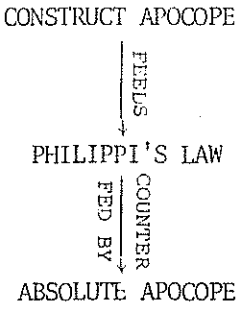

Figure 1

Since the loss of case endings is a necessary pre-condition for epenthesis, it seems reasonable to assume that the construct state was the leader in the area of epenthesis as well:

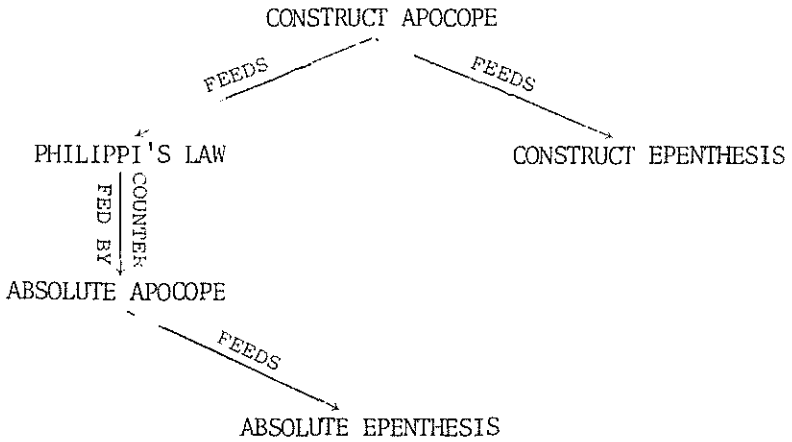

Figure 2

If so, we may answer question 2 simply by assuming that stress-shift occurred after nouns in the construct state underwent epenthesis but before nouns in the absolute state did so:

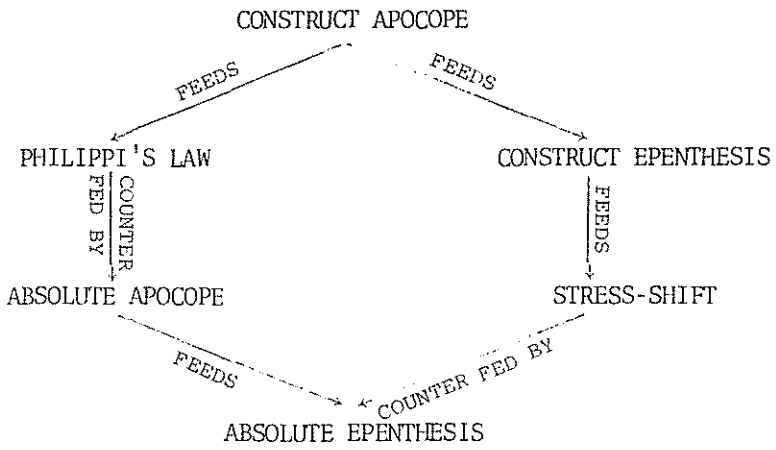

Figure 3

The claim is, then, that the stress did not shift to the epenthetic vowel in forms like *ḥɛ̆ðɛʀ*, *hɛ̆vɛl*, etc. because these forms did not get their epenthetic vowels until after the extinction of stress-shift.

It should not surprise us to learn that epenthesis proceeded in stages. The two stages we have identified coincide more or less with the stages discovered by Bauer and Leander (op. cit., 213). The existence of a third stage has been tentatively proposed (Harris 1941:145), based on the contrast between the behavior of the *bgd kpt* stops following an epenthetic vowel in a noun and the behavior of those consonants following an epenthetic vowel in a verb (cf. especially the minimal pair *ɬɔqāḥaθ* 'to take'[18] : *ɬɔqāḥat*[19] 'you (f.) took'). This contrast would seem to prove that epenthesis took place later in the verb,[20] too late to be

[18]In Hebrew, infinitives pattern with the nouns because they are derived from verbal nouns.

[19]The unspirantized *t* in this form is a regular feature of the 2fs perfect suffix, cf. *šɔxāḥat* 'you forgot', *šɔlāḥat* 'you sent', *pɔθāḥat* 'you opened', *yɔðāᶜat* 'you knew', *yɔγāᶜat* 'you toiled', *šɔmāᶜat* 'you heard', *higgāᶜat* 'you arrived', *hiśbāᶜat* 'you satiated', *pɔšāᶜat* 'you transgressed'.

[20]Just as the early date of construct epenthesis is a function of the early date of construct apocope, so too the late date of epenthesis in *wayyiḥad* (note, in addition to the unspirantized *d*, the unlengthened, or at least unlowered, *i* of this form and others like it in contrast with the *e* of *yeḥām*) and *ɬɔqāḥat*, *šɔxāḥat*, etc. may be a function of the late date of apocope in final-*w*,*y* verbs and in the second person feminine singular of the perfect. Evidence for the late date of apocope in the latter comes from the unspirantized (and simplex?) *t* of *nɔθat(t?)* 'you gave', in which apocope took place too late for the final *t* to be affected by spirantization (contrast *mattɔ̄θ* 'gift'), either because spirantization was already extinct or because geminate simplification, a prerequisite for spirantization, was already extinct. This theory is not necessarily in conflict with Blau's theory that apocope took place EARLIER in the verb than in the absolute-state noun (1972: 65), because that theory was put forward to account for forms like *šɔmār* and *yixbāð* in which we find *a* instead of *ɔ* in syllables which were originally open. There is no good reason for assuming that apocope in *wayyiḥad* and *ɬɔqaḥat* was as early as apocope in *šɔmar*

affected by spirantization.[21] If so, we obtain a tri-partite division:

CONSTRUCT EPENTHESIS	*qəḥāθ*
----------STRESS-SHIFT	
ABSOLUTE EPENTHESIS	(*ɬɔ*)*qāḥaθ*
----------SPIRANTIZATION	
VERB EPENTHESIS	*ɬɔqāḥat*

Figure 4

It is sobering to realize that, were it not for the fact that two of the sound changes which took place while epenthesis was in progress (viz. stress-shift and spirantization) happened to be fed by it, the Massoretic reflex of **qaḥt* would have been identical in the construct, in the absolute, and in the verb, and we might never have suspected that epenthesis cannot be assigned to only one position in the sequence of Hebrew sound changes. The possibility that there are other such changes of which we are not aware is disturbing.

4.2. Question 3 (Change Favored by a Final Resonant)

The relationship between sonority and epenthesis has long been recognized by linguists. Sievers (1901a:294-5) and especially Jespersen (1913:191ff) have interesting things to say

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and *yixbað* — in fact there is evidence suggesting the opposite. It is quite likely, for instance, that in Jeremiah's dialect neither *wayyiḥad* nor *ɬɔqaḥat* had undergone apocope, cf. forms like *wattizni-šɔ̆m* 'and she (!) committed fornication there' (Jeremiah 3:6) and *ʾal-tĕmḥi* (paroxytone!) 'do not erase (masculine!)' (18:23), for the former, and *ketivs* like *ɬmdty* (2:33), *qrʾty* (3:4), *šmᶜty* (4:19), *hɬkty* (31:20), *hrbyty* (46:11) and probably also forms like *šɔvarti* (2:20) and *nittaqti* (loc. cit.), for the latter. Forms like *ɔmâr* and *yixbāð*, on the other hand, show no sign of having preserved their final vowels in Jeremiah's dialect or any other for that matter. Thus, Blau's theory about the date of apocope in verbs should be restricted to the verb forms from which his evidence is drawn, evidence which, incidentally, can be explained on the basis of accent rather than syllable structure (cf. Brockelmann 1903:9fnl and Nyberg 1952:§15a,b).

[21] This solution presupposes that spirantization was no longer productive at the time of the Massoretes, but as Blau has pointed out (private communication):

> "In Aramaic, at any rate, spirantization was a living feature even in Saadiah's time (v. his commentary to Sefer Yeṣirah). It is to be assumed that the same was the case in Hebrew, so decisively influenced by Aramaic."

This is a serious objection which I am unable to dispose of in a convincing manner. It is true that it is a long way from the Aramaic vernacular of women in 10th-century Baghdad to the Biblical Hebrew reading tradition of the Massoretes in 8th-century Tiberias, but the gap is narrowed considerably by the existence of segolated but unspirantized verbs in Biblical Aramaic (*hištɔ̆xāḥat* 'you have been found', Daniel 5:27) and Babylonian Hebrew (*yāḥad* 'let it rejoice?; let it be united?', Job 3:6 in Yeivin 1973a:54,130). I have decided to retain Harris' explanation because I fail to see any viable alternative to it: the various analogies that might be proposed to account for the unspirantized *t* in *ɬɔqāḥat*, *šɔlāḥat*, *pɔθāḥat*, etc. are not capable of accounting for the unspirantized *d* in *wayyīḥad*.

on this subject, but, for our purposes, the most useful discussion is that of Bloomfield ([1933]1965:384):

> "When a relatively sonorous phoneme is non-syllabic, it often acquires syllabic function;[22] this change is known by the Sanskrit name of *samprasarana*. Thus, in sub-standard English, *elm* [elm] has changed to ['elm̩]. This is often followed by another change, known as *anaptyxis*, the rise of a vowel beside the sonant, which becomes non-syllabic. Primitive Indo-European *[agros] 'field' gives pre-Latin *[agr]; in this the [r] must have become syllabic, and then an anaptyctic vowel must have arisen, for in the historical Latin form *ager* ['ager] the *e* represents a fully formed vowel. Similarly, Primitive Germanic forms like *['akraz] 'field', *['foglaz] 'bird', *['tajknan] 'sign', *['majθmaz] 'precious object' lost their unstressed vowels in all the old Germanic dialects. The Gothic forms [akrs, fugls, tajkn, majθms] may have been monosyllabic or may have had syllabic sonants; anaptyxis has taken place in the Old English forms ['ɛker, 'fugol, 'ta:ken, 'ma:ðom], though even here spellings like *fugl* are not uncommon."

That the same relationship between epenthesis and sonority may have existed in Arabic (in the pausal forms of nouns on the pattern CVCC*un*) was pointed out already by Brockelmann (1908:I, 209) and Schaade (1911:58), and Harris (1936:34) was able to demonstrate its existence in Punic as well:

> "Beginning with Punic there are traces of anaptyxis in doubly closed syllables, similar to the occurrence of anaptyctic vowels to simplify the pronunciation of the Hebrew segolates. As would be expected, the words in which this occurs are those in which the last consonant is more sonorous than the preceding, thus making a group which is normally not a single syllable at all and which is very difficult to pronounce. For קבר **qabr* 'grave,' there is the Punic variant קברא, with an anaptyctic vowel. In Neo-Punic, **nidr* 'vow' is often written נדער; **ʃikr* 'memory', מכער; and **ʿaśr* 'ten', עסער."

Evidence that the relationship may have also held in Hebrew was adduced by Speiser ([1926] 1967) in 1926. (Blake's unsuccessful attempt (1911:219) to prove that Tiberian Hebrew had syllabic resonants in word-final position need not detain us). Speiser observed (op.cit., 390-2) that in Hebrew nouns and verbs which end in a consonant cluster, i.e., forms which should have undergone epenthesis but didn't, the first consonant of the cluster is generally more sonorous than the second (although it is clear that other factors besides sonority are at work in these forms[23]). The forms in question are: *nɛrd* 'nard', *ʾard* 'Ard (pr.n.)', *yɛrd*

[22]Cf. Bell 1970 for a detailed analysis of this change, based on data from a large group of languages.

[23]It is no accident that the epenthesis-resistant cluster in four of the forms is *rd* and that it is a voiceless sibilant (generally *š*) plus a voiceless stop (generally *t*) in five or six of the others. Since *r* and *d* are homorganic, epenthesis between them would produce a sequence close to the C_XVC_X sequence which many languages tend to eliminate (probably because of the delicate coordination of opposing muscles required to move an articulating organ back and forth quickly, cf. "tongue-twisters" like "Peter Piper picked a peck of pickled peppers") by metathesis (cf. the "2-2 Contraction" rule of Arabic (Hetzron 1974:6-7)), syncope (Bell 1970:12), or haplology. (Counter examples like *mɛ̄rɛd*, *zɛ̄rɛd* (pr.n.) and *śɛ̄rɛd* (pr.n.) result from the momentum that Wang (1969:22) has dubbed the "snowball effect" and do not affect the validity of the argument.) A similar point is made by Sievers (1901a:295):

'have dominion', *yarad* 'give dominion', *qošṭ* 'truth', *košt*[24] 'putchuck', *yeśṭ* 'turn aside', *yešt* 'drink', *wayyašq* 'gave to drink', *wayyevk* 'wept', *yaɓt* 'make wide', *wayyiɓt*[25] 'was enticed', and the entire group *qotalt* (but cf. *wayyišb*[26] 'took captive'). From this evidence and some rather too carefully selected parallels in Arabic, Ethiopic, and Akkadian, Speiser deduced (ibid., 392-3) that:

> "When two consonants were left in the Semitic languages at the end of a word, there arose the need to develop a secondary vowel in the final syllable if the last consonant was more sonorous than the preceding one. For practical purposes it may be said that this was the case between a stop or sibilant and a following liquid or nasal. Thus arose forms like Arab. *mahal*, Hebrew **ʾaben* > *ʾeḇen*, Akk. *ṣalam*. That there was no phonetic need for the development of a segolate vowel if that order of consonants was reversed is proved by the fact that in modern Arabic there is no anaptyxis under such conditions, hence we get here forms like *qalb*, *milḥ*. The same is true of the short imperfect of the Hebrew verbs of the type *tertiae wy*, hence *yiben* occurs in that language alongside of *yarad* and *yešt*[27] .... And finally, in some languages the vowel was extended to all nouns capable of segolization, notably in Hebrew where *ʿeḇeḏ* is now found by the side of *ʾozen*, *geḇer*."

"Svarabhakti tritt um so leichter ein, je grössere Schwierigkeiten sich einer raschen Umsetzung der Articulationsstellung darbieten, d.h. je grösser die Articulationsdifferenz der Nachbarlaute ist. Zwischen nahezu homorganen Lauten tritt sie daher ausserst selten auf, so etwa zwischen *r + d*, *r + t*."

As for the failure of *qošṭ*, *košt*, *yeśṭ*, *yešt*, *wayyaša*, and *wayyišb* (ii pronounced [wayyišp]) to undergo epenthesis, it is of a piece with the irregular syncopation of vowels in the environments *šš_t* and *š_tt* in languages where syncope is normally restricted to (short) vowels in two-sided open syllables, e.g. P-S **ʾišša-* > **ʾišta-*, **šišša-* > **šišta-* (Steiner 1975:2,5-6,8), Hebrew **šittayim* > *štayim*, Syriac *šittā* > *štā*, and perhaps also Akkadian *ina šattaqad* > Aramaic ʾɛštɔqaḏ (Speiser [1926]1967:377fn16). Both of these phenomena, like the metathesis which produced Biblical Aramaic *ʾištiw* 'they drank' (< **šštiw*), reflect a preference for uninterrupted sequences of voiceless sibilant (esp. *š*) followed by voiceless stop (esp. *t*), but the reasons for this preference are not entirely clear.

[24]A Mishnaic term attested in *ʿUqṣin* III.5. For the vocalization, cf. Mishna Codex Parma B.

[25]That *yaɓt* and *wayyiɓt* already had fricative *ɓ* (instead of earlier *p*) by the time verb epenthesis took place follows from the relative chronology proposed by Harris (cf. above, p. 9) on the basis of forms like *yiḥad*.

[26]This is probably an exception, but it is also possible that final *b* was devoiced in this word.

[27]Speiser appears to be less than candid when he contrasts *yārad* and *yēšṭ* with forms like *wattēmer*, *yīven*, *yīɓen* (ibid., 391,393), leaving his readers to infer that the law of sonority is a synchronic fact within the realm of final-*y* apocopated imperfects in Hebrew. Actually, there are numerous counter-examples to the law of sonority even in this limited sub-class (e.g. *tīreṣ*, *yīrev*, *wayyīmeṣ*, *tēreɓ*, *wayyīreɓ*, *wattēmes*, *terev* and perhaps also *wayyišb*, cf. fn. 26). It is true that all or most of the counter-examples exhibit "over-epenthesis" with respect to the law of sonority rather than "under-epenthesis," but this is a much weaker statement than the one which is implicit in Speiser's discussion.

Speiser's hypothesis is reasonable, even though it goes far beyond the evidence adduced by him. Moreover, it provides a simple answer to question 3, for if segolates ending in a resonant were the first to undergo epenthesis, then it is possible to assume that they were the only ones (aside from segolates with a medial laryngal like *baᶜd* and *qaḥt*) which underwent epenthesis early enough to be affected by stress-shift. Accordingly we must modify the diagrams on pp. 9 and 10 as follows:

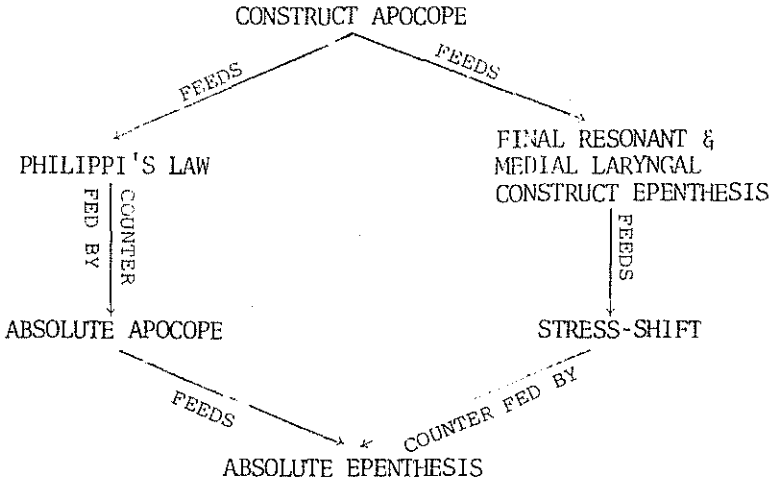

Figure 5

FINAL RESONANT & MEDIAL LARYNGAL CONSTRUCT EPENTHESIS	*qŏḥāθ*
---------- STRESS-SHIFT	
ABSOLUTE EPENTHESIS	*(lɔ)qaḥaθ*
---------- SPIRANTIZATION	
VERB EPENTHESIS	*lɔqaḥat*

Figure 6

Further evidence for Speiser's hypothesis comes from a small group of segolate toponyms, alluded to above (§3.1), which underwent epenthesis early enough to be affected by stress-shift: *šĕxĕm, gŏvāl*, and, less certainly, *ʂŏδōm*. That these toponyms are indeed segolates is perfectly clear (except in the case of *ʂŏδōm*) from allomorphs which occur with locative and gentilic suffixes (*šĕxmɔ(h), givlî*, and *ʂoʊmîθ*[28]), from transcriptions in the Amarna letters

[28] This form is listed, without reference, by Yeivin 1973a:145. The source is apparently *Sifra*, Codex Assemani 66, p. 52, where examples appear in lines 8 and 9. The form is spelled with a *šăwɔ* after the *ō*, but *šăwɔ* in this manuscript, as in all or most other

(Šakmi, Gubla) and other ancient documents (cf. Koehler, Baumgartner, et. al. 1967, s.v. *gəvâl*), and, to a lesser degree, from variant spellings in the Isaiah Scroll (*śwdm*) and the Genesis Apocryphon (*śwdm*, *śwdwm*) (cf. Kutscher 1959:83-4). Here again we find that all of the forms in question end in a resonant.

It remains to be said that, while it is clear that *śəxém*, *gəvál*, and possibly *śəðóm* underwent epenthesis earlier than absolute-state COMMON nouns ending in a resonant, it is not all clear why this should be so. Could it be that proper nouns were used in "pausal" form (i.e. without case-endings) in ancient Hebrew as Brockelmann (1903:5fn1) suggested? Or does the exceptional behavior of these place-names merely indicate that they were borrowed from the dialects of the cities to which they refer (Malone 1971:56, Bauer and Leander [1922]1965:580)? Further research is clearly called for.

A more difficult problem is posed by the existence of COMMON nouns stressed on their epenthetic vowel in the absolute state as well as the construct. It is well-known that nouns ending in *y* generally fall into this category. Since *y* is even more sonorous than the liquids and nasals, it is not surprising that these nouns should be more susceptible to early epenthesis and, consequently, to stress-shift. The only question is whether the oxytone allomorphs spread to the absolute state by analogy[29] or whether epenthesis preceded apocope in final-*y* nouns.[30]

Nouns with medial *ʾ* are also regularly stressed on their epenthetic vowel in the absolute state, and much of what has been said about final-*y* nouns applies with slight modification to this class of nouns as well. We might also note that the relationship between the medial-*ʾ* nouns and the other medial-laryngal nouns is very similar to the relationship between the final-*y* nouns and nouns ending in a liquid or nasal.

The most difficult problem of all is posed by nouns which are stressed on their epenthetic vowel in the absolute state, but yet do not fall into either of the above classes. Some of these (*gəvár*,[31] *śəlōw*) end in a liquid or semi-vowel, but some (*dəvâš*, *śəvôx*, *śəvâx*?) do not.

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manuscripts with Early Babylonian vocalization (Yeivin 1973a:70), often stands for etymological and phonetic *φ*. The vocalization with *o* (although it is possible that in line 9 this has been corrected to *u*) is puzzling (one would normally expect *u* in the Babylonian system), but not entirely unparalleled; cf. Yeivin 1973a:63(several examples),202 (*ʾoxle(y)* 'foods'), 204 (*tonpo(h)*). Incidentally, the form we are discussing is not a *hapax*. There are many examples of graphemic <swdmy> scattered throughout Rabbinic literature (Kutscher 1959:84,Sokoloff [1969]1972:295) which are clearly to be read *śuðmi* (Kutscher loc. cit.) or *śoðmi*. Finally, we should note that if we accept Kutscher's derivation (loc. cit.) of *śwdmy* from an original **śudumiyyu*, the Sifra's vocalization becomes less difficult to explain, although one might still have expected *śuðmiθ* or *śəðomiθ*. It may, therefore, be necessary, in the end, to abandon our classification of the name *śəðóm* as a segolate.

[29] Analogy was more likely with final-*y* nouns than with, e.g., final-*r* nouns, because more final-*y* construct forms had undergone epenthesis.

[30] In the latter case, we would posit *gadyu* > *gadiu* > *gadiyu*. Changes of this type (which increase the number of syllables in a word) are discussed by Jespersen (1913:193,198-9). This solution would allow us to account for forms like *ṣəviyyó* (Song 4:5,7:4) and *gəðiyyoθâyix* (Song 1:8) without invoking analogy.

[31] This form, which occurs in the phrase *gəvár tomim* (Psalms 18:26) is often said to be a construct form. This view presupposes that *tomim* can be a noun as well as an adjective, an assumption which is highly questionable. Not surprisingly the Hexapla vocalizes γαβρ Θαμιμ.

These forms are anomalous in other ways as well: 1) pre-tonic vowels, even *a*,[32] have been reduced (Malone 1971:56fn40), even though the tonic vowels are etymologically short,[33] and 2) stressed *a* has not been replaced by ɔ (except before *w*[34]) even though these are nominal forms. For these forms, the assumption of inter-dialectal borrowing (Malone 1971:56, Bauer and Leander [1922]1965:580) would seem to be the only solution.

4.3. Question 4b (the Vowel *a*)

If we accept Malone's assumption (1971:46) that the original epenthetic vowel was uniformly *e*, we must explain how this vowel was lowered to *a* in *ḥăḏâʀ*, etc. Two possibilities come to mind. On the one hand, we might attribute the lowering of *e* to the presence of *ʀ* (and, of course, the laryngals; cf. Malone's rule (hH), loc. cit.), especially if it took place while the *e* was still unstressed, cf. **wayyâsiʀ* 'he removed' > *wayyɔ̂saʀ*, and **wayyâᶜiʀ* 'he wakened' > *wayyɔ̂ᶜaʀ*. If, on the other hand, the lowering of *e* took place after the stress had shifted to it, Philippi's law would seem to provide the answer.[35]

Neither of these solutions is without its difficulties. The former solution is incapable of explaining why the epenthetic vowel of *śŏḏân* was lowered, while the latter solution fails to explain why the epenthetic vowel of *hăvêl* was NOT lowered. Nevertheless, the latter solution seems preferable, since there are other exceptions to Philippi's law which must be accounted for in any case: **ʾăvêl*[36] (note the striking phonetic similarity to *hăvêl*!), *ᶜăqêv*, and *ḥămêš*. Rather than explain these forms away as analogical restorations (a strategy which, in any event, will not work with *hăvêl*, since the corresponding absolute form is not **hɔvêl* but *hêvel*), we propose a solution based on the possibility that Philippi's law and stress-shift partially overlapped in time.

A glance at figure 5 reveals that Philippi's law and stress-shift at least fall within the same general time-period (after construct apocope and before absolute epenthesis[37]), and that stress-shift could easily be the later of the two. At the same time, it is well-known that stress-shift feeds Philippi's law. We may therefore hypothesize that stress-shift began in time to supply SOME customers to Philippi's law, but that the latter became extinct before the former had worked its way through the entire class of bi-syllabic construct forms.[38] In other words, it is possible that we have COUNTER-feeding here as well

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[32] Short *a*, the most sonorous of the short vowels, is generally immune to pre-tonic reduction in Biblical Hebrew. Final-*y* segolates like *gŏḏî* (< **gadyu*) and *ṣŏvî* (< **ṣabyu*) are only apparent exceptions to this rule, since the original *a* of these forms was raised to ɛ by vowel harmony (cf. *kɛlyŏxɔ*, *tɛlyŏxɔ*, *pɛʀyŏxɔ*, and *gêŏî*, *kêlî*, *pêʀî*, and the proper nouns *ʾɛvyɔsɔɕ*, *ʾɛvyɔθɔʀ*) before being reduced.

[33] This last detail, unmentioned by Malone, is crucial, cf. fn. 15, above.

[34] The ɔ in *śŏlɔ̂w* may be a product of assimilation (*a* becoming rounded before *w*) at a late stage, rather than a product of stress-lengthening, cf. Blau 1967:63, and add *wayŏθɔ̂w* (I Samuel 21:14) to the examples cited there.

[35] This solution was suggested to me by Norman Didia, a student of mine at Touro College.

[36] The form which is actually attested is *ʾavɛl-*, i.e. with ɛ and *maqqeɕ*; cf. p. 7 above for other examples of this alternation.

[37] I.e., the first stage of absolute epenthesis — affecting nouns ending in a resonant. This must have taken place very soon after absolute apocope. Accordingly, the time-period covered by this diagram is not so long as to invalidate the argument which we wish to base on it.

[38] Wang 1969, which I came across after writing these lines, argues, on theoretical grounds, that competition between (= the overlapping of) sound-changes is one of the causes of sound-change residue. The Hebrew data presented here certainly seem to support Wang's position.

as feeding[39] — and that *hăvēl*, **ʾăvēl*, *ʿăqēv*, and *hămēš* are "products" of this counter-feeding. As such, *hăvēl* would no longer stand in the way of a solution to question 4b based on Philippi's law.[40]

5. CONCLUSION

This investigation has shown that forms like *hăāār* and *hăvēl* are relics of an early stage in the development of Hebrew. The occurrence of these forms in Tiberian Massoretic Hebrew is significant, firstly because some of them (e.g. *šəγār*, *zərāʿ*, *nəṭāʿ*, *ḥasār*) must have already disappeared from colloquial Hebrew by the Mishnaic period, and secondly, because a number of them have been replaced by their absolute-state co-allomorphs in the Babylonian tradition of Biblical Hebrew, e.g. *hăvăl* (Kahle [1902]1966:171-2), *šăγăr* (Yeivin 1973a:193), *šăvāʿ ʿśrh*, *šăvāʿ mʾwt*, *tešāʿ mʾwt* (ibid., 220).[41] These facts must now be added to the growing body of scientific evidence (cf. esp. Kutscher 1959:23-52) indicating that the Tiberian vocalization is a faithful, even slavish, reproduction of a stubborn oral tradition which succeeded in preserving ancient forms even after they had disappeared from the Hebrew spoken by the bearers of that tradition.

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[39] This should not be confused with Koutsoudas, Sanders, and Noll's "feeding and counter-feeding" (1974:2), which is a symmetric relation between two NON-OVERLAPPING rules. What we are proposing is that PART of stress-shift precedes the extinction of Philippi's law (and thus feeds it) and part of stress-shift follows it (and thus counter-feeds it).

[40] Another solution to question 4b might be to reject Malone's assumption that there was only one epenthetic vowel and to assume that, in the earliest period, the epenthetic vowel took on the coloring of the stem vowel (as it does in some Arabic and Akkadian dialects), except when the word ended in a laryngal or *r*. This assumption is made plausible by the fact that *hăvēl* is apparently a *qitl* form (cf. *hεvlō*, *hεvlέxɔ*), while *śəγān* seems to go back to a *qatl* form (cf. Assyrian *šaknu*, from which it is borrowed). But construct epenthesis would still have to be ordered before stress-shift, and stress-shift before the extinction of Philippi's law, and, accordingly, the unlowered *e* of *hăvēl* would still be a problem. This "solution" is therefore illusory.

[41] Cf. also *śəγān* (abs.) for *śέγεn* in *Sifra*, Codex Assemani 66, p. 180. Older forms preserved by the Babylonian tradition are *bəʿāā* (Yeivin 1973a:198), *śəḥār* (ibid., 197), and *ḥsār* (Yeivin 1973b:168).

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