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Stop Being Fricative! The Hebrew *Šĕwā' Medium*, Syllabic Consonants, *Tidʿāl* and the Aesthetics of Linguistic and Exegetical Models[*]

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Introduction

The objective of this article is to discuss two cases of seeming "irregularity" in the Tiberian Masoretic stop and fricative systems, especially concerning the relationship between those two systems—and to ponder the

[*] This article was conceived and written by Wikander, who is responsible for the formulation of the text and the underlying ideas. The section called "Surface Realizations and Autosegmental Phonology" was, however, created in dialogue with Aljoša Šorgo, and the ideas in that segment are mostly his, as is the specific formulation of the syllabification rule there suggested.

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methodological implications of this for the study of Masoretic orthography and analysis. The first case has to do with the question of the *šĕwā' medium* (known in Hebrew as *šĕwā' měraḥēf*, "hovering *šĕwā*"), the strange and seemingly incongruous phenomenon giving rise to fricativized *beghadhkefath* letters appearing where they logically "shouldn't," i.e. where there is no preceding audible vowel (cases such as *birχōθ*, "blessings of" or *biqβūrāθô*, "in his grave")[1]—especially cases like these, where Tiberian vocalization produces an *i* due to the so-called *rule of šĕwā'*, which stipulates that two audible *šĕwā's* cannot be tolerated in successive syllables (a rule operating together with the *law of attenuation*, which turns etymological **a*-s in closed unstressed syllables into *i*). The second question has to do with a specific word showing vacillation between a stop and a fricative in various traditions of Hebrew, viz. the royal name preserved in Tiberian Hebrew as *Tiḏʿāl* in Genesis 14. This name, which is often (and in my view correctly) regarded as a Hebrew version of the Hittite royal name *Tudḫaliya*-, has been argued to preserve an early differentiation between etymological *ʿayin* and *ġayin* in its LXX Greek version, Θαργαλ. The discrepancy between the Tiberian and LXX versions of this name may tell us more about the relationship between certain stops and fricatives in the history of Hebrew than has previously been thought.

As a bridge between the two main parts of the article, I will also touch upon some methodological and perhaps even philosophical issues that arise when discussing these questions, regarding how matters of phonological reconstruction may disclose certain ideas of an almost aes-

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[1] Here, I use a sort of cross between the standard "Anglo-American" scholarly transliteration of Classical Hebrew (using *ĕ* for *šĕwā'* etc.) and a more phonetic-style rendering—the latter in the case of the *beghadhkefath* letters, whose fricative variants I represent as *β*, *γ*, *ð*, *χ*, *f* and *θ*, respectively. Despite the fact that I discuss the Tiberian tradition, I keep to the common rendering of *qāmeṣ* as *ā* (instead of *å*, *vel sim.*), even though the Tiberians pronounced it as a rounded back vowel. This is just a matter of keeping closer to standard transliteration. I use capital letters for the syllabified consonants that I will argue for later. In certain cases, I use overt IPA transcription, using the standard brackets [...].

thetic nature on the part of both the modern scholar and the historical tradents of the textual material.

The Sound of Silence: The *Šĕwā' Medium* and "Magical *Ḥireqs*"

The first question concerns Hebrew words in which, etymologically and structurally, two pronounced *šĕwā's* would have followed each other—an impossibility due to the rules of Tiberian syllabification—and how this relates to the question of *beghadhkefath* spirantization. One such example is the construct form that in pre-Hebrew must have sounded something like **barakōt* ("blessings of," ultimately from Proto-Northwest Semitic **barakātu*). Because this is a noun in the construct state, the vowels thereby undergoing reduction in open syllables, this could have been expected to turn into ***bĕrĕχōθ* (with the two postvocalic *beghadhkefath* letters *k* and *t* being turned into their respective fricative allophones). However, as Tiberian Hebrew does not tolerate two audible *šĕwā's* in succession, the second one is deleted from pronunciation (becoming known as a *šĕwā' medium*[2]—which is "there, yet not there"), and the law of attenuation (which turns historical **a*-s in to *i*-s in

[2] Introductions to the phenomenon can be found in many standard handbooks; see, e.g., Joshua Blau, *Phonology and Morphology of Biblical Hebrew: An Introduction* (Winona Lake, IN: Eisenbrauns, 2010), 114–115; Ivan Engnell, *Grammatik i gammaltestamentlig hebreiska* (Stockholm: Nordstedts, 1963), 21; Willhelm Gesenius, E. Kautzsch, and A. E. Cowley, *Gesenius' Hebrew Grammar: As Edited and Enlarged by the Late E. Kautzsch ... Revised in Accordance with the Twenty-Eighth German Edition (1909) by A.E. Cowley*, 2nd ed. (Oxford: Oxford University Press, 1910), 51–52 (§10*d*) (in the latter case, though, wanting to do away with the term itself, though still describing the same phenomenon—while arguing that the fricativizing vowel was entirely elided and that this means that the fricativization is "older than the elision," with reference to Sievers); Paul Joüon and Takamitsu Muraoka, *A Grammar of Biblical Hebrew.* 2nd, rev. ed., SubBi 27 (Roma: Editrice Pontificio Istituto, 2006), 49–50 (§8*e*, with the same attitude as Gesenius, Kautzsch, and Cowley).

unstressed closed syllables)[3] or the "rule of *šĕwā'*" turns the entire word into *birχōθ*, with the *k* still spiranticized into *χ* even though there is no longer any pronounced vowel directly preceding it.[4] Another example consists of imperatives such as *qirβû* ("draw close! [plural masculine]"), which stands in a regular morphological relationship with the corresponding imperfect form *tiqrĕβû*; as the *qal* imperative in Hebrew normally is synchronically identical with the imperfect without its prefix, the morphologically (as opposed to phonologically and phonetically) expected synchronic form could be argued to be ***qĕrĕβû*, again a phonotactic impossibility due to the two pronounced *šĕwā's* in consecutive syllables—solved by turning the first *šĕwā'* into *ḥīreq* and the second into a *šĕwā' medium*, producing the actually attested form, *qirβû*. An even more illustrative example would be the (biblically unattested but formally certain) feminine singular imperative *qirβî*, which would form a minimal pair (in orthographical terms, at least) with *qirbî*, "my midst."

Even though the etymological background of these cases is clear, these phonotactic structures constitute an anomaly within the confines of the Tiberian phonological system. The situation here at hand—spirantized/fricativized *beghadhkefath* letters appearing without a synchronically pronounced vowel preceding them—breaks the usual rules of the

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[3] On the law of attenuation, see now the highly illuminating account in Benjamin D. Suchard, *The Development of the Biblical Hebrew Vowels* (PhD diss., Leiden University, 2016), 189–212.

[4] This historical analysis – that these cases go back to a subsequently elided vowel having already fricativized the following stop before disappearing – originally goes back all the way to Eduard Sievers, *Metrische Studien I: Studien zur hebräischen Metrik: Erster Teil: Untersuchungen,* Abhandlungen der philologisch-historischen Classe der Königl. Sächsichen Gesellschaft der Wissenschaften 21 (Leipzig: B.G. Teubner, 1901), 22–23. A concise description of the whole process generating *birχōθ* and similar forms can be found in Hans Bauer and Pontus Leander, *Historische Grammatik der hebräischen Sprache des Alten Testamentes* (Halle: Max Niemeyer, 1922), 240–241 (§26*w*'). They describe the process in terms of only the second vowel being elided, but this means essentially the same thing, as two vocal *šĕwā's* in successive syllables are not tolerated. See also page 595 (§74*a*') in the same publication.

synchronic Tiberian sound-system, and is often regarded as a sign of the dual pronunciation of these letters actually being on the way to becoming phonemic (more on this later).

A New Proposed Analysis: Syllabic Consonants

However, positing what is in effect a new set of rule-breaking phonemes for these cases need not be the most parsimonious route to take. I would like to propose a new type of analysis of this sort of syllabic structure, one involving no "deleted *šĕwā'*s" that are "magically" turned into *ḥīreqs* (or in some cases *paθaḥs*). Rather, a fitting analysis would be to regard these cases as involving *syllabified consonants* (or at least consonants forming syllabic nuclei), which themselves turn subsequent *beghadhkefath* letters into fricatives (or perhaps better: are vocalic enough to keep them fricative).

The reason for this is the place in which the "*šĕwā' medium* + fricative stop" type syllabic structure has in the Tiberian phonological system. If one keeps with the classical analysis—that the vowel that has subsequently been made silent ("medium") fricativized the following stop before quiescing—one would in effect have to presuppose that the Tiberian Masoretes performed a type of etymological/morphophonemic analysis every time they encountered such a word (to "change it back" into its etymological shape, or rather: to keep it from changing into the form expected by phonologically automatic processes, which would have yielded a form with *dāγēš lene* in the relevant consonant, i.e. the stop allophone). To be sure, the Masoretes could have received "by tradition" a fricativized form, but the odds of that form staying fricative would have been slim, given the pervasive "fricative = audible vowel preceding" rule of the Tiberian system. The main attitude of the Masoretes seems to have been to write down *what they thought they heard* (in the sense of *their subjective feeling for what was demanded for the realization of the underlying phonology*), even though the grammatical rules strictly mandated something else (the use of *ḥāṭēfs* instead of *šĕwā'*s under guttural letters is a classic example of this). I would argue that the Tiberian

system of notation included some features that were closer to being *phonetic* than *phonological* in a historical sense.[5] Therefore, given that the

[5] Thus, my approach here is somewhat different from, e.g., that employed in Benjamin D. Suchard, "Sound Changes in the (Pre-)Masoretic Reading Tradition and the Original Pronunciation of Biblical Aramaic," *Studia Orientalia Electronica* 7 (2019), 52–65 (55), where it is posited that "... the Tiberian vocalization only marks phonemic contrasts, which sits well with the fact that speakers are typically unaware of the allophony they produce and the phonological rules they employ." Suchard mentions a number of demonstrable differences in surface realizations that are not recorded in the Tiberian pointing but which can be reconstructed with the help of mediaeval transcriptional material (following Geoffrey Khan), and there I agree, but I would not say that this necessarily implies that the Tiberian pointing as such was *only* based on phonemic contrasts, simply that there may have been additional allophonic processes that were not recorded in the writing at all. On the question of the *ḥāṭēfs*, Suchard argues in another article—with some hesitation—that they probably were phonemic in some contexts (as they exceptionally appear in other contexts than near gutturals, their usual—and predictable—distribution): see Benjamin D. Suchard, "The Vocalic Phonemes of Tiberian Hebrew," *HS* 59 (2018), 139–207 (202–203). He also refers to Geoffrey Khan's ("Syllable Structure: Biblical Hebrew," in *Encyclopedia of Hebrew Language and Linguistics*, vol. 3, ed. G. Khan, Sh. Bolozky et al. [Leiden: Brill, 2013], 666–676 [666–668]) argument that some such *ḥāṭēfs* must be regarded as phonemic, as they appear to have influenced the realization of /r/ in Tiberian Hebrew in a way only consistent with an underlying, syllable-creating phoneme (determining whether the /r/ was realized as uvular or alveolar—cf. footnote 67 in the present article). Although I find those arguments in themselves reasonably convincing, it does not change the fact that this phonemization of the *ḥāṭēfs* was quite marginal, and that the overwhelming number of *ḥāṭēfs* was entirely predictable from the surrounding phonological environment. N.b. that Khan himself mentions ("Syllable Structure," 667) that the marking of *ḥāṭēf paθaḥ* under consonants other than the guttural ones "is not consistent and is written more frequently in some manuscripts than others" (with reference to Israel Yeivin, *Introduction to the Tiberian Masorah*, Masoretic Studies [Missoula, MT: Scholars Press, 1980])—and later, on p. 673, that syllables with non-guttural-conditioned *ḥāṭēfs* are of "the non-canonical weight of a single mora" and thus "highly marked," which indicates their unusualness. All in all, while I agree that much of the Tiberian pointing system does indicate the underlying phonemic system, it cannot be presupposed that it does so *exclusively*. As mentioned in the main text, the continuous operation of post-vocalic fricativization as an automatic surface-filter creates many pointing differences that must be regarded as entirely allophonic, for example. Also, as concerns the question of the allophonically conditioned pronunciation of /r/ (present in transcriptions and mediaeval descriptions but not indicated in the Tiberian pointing

beghadhkefath spirantization rule was synchronically operative as a phonological surface filter (as opposed to a grammatical rule) within the Tiberian morphophonological system, one would have expected the Masoretes to "correct" the letters back into the corresponding stop version, since no audible vowel directly preceded them—which would have meant them having to have applied the above-mentioned etymological or morphophonemic analysis to refrain from doing so.[6] This is hardly a parsimonious scenario.

Rather, the facts suggest the probability of the Masoretes actually having subjectively "heard" something that they regarded as a vowel before the fricativized consonant.[7] This vowel-like element, I suggest, was

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itself), it must be remembered that pronunciation among Masoretes may have varied (as I argue below in the main text), which means that the surface realization of /r/ need not necessarily have been identical among *all* proponents of the Tiberian reading tradition (which makes the distinction somewhat less probative). Also, the Khan-Suchard argument for at least some *ḥāṭēfs* being phonemically underlying (due to their effect on the pronunciation of adjacent /r/ in Tiberian Hebrew), whereas many need not have been (being conditioned by gutturals), could actually be used as an argument for Masoretic pointing both having features representing underlying phonology and ones related only to surface realization.

[6] Interestingly, according to Alan S. Prince, *The Phonology and Morphology of Tiberian Hebrew* (PhD diss., Massachussetts Instititute of Technology, 1975), 81, there is data suggesting that there were "some die-hard surfacists among the earlier medieval grammarians" who insisted on actually pronouncing *šĕwā' medium* as a vocal *šĕwā'* simply to make the rules add up, so to speak (when the next letter was fricativized). If this is correct, it underscores that the fricativization rule was regarded as being quite "alive" in the language (and not just an earlier process), and that the apparent lack of match-up was seen as a problem. This, in a way, would be a type of reverse of the idea of "correcting" the fricatives back into stops (theoretically discussed earlier in the present article), and would show that such inclinations could exist. The fact that the Masoretes did *not* do this (nor follow the "die-hard surfacists") is, I argue, highly relevant.

[7] The early suggestion along similar (but still significantly different) lines, that the Masoretes heard some sort of "half-reduced *šĕwā'*," has rightly been abandoned in modern scholarship—see the above reference to Sievers, *Metrische Studien*, which did in fact polemicize against this possible (though highly unlikely) position. One could perhaps say that the position I argue here gets close to it, but I would like to underscore that my point here is an underlying syllabic consonant, not some sort of "half-pronounced" *šĕwā'*, so to speak. See also the previous footnote.

the simplest one possible: a syllabic version of the consonant itself. The various ways of writing this pseudo-vowel (*ḥīreq* before, *paθaḥ* before) should then only be regarded as allophonic surface realizations or even as graphic conventions, possibly influenced by etymological associations (such as in the example *malχê*, "kings of," for which there is an entire battalion of forms with actual *paθaḥ* that could act as analogical templates—*malkî*, etc.). Note also that the choice of *a*- or *i*-vowels in the root syllable in such cases seems partly motivated by purely phonetic concerns (a second root consonant being *l* or *r* favoring *a*, for example, as guttural letters), a fact established by Yuditsky.[8]

This may perhaps seem like an outlandish suggestion at first, but an illustrative typological parallel to this analysis can be found in Sanskrit and Avestan, both of which possess a syllabic version of *r* or a historical descendant thereof. In Sanskrit spelling, there is a special sign for this phoneme, transcribed *ṛ*, but in Avestan, it has developed into what is graphically rendered as *ərə*, which shows that typological similarity to the Hebrew situation quite clearly. Also, one should note that many reading traditions of Sanskrit tend actually to pronounce *ṛ* as [ri].[9] This is a perfect parallel to the situation that I want so suggest for Tiberian Hebrew: the underlying phonemic reality of the language was still a syllabic *r*, notwithstanding its synchronic realization as *ərə* or [ri].

In fact, the tension created between what the Masoretes appear to have "heard" (as in "wanted to transcribe") and what the morphophonological system demanded indicates a very sophisticated problem sphere as regards how we as scholars are to regard their activity. The

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[8] Alexey (Eliyahu) Yuditsky, "'Al 'ĕkhôt tĕnû'â biltî mûṭ'emet šel-lĕyad *r* wĕ-'îṣûrîm 'ăḥērîm" (Modern Hebrew; "On the Quality of Unstressed Vowels in the Vicinity of *r* and Other Consonants"), *Leshonenu* 73 (2010), 55–68 (57–59; cf. also the table on pp. 64–65). I also refer to the convenient overview and development of the idea in Suchard, *Development of the Biblical Hebrew Vowels*, 197–199. Note that Suchard explicitly invokes paradigmatic leveling as an argument in cases that do not fit with Yuditsky's suggested rules, much as I do in the main text above.

[9] Or, in some pronunciation traditions, [ru] or even [ro].

Tiberian reading tradition not being directly attested in any living tradition of Biblical Hebrew, we have to put ourselves, as it were, *in loco masoretarum* when analyzing their transcription principles and the relationship between the (synagogally prescribed?) pronunciation that they wanted to convey and preserve and the phonological rules that they wanted to see or impose on the material. Another type of evidence is supplied by contemporary transcriptional material, studied in the work and scholarly tradition of Geoffrey Khan.

Note again the usage of *ḥāṭēfs* instead of "normal" vocalized *šĕwā'*s under guttural letters as a sign of the tension between the two abovementioned ideals: simply using a normal *šĕwā'* would often have violated the acoustic properties of what the Masoretes actually heard, whereas using a simple, full vowel would have broken the morphophonological rules that the Tiberians were apparently aware of.[10] Thus, *ḥāṭēfs* provided an ideal middle ground between mophophonological prescriptivism and phonetic transcription; my argument for syllabic consonants entails something similar: the Masoretes transcribed what they "heard," but the underlying phonological reality in that case was something different (we will return in greater detail to what "heard" may actually have entailed in this case). Also, one should remember that the assumed consensus culture of "the Masoretes" (as a unified collective) should not be postulated as an *a priori* axiom: there certainly is large degree of analytical uniformity present in the work of the Tiberians, but one need not necessarily assume that all the people engaged in the establishment of the vocalized, Tiberian text must have agreed on each and every point, which allows for some possibility in variation. The positions on which they agreed may in some cases have represented compromises between rivalling views and analyses.[11]

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[10] A similar analysis is found in Bauer and Leander, *Historische Grammatik*, 111 (§7*l'*).

[11] I would like here to refer to the view of Blau, *Phonology and Morphology*, 117–118, who argues that the use of *šĕwā'* in Tiberian Hebrew is a sign that the language

Of course, it could be objected that, on a phonological level, what is going on is rather a matter of *two* surface filters being active and not one: first the fricativization of stops and subsequently the vowel deletion rule that hides the historic reason for that fricativization and "cheshirizes"[12] it (so that the order of the rules represents not only a historical sequence but a layered system of synchronic rules).[13] This, by itself, is quite a possible scenario, but the problem with that analysis is that Masoretic Hebrew was not a natural, spoken language but one "generated" through the complex interplay of handed-down reading tradition and the grammatical/phonological sensibilities of the Masoretes themselves (who, of course, where not native speakers of Hebrew), superimposing their somewhat idealized system on the consonantal text that they had received. The way in which the fricitivization/spirantization rule operates seems to constitute a totally automatic surface filter, applied at the "end stage" of phonological generation—as shown, for example, by the fact that it normally operates across word boundaries when a preceding word ends in a vowel—which makes it less likely that the historical

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encoded by the Tiberian Masoretes is in a stage of transition and does not represent a single, unified vocalic system. However, I do not necessarily agree with his use (pp. 79–80) of this idea of "transitionality" as an argument for *beghadhkefath* spirantization being allophonic word-initially (due to the sandhi-like surface filter of a previous vowel) but semi-phonemic in medial and final position (due to seeming exceptions such as *šĕwā' medium*). Each such situation has to be examined on its own in relation to the surface filter rule (see later in this article).

[12] "Cheshirization" is a somewhat jocular term for the phenomenon of phonological changes that are conditioned by certain contexts, after which these contexts have subsequently disappeared, leaving only the conditioned result behind as evidence of having existed – the inspiration being, of course, the Cheshire Cat of *Alice in Wonderland* fame (the cat that disappears, leaving only its smile behind, prompting Alice to think: "I've often seen a cat without a grin ... but a grin without a cat! It's the most curious thing I ever saw in my life!"). The term was coined by James Matisoff, "Areal and Universal Dimensions of Grammatization in Lahu," in *Approaches to Grammaticalization*, ed. E.C. Traugott and B. Heine (Amsterdam/Philadelphia PA: Benjamins, 1991), 383–453 (443).

[13] I would like to thank Nicholas Zair for neatly formulating this objection.

vowel reduction giving rise to *šĕwā' medium* was operating as an equally synchronic surface process (and one following *after* the fricativization, at that). The fricativization applies across the board, so to speak, in a way that the vocalic reduction can hardly be said to do.[14] For some other possible evidence for the "internalization" of the relation between fricativizing and preceding audible vowels, see also footnote 6, above.

In this context, note specifically that the fricativization also affects old loanwords, such as the Persian *paθ-bay* in Dan 1:5 ("food, morsel, provisions," from Old Persian *patibaga*); there is one single case in which a loanword (also a Persian one) demonstrably kept a unique—and non-fricativized—*beghadhkefath* letter all the way down into the Tiberian manuscripts, viz. the *p* in *'appaðnô* ("his palace/hall," Dan 11:45, from Old Persian *apadāna*, notably not with a geminate *p*), which according both to mediaeval texts and the much earlier reports of Jerome was pronounced in an uncharacteristic way: as an unaspirated, emphatic *p*, a phoneme occurring only here in the entirety of the Hebrew Bible. This would therefore probably be a tradition going back all the way to the Persian source word, preserving its unaspirated *p*.[15] The

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[14] Note the differing analysis found in Geoffrey Khan, "How was the *Dageš* in Biblical Hebrew בְּתִּים Pronounced and Why is it There?," *JSS* 63 (2018): 323–351 (328), where it is argued that cases of spirantization after *šĕwā' medium* indicate that the vowel loss postdates the working of the spirantization process in these words (which was also the position apparently taken by Gesenius, Kautzsch, and Cowley—see above, footnote 2). As argued in the main text, I believe that the persistent effect of spirantization across word-boundaries militates against such an approach—at least against one that views the spirantization as a one-time event, so to speak, that necessarily stopped operating after the vowel loss. See also footnote 6, above. Khan does, however, also adduce forms like 2nd fem sing *lāqaḥat*, which I do believe is relevant in the present context, but in a rather different way (see below, under the heading *Fewer Unnecessary Phonemes?*).

[15] On this, see Geoffrey Khan, *A Short Introduction to the Tiberian Masoretic Bible and its Reading Tradition*, Gorgias Handbooks (Piscataway, NJ: Gorgias Press, 2013), 55. Indeed, Khan uses this word and its ancient pedigree as a sign of the conservatism and stability of the Tiberian reading tradition itself. For the original Persian form of the word, cf. *HALOT*, s.v. **'appeðen*).

existence of such a conservative feature in a loan-word means that such a phenomenon could have "preserved" other loan-words from fricativization if that process were just something of the past and not a living, final step in phonetic generation, yet *paθ-baγ* shows us that this normally did not happen. If a word was felt as being "alien," it could demonstrably have had its stop pronunciation preserved by the introduction of an atypical phoneme, but in most cases, the loanwords were totally assimilated to the Tiberian sound system and thus underwent fricativization where appropriate. This, though partly an *argumentum e silentio*, also suggests that the fricativization was an automatic, surface rule, exceptions from which were highly strange and noteworthy to the Tiberians themselves.

Thus, one would have expected the Masoretes to "correct" a fricative following a *šĕwā' medium* back into the corresponding stop, which they obviously did not do—unless they somehow experienced that there was some sort of underlying vocalic syllabic structure (which is what I am suggesting here that they did).[16]

In actual surface phonetic realization, it is of course harder to think that stops were in some cases realized as true syllabics—they may, in fact, even have involved a phonetic [i]. But this is a question of *phonetic realization*, not of the underlying phonological system, which is easier explained by regarding the consonant itself as being syllabic and thus inducing fricativization of a following *beghadhkefath* letter. The Masoretes were working with phonology, to be sure, but in their *niqqûḏ*, they were closer to phonetics than phonology in this case (we will soon be returning to the question of the exact "levels" involved).[17]

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[16] But see the next section for the strange behavior of construct infinitives in this context and the "flames" word also discussed later in this article.

[17] Interestingly, the idea that Hebrew may at some point have had syllabic consonants in its prehistory is also mentioned (very obliquely) in A. Murtonen, *Hebrew in its West Semitic Setting: A Comparative Study of Non-Masoretic Hebrew Dialects and Traditions*, part 2–3 (Leiden/New York, NY: Brill), 174. There, however, the argument concerns far earlier periods than the Masoretic stage discussed here.

One may note with some interest that a suggestion of a syllabic version of at least resonant consonants was made already in the grammar of Bauer and Leander (1922), referring, however, not to the Tiberian Masoretic tradition but to the one represented by the *Babylonian* Masoretes. The grammar mentions cases in which two consonants would have been followed by a *šĕwā'* but where a an actual vowel was written between them instead (the paradigmatic example being *Yirimyāhū* for Tiberian *Yirmĕyāhû*), and adds: "Da von den beiden zusammenstossenden Konsonanten der zweite gewöhnlich *r*, *l* oder *m* ist, so hat man sich wohl ein sonantisches *r̥*, *l̥* oder *m̥*, als Zwischenstufe zu denken."[18] This analysis is quite similar to the one proposed here, with the differences that (a) the present proposal involves the Tiberian tradition and not only the Babylonian one and (b) it widens the idea beyond resonants, at least as far as the underlying level of syllabification is concerned (see further below, in the sections on the "Skin'em Levi" consonants and on autosegmental phonology for further discussions of the surface phonetic realizations of resonant and non-resonant syllabic consonants).

Morphophonological Conundra

One could object that the above model involving syllabic consonants is too complex an explanation of the phenomenon here under scrutiny. In cases like *biqβūrāθô* ("in his grave"), one could easily imagine a conscious etymologization on the part of the Masoretes having played a role, keeping the *bēθ* fricative due to its being so in the form without the prefixed preposition (*qĕβūrāθô*). In cases like the above-mentioned imperative *qirβû*, however, such an explanation is much harder to maintain. There, the (synchronically, at least) underlying spirantizing form of the word corresponds mechanically to another inflectional paradigm altogether, the imperfect *tiqrĕβû*—due to the general synchronic principle

[18] Bauer and Leander, *Historische Grammatik*, 211 (§20*h-j*).

in Biblical Hebrew that most imperatives look like the imperfect with its prefix taken away—a form which would have had to have been artificially constructed for every relevant verb and the phonotactics of which would have had to have to been reprojected onto the imperative by the Masoretes to "keep" the *β* fricative in spite of the general rules. This is hardly a credible analysis of what is going on.

An especially intriguing problem connected with this whole issue is the fact that infinitives construct with a prefixed preposition behave strangely in relation to *šĕwā' medium*. When the prefixed preposition is the most common one, *l-*, the resulting form (e.g. *liχtoβ*, "[in order] to write") is treated as though the *šĕwā'* were really quiescent (with no fricativization of the second radical, *t* in this case), but when the other two one-letter prepositions a (*b-* and *k-*) are involved, the resulting forms *do* show fricativization (and thus, in the classical analysis, *šĕwā' medium* must be in evidence): *biχθoβ* ("in writing") and *kiχθoβ* ("like writing/when writing").[19] This strange and asymmetric state of affairs probably has to be explained as a case of analogy: given that the infinitive construct with *l-* is so very common as almost to become a verbal form in and of itself (which, indeed, it practically does in later stages of Hebrew), it is easy to imagine a scenario in which the analogical pressure for this phonetic realization came not from the historically parallel ones with *b-* and *k-* but rather from the imperfect of the *o*-stem verbs (*yiχtoβ* etc., from historical **yaktubu*).[20] The non-fricativization of

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[19] Bruce K. Waltke and M. O'Connor, *An Introduction to Biblical Hebrew Syntax* (Winona Lake, IN: Eisenbrauns, 1990), 599 (§36.1.1d). The specific example forms for "in writing" and "like writing" are unattested in the Bible, but quite certain due to the general patterning with other verbs. The same examples are used in Gesenius, Kautzsch, and Cowley, *Hebrew Grammar*, 348 (§114*f*), n. 8, which also suggests a real verb form coming into effect in the case of *l-* and seeing the different treatment of *beghadhkefath* as a sign of this.

[20] One may also note that the difference in treatment of the infinitive construct depending on the choice of preposition is correlated with whether or not that preposition does itself involve a *beghadhkefath* letter: *kĕ-* and *bĕ-* do—and those two do cause spirantization of the initial letter of the infinitive—whereas *lĕ-* does not—and

the middle radical in this form is thus no obstacle to the phonological analysis offered here: it is the result of an analogical restructuring, which in itself says something about how the Masoretic phonological tradition viewed the forms in question (i.e. as being closer to the actual finite inflectional system than to the "gerund-like" constructions of the other two prepositions). However, one could argue that it is *here*, not in cases like *qirβû* etc., that we find steps towards a phonemization of the fricative allophones: after the analogy had done its work, separating *biχθoβ* from *liχtoβ*, one almost ends up with a minimal pair. Yet, the process leading to this is still analogical at the level of Tiberian pointing. And, again, it bears pointing out that this difference in form could not have arisen due to "dual surface filters" but needs a morphophonological process to produce the data at hand (as two patterns that were structurally identical from a historical point of view turn out in different ways,[21] apparently due to the morphological connection made synchronically by the Masoretes—or, theoretically, some predecessor of theirs).

Allophonic Analogy (?)

The idea of an allophonic difference in pronunciation spreading by analogy may seem strange at first sight; analogies normally affect

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causes no spirantization. Whether this is really a relevant correlation is much less certain, however, as the letters making up the prepositions need not be spirantized themselves, which means that some sort of sandhi-like spread of the feature [SPIRANTIZED] seems less likely here than the morphophonological conditioning mentioned in the main text.

[21] One could, of course, argue that the three prepositions originally possessed some difference in vocalization at the proto-level, that could possibly influence their treatment here. However, as pointed out in Leonid Kogan, *Genealogical Classification of Semitic: The Lexical Isoglosses* (Boston, MA/Berlin: de Gruyter, 2015), 119, n. 341, the original vocalization of **b*- and **l*- is highly uncertain and seems to have varied among languages in unpredictable ways (some showing original **i*-vowels and some showing **e*, with syllabic transcriptions of Ugaritic even giving the strange pairing of *bī*- versus *lē*- [n.b. the long *ē*!]). Different forms of analogical replacement must have taken place in various places, and thus, no firm argument should be based on these discrepancies.

morphemes, not allophones.[22] But again, there are typological (extra-Semitic) parallels to such a development. One can be found in the complex sandhi rules of classical Sanskrit, which include the (on the surface) quite strange rule that word-final *-ān* is transformed into *-āṃs* before an unvoiced dental stop. The rest of the sandhi system tends to be quite logical from a phonetic point of view, but this case is baffling. The reason for the strange sandhi development is, however, etymological: one of the most common sources of word-final *-ān* in Sanskrit is the Proto-Indo-European thematic accusative plural masculine ending **-ōns*. It is the final sibilant of this ending that is preserved before dental stops, such as in the sentence *tāṃs titikṣasva*, *bhārata* ("Endure them, son of Bharata!"), appearing in the second chapter of the Bhagavad-Gītā. However, this sandhi-ized structure spread throughout the Sanskrit phonological system, so that words ending in *-āṃs* appeared where it was never etymologically motivated, the rule even extending to all cases of final *-n* before unvoiced dental stop (and, with a slight variation, palatal or retroflex stop).[23] As sandhi is basically a purely allophonic process (at least at first), this process provides a nice parallel to the *beghadhkefath* case, showing that allophones can be analogically induced and spread based on etymological considerations. A similar "analogical allophone" is in evidence in the possessive suffixes *–χā/–χem/-χen*, which appear with a fricative *χ* even if the word stem ends in a consonant (as in the object marker with 2pl suffix: *'eθχem*, *'eθχen*). In that case, however, one could well speak of the entire morpheme being in involved in the ana-

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[22] One may, however, note with some interest that E. A. Speiser argued already in the 1920's that basically *all* cases of *šĕwā' medium* were due to analogy! By reason of this, he was one of those calling for a dismantling of the category. See Ephraim Avigdor Speiser, "The Pronunciation of Hebrew According to the Transliterations in the Hexapla," *JQR* 4 (1926), 343–382 (373–378).

[23] For the Sanskrit rule and its background, see, e.g., Arthur A. Macdonnel, *A Sanskrit Grammar for Students* (New Delhi: Motilal Banarsidass, 1991 [1927]), 18, n. 1. Macdonnel also mentions the old nominative singular **-ns* of *n*-stems as part of the background, but the argument works in that case, too.

logy.[24] To be sure, that case and the Sanskrit one are not exactly parallel (being actually somewhat opposite in their development), as the Sanskrit case involves a conditioning environment, but the point is that the example illustrates that a normally allophonic process can be influenced by etymological "background noise." The existence of such cases do not necessarily constitute a defeater for the fact that most cases of the interchange between fricative and stop actually are allophonic.

One argument that might make this sort of process more likely is the fact that the process of *beghadhkefath* spirantization was probably in origin a phenomenon appearing due to adstrate influence from Aramaic.[25] That is: its beginnings are not entirely "at home" in Hebrew itself, which could increase the probability of allophones behaving in a way not entirely concordant with their normal conditioning factors.

The parallel from Sanskrit "allophonic analogy" and the sandhi system is illustrative from a wider perspective: it highlights how a spelling system which is to a large extent phonemic in its construction can still include features that do not express the underlying phonology in a clear-cut way (but rather surface phonetic phenomena), and how sometimes, phonetic processes can get retrojected onto and influence the underlying phonological structure in a kind of dialectic process—a process especially likely in the case of canonized, reading-tradition based languages in which the ancient phonological system(s) of the various periods behind the text meet the synchronic pronunciation of the codifiers. This, I suggest, is a quite relevant principle and possibility in the case of Tiberian Hebrew spelling, as well.

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[24] Joüon and Muraoka (*Grammar of Biblical Hebrew*, 77 [§19*f*]) talk of the realization of the *beghadhkefath* letters sometimes being "built into a morphological and lexical pattern," which is similar to what I am referring to here.

[25] See, for example, Paul Kahle, *The Cairo Geniza*, The Schweich Lectures, 1941 (London: Oxford University Press, 1947), 102–108.

The Evidence of *Qiṭṭālôn* Nouns and *Dāgēš Forte Dirimens*

One type of noun that may initially be seen as a stumbling block to the analysis offered here—but may actually rather provide direct support for it—is those belonging to the *qiṭṭālôn* pattern. Such nouns have a geminated second radical in the singular absolute state, whereas the gemination is mostly lost in all other forms (the standard example is *zikkārôn*, "remembrance," and its construct, *ziχrôn*). If a *beghadhkefath* letter is found in radical slot number three, one would imagine—based on the behavior of other nouns in the construct state or other forms with a rightward shift in accentuation—that this letter would still be fricativized even though the gemination is gone and the reduced vowel after it (normally a *šĕwā'*) would become quiescent (thus a classic case of *šĕwā' medium*).

However, this is not what happens. In the forms actually attested of *qiṭṭālôn* nouns with a *beghadhkefath* letter as the third radical, something entirely different tends to occur: the *dāγēš* is still there, even though it "should not":[26] see examples such as *'iṣṣĕβônēχ* ("your hardship," Gen 3:16, from *'iṣṣāβôn*), the construct singular *'iṣṣĕβôn* of the same word (in Gen 5:29) and the plural *ḥiššĕβōnôθ* ("strategems, plans, inventions") in Qoh 7:29 and 2 Chr 26:15. In these words, the root-final *beghadhkefath* letter is indeed fricativized, but the *dāγēš* in the preceding consonant is still there (even though it ought not to, due to the normal patterning in *qiṭṭālôn* nouns), and thus the *šĕwā'* is audible and technically not a *šĕwā' medium* at all (which, again, would have been expected in *qiṭṭālôn* nouns).

The unexpected *dāγēš* in these forms is usually explained (or rather just referred to) as a "*dāγēš forte dirimens*," meant to remind the reader

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[26] The reasons for this discrepancy have not always been sufficiently appreciated. For example, Joüon and Muraoka (*Grammar of Biblical Hebrew*, 240 [§88*b*]) simply state that the construct or suffixed forms of *qiṭṭālôn* nouns are "usually without doubling," without further comment.

that the following *šĕwā'* is indeed audible.[27] However, I would like to propose a different analysis: maybe it is actually there to show that the consonant is syllabic? This type of spelling may well represent an actual attempt by the Masoretes to encode this strange syllabic structure in writing.[28] Thus, far from providing evidence against the present suggestion, these words may in fact represent evidence *for* it.

Speaking of "*dāγēš forte dirimens*," it is interesting to note that this rather "weird" phenomenon occurs especially often in the letters *lāmeð*, *mēm*, *nûn*, *qôf* and the various sibilant letters, and that it appears in construct forms such as *'innĕβê* ("grapes of," Lev 25:5, Deut 32:32) and *'iqqĕβê* ("heels of," Gen 49:17), which "ought" to have been ***'inβê* and ***'iqβê*, no *dāγēš* but fricativized third radical—and thus, by implication, *šĕwā' medium*.[29] Using this device in sonorant letters such as these (as well as the highly marked uvular [ʔ] emphatic *qôf* and the sibilants,

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[27] See, e.g., Viktor Golinets, "*Dageš*," *Encyclopedia of Hebrew Language and Linguistics*, vol. 1, ed. G. Khan, Sh. Bolozky et al. [Leiden: Brill, 2013], 649–654 [652]). I here render the *dirimens* as gemination in the transliteration, even though that is probably not really "what it means": this is just a matter of convenience.

[28] Compare, in a type of roundabout way, with the "orthoepic" strategies that Geoffrey Khan has suggested as the reason for historically unmotivated geminate readings in the Tiberian tradition in cases where a syllable-initial consonant has *dāγēš lene*, in order to maximize the difference between spirantized and non-spirantized forms—Khan refers to this phenomenon as "extended *dagesh* forte" (see Geoffrey Khan, "Orthoepy in the Tiberian Reading Tradition of the Hebrew Bible and Its Historical Roots in the Second Temple Period," *VT* 68 [2018], 378–401 [380–383] and the longer exposition in Geoffrey Khan, "Remarks on the Pronunciation of Dageš in the Tiberian Reading Tradition of Biblical Hebrew," in *Semitic, Biblical, and Jewish Studies: Festschrift for Richard C. Steiner*, ed. Mordechai Z. Cohen, A. Koller, and Adina Moshavi [Jerusalem/New York, NY: Bialik and Yeshiva University Press, 2018], 433–441 [*non vidi*]). The analogy is only an imperfect one, it must be granted, as the *dāγēš forte dirimens* was not, it must be presupposed, actually *pronounced* as a gemination (which Khan's Karaite transcriptions into Arabic suggest that the "orthoepic" and "extended" *dāγēš forte* was), but it would still be a case of *dāγēš* being used to underscore certain other linguistic phenomena (in this case, I argue, consonantal syllabicity, which does, after all, possess a kind of associative similarity with gemination).

[29] See Joüon and Muraoka, *Grammar of Biblical Hebrew*, 74 (§18*k*) for the common letters with *dāγēš forte dirimens* and the two construct plural examples.

which are obviously continuants) may represent another case of the Masoretes trying as best they could to encode a syllabic consonant.[30]

More Difficult Cases—And More on the "Skin'em Levi" Consonants

There is another type of *šĕwā' medium* structure that needs to be explained in a way similar to this if the present analysis is to be adopted. This consists in words in which grammatically predicted preforms have a geminate letter (normally a non-*beghadhkefath* one, and not seldom a resonant) with a vocal *šĕwā'*, but in which the letter itself subsequently becoming degeminated, which leads to the *šĕwā'* quiescing even though a subsequent *beghadhkefath* letter retains is fricative pronunciation. This occurs quite regularly in the *wayyiqṭol* forms of verbs in the *pi'el*: as an example, we can take the word *wayyallaḥ* ("and he shaved himself") from Gen 41:14. Under the standard theory, this word simply represents degemination and *šĕwā'*-silencing based on the preform **wayyĕyallaḥ* with the fricativized *gīmel* (that is, *ɣ*) kept the way it was. In this case too, it could be possible to argue that analogical processes are at work: the analogical pressure from the *wa*-less form *yĕɣallaḥ* would certainly be great indeed. This solution may seem *ad hoc*, but note that a process such as this *must* be posited to explain such cases as the *liχtoβ*/*biχθoβ* distinction delineated above. Note that forms like *'ălammĕðâ* (cohortative 1 sing., "let me teach"), *yĕlammĕðûn* (imperfect 3 plur. masc., "they will teach"), etc., do not show degemination of the middle

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[30] Bauer and Leander (*Historische Grammatik*, 211–212 [§20*j*–*k*]) discuss the intrusive reduced vowels after sibilants, resonants and *q* (on which see also further below) and their often being marked by *dāɣēš forte dirimens* when the ordinary *šĕwā'* sign was used; they argue that the use of the *dāɣēš* is due to the fact that the *šĕwā'* sign "in der tib. Schrift zweideutig geworden war." They also point out that this behavior appears after word-internal closed syllables that do not carry the stress ("am Ende druckloser Silben in Wortinnern").

radical before the fricativized *beghdhkefath* letter (and thus no *šĕwā' medium*) even though they theoretically could—fitting, as no such analogical pressure would have been present in those cases.

In discussing these cases, it may be fruitful to think of which letters undergo this degemination process—or, rather, possess the possibility of doing so (as it does not happen in every case). This group of letters is sometimes grouped together using the mnemotechnic phrase *Skin'em Levi*—which again refers to the unvoiced sibilants (*s*, *š*, *ś* and *ṣ*),[31] *q*, *n*, *m*, *l*, *w* and *y* (the same letters appearing with *dāγēš forte dirimens*, which is certainly not a coincidence). It is highly interesting to note that these letters are all—with the exception of *q*—either sibilants or resonants, in a word: continuants. Almost all of them are, therefore, excellent candidates for syllabicity.[32] Thus, one could well believe that a word such as *mil'û* (for perfect *pi'el* 3pl *millĕ'û*, "they filled") represents underlying /mL'û/.[33] The *i*-vowel here is, after all, the same filler-vowel used in the earlier cases we have looked at, and need not necessarily represent the etymological vowel of the *pi'el* form. A word of this type with a *šĕwā' medium* structure like *hamðabbĕrîm* (for **hammĕðabbĕrîm*, "the speak-

[31] And in rare cases *z*, as in 2 Sam 22:40, which has *wattazrēnî* for **wattazzĕrênî*. The following resonant could well be part of the reason here. A practical presentation of the *Skin'em Levi* rules, with textual examples and references to relevant literature, can be found online in J. Beckman "SQin eM, LeVY" (2011), https://www.hebrewsyntax.org/hebrew_resources/sqin_em_levy.pdf. The mnemonic is also used, e.g., in Jo Ann Hackett, *A Basic Introduction to Biblical Hebrew* (Peabody, MS: Hendrickson, 2010), 89, n. 2 (not mentioning all sibilants, though). For presentations of the phenomenon in the standard grammars, see, e.g., Joüon and Muraoka, *Grammar of Biblical Hebrew*, 75 (§18*m*) and Gesenius, Kautzsch, and Cowley, *Hebrew Grammar*, 74 (§20*m*).

[32] The same would, of course, also apply to *r*, but that letter can obviously not appear in this list, as it may normally never have a *dāγēš* at all in Tiberian Hebrew. The same goes for the guttural fricatives.

[33] Note that this form shows that the phenomenon is more widespread than mentioned, e.g., by Golinets, who says that the degemination happens only in the *wayyiqṭol* forms and participles of the *pi'el* and *pu'al* stems (as well as after the definite article, and some other cases in the Codex Leningradensis)—see Golinets, "*Dageš*," 650–651.

ers"), which occurs in Exod 6:27 and 2 Chr 33:18, would then represent /hMðabbĕrîm/, with the *M* forming a syllabic nucleus. Joüon and Muraoka simply refer to this phenomenon as "semi-gemination" or "weak gemination,"[34] which is a mere *obscurum per obscurius* and hardly an explanation at all. Interestingly, Joshua Blau in a way almost hints at the analysis offered here when he discusses degemination: he argues that the reason for the degemination is "the difficulty of pronouncing a double consonant with the help of only an ultra-short vowel," but then adding that the *beghadhkefath* letters, being stops and therefore having less phonetic duration than continuants, would be even more difficult to pronounce geminated without a full vowel, which (to Blau) makes their non-degemination surprising.[35] Given what I have argued here, the question is rather one of the phonetic duration of continuants making them excellent candidates for syllabicity (which would be rather more difficult for stop versions of the non-emphatic occlusives—remember again that the cases in which *beghadhkefath* letters show *šĕwā' medium*/are syllabic, they perforce always appear in their fricative/continuant forms).

This means that—again—what may have seemed like a possible objection against the "syllabic consonant" interpretation actually turns out to provide support for it. The *q* is, it must be admitted, somewhat strange in this context, but I would suggest that a backed/uvular pronunciation of this consonant helped attract *šĕwā'*-like vocalic elements due to its markedness (something that I have already argued in an earlier article on the pronunciation and history of the Hebrew emphatics).[36]

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[34] Joüon and Muraoka, *Grammar of Biblical Hebrew*, 75 (§18*m*), with reference to the longer exposition on p. 70 (§18*b*), which still only talks (in uncharacteristically vague terms) about "some kind of lengthening of the consonant," while not being true gemination. This "some kind of lengthening" is, I propose, exactly the syllabicity that I am arguing for in this article.

[35] Blau, *Phonology and Morphology*, 80.

[36] See Ola Wikander, "Emphatics, Sibilants and Interdentals in Hebrew and Ugaritic: An Interlocking Model," *UF* 46 (2015), 373–397 (379–380) (discussing

Note specifically words such as *ʾălaqŏṭâ-nnāʾ* ("let me glean," Ruth 2:7), which shows degemination of the *q* in combination with a *ḥāṭēf*, which could suggest an underlying /ʾălQṭâ/. If this is the case, the present problem would also provide indirect support for the idea that *q* was a uvular stop in Tiberian Hebrew as opposed to an ejective velar (ejectivity being the older pronunciation of emphatic letters in Semitic and sometimes suggested as the realization in Classical Hebrew as well). It is quite difficult to imagine a velar ejective stop [kʾ] being used as a syllabic nucleus by virtue of attracting (or rather generating) epenthetic vowels, whereas such a possibility fits rather well with a marked back obstruent [q].[37]

Another type of verbal pattern that is interesting from the present perspective is I-guttural imperfects of the type exemplified by *yaʿamðû* ("they [will] stand") or (with two *beghadhkefath* letters) *yaʿaβðû* ("they [will] serve"). This type of verb would, if it had strictly followed the paradigm of the strong verb, have produced a form ***yaʿmĕðû* (cf. the similarly formed *nifʿal* perfect type *neʿezβâ*, "she was forsaken," also with *šĕwāʾ medium*). As pointed out by Gesenius, there are cases where this substitution of the vowel pattern does not occur, such as in *yaḥbĕlû* ("they [will] take as a pledge").[38]

What are we to think of these patterns? Based on the arguments put forward earlier, an easy possibility presents itself, viz. to view the "revowelled" cases as instances of consonantal syllabic nuclei as well. *Yaʿaβðû* would then, at a phonological level, represent /yaʿBdû/, with the /b/ being pronounced as its fricative allophone *β*, which would make the consonantal pronunciation easier, and then itself fricativizing /d/ into *ð*

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words such as *qoṭoβχā* ["your destruction"/"your sting," Hos 13:14] and *qoroβχem* ["your drawing close," Deut 20:2]).

[37] A uvular (or post-velar) pronunciation is also supported for Tiberian Hebrew in Khan, *A Short Introduction to the Tiberian Masoretic Bible*, 91–92, based on a mediaeval description.

[38] Gesenius, Kautzsch, and Cowley, *Hebrew Grammar*, 166 (§63*g*).

(like in words such as *liðβar*, "to the word of," which would represent *lDbar* with the /d/ realized as syllabic *ð*).

Interestingly, Alvestad and Edzard note that roots beginning with *ḥ* normally do not show the apparent anaptyctic vowel (as we saw in the case of *yaḥbĕlû*), though there are exceptions (they mention, among others, *wayyaḥalmû*, "and they dreamt," from Gen 40:5).[39] After a survey of the evidence, they conclude that these exceptions, where the anaptyctic vowel is written in I-*ḥ* roots as well, tend to appear in cases where the following second radical is high in the sonority hierarchy (almost all cases concern resonants—the only other examples being from the root *ḥṭ'*, "to sin").[40] This would fit extremely well with the analysis that we are really dealing with a consonantal syllabic nucleus, i.e. /wayyaḥLmû/ etc.

Fewer Unnecessary Phonemes?

An added benefit of the analysis offered here is the removal or at least lessening of the need to view the fricativized variants of the *beghadhkefath* letters as eventually being marginally phonemicized due to deletion of conditioning vowels in Tiberian Hebrew.[41] If most cases of *šĕwā' medium* really represent something else (syllabification/vocalization of consonants), this typological anomaly disappears. To be sure, there are cases of "phonemic" fricativized letters in a very few other types of cases as well, such as in the distinction between *lāqaḥat* ("you took," perfect *qal* 2fs) and *lāqaḥaθ* (*l* + infinitive construct *qal*), but it would be rather easy to explain the anomalous form *lāqaḥat* not as a case of phonolo-

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[39] Silje Alvestad and Lutz Edzard, *la-ḥšōḇ, but la-ḥăzōr? Sonority, Optimality, and the Hebrew* פ״ח *Forms*, Abhandlungen für die Kunde des Morgenlandes 66 (Wiesbaden: Harrasowitz, 2009), 92–93.

[40] Alvestad and Edzard, *la-ḥšōḇ, but la-ḥăzōr*, 94–95.

[41] Thus, e.g., Richard C. Steiner, "Ancient Hebrew," in *The Semitic Languages*, ed. R. Hetzron, Routledge Language Descriptions (London/New York, NY: Routledge, 1997), 145–173 (147).

gization of the *t-θ* distinction but rather as a reflection of the simple fact that the underlying phonological form was actually *lāqaḥt* all along. I would suggest that the intrusive (and non-fricativizing) *paθaḥ* here is to be viewed as an analogue of *paθaḥ furtivum*, and not as a "real" vowel (note that both are used to break up hard-to-pronounce final phonological structure that are generally not allowed in Tiberian syllabification), which may well have appeared as an even "later" realization filter than the fricativization.[42] Thus, it appears that, in these cases, the distinction between plosive and fricativized pronunciation of the *beghadhkefath* letters basically *is* allophonic in Tiberian Hebrew. To be sure, it *was* later phonologized (most clearly, of course, in Modern Israeli Hebrew), but there is no certain need to retroject this development onto the Tiberian system itself. We must not presuppose that the Tiberian writing system necessarily reflects the phonologically underlying system in this case. Indeed, Geoffrey Khan also points to cases of vacillating between stop and fricative realization in Tiberian cases of *šĕwā' medium*, such as in the words *rišfê* ("flames of"), which also appears (in Cant 8:6) as *rišpê*.[43] Sporadic cases of non-fricativization such as that one could, I would say, provide isolated examples of the Tiberian Masoretes actually (and erroneously) carrying out the above-mentioned "correction" that they generally did not in cases such as these.

Cases like the vacillating *rišpê*/*rišfê* example militate against an incipient phonemization, as the two words would in practice form a minimal pair with no difference in meaning and the distinction between *p* and *f* carrying no functional load. The vacillation would, however, make ex-

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[42] Note that Khan ("Syllable Structure," 670) views the final consonant of syllables with *paθaḥ furtivum* as extrasyllabic and the vowel as a case of surface phonetic epenthesis.

[43] Khan, "How was the Dageš in Biblical Hebrew בְּתִּים Pronounced and Why is it There?," 328; *A Short Introduction to the Tiberian Masoretic Bible*, 94. A similar strange interplay between fricativized and non-fricativized forms can be found in the words *kaðχōð* and *kaðkōð*, occurring in Isa 54:12 and Ezek 27:16, respectively, and both meaning "pinnacle," in the singular absolute state (see Golinets, "*Dageš*," 652). Again, this type of strange and unmotivated interchange does not suggest phonemicity.

cellent sense if the Tiberians subjectively "heard" a syllabic consonant, their phonetics being unsure if it ought to be treated as a vowel or a consonant (and, thus, whether or not it ought to fricativize the following consonant). Similarly, one could argue that the difference discussed above between *biχθoβ* and *liχtoβ* (and similar cases) represents another concrete example of this type of morphophonological Masoretic "correction" to a stop pronunciation, as the forms with *l-* would then have been interpreted as no longer containing a syllabic consonant with an epenthetic surface *i* to make it pronounceable but as an actual closed syllable with *i* (based on the analogy with *yiχtoβ* discussed earlier), which would then in itself work as a synchronic motivation for the stop pronunciation of the *beghadhkefath* letter.

In a case such as *qirβû*, thus, the underlying phonological structure is *qRbû*, or (if one prefers that way of expressing oneself) *qĕrĕbû*, without any phonologized fricative whatsoever.[44] The *surface phonetic realization* of this syllabic sequence is *qirβû* (or, to be more accurate, a sequence of phones that the Tiberians chose to transcribe in that way), but that is something else altogether. There is really no "*šĕwā' medium*" here, nor is there a separate phoneme *β*. The same applies for a theoretical feminine imperative *riχβî*, "ride!," which would underlyingly represent *rKbî* or *rĕkĕbî*.

If one wants to argue for an incipient phonemization of the *beghadhkefath* spirantization, one must look elsewhere: in the case of the above-mentioned "analogically induced allophones," one could speak of such a process beginning to operate, as the suffixes *–χā*/*–χem*/*-χen* are

[44] The basic fact that even a pronounced *šĕwā'* must be regarded as representing underlying, synchronic zero at the phonological level is pointed out, for example, in Geoffrey Khan, "Shewa: Pre-Modern Hebrew," in *Encyclopedia of Hebrew Language and Linguistics*, vol. 3, ed. G. Khan, Sh. Bolozky et al. (Leiden: Brill, 2013), 543–554 (554). Even though it may occur in places that had an actual, phonological vowel in pre-Tiberian Hebrew, those vowels themselves are not the predecessors of the *šĕwā's* that actually are pronounced, which must be regarded as *svarabhakti* vowels.

actually starting to acquire a phonological shape with "canonical" *χ*[45] (and possibly also the incipient "almost minimal pairs" in infinitives after prepositions; these could, however, alternatively be explained as a phonologized difference between a syllabic consonant and an actual, phonemic *i*, as we have just seen). The "*i* plus *šĕwā' medium*" cases are, however, in themselves no good argument for the phonologization of the fricative allophones, and the case with the suffixes was probably still an analogical process when the Tiberians were working.

Surface Realizations and Autosegmental Phonology: Three Levels

The question is, however, how this suggested deeper phonological structure relates to phonetic reality. In at least a few of the proposed cases (though not very many), the syllabic consonants would be stops (this would, however, only happen in the case of *ṭ* and *q*, because of the spirantization of the *beghadhkefath* syllabic stops themselves when appropriate), which is typologically highly uncommon and would create an oddity in the system. In most cases, the syllabic nuclei would be continuants (fricativized *beghadhkefath* letters, sibilants or resonants), which fits very well with an analysis involving syllabicity, but cases with *ṭ* and *q* are more difficult, and the Masoretic spelling actually attested needs a good and succinct explanation. Thus, one would like to formulate a more probable way in which this structure was realized phonetically (or at least graphically) even though the underlying structure was one of syl-

[45] I am not quite convinced by the suggestion (found in John T. McCarthy, "OCP Effects: Gemination and Antigemination," *Linguistic Inquiry* 17 (1986), 207–263 [235]) that words ending in *k* and followed by the *χā*-suffix are to be pronounced without an intervening *šĕwā'* despite the homorganic stops, like the one meaning "she will bless you" in Gen 27:4 being supposedly read *tĕβārexχā*, which would, in essence, create a geminated *and at the same time* fricativized *beghadhkefath* letter, a phonological structure quite alien to the Tiberian sound system.

labic consonants—as well as one that would explain the Masoretic choice to spell it with an *i* or *a* before what is here analyzed as a syllabic consonant.

The present suggestion of consonants forming actual syllabic nuclei in clusters in cases with apparent *šĕwā' medium* could be formulated in another way as to its actual phonetic realization (and thereby, its relationship to actual Tiberian vocalic spelling), basing it on the concept of extrasyllabicity (i.e. underlying consonants that break the normal syllabic structure of the language and thus have to be "taken care of"). Any cluster of three consonants before a vowel violates the syllabic structure usually present in Masoretic Hebrew (the proposed syllabic consonants would be a case of this).

In this type of structure (the ones that show traditional *šĕwā' medium*),[46] the graphic surface realization of the proposed /CCC/ sequence (where C stands for consonant) could be argued to obey the following rule:

> In a pre-vocalic cluster of three consonants, assign syllabicity to the middle one. Epenthize anaptyctic vowel before syllabic consonant. Every syllabic consonant is fricativized if possible, as is the consonant following it.

This, it must be emphasized, is simply a mechanical statement of the rule giving rise to the attested Tiberian spelling of these structures—however, and this is important: it is a statement that begins with the underlying assumption of a /CCC/ structure, as opposed to one operating with fricativization followed by subsequent vowel loss. Thus, this rule generates phonetic surface realizations identical with Tiberian spelling while still operating on the basis of there having been underlying syllabic consonants.

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[46] I.e., ones that do not solve the extrasyllabicity in the usual and uncomplicated way, by inserting a normal vocal *šĕwā'* after the final consonant and creating a separation of syllables that way (such as the totally ordinary *tirkĕβû*, which needs no further explaining). It would be quite possible to argue that this completely normal syllabic structure represents an underlying syllabic consonant as well (i.e. *tirKbû*).

What this would mean, in essence, is that we need to postulate not only the underlying phonological level /CCC/ and the "overt" surface level /CiCC/ or /CaCC/, but also a kind of "middle ground" level, which is the level of syllabification. It is at this middle level that the syllabic consonants exist: the occurrence of syllabic consonants is entirely predictable due to the phonotactic context (which is why this is not the deepest level of underlying phonology), but neither is it the surface phonetic realization, which the Tiberian Masoretes reflected in their vowel pointing in these cases. Thus, this middle, "syllabification" level would, in a way, be analogous to suprasegmental concepts such as tonal contours, vowel harmony, the Danish *stød* laryngealization and similar phenomena, the basis of so-called *autosegmental phonology*, a theoretical framework that allows for (and even demands) the separation of phonological structures into different tiers that operate in tandem (with the sound segments themselves representing one tier, for example, and stress or tone another).[47]

If we look at the word written in Tiberian Hebrew as *birχōθ*, we can see the three levels of the present analysis in action thus:

Deep, phonological level:	*brkōt*
Middle, syllabified level:	*bR-kōt*
Final, purely phonetic level:[48]	*birχōθ*

To illustrate the processes even more clearly, we can look at the abovementioned feminine singular imperative *riχβî*, which includes two *beghadhkefath* letters in a row:

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[47] The foundational text of this theoretical current is John A. Goldsmith, *Autosegmental Phonology* (doctoral dissertation, Massachusetts Institute of Technology, 1976). An example of an application of the theory to Semitic languages (specifically to the root-and-pattern or transfixing morphology of the family) can be found in John J. McCarthy, "A Prosodic Theory of Nonconcatenative Morphology," *Linguistic Enquiry* 12 (1981), 373–418, which uses the idea of different tiers as an analysis of the root consonants and vowel patterns so common in Semitic morphology.

[48] Including anaptyctic vowel and fricativization based on the middle level form.

Deep, phonological level:	*rkbī*
Middle, syllabified level:	*rK-bī*
Final, purely phonetic level:[49]	*riχβî*

It must have been the middle, syllabified level that the Masoretes subjectively "heard" as the basis for the subsequent fricativization. Thus, we have now arrived at a formulation and analysis that (a) gives a parsimonious interpretation to the *šĕwā' medium* phenomenon without positing unnecessary non-occamistic phonemes, while still giving heed to the actual phonetically filtered spelling of the Tiberian Masoretes. As mentioned in footnote 46, it would be quite easy to posit that even "normal" underlying /CCC/ structures (such as in *tirkĕβú*) actually involve syllabic consonants on the second level (though solving the extrasyllabicity in a different and easier way—by simply putting a normal vocal *šĕwā'* after the second consonant).

Living Fricativization and Words Like *kāθĕβû*

One problem with the scenario espoused in this article—and one also involving extrasyllabicity—could be found in words such as *kāθĕβû* ("they have written"), *yārĕðâ* ("she has gone down"), *yēlĕχû* ("they [will] go") and similar words in which a long vowel (often written with *meθey* on the first vowel, indicating a secondary stress) is followed by a *šĕwā'* in the next syllable, a *šĕwā'* that historically represents an elided vowel. In Sephardi/Mizraḥi-based "Hebrew school grammar," these words are pronounced with a vocal *šĕwā*, which fits well with the fricativized letter following it: *kā-θĕ-βû*: indeed, this is often what the *meθey* is taking as indicating.[50] This creates no problem at all for the present purposes.

However, there is contemporary data that suggests that in the actual Tiberian reading tradition, these *šĕwā's* were in many cases silent: *kāθ-*

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[49] Including anaptyctic vowel and fricativization based on the middle level form.

[50] Thus, e.g. Blau, *Phonology and Morphology*, 116 (though noting the problem caused by the irregularity of *meθey* marking).

βû.[51] This could be taken to imply that the loss of the old vowel in the middle of Proto-Northwest Semitic **katabū* and **yaradat* post-dated the fricativization, which could allow for the above-mentioned interpretation that the fricativizing vowel in *šĕwā' medium* cases was "once there" but disappeared after influencing the following consonant. These cases could be explained through analogical influence ("allophonic analogy") from the morphologically underlying pausal forms *kāθāβû* and *yārāðâ*—or from the whole paradigm, which shows a fricative throughout. This explanation would, however, be somewhat *ad hoc*, as a such analogies could be adduced to explain unexpected fricatives in other verbal forms as well (including the ones argued above to contain syllabic consonants). More on point, in over-long syllables as these (like *kāθ-*), the consonant preceding the *šĕwā'* may actually have functioned "semi-vocalically" in a sense similar to what I suggested for the other cases with *šĕwā' medium*. Note that Geoffrey Khan himself views this type of syllable as including an extrasyllabic element,[52] which would mean that it would be a perfect candidate for exhibiting the same type of "syllable-like" behavior (fricativizing the following consonant) as an actual vowel: *kā-θ-βû*, so to speak. These explanations could, of course, be rejected, but it is hard to get around the fact that the fricativization rule is alive and active in the language in a way that the vocalic reductions seem not to be. Again: unless some sort of "vowel-like" element was there, it is hard to see why the forms were not "corrected" to *kāθ-bû* etc. on a large scale (see the above-mentioned *rišpê* for *rišfê*, which shows that such a "correcting" tendency could very well make itself heard as an exception—and note again the *biχθoβ-liχtoβ* difference also discussed earlier). Thus, words like *kāθ-βû* are in a way themselves signs that extrasyllabic consonants could be regarded as syllable nuclei.

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[51] See Khan, "Shewa: Pre-Modern Hebrew," 545. The same type of reading is espoused (without argumentation) in Golinets, "*Dageš*," 652.

[52] Khan, "Syllable structure," 670. At the bottom of the page, he explicitly mentions words like *šāmĕrû* (my orthography) as having an extrasyllabic *m*, i.e. a segmentation *šā-m-rû*. This would imply *kā-θ-βû* for what is "normally" transliterated *kāθĕβû*.

All of this illustrates the difference between underlying phonemic reality and surface realization, and raises interesting questions concerning the analytical ideals both of modern scholars and mediaeval Masoretes, a question to which we now turn.

"The Beauty of Ideas" and Linguistic Realities

In a classic sketch from the tv program *A Bit of Fry and Laurie*, the two titular comedians (Stephen Fry and Hugh Laurie) artfully parody hyper-aestheticizing academics or cultural figures by discussing—in a faux-cultural snobbish diction—"*The Beauty of Ideas and the Idea of Beauty*." Having formulated an analysis such as the above—which is conceptually simple yet requiring a number of specific explanations of seeming exceptions—one cannot help thinking of these more abstract and aesthetic considerations and the role the play in the genesis of scholarly thought, and of the motivations for positing such analytical models in the first place.

A striving for parsimony using a very simple meta-explanation often generates multiple sub-problems, which have to be tackled in what is hopefully a not too *ad hoc* manner. Natural languages—and perhaps even more scribally transmitted languages such as Classical Hebrew—often possess larger amounts of irregularity than one would like. I am reminded in this context of Angela Breitenbach's disquisition[53] (based on Kant) on the idea of "beauty in mathematics" (a common meta-scientific trope) being based not in the aesthetic "beauty" of the mathematical objects themselves in a sort of Platonist way but in the aesthetic pleasure derived from the cognitive processes used to arrive at mathematical demonstrations, i.e., the interaction between the mathematical objects and the human creative activity of logical cognition. The same may be said of the relationship between a linguistic system and the for-

[53] Angela Breitenbach, "Beauty in Proofs: Kant on Aesthetics in Mathematics," *European Journal of Philosophy* 23 (2013), 955–977.

malizing analysis thereof: the creation of a "right-angled," regular and comprehensive analytic model is to a large extent an aesthetic endeavor, and must be allowed to be so—not forgetting the lack of complete overlap sometimes found between such analytical systems and the multifarious reality of language. The difference between stop and fricative pronunciation in Tiberian Hebrew is *basically* allophonic, but there are signs of a process towards phonemization, though still mostly governed by allophonic rules and some analogical processes. This is, perhaps, not as neat as one would like, but it is a testament to the special character of Tiberian Hebrew, which is both a codification of (one interpretation of) the phonology of a dead language and a superimposition of synchronic reading rules onto that system. This means that there are methodological differences between studying it and a natural, spoken language from a phonological perspective. Too often, modern interpretations of ancient scribal practices are based on subjective (and often unstated) presuppositions of what a "good" or "beautiful" system of transcribing a spoken language should look like, as though the scribes of former days were trained in modern phonological analysis. To be sure, they often used quite phonemic spelling systems, but absolute consistency cannot and should not be expected.[54]

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[54] One notable field in which this tendency makes itself heard is the phonological study of cuneiform languages such as Akkadian and Hittite, in which one sometimes comes across an attitude that appears to represent a wish to find phonologically significant information in every spelling variation. We may of course hope and wish that ancient scribes always used systematic rules for each and every spelling and their correspondences with the spoken phonology, but this does not in itself make such a situation real, something that is readily apparent from studying the extremely varied spellings occurring in later languages such as English or Swedish (one example is the English spelling of the word *son* with an *o* instead of historical *u*, due mainly to purely aesthetic reasons relating to how the word was perceived as "looking" on the page). I would like here to refer to the views of Craig Melchert, *Anatolian Historical Phonology*, Leiden Studies in Indo-European 3 (Amsterdam/Atlanta, GA: Rodopi, 1994), 2, who cautions against "seek[ing] a linguistic explanation for any orthographic variation...," while himself "prefer[ring] rather to seek first an orthographic motivation ... and to admit linguistic variation only when absolutely necessary." In light of my arguments in

In a way, the above-mentioned "aestheticizing" process of constructing a subjectively beautiful system may well have been present in the work of the Masoretes themselves. Thus, the "aesthetic bias" (so to speak) in the analysis of the Hebrew phonological system may have occurred on two levels: both a mediaeval and a modern one (and possibly also at the ancient scribal level).[55]

A question also arises related to parsimony and Occam's razor: is it more parsimonious to postulate marginal phonemes and rule breaking *šĕwā's* or to posit that consonants in Masoretic Hebrew could sometimes be thought of as syllabic? The former demands more sound changes, but the latter demands a specific analysis of the underlying phonology that is not directly represented in the orthographic and transcriptional data. Which, then, is the more parsimonious? That is not easy to judge. A point to realize from this is that parsimonious simplicity in one part of an idealized system can often create difficulties or even redundancies in another. Actual linguistic systems aren't always as aesthetically pleasing as we would like.

One could argue that the cases in which the fricative allophones seem to be closer to becoming phonemes (*–χā*/*–χem*/*-χen*, for example) would provide a sort of Occamististic reason for them to be regarded as such in the other cases as well.[56] Indeed, this would be more economical

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the main text, however, I would also like to point out that such an Occamistic view of the actual phonology of a dead language need not always lead to correct results in any given case, as the definition of parsimony can vary depending on what factor one is analyzing, which means that explanatory power always has to be a deciding factor.

[55] Note that Alvestad and Edzard (*la-ḥšōḇ, but la-ḥăzōr*, 74-75) discuss *šĕwā' medium* cases such as *malχê* in terms of analogy with the rest of the paradigm, and explicitly argue (based on a suggestion by Shmuel Bolozky) for the Masoretes in some cases changing vocalizations of I-*ḥ* verbs to fit with their feeling of what the spirantization rules ought to have produced. This argument is not identical to the present one, but it goes in the same direction: one involving the Tiberians confronting numerous cases of tension between their "heard" tradition, the demands of the phonology and morphological rules, and, perhaps, a sort of "aestheticizing" process in action.

[56] A fact pointed out to me by Benjamin Suchard (p.c.).

if Tiberian Hebrew were a natural, spoken language, which it is not. Given the "meta-scribalistic" nature of Tiberian Hebrew, I find it eminently plausible that there are what seem to be internal inconsistences at certain points of the system (note the unique emphatic, unaspirated *p* of *'appaðnô* discussed above!). Even if one goes with the "traditional" view of *šĕwā' medium*, one ends up with a complex and strange situation with a number of new phonemes with very low functional load and an old fricativization rule that still works automatically in some—indeed, most—contexts, but not others, which in itself creates an anti-Occamistic irregularity in the system. Again, we would *like* for the system to be more "coherent," but that is not always the case. The Tiberian Masoretes had a pronunciation system that they had received by tradition, and mostly, they did a wonderful job of trying to encode that. But one should not marvel at the possibility that there are cases in which the process was not absolutely perfect (note, as another example of this, the polyvalent use of *dāγēš* to represent different phonological features).

This is also relevant for the question of how the proposed syllabic consonants were spelled. We have seen different cases in which they seem to be marked both with a *dāγēš forte dirimens* (itself an anomaly) and without a *dāγēš* (the latter occurring much more often). This duality is a sign of the inability of the Masoretic pointing exactly to replicate the underlying phonological system of the language. Even though the Masoretes tried to create an aesthetically pleasing (and in many cases phonemic) system, it did not always portray the underlying language quite as well as we would have liked, and surface phenomena sometimes interacted with the underlying phonology.

This type of observation may also be of relevance to other parts of the exegetical endeavor. Take, for example, the reconstructions of redactional criticism, which in their more extreme forms seem sometimes to be at least partly based in aestheticizing ideas about how the genesis of textual material "ought" to look as opposed to completely empirically based reasoning. Much redaction-critical work is based on extensive postulation of models that may or may not possess explanatory power—

however, it can not seldom be said that the models themselves are based to a large extent in the aesthetic (or sometimes even ideological) ideas of the scholars constructing them, rather than being the direct, deductive results of the evidence.

My point here is not that these aesthetic "prejudices" need necessarily be a bad thing; my point is, rather, that they are sometimes unavoidable, and that we as scholars should always keep that fact in mind.

Tiḏʿāl and Preserved Velar Fricatives

We now move on to a case that concerns not the fricativization of stops, but the preservation of old Semitic fricatives that were subsequently lost in Hebrew, viz. the *ġayin* phoneme, which was present in earlier Hebrew and Aramaic (as it is in Arabic and Ugaritic), but subsequently merged with *ʿayin* (as it apparently did even earlier in Phoenician).
The name *Tiḏʿāl*, which appears in Gen 14:1, has been plausibly explained as a borrowed Hebrew version of the Hittite royal name Tudḫaliya-[57] and is also represented in a Ugaritic version as *tdġl* or

[57] This view can be found in many places; one example is Charles Burney, *Historical Dictionary of the Hittites*, 2nd ed. (Lanham, MD: Rowman & Littlefield, 2018), 54. The identification was argued as early as in Franz M. Th. Böhl, "Die Könige von Genesis 14," *ZAW* 36 (1916), 65–73 (68) – but as an undeveloped suggestion, it is even prior to that, having been suggested in A. H. Sayce, "Was Tidal, King of Nations, a Hittite?," *Expository Times* 19 (1908), 283 (Sayce wrote the Hittite name as Dud-Khaliya and Böhl as *Du-ud-ḫa-li-ia*, the latter of which does indeed render one Hittite spelling of it). The alternative suggestion to equate *Tiḏʿāl* with the obscure ruler Tudḫula mentioned in the so-called Spartoli Tablets (a possibility mentioned, though not endorsed, in Gard Granerød, *Abraham and Mechizedek: Scribal Activity of Second Temple Times in Genesis 14 and Psalm 110*, BZAW 406 [Berlin/New York, NY, 2010], 114) is much less likely; the Hittite royal name would have been quite well known in a Late Bronze Age milieu, and it is thus quite plausible that it would have been passed on through narrative tradition. A recent publication supporting the identification with Tudḫula (which ultimately goes back to T. G. Pinches in 1897) is Gérard Gertoux, *Abraham and Chedorlaomer: Chronological, Historical and Archaeological Evidence* (*sine loco*, 2015), 34 (*et passim*). One may note with some interest that Sayce actually suggests identifying Tudḫaliya with the Tudḫula of the Spartoli tablets in his early comment (or Tud-ghula,

ttġl;[58] it is transcribed in the Greek of the LXX as Θαργαλ. This Greek rendering has been argued to represent an instance of retention (using *gamma*) of the *ġayin* phoneme, which would fit well with the velar/uvular fricatives in the Hittite and Ugaritic forms of the name.[59] However, the onomastic form has more peculiarities, which make it worthy of further consideration.

Regarding the Greek transcription, there have been two trajectories of interpretation. One of them is to follow Joshua Blau (1982), and to regard the *gamma* as signifying the persistence of a learned reading tradition preserving the original velar/uvular fricative *ġayin* of the name (as reflected in the Ugaritic version of the same). As is well known, there are many cases in which the transcriptions of the LXX show an etymologically consistent distinction between *ʿayin* and *ġayin*, whereas the Hebrew consonantal text shows no such difference and the mediaeval reading traditions have merged them in all places.[60] If Blau is right, then, the rendering Θαργαλ shows a persistence of the original phonological shape of this foreign name.[61]

The difference between the Tiberian *dāleθ* and the Greek *rhō* could easily be due to the similarity between *dāleθ* and *rēš* in Hebrew square script (making a confusion either on the part of early copyists or on the part of the Greek translators themselves a plausible explanation of the

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as he writes the latter), and indeed uses that purported identification as a step in arguing the connection between the Hittite name and Genesis 14!

[58] On the Ugaritic forms, see Frauke Gröndahl, *Die Personennamen der Texte aus Ugarit*, Studia Pohl 1 (Roma: Pontificium Institutum Biblicum, 1967), 268–269.

[59] A similar form, *Tergäl*, can be found in the Geʿez version of the Book of Jubilees (13:22); it is without a doubt dependent upon the LXX version of the name.

[60] On the preservation of *ġayin* in earlier Hebrew, see, e.g., Joshua Blau, "On Polyphony in Biblical Hebrew," *Proceedings of the Israel Academy of Sciences and Humanities* 6 (1982), 105–183 and Richard C. Steiner, "On the Dating of Hebrew Sound Changes (**ḫ* > *ḥ* and **ġ* > ʿ) and Greek Translations (2 Esdras and Judith)," *JBL* 124 (2005), 229–267.

[61] One may note that by Josephus' time, the distinction was no longer upheld, as he gives the name in the genitive as Θαδάλου (*A.J.* 1.9.1 §173), showing no trace of a *gamma*.

discrepancy). Note, however, that Richard Steiner has suggested that the *r*-version could actually go back to a change in pronunciation in Hieroglyphic Luwian, which did indeed change many *d*-sounds into *r* (Luwian being a possible avenue of transmission for the Anatolian name).[62] However, one must agree with Joosten that this proposal, ingenious though it is, it too speculative to be convincing.[63] Crucially, it would depend on an (unattested) version of the Hittite name in *one specific Anatolian dialect* having influenced the *Hebrew* reading tradition, a proposition which stretches credibility. Also, the Hieroglyphic Luwian rhotacism occurred in *intervocalic* position, and not between a vowel and a consonant, as is the case here.[64]

Against the interpretation of the Greek *gamma* going back to the original velar/uvular fricative being preserved in Hebrew phonology (as opposed to spelling), Jan Joosten has argued (based on arguments by James Barr)[65] that the shape of the Greek name form militates against such an interpretation. According to Joosten, the graphical confusion

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[62] Steiner, "On the Dating of Hebrew Sound Changes," 247, n. 96.

[63] Jan Joosten, "The Septuagint as a Source of Information on Egyptian Aramaic in the Hellenistic Period," in idem., *Collected Studies on the Septuagint: From Language to Interpretation and Beyond*, FAT 83 (Tübingen: Mohr Siebeck, 2012), 211–225 (218, n. 38).

[64] On this process in general, see, e.g., Annick Payne, *Hieroglyphic Luwian: An Introduction with Original Texts*, 2nd rev. ed., Subsidia et Instrumenta Linguarum Orientis 2 (Wiesbaden: Harrasowitz, 2010), 16 and Ilya Yakubovich "The Luwian Language," *Oxford Handbooks*, Online: http://www.oxfordhandbooks.com/view/10.1093/oxfordhb/9780199935345.001.0001/oxfordhb-9780199935345-e-18 (2015), also available (with pagination and minor differences) at http://web-corpora.net/LuwianCorpus/library/Luw-grammar.pdf, p. 23, interpreting the rhotacizing change as the /d/ turning into a flap; whether Hebrew would transcribe an alveolar flap as an <r> is rather an open question.

[65] Joosten, "The Septuagint as a Source of Information on Egyptian Aramaic," 217–218. The article to which he refers is James Barr, "'Guessing' in the Septuagint," in *Studien zur Septuaginta: Robert Hanhart zu Ehren*, Mitteilungen des Septuaginta-Unternehmens 20; Abhandlungen der Akademie der Wissenchaften in Göttingen, Philologisch-Historische Klasse 3: 190, ed. D. Fraenkel, U. Quast and J.W. Wevers (Göttingen: Vanderhoeck & Ruprecht. 1990), 17–34.

between the Hebrew *dāleθ* and the *rēš* that the LXX translators apparently saw before them (prompting their use of the Greek *rhō*) does not fit the idea of a consistent reading tradition-based distinction between etymological *ʿayin* and *ġayin* (if such a tradition existed, the argument goes, the confusion between "simpler" letters would not be an issue—the reading tradition would have corrected them). Rather, Joosten argues, the many cases in which the LXX translators get the distinction between the letters *ʿayin* and *ġayin* right is due to their own knowledge of contemporary Aramaic, in which the distinction was still alive (as shown by Steiner, to whom Joosten refers). The "correct hits" in the distinction would have due, then, not to any sophisticated and detailed tradition of Hebrew pronunciation but rather to (mostly correct and perhaps unconscious) etymologizing on the parts of the translators. As, obviously, no such Aramaic etymological cognate was available for *Tiðʿāl*, one has to infer that the LXX translators would then just have chosen the *gamma* on a whim (which turned out, by pure chance, to be correct).

However, I would argue that the opposition between these points of view need not be as absolute as it may at first appear. There are two intermediate possibilities here that should not be ignored. The first is the simple fact that an existing (perhaps somewhat shaky) reading tradition could have been "buttressed" by the actual Hebrew manuscript that the translators had before them (for example one substituting a *rēš* for a *dāleθ* in this instance, which is not uncommon).[66] The tradition and the manuscript could have been "mixed together." This would mean that the tradition may have been uncertain as to whether *r* or *d* was to be

[66] Note that it has been suggested (James C. VanderKam, "The Textual Affinities of the Biblical Citations in the Genesis Apocryphon," *JBL* 97 [1978], 45–55 [51]) that the Qumran Genesis Apocryphon reads the name with a *rēš* as well, which would directly attest to such a Semitic-language textual tradition (1QApGen 21:23). However, the reading is unclear, and the normally adopted interpretation is that the letter is, indeed, a *dāleθ* in the Genesis Apocryphon—see Joseph A. Fitzmeyer, *The Genesis Apocryphon of Qumran Cave 1 (1Q20): A Commentary*, 3rd ed., BibOr 18/B (Roma: Editrice Pontificio Istituto Biblico, 2004), 232.

read, and a manuscript made the translators choose. However, one could also imagine that the tradition that the LXX translators represented in this case actually *preferred* an *r*. Why would this be? This question brings us to the second possibility.

This even more intriguing possibility is that the use of *rhō* was at least in part motivated by phonological concerns itself. One such could have been assimilation: a *ġayin*, which was often pronounced as a uvular fricative [ʁ] is, after all, phonetically very close to a uvular trill [ʀ], which could have provoked a shift to an alveolar trill instead of a dental stop. Combined with a dissimilatory influence from the unvoiced dental stop /t/, this could well have turned the phonetically complex [tadʁal] into [tarʁal]—or even something like [taʁʁal] or [taʀʀal], with two sounds perhaps coalescing into one and the -ργ- being used to represent this double trill-like fricative in Greek transcription.[67] If, additionally, there also was a manuscript tradition showing *rēš* instead of *dāleθ* here, the combined pressures of the assimilatory/dissimilatory process delineated above and the graphical data in front of the translator would have worked in tandem to create the form Θαργαλ, which would then be both linguistically innovative and archaic at the same time. It would also show an interesting example of the way in which historical phonology, reading tradition and orthographic processes can intersect to create an actual, attested word form.

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[67] This possible realization as a geminate uvular trill in "LXX Hebrew" is, of course, a different process than that which may have been involved in the suggested uvular pronunciation of *rēš* in Mediaeval Tiberian phonology (as the time depths are completely different). It is, however, interesting to note that Geoffrey Khan has argued that the distinction between what he regards as an apical (and, incidentally, also emphatic) pronunciation of the letter in Tiberian Hebrew and the uvular realization (which he regards as the normal one in that tradition) was due to contact with homorganic (alveolar or dental) stops: *d*, *z*, *ṣ*, *t*, *ṭ*, *s*, *l*, and *n* inducing the apical realization—see Geoffrey Khan, "The Pronunciation of the *reš* in the Tiberian Tradition of Biblical Hebrew," *Hebrew Union College Annual* 66 (1995), 67–80 (75–80). This would, in essence, an inverted case of what I am arguing concerning a uvular *rēš-ġayin* combination in the LXX rendering Θαργαλ, suggesting that this type of assimilatory effects on rhotic consonants are not unheard of in the history of Hebrew.

If this analysis holds true, it would mean that the ργ combination is due neither to misreading nor haphazard guesswork, but to rather sophisticated phonetic processes on the parts of the LXX translators. This would heighten the probability for an actual "reading pronunciation" having existed, and would thus lend credence to Blau's arguments. However, the same spelling would also argue for the proposition that those same translators did not always understand the reading tradition that they had received; much like the Tiberian Masoretes appear to have done in the question of the *šĕwā' medium*, they wrote down what they "heard," in the sense of "what they found to be there in the system," even when the spelling system that they were using could not quite record that sound sequence.[68]

In Conclusion

In both of the cases discussed above, the rise and fall of different fricative phonemes in the history of Biblical Hebrew forces us to think of the conundra created by standardized spelling systems being used while the phonological system itself was in flux, and of the difference between underlying phonological data and the surface forms—and the question of what is actually being written down. These factors force us to face the different levels of systemic "beauty," spoken or recited language, and philological/traditionalist codification of texts (and highlight the infeasibility of separating this type of linguistic material from the textual tradi-

[68] Interestingly, both processes discussed in this article have an illustrative parallel in a later instance of Hebrew vocabulary, viz. in an expression borrowed into the Judeo-Arabic dialect of Morocco. Here, the Hebrew phrase *ziχrô liβrāχâ* ("may his memory be a blessing") appears as *sḥo lbrāḥa* (with Arabic *ḥ* for the same sound as *χ*, which has devoiced the adjacent sibilant). Note here how the Hebrew combination *χr* has coalesced into a simple uvular fricative (as I suggest in the case of [таʁʁal]) and how the *šĕwā' medium*-sequence *liβr-* appears in Judeo-Arabic as the vowel-less CCC sequence *lbr-*, the latter perhaps suggesting a preservation of that structure into modern times in that tradition. I would like to thank Jonas Sibony for bringing this Judeo-Arabic phrase to my attention.

tion of its tradents). They also illustrate types of processes that may have been active at earlier points in the transmission of Northwest Semitic traditional literature at well, showing the relevance of this type of analysis to the larger field of long-term transmission of this type of literary tradition, which involves both oral and written textual survival—with a complex interplay between the two. Perhaps too often, historical linguistic study of ancient Near Eastern texts is based on an unstated yet great faith in the accuracy of spelling systems and a systemic view of the orthography in its relation to underlying phonology and surface realization, tending sometimes to attribute any difference in spelling to actual linguistic variation; cases such as these teach us always to remember the possibility of complex interplay between linguistic and sometimes semi-constructed surface realizations, writing systems (however phonemic in their theoretical principles) and the underlying phonemic realities.

Addendum: Regarding *šĕwā' medium*, two more forms should be addressed. One is the long imperative 2ms of the *koθβâ* type (with *o* rendering *qāmeṣ ḥāṭûf*). Such forms show *šĕwā' medium*, yet the vowel preceding it is not the predictable epenthetic *i* or *a*, but one representing the real etymological vowel of the form (**u*, appearing as *qāmeṣ ḥāṭûf* in this context). This seeming exception can be explained as the fricative third radical being analogical to all the other imperative forms (including the 2fs, *kiθβî*, which fits exactly with the idea of a syllabic consonant) or—more specifically and perhaps less likely—as an etymological persistence of the original vowel influencing the choice of synchronic, epenthetic vowel at the surface level though the form has a syllabic consonant because of analogy with *kiθβî* and *kiθβû*. The other case consists of construct infinitives with personal suffixes, such as *koθβî* ("my writing"), etc., to which the same explanations apply (especially the first, purely analogical one, given the synchronic pattern of the infinitive *kĕθōβ*, with fricative third radical). On the 2fs imperatives, it should be noted that I do not necessarily agree with the idea that the internal *i* vowel represents an alternative imperative stem of a type **qiṭil-*, but rather a reduced "short" imperative **q(u)ṭulī*, which also fits with the pausal version, *qĕṭōlî* (cf. the discussion in M. M. Bravmann, "The Forms of the Imperative (and Jussive) in the Semitic Languages," in idem, *Studies in Semitic Philology*, SSLL 6 [Leiden: Brill, 1977], 195–199 [198]). This strengthens the idea that *koθβâ* is somewhat anomalous, keeping the first vowel of the **quṭul-* in its etymological shape, whereas all the other imperative forms reduce it to zero (*šĕwā'* or syllabic consonant), and thereby making its final radical an excellent object of analogical fricativization when applicable.