ON THE HISTORY OF THE BABYLONIAN JEWISH ARAMAIC READING TRADITIONS: THE REFLEXES OF *a AND *ā†

DANIEL BOYARIN, Jewish Theological Seminary of America, New York

To the master, Professor H. L. Ginsberg, on his seventy-fifth birthday

The vowel system of Babylonian Jewish Aramaic (BJA) is only imperfectly known. There are, however, several “reading traditions,”1 preserved orally or in writing, with varying claims to authenticity. Among these are the Babylonian vocalization of Targums Onkelos and Jonathan (TOJ),2 the vocalization of the codex unicus of the Geonic work Halakhot Peseqot (HP),3 and the oral tradition for reading the Talmud preserved by the Yemenite Jews (Y). Of lesser importance are the vocalization of Codex Paris 1402 of Halakhot Gedolot (HG)4 and that of various Geniza fragments of Talmud (F).5 These sources converge with dramatic frequency in the picture they provide of BJA phonology and morphology. However, they also diverge quite significantly in many major details. It follows, therefore, that in order to make use of these sources in reconstructing the phonology and morphology of BJA, some theory of their historical relationships must be assumed. One such theory has already been offered by Shlomo Morag in a series of publications. Perhaps the crucial issue in question is the status and development of pre-BJA *a and *ā implied by the sources.6 This paper will examine afresh the data and analyses presented by Morag and offer an alternative theory.

† I am greatly in the debt of Professors H. L. Ginsberg, Moshe Goshen-Gottstein, Joseph Malone, Michael Sokoloff, and Ms. Malcah Yaeger for their comments on earlier versions of this paper. Professor Richard Steiner spent so many hours discussing the subjects of this paper with me that it is hard for me to sometimes say what is mine, what is his, and what is ours. Jonathan Boyarin did what he could with my English style, and I thank him. Of course, I am solely responsible for any remaining mistakes and infelicities. Part of the research for this paper was done with the aid of a grant from the Memorial Foundation for Jewish Culture.

While this article was in press, I was able to show it to Professor Morag, who has established secure foundations for the reconstruction of Babylonian Jewish Aramaic vocalization through his recognition of the importance of both HP and the Yemenite traditions. He made many valuable suggestions for which I am grateful. I have been able to make some changes in the proofs in response to his suggestions, and there are others I would have made were it possible. Instead, I have inserted some new notes and clarifications, and remarks inspired by his suggestions have been added to my original notes.

1 The term is Shlomo Morag’s (see “Oral Tradition as a Source of Linguistic Information” in J. Puhvel ed., Substance and Structure of Language, (Berkeley, 1968) and “Oral Traditions and Dialects,” Proceedings of the International Conference on Semitic Studies [Jerusalem, 1969]). However, I am using it specifically to refer to traditions of the West Semitic type where much of this phonological morphology is normally unexpressed in writing and must be realized in one of two ways, either by rote memorization of the pronunciation of specific form-tokens and/or types, or by the application of rules. A written vocalization-pointing can be seen then as the expression in writing of “reading tradition.”

2 See the introduction to my edition of Targum Onkelos: A Collection of Fragments in the Library of JTSA, New York, 4 vols. (Jerusalem, 1976) and works cited there.

3 Sefer Halachot Peseqot by Bar Jehovah Gaon (Jerusalem, 1971).

4 Sefer Halachot Gedolot (Jerusalem, 1971).

5 Morag has gathered and discussed many of these fragments. (See his “Towards the Vocalization of the Babylonian Talmud in the Geonic Period” [in Hebrew], World Congress of Jewish Studies, vol. 2 [Jerusalem, 1968], pp. 89–94).

6 In the present paper the following notation will be adopted: / will be used for proto- and pre-phonemes and forms. Otherwise, italicized letters will be used, unless specific reference is being made to the phonological status of a given phone, in which case / / will


1. Morag’s View of the Vocalization Tradition of BJA

1.1 The Yemenite oral tradition serves as the cornerstone of Morag’s theory. His view is summed up by the following statement:

Whereas the authenticity of the Yemenite oral tradition of post-Biblical Hebrew may be tested by comparison with vocalized Mss., the same is not true for the Yemenite tradition of the Aramaic of the Babylonian Talmud. Our opinion of the value of the latter must be based on internal evidence only in the absence of vocalized Mss. with which it could have been collated. Having examined the nature of this tradition as it stands, we may say that in many of its features it would appear to constitute a genuine reflection of a Babylonian Aramaic dialect, as learned by Yemenite scholars, assiduous readers of the Talmud, who went to Babylonia for the purpose.7

Presumably by “the absence of vocalized Mss.,” Morag meant Talmudic manuscripts per se, for it was only shortly later that he proposed a comparison between Y and HP and used it to validate the antiquity and stability of the former.8 However, here a paradox results, for vis-à-vis the *a-*ã question, Morag did not abandon the primacy of the Yemenite tradition. Indeed on this matter, the opinion that Y preserves a more ancient tradition than HP emerges from his discussion. Such a view is, of course, not excluded but a priori does seem rather less plausible than the position defended below, namely that HP is more faithful to the structure of a living dialect of Geonic Aramaic, while Y is an archaizing tradition.

1.2 According to Morag, Y preserves a tradition of a dialect in which very limited merger (Morag’s “neutralization”) of the *a-*ã opposition took place. The environments of merger are: (a) penultimate open syllables preceding open syllables (/.-CV##); and (b) some open syllables preceding shwa, with concomitant syncope of shwa (*ãCoCV > ãCV).

1.2.1 Morag tenders the following explanation of the mergers:

(a) he assumes a length and quality contrast between /ã/ and /a/. On the basis of comparative evidence, stress is posited for penultimate open syllables, where the final syllable is also open. Because of the stress on these syllables, the feature of length is neutralized, “/a/ also being realized as a long vowel in this position. The /ã/-/a/ contrast has thus turned out to be based upon one phonemic factor only, the qualitative. It seems, however, that this feature alone was not sufficient to retain the contrast in question, all the more so since both phonemes were realized as low vowels of approximately the same highness”;9

(b) the shift of ãCoCV > ãCV is explained by syncope of the shwa followed by vowel shortening in the now closed syllable.10 This shift is only sporadic in Y.

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10 Ibid., p. 229.
1.2.2. Morag employs analogy to account for other instances in which Y ā replaces historical *ā. Thus in the sound participle, the shift of *qātel > qātel, where the conditions of merger are not met, is explained as owing to the analogy of the III Y verb where they are, viz. *qāte > qāle.\textsuperscript{11} In sum, then, in Y, according to Morag we are dealing with a very limited sound shift partially morphologized.

1.3 In HP the environments in which [a] replaces *ā are wider than in Y. In the environment *āCoCV, the shift occurs virtually without exception, and Morag accounts for the rest by positing further analogical processes. For example, the feminine plural suffix, *ān, is an. Morag somewhat tentatively explains this again as resulting from analogy to the III Y participle in two stages. First, the masculine plural qatan influenced the feminine *qātān > qatyan. Then the feminine plural suffix in all other forms followed suit.\textsuperscript{12}

Morag claims that all other incidences of the two phones in both traditions appear practically identical. The opposition remained phonemic in both traditions.\textsuperscript{13} Since in Morag's view the main environment in which Y has ā versus a in HP is the feminine plural suffix, it follows that HP represents a dialect which is a later stage of development than Y, precisely by virtue of having carried the *āCoCV shift to completion and having carried analogy further as well. The Yemenite realization of gāmes as ā in Bible and Targum would then be simply a survival. This is the same phon(eme) which existed in the dialect of BJA and reading of Bible and Targum, which they received from the Geonim.\textsuperscript{14} This account has obviously great implications for the description of BJA, as it would tend to establish Y as the frozen record of a living dialect more ancient in visage than HP and therefore presumably closer to the language of the Talmud itself. However, as I shall try to show, Morag's theory is not the only way to account for these data.

2. The Rationale for an Alternate View

2.1 The Conditioning of the Neutralization

Morag's explanation for the shift of ā > a in penultimate open syllables is not satisfying for the following reasons:

(a) it assumes a double contrast between /ā/ and /a/; both length and quality are distinctive.\textsuperscript{15} This is itself uneconomical, unless supported by the system as a whole, while in BJA there are no other length contrasts.\textsuperscript{16} However, here, the sole purpose

\textsuperscript{11} Ibid., pp. 223-24.
\textsuperscript{12} Ibid., "Phonology of Babylonian Aramaic," pp. 74-75.
\textsuperscript{13} Ibid., "Vowel System," p. 221.
\textsuperscript{15} Ibid., "Vowel System," p. 228.
\textsuperscript{16} To be sure, such a system is often assumed for Eastern Syriac as well. Birkeland himself characterizes it as "very curious" (see H. Birkeland, "The Syriac Phonematic Vowel Systems," Festschrift til Professor Olaf på hans 89-Årsdag [Oslo, 1947], pp. 13-39, esp. 14). It is not at all clear from his discussion what necessity there is for assuming a length contrast between his /ā/ and /ā/, since he avers that "no doubt a:ā and e:e express qualitative differences as well." Surely the fact that the Syrians call the supposed short member of the second pair long and the long member short hardly supports a length contrast theory. Confusions and variation between the members of the pairs also does not prove that the opposition was quantitative (see further, sec. 3.3.1 below).
of this assumption is to motivate the shift, and there is no other evidence for
length in this vowel at this time;\(^{17}\)

(b) it involves the assumption of a stress placement rule, again solely to motivate
the hypothesized development;\(^{18}\)

(c) it posits that given stress lengthening of a short vowel in a certain position and
its consequent merger with a long vowel, the product of the merger was paradoxically interpreted as belonging to the short vowel phoneme.\(^{19}\)

Moreover, if the durational increment which triggered the vowel shift were caused by
stress, one would expect that other stressed syllables would likewise undergo the shift.
Since there is no evidence for a shift of other stressed \(\ddot{a}\)'s, nor for a distinction between
the stress-type evident here and the stress-type evident for nonshifting \(\ddot{a}\)'s, we will have to look elsewhere for the motivating factor or factors.

2.2 The Use of Analogy

As seen above (secs. 1.2.2 and 1.3), by positing a very limited phonetic shift, Morag is
forced to assume rather complicated analogical processes to explain other \(\ddagger \alpha > \alpha\)
transfers. These analogies do not appear to have a clear rationale, such as regularizing
paradigms or the like, and therefore an explanation which obviates the need for them
would seem a priori preferable.\(^{20}\)

2.3 Unexplained Forms

In the following forms in HP the shift \(\ddagger \ddot{a}\) to \(\dddot{a}\) has to be explained on an ad hoc basis if
Morag's reconstruction be accepted:

\[(a) \ k\ovu\dddot{a} \le\h, \ "\text{according to him}" \ < \ *k\ov\dddot{a} \le\h (\text{pp. 5, 231})\]

\[(b) \ k\w\ovu \le\h, \ "\text{he became}" \ < \ *h\ov\dddot{a} \le\h (\text{pp. 23, 206})\]  


\(^{18}\) With his usual scrupulousness, Morag (in "Vowel System," p. 237, n. 57) duly notes that there is
no support from \(\ddot{Y}\) for his posited stress rule. He does not try to find evidence for it in Western Syriac.
Whether or not this stress rule can be posited for proto-Aramaic is questionable (but cf. Birkeland,
"Syriac Phonemetic Vowel Systems," p. 17), and diffusion seems out of the question because of the lack
of geographical continuity between the areas of BJA and Western Syriac. Incidentally, proto-Eastern
Aramaic will not do, since as I have tried to show in a forthcoming paper ("Is Syriac Eastern Aramaic,"
delivered at the A.O.S., March 1976, Philadelphia), there simply was no such entity.

\(^{19}\) Morag seems to anticipate this difficulty by his complex account of "neutralization" of length
rather than lengthening, which seems to point to a resulting phonological entity belonging neither to a nor
\(\ddot{a}\) but to both, an archiphoneme, \(\ddot{a}\) (see N. S. Trubetzkoy, \textit{Grundzüge der Phonologie}, 4th ed. (Göttingen,
1937), pp. 71–75. This, at least, could possibly explain the choice of the sign for the less marked vowel to
indicate the suspended opposition (see Trubetzkoy, ibid., p. 73, par. b). Since, however, many of these
\(\ddot{a}\)'s alternate morpho-phonemically with \(\ddot{a}\) and not \(\ddot{a}\), I would still expect the \(\dddot{a}\) graph to have been chosen,
e.g., for example, German \textit{Land, Bund}, not Lant, Bunt,
of course. In short, I believe even this interpretation of
Morag's theory raises as many problems as it solves.

\(^{20}\) See, e.g., P. Baldi, "The Latin Imperfect in *ba,"
\textit{Language} 52 (1976): 839–51, esp. 845: "It seems to me methodologically reasonable that we should
invoke analogy only when all structural possibilities have been exhausted .... But it must be conceded
that sound change, while less than perfectly regular,
at least displays consistent tendencies; and we are
always on firmer ground with phonetic explanations
than with analogical ones," See also J. J. Ohala,
"Phonetic Explanation in Phonology," Anthony Bruck
et al., eds., \textit{Papers from the Parassession on Natural
Phonology} (Chicago, 1974), pp. 251–75, esp. 268.

\(^{21}\) For (a) and (b) one could claim that the \(\text{b}/h\)
had been lost at the end rendering the ultimate
syllable open, thus meeting the conditions of the shift. However, HP seems to retain final \(\text{b}/h\), as
attested by such forms \(\text{as t\ov\d\text{d}nah} \ "\text{he teaches it},"
not \(\text{t\ov\d\ddot{a}nah}\). Furthermore, one would have to maintain
that both the stress rule and vowel shift rule were
synchronically active after the loss of \(\text{b}/h\) for this
explanation to work. This solution is, of course, not
impossible, but certainly uneconomical.


(c) ḥṣḥṭar, "after" < ḥṣḥṭar (p. 40)
(d) ṣnṣṣr, "he will join" < ṣnṣṣr (p. 209)
(e) β̄nax, "your sons" < β̄nāx (p. 219)
(f) Ṽlmiqam, "to stand" < Ṽlmiqām (p. 214)

For each of these forms an individual explanation can be found to solve the problem (see notes). These individual explanations represent at least a complication of the theory. Taken together with the two preceding arguments, especially the first, they lead us to search for a different explanation.

3. The Present Hypothesis

3.0 The present theory posits three stages in the development of Babylonian Jewish Aramaic vocalism: an archaic one, more or less represented by the Babylonian pointing of the Targums; a later stage of the spoken language, represented by the pointing of HP; and an archaizing tradition, represented by other Geonic fragments, HG, and perhaps best by Y, arising after the death of the spoken language in Babylonia and in Yemen. This tradition is characterized by systematic (nonrandom) interference between the TOJ system and the HP system.

3.1 On the basis of this hypothesis, a model of the history of *a and *ā may be constructed. At the earliest stage, when the vocalization signs of TOJ were invented, *a and *ā were phonemically distinct, with the opposition transmuted from a qualitative to a quantitative one. At the stage of HP, these two phonemes had unconditionally merged, but this new phoneme /a/ had an allophone [ɔ] (signified by miqṣṣ pumma, the Babylonian ganes, in HP). The reading of Bible and Targum, however, maintained a phonemic

22 Forms with short a in bāṭar are attested as Sura’ variants in the Massora to TO (see Landauer, p. 19). As far as I can tell, however, these are only forms with suffixes in which the shift is predicted by Morag, e.g., *bāṭeroh > bāṭrohi. Perhaps Morag would explain bāṭar as back formation from such suffixed forms.
23 For (d) one could easily assume that the vowel was never long and that /r/ was doubled in the language. This solution however creates problems for another part of Morag’s hypothesis, since he claims phonemic status for the /k/–/q/ opposition in Y on the basis of a contrast between “pārēq, ‘is separated’ . . . (act. part. of the simple stem . . .)– pārēq, ‘expounds’ . . . (act. part. of the gattel stem . . .)” (“Vowel System,” p. 221). If we assume that the latter was pārēq historically, then in any case pārēq is a secondary development influenced by Targumic and/or Biblical Aramaic, and we are back where we started from.
24 Resulting from contraction of *banaix, the vowel is *ā in both the Qre of Biblical Aramaic and in Targum. One could claim, however, that in some dialects an exceptional short vowel resulted, cf. Syriac ‘ax from *aix, “how.”
25 Perhaps to be explained as owing to analogy of the Med. Gem. verbs. Syncretism of these two classes was a prevalent phenomenon in the language.
25a I should like to emphasize that the difference between my view and that of Prof. Morag is one of detail. In the main, my paper is based on and accepts his description and evaluation. However, he believes that HP and Y are parallel subdialects. Although he has never expressed or implied a judgment that Y is more ancient in origin than HP, this view does seem to follow from his explanations. If certain aspects of the linguistic patterning in HP are explained as resulting from further development of diachronic processes occurring in Y, Y is in these respects at least an earlier état de langue. I do not wish to claim that Y is substantially different from HP and therefore unreliable. Quite the contrary, as Prof. Morag has shown, Y is nearly identical with HP. I believe, however, that they are not subdialects but rather the same dialect. I have tried to explain the differences between the two as owing to systematic interference between the Yemenite reading traditions of Aramaic (or their Babylonian ancestors). This interference was originally pointed out by Prof. Morag, and I have really only extended its scope. In my view, it does not nearly approximate the extent of such interference in HG and F, but it is present and significant (see below sec. 3.5). Y is invaluable for reconstructing the grammar of BJA and indispensable in those matters for which HP provides no attestation. My feeling is that in areas where they diverge, HP should be preferred over Y as representing something closer to living speech.
opposition between these phones, at least among the learned elite. In the final stage (Y, HG, F), the HP tradition had been partially reformed after TOJ according to a coherent set of principles, and the opposition between /a/ and /ã/ or /ɔ/ was rephonologized. What evidence is there for such a model?

3.2.0 Two types of evidence may be offered in support of the claim that TOJ represents an archaic form of Babylonian Aramaic vocalization. These are: (1) isoglosses which may be established between details of TOJ vocalization and known Babylonian features, and (2) the pataḥ-segal merger in the Babylonian vocalization system, which almost surely must be explained by the interference of the then current structure of Babylonian Aramaic vocalism.

3.2.1. The Babylonian provenience of the TOJ vocalization is by now accepted in all quarters, although many think the consonantal text to be Palestinian (see the contribution of M. Goshen-Gottstein). It is therefore perhaps somewhat surprising that virtually no attempt has been made to identify various features of that vocalization with known features of Babylonian Aramaic.

In two recent papers, I have pointed out some connecting links between TOJ and BJA. These include:

(a) the forms qatalaθ-qataliθ for the third person feminine singular and first person preterite of the verb, vs. qa/iθlaθ-qa/iθliθ in all other dialects;
(b) the form /-ãx/ for the second person suffix on plural nouns, otherwise only attested in the Qre of Tiberian Biblical Aramaic;
(c) the form /qâtan/ for the masculine plural participle of III Y verbs;
(d) the vocalization of the conjunction /w-/ with /i/ before consonants followed by sheva, e.g., /wiñâ:n/, “and daughters”;
(e) epenthesis of /i/ after /i/, e.g., /yi`iruq/, “he will run away”;?
(f) frequent replacement of CVCCsCV by CVCCiCCV, as in e.g., /maðînha`e/, “Easterners” < /maðînha`e/. This epenthesis is well attested in HP by both the writing of y and the vocalization; cf. e.g., /munqi`yeh/, “to read it” (HP, p. 184);
(g) avoidance of reduced vowels after laryngeals and pharyngeals, e.g., /inòs/, “man” not */əwnòs/ or */ənòs/.

It must be admitted that none of the above prove Babylonian origin for the vocalization tradition, as they could all be secondary impositions of Babylonian forms on a basically Palestinian text, but such a wealth of isoglosses shows, at the very least, a very heavy influence of Babylonian speech on the oldest attested TOJ vocalization.

26 See the introduction to my edition of Tarjum Onkelos, 3–4 and my article “Studies in Babylonian Aramaic,” Léshonenu 35 (1976): 172–77, esp. here 175–76. Lest there be any misunderstanding, I wish to make it clear that I have never claimed that TOJ may be used as direct evidence in the sense that Y and HP are. However, Prof. Morag suggests that the possibility of a Palestinian substratum, as it were, has not been taken seriously enough by me. The point is well taken, and I would say now that TOJ should only be used to corroborate other sources of evidence.

27 The Qre of TBA shows other Eastern features as well; note for instance the shift ãge > ãe in the gentilic. Of course, I am not claiming to have discovered all of these correspondences, merely their systematic importance. See cited papers for reference to previous work.

28 Since (d) and (e) occur in Babylonian Hebrew also, one could see that tradition as their source in TOJ and BJA as well. This has been pointed out to me by Professor Sokoloff. It seems much more likely, however, that these were features of native Babylonian Aramaic speech which affected the Hebrew vocalization tradition rather than the converse.
3.2.2. It is well known that the Babylonian sign *miftah pumma* covers the territory of Tiberian *patah* and at least some of Tiberian *segol.* This by itself does not prove that the Babylonian sign indicates merger of two vowels since in many lexical items Tiberian *segol* is derived from a historical short *a* through various phonetic processes. However, there is at least one class of forms in which Babylonian *miftah pumma* is equivalent to a Tiberian *segol,* not from an earlier short *a,* namely verbs and nouns from III Y roots which earlier had a diphthong */-ay/.* An example of this would be נֹרֶר in Tiberian vocalization < */yir'ay/,* with נֹרֶר in Babylonian.* There js, therefore, little doubt that Babylonian Hebrew represents in this regard a later stage than Tiberian and that there was in Babylonian Hebrew a merger of */e/ and */a/.* The most probable explanation for the loss of a phoneme of Hebrew in Babylonia is the lack of that phoneme in Babylonian Aramaic. It does not seem at all likely that the originators and transmitters of the Babylonian Hebrew tradition would have maintained one Palestinian phonemic distinction *not current in their speech* while losing another. It follows, therefore, that the system of vocalization signs—invented for Hebrew and adopted for Targumic Aramaic—most probably represents a Hebrew whose vocalic system had been adapted to the vocalic system of Babylonian Aramaic at the time of its invention. We may conclude with some degree of confidence, therefore, that the *miqpaš pumma* (= Tiberian *qames-ā*) and *miftah pumma* (= Tiberian *patah-ā*) represented distinct phonemes of Babylonian Aramaic at the time of their invention. Since in Targumic Aramaic the two signs are placed quite where we should expect them on historical grounds, we assume no significant conditioned mergers either. The opposition was no longer quantitative but qualitative as will be shown now.

3.3 The Phonetic Value of *miqpaš pumma*

3.3.0 The question at hand in this part of our discussion is one that has been treated frequently in the literature. Some scholars have maintained that the opposition of *miqpaš pumma* and *miftah pumma* was essentially quantitative.* Others have maintained that the opposition was indeed qualitative, but both were front vowels of different height, *miqpaš pumma,* a low vowel and *miftah pumma,* slightly higher, perhaps */a/* and */æ/.* Morag has maintained, following Klar, that in fact the *miqpaš pumma* was a back vowel and the *miftah pumma* a front vowel, */â/* and */ā/*. In my opinion, all the evidence points to the last view. Accordingly, I shall review here the evidence which has been offered so far and present some more supporting evidence.

3.3.1 Our first task is to examine the evidence for the contrary hypothesis, that *miqpaš pumma* was a front vowel, long or length-indifferent. Kahle presented the following arguments in favor of this view:*33

(a) the pronunciation of the Spanish Jews and others Jews who read *qames* as */a/* must have been derived from Babylonian models;

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(b) the Babylonian *naqdanim* (vocalizers) did not use the *miqpaš pumma* to represent short /u/ as did the Tiberians; and
(c) the Eastern Syrians read the sign paralleling *qames* and *miqpaš pumma* as a front vowel.

Yalon already discredited the first argument very simply. In the first place, it is by no means a necessary assumption that the “Spanish” pronunciation has Babylonian origins. We possess a Palestinian vocalization tradition which merges the reflexes of *a* and *ã*. Moreover, the structure of the Palestinian vocalization, with its merger of *segol* /e/ and *šere* /e/, is much closer to the “Spanish” pronunciation than is the Babylonian. As a matter of fact, the “Spanish” reading cannot possibly be derived from the Babylonian since in the latter *patah* and *segol* are merged and not in the former. We may sum up these relationships by the following statement.

The Tiberian seven vowel system (fig. 1) becomes five vowels in the Palestinian system by merging /a/ and /ã/ on the one hand and /e/ and /ɛ/ on the other (fig. 2). This, of course, fits the Spanish tradition exactly. However, in Babylonia, /a/ and /ɛ/ are merged, yielding a six vowel system quite different from the Spanish pronunciation (fig. 3). Morag has refuted Kahle’s second argument. Whether the *miqpaš pumma* was a

**The Tiberian Vowel System**

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<th>i (1)</th>
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**Fig. 1**

**The Palestinian and Spanish Hebrew Vowel System**

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**Fig. 2**

**The Babylonian Hebrew Vowel System**

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<td>ą (3, 4)</td>
<td>å (5)</td>
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**Fig. 3**

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36 Ibid., p. 102.
back or front vowel, the Babylonians did not use it for a lowered short /u/ because short /u/ was not lowered in their pronunciation. There is, therefore, no evidence whatsoever from this nonuse of the sign.\textsuperscript{37}

As for the third argument that in Mesopotamia, in general, historical long *ā* was a front vowel, there is evidence that at an earlier period in East Syriac as well *ā* had become ā. Thus before /w/, the East Syrians use the sign corresponding to long *ā* in morphemes where etymologically and in West Syriac short *ā* obtains.\textsuperscript{38} This phenomenon is almost certainly to be explained as assimilatory labialization,\textsuperscript{39} and the resulting vowel must have been a back round vowel at the time, proving that the sign represented such a sound then. Furthermore, Greek words with o and w are written with this sign as well.\textsuperscript{40} All this suggests very strongly that at an earlier period this phoneme was a back round vowel ā which later merged with a, precisely the development we are suggesting for Babylonian Aramaic.\textsuperscript{41} After all, it seems much more likely that the pronunciation of Θεόδωρος as teodar us is an inner Syriac development than that they ignored their /o/ vowel and chose a front /a/ to transcribe Greek w. In fine, Kahle’s arguments may be said to contribute nothing to the view that Babylonian miqpaš pumma was a front vowel or central vowel and may even favor the opposite view.

Yalon also claims to have proven “that the pronunciation of the Babylonian qames equals Tiberian patah.”\textsuperscript{42} His major proof seems to be the use of aleph as a vowel letter for qames and a sign derived from aleph as its diacritical vowel sign.\textsuperscript{43} Let us examine this argument. Aleph has a history as a vowel letter for *ā* in Aramaic,\textsuperscript{44} and a mere phonetic shift in the pronunciation of this vowel would have affected only the phonetic value of the graph, much as the English vowel signs changed their phonetic values with the great English vowel shift. With some redundancy, Yalon also argues that the same aleph spelling proves that in Babylonian Aramaic the reflex of *ā* was a low front vowel, and since Qirqisani said that the Babylonians’ reading of Hebrew was similar or close to Babylonian Aramaic, it follows that a similar pronunciation obtained in the former. This argument is weak for two reasons. First, the pronunciation of Babylonian Aramaic is itself the issue, and aleph spellings prove nothing. Secondly, when Qirqisani says “similar” and “close,” it does not by any means imply identical (see sec. 3.3.2 below). Finally, Yalon argues from the interchanges between [ā] and [â] in the Yemenite reading of BJA, an argument already rejected by Morag\textsuperscript{45} and further refuted below.

A stronger argument is that of Kutscher,\textsuperscript{46} who contends that in Yemen one outlying

\textsuperscript{37} Ibid., p. 103.
\textsuperscript{39} Cf. Morag, “Vocalization of Babylonian Talmud,” p. 85, n. 75.
\textsuperscript{40} Nöldeke, Syriac Grammar, p. 34.
\textsuperscript{41} This view seems to be consistent with that of Nöldeke who states, “No one of these systems (i.e., Nestorian or Jacobite) carries out a distinction between long and short vowels ... in neither case is the quantity of the vowel considered, but merely the quality” (Syriac Grammar, p. 9). Even Birkeland, who argues for a length distinction on the grounds that the Nestorians use terms for “short” and “long” (see Syriac Phonematic Vowel Systems, pp. 18–19), reversing, however, their historically correct application, which to me, as it did to Nöldeke (Syriac Grammar, p. 9), suggests strongly the opposite (i.e., that length was totally irrelevant) concludes, “no doubt a: and e: express qualitative differences too. And as this kind of difference is the only one that exists between the rest of the vowels, it must also be the one that is of main relevance as regards a and e.” (Birkeland, Syriac Phonematic Vowel Systems, p. 14; see also Morag, Hebrew Language Tradition, p. 105, n. 2.)
\textsuperscript{42} Yalon, “Qametz,” p. 278.
\textsuperscript{43} Ibid., pp. 256–66.
\textsuperscript{44} See F. M. Cross, Jr. and D. N. Freedman, Early Hebrew Orthography (New Haven, 1952), pp. 33–34.
\textsuperscript{45} Morag, Hebrew Language Tradition, pp. 103–4.
district, Ḥabbān, can be shown to have maintained a purer Babylonian reading of Hebrew. Since that district reads qames as a as against all other Yemenites who have ä, Kutscher argues that the ä must be the innovation under Tiberian influence. While the argument seems sound on dialect geography grounds, it is by any means watertight and is far outweighed by evidence in the other direction.

To sum up the results of this section, it seems that it may be said that only one significant piece of evidence has been offered to support the claim of a low front realization of miqpaš pumma.\(^{46a}\)

3.3.2 In this section I shall endeavor to show that there is strong evidence for a back round realization of miqpaš pumma. The evidence falls into three categories: (1) the testimony of Qirqisani, (2) naive spellings of w (waw) for *ā and qames for o, and (3) use of the miqpaš pumma sign to indicate labialization of a.

(1) Benjamin Klar seems to have been the first to recognize the significance of the passage from Qirqisani in this regard, but his interpretation requires modification in certain details.\(^{47}\) In order to justify my interpretation, fairly extensive quotation is required:

What may be said in this matter (viz. the change in Babylonia from Tiberian pronunciation) is this: that the people, when they had long sojourned in Iraq, their reading became Nabateanized,\(^{48}\) since our eyes see that the reading of the Babylonians is close to the Nabatean language. Similarly we find the reading of the people of every place becoming close to the language in which they have been brought up, e.g., the people of Hijaz and Yemen do not maintain veth, pronouncing in its place beth, and the reason is that they grew up among Arabs and became accustomed to their language, since there is no veth in Arabic . . . And similarly also the Byzantines\(^{49}\) have come to the point where they do not retain qames because it is not in the Byzantine language. . . . And many of the Jews of Iraq who grew up among Nabateans substitute qadeš for qadoš . . . . And there does not remain in this generation even one philologist or grammarian of the men of Iṣfahan, Baṣrah, Tustar or anywhere else, who do not prefer the Palestinian reading, and who do not recognize that it is the true one, and who do not see that the true nature of grammar only becomes clear in accordance with it. Indeed a group of their elders, who do not read Palestinian, and who read Babylonian, and have heard about Palestinian only through rumour, when they wish to speak of matters concerning language and grammar, they speak only of the Palestinian language and none other (italics mine).\(^{50}\)

\(^{46a}\) Prof. Morag informs me that in fact in the Habbān pronunciation qames was a low back vowel (differing from the other Yemenite traditions only in that it was not rounded) and always distinct from patah. It does not therefore truly constitute counter-evidence to Morag’s (and my) view that Babylonian Aramaic had originally a low back vowel for qames, i.e., as the reflex of *ā. As Prof. Morag points out, the unrounding of this vowel would have been a likely step in the direction of the merger for which I have argued in any case.

\(^{47}\) B. Klar, Matters of Massorah and Pronunciation in Qirqisani (Tel Aviv, 1954), pp. 320–28.

\(^{48}\) I.e., closer to Babylonian Aramaic. The use of the term Nabatean for Arabic Aramaic was very common in this period. (See Klar, Matters of Massorah, p. 326, n. 36.)

\(^{49}\) cf. “Romana,” i.e., Jews of Byzantine and perhaps Christian Europe (see ibid., p. 325, n. 31).


Professor Steiner collated my translation of Klar’s Hebrew with the original Arabic and made many important corrections. I checked some points with Professor Moshe Zucker also.
The implications of the first italicized passage seem clear enough. Qirqisani’s main objective is to prove the secondary and corrupt nature of the Babylonian reading. Although Qirqisani quotes mispronunciation of qames in Byzantine as an example of corruption and moreover gives a different example of corruption of Hebrew in Babylonia, he does not adduce mispronunciation of qames in Babylonia. One may surely deduce that in Qirqisani’s opinion the Babylonians had at least not obliterated the correct realization of qames, to wit Tiberian á. So, indeed, deduced Klar.51

The second italicized passage caused the difficulty with Klar’s interpretation. Klar apparently thought that the “preference” mentioned in the preceding sentence meant that they actually adopted the Palestinian reading. He therefore concluded that there was an “academic pronunciation of Hebrew” in Babylonia similar to that of Europe.52 Consequently, he added in parentheses the word “naturally” after the words “who do not read Palestinian,” taking the passage to mean that these sages do read Tiberian, albeit as a sort of affectation. Yalon therefore discounted the significance of the first passage, claiming that Qirqisani absolves the Babylonians of guilt in regard to the qames by virtue of their sages.53 The passage, however, does not require the addition of the word “naturally” because “preference” here does not imply adoption. These philologists, grammarians, and sages did not read Tiberian at all, as Qirqisani says clearly when not parenthetically embelished. The first group paid mere lip service to the superiority of the Tiberian reading, while the latter carried on their grammatical discussions following its rules. None of these groups had an “academic pronunciation,” and therefore if Qirqisani does not take the Babylonians to task for mispronouncing qames, we may safely conclude that according to his knowledge, á existed there as in Tiberias.54

(2) The second type of evidence in this matter consists of vulgar texts from Babylonia in which w (waw) is “miswritten” in place of historical *á and in some of which the qames sign is used for *o. There are three main sources of this type of spelling: a Hebrew incantation found in the Cairo Geniza with some vocalization, a schoolboy’s copy of some Hebrew prayers with vocalization (also from the Geniza), and mixed Aramaic and Hebrew incantations written on excavated bowls. The former two, although vocalized with Tiberian signs, are proven Babylonian by segol-pataḥ inversions.55

In the Hebrew incantation published by Gottheil and Worrell,56 w is used quite extensively for *á and qames for o. There can be little doubt that in the Hebrew pronunciation of the scribe *á and *o had merged, surely arguing for an earlier [á] or the like. Yalon tries to discount this argument claiming that “this text is no support for a Babylonian qames equaling holom; quite the contrary, the abundance of inversions shows the opposite.”57 If I understand Yalon’s cryptic statement correctly, he wishes to say that since for this scribe *á and *o were merged completely, and since there is no other evidence of such a general merger in Babylonia, we are dealing with a special case, a local dialectal or even idiolectal phenomenon. This same problem will be accounted for quite differently below (see sec. 3.4.2). Identical considerations will apply to the fragment

51 Klar, Matters of Massorah, p. 327, n. 37.
52 Ibid., p. 328, n. 44.
53 Yalon, “Qametz,” p. 262.
54 I thank Professor Steiner for this insight.
of Hebrew liturgy published by Sharvit, in which the same inversions certainly prove that for its writer as well gameš and holam were one.

The other type of evidence of this sort, namely the Aramaic-Hebrew incantation bowls is somewhat more difficult to interpret. There are no vocalization signs in these texts and therefore no inverse spellings of gameš for o to prove merger.58 There is, however, extensive use of w where historically *ā obtained. These spellings show that at least some reflexes of *ā had merged with o or were phonetically close to that vowel.59 Yalon has also tried to dismiss the evidence of these texts on the grounds that the phenomenon occurs in only a minority of them.60 Lack of an inverted or naive spelling in a text, however, is never evidence of nonmerger of phonemes. Since there is no counter-evidence, as shown above, there seems to be no reason to posit special dialectal status for these and the above-discussed texts. They are special only in that their writers were clearly from among the less tutored members of Babylonian Jewry.

(3) We turn finally to the use of gameš to indicate labialization of an original a before w.61 Morag already argued from such use of miqpās pumma before w in HP that it must have been then a back round vowel. Morag’s argument may be supported by the Babylonian biblical vocalization where the same is true. Thus *mawgθ and *awen are mawθ and ‘awen in Babylonian Hebrew just as they are in Tiberian.62 We conclude that the Babylonian Massoretes were aware of the rule of labialization and chose to show its effect by using miqpās pumma, which must have marked therefore a labial (i.e., back round) vowel in their reading.63

3.3.3 I believe, therefore, that it is with some confidence that we can establish the value of miftah pumma as a low front vowel and miqpās pumma as a low back round vowel, in accordance with the views of Klar and Morag.64 Since we have argued that this vocaliza-


59 It appears now that under various sociolinguistic conditions even speakers who are classically aware of allophonic differences and phonetic values and, therefore, that spelling can sometimes show synchronic and diachronic phonetic changes. See my article referred to in n. 58 above and references cited in that work.

60 Specifically, Yalon claims that the corpus of texts which he examined (W. H. Rossell, A Handbook of Aramaic Magical Texts [Ringwood Borough, New Jersey, 1953]): “in four of them there are an abundance of examples for our matter... the exceptional minority, four out of thirty-two, prove the rule about the majority” (see Yalon, “Qametz,” p. 276). This statement raises several problems, aside from the fact that it is based on a particularly weak argument from silence. First, the corpus examined by Yalon is not exhaustive by any means, so statistically based arguments are suspect. Secondly, these spellings appear in somewhat more than four of those texts; ten would be a more appropriate estimate (give or take one or two because of problems of reading and/or interpretation). It is just that in these additional texts there are not an “abundance” of examples. There seems therefore little reason to regard these as some sort of exceptional phenomenon.

61 Morag supports this view as well by the names of this vowel in Babylonian, miqpās pumma = closure of the mouth, *ima = tightening, both of which seem to refer to a higher or closer vowel. My feeling is that he is on safer ground interpreting the vowel names on the basis of his identification of the phonetic value of the vowel than the reverse. Compare, for instance, the Syriac “long” for short (see above n. 41). The comparison is not strictly relevant, of course, but, nevertheless, suggests caution in deriving phonetic information from ancient terminology (see also n. 75 below).

62 Z. Ben-Hayyim ("The System of Vowels of R. Sa'adya Geon," Eshonenu 18 [1953]: 89–96, esp. 90–91) and A. Dotan ("Masorah," Encyclopedia Judaica, vol. 16 [Jerusalem, 1972], p. 1,443) also interpret the "Babylonian gameš" as d. However, even such recent writers as Yeivin (see "Babylonian Vocalization," p. 44) and Sharvit (see "Qametz and Holom, and Segol and Patah," p. 554) have regarded the question as a standoff.
tion manifests the vocalic system of BJA at a relatively early period, we conclude that BJA had at an early date a six vowel system (fig. 4). This system did not remain stable however.

The Archaic Babylonian Aramaic Vowel System

<table>
<thead>
<tr>
<th>i (1)</th>
<th>u (6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>e (2)</td>
<td>o (5)</td>
</tr>
<tr>
<td>å (3)</td>
<td>å (4)</td>
</tr>
</tbody>
</table>

Fig. 4

3.4 The Merger of å and å

3.4.1 In the manuscript of HP, miqpaš pumma and miftaḥ pumma are never in a position of contrast (except for one minor exception, which can, moreover, be explained phonetically, see below). In certain positions, miqpaš pumma is uniquely determined, viz. in word final accented position and in penultimate syllables followed only by /ʼ/ and a back vowel. The occurrence of /å/ in these positions seems phonetically conditioned, in the former case by word final lengthening leading to raising and rounding and in the latter case by vowel-vowel assimilation.

In all other phonetic surroundings where miqpaš pumma appears, it is as a variable conditioned variant of miftaḥ pumma. The basic conditioning factor is that the following consonant must be a voiced continuant (or ʌ). As discussed at length in my forthcoming paper “Variable Rules in Philology,” these consonants have a cross-linguistic tendency to lengthen preceding vowels, and phonetically longer vowels often approximate to the outside of the vowel envelope and rise. The precise effect of some of these consonants, e.g., n and r in raising a to o in Babylonian Aramaic and other Aramaic dialects is well known. Morag already discussed this effect with regard to following /w/, where of course assimilatory rounding plays an even stronger role.

What is most interesting with regard to this effect in HP, however, is the fact that secondary conditioning factors can also be isolated, i.e., given the presence of the prime factor (the following consonant), other phonetic features in the word increase or decrease the frequency of å. Thus if the preceding consonant or next following vowel are labial, /m, w, ʌ, u, o, ə/, the numbers of miqpaš pumma are dramatically increased. If the next following vowel is a front vowel, however, incidence of miqpaš pumma is reduced almost to zero.

65 This vowel chart represents to a certain extent an idealization. The /å/ may have been higher; the /å/ may have been further back. In view of the evidence for fronting of the /o/, the relevant contrast may not have been front-back so much as round-spread (see also Morag, “The Tiberian Tradition of Biblical Hebrew: Homogeneous and Heterogeneous Features,” Perqim 2 [1972]: 105–44, esp. 127–28). Compare Modern Azerbaijani Jewish Aramaic where the back vowels vary “freely from back to central to centralized front in N (= the Northern dialects)” (see I. Garbell, The Jewish Neo-Aramaic Dialects of Persian Azerbaijan [The Hague, 1965], p. 26).

66 See my article “Variable Rules in Philology,” sec. 2.4.


68 For a full discussion, examples, and statistical tests of the significance of these effects, see my forthcoming “Variable Rules in Philology,” sec. 2.3. Here suffice it to say that the confidence levels achieved by approved statistical measures of significance were never larger than .01.
The only occurrences of miqpaš pumma in the MS which do not seem to be clearly phonetically conditioned are in the so-called infinitive absolute, e.g., mitlaš yaḥṣānān = sitting we sit (p. 214), as opposed to the normal infinitive used as a verbal complement, for example, īsmīqbal = to immerse (p. 214). However, this should also be explained as synchronically phonetic conditioning. First, from a historical point of view it is clear that the “infinitive absolute” arose as a special form because of phonetic conditioning. To the best of our knowledge, earlier Aramaic had only one form of the infinitive with a short a. The only dialects in which the lengthened *ā is known are the Targum vocalization and HP (and Y). I assume, therefore, that under the special circumstances of syntactic emphasis for which the infinitive absolute was employed, the vowel came to be lengthened. There is no reason not to believe that the same phonetic conditions still obtained at the time of HP. 69

Thus I conclude that it is reasonable to assume that in the dialect of HP miqpaš pumma and mišlak pumma represent allophones of a single phoneme. The phonemes /ā/ and /ā/ of the earlier language had merged yielding a five vowel system (fig. 5). 70

The Later BJA Vowel System

\[
\begin{align*}
\text{i} & \ (1) \\
\text{u} & \ (6) \\
\text{e} & \ (2) \\
\text{o} & \ (5) \\
\text{a} & \ (3, 4)
\end{align*}
\]

Fig. 5

The plausibility of a development of this sort having taken place is much increased by comparative evidence. In the Jewish modern Aramaic dialects of Azerbaijan, described by Garbell, 71 the same five vowel system obtains, having been generated by the same *ā-*ā merger hypothesized for BJA. Unfortunately, I am not in a position to prove that the reflex of *ā had been ā earlier in that language’s history, but this seems at least very likely. Even more striking, however, is the fact that many of the identical allophonic rules occur. Thus the phoneme /a/, whose basic allophone is an “unrounded low front to front central a,” is actualized in word final “as a rounded low back, slightly nasalized [o] and by the female speakers of the U sub-dialect in N [Northern dialects] as a rounded back lower-mid [ɔ].” Furthermore, “[ɔ] is also an allophone of /a/ in the sequence /-ao-/, [baɔx],” to which compare the effect of w in HP. And also “/a/ has a centralized allophone [ə] . . . in S [Southern dialects] preceding /-Ci-/: jariq [jəriq] . . . ,” 72 to which

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69 Lest this be regarded as a deus ex machina, I refer to Goldberg’s statement—made for purely grammatical reasons: “the front-extrapoled infinitival subject [in BJA] is not marked as isolated by any specific particle, but was in all probability made to be heard as such by dint of some characteristic intonation” (see G. Goldberg, “Tautological Infinitive,” Israel Oriental Studies 1 [1971]: 36–85, esp. here p. 44 [italics mine]). It is striking how the synchronic syntactic investigation and diachronic phonetic aspects coincide.

70 The proximate phonetic explanation for this merger is the fact that for a combination of articulatory and perceptual reasons, languages prefer to have more front-back distinctions at higher than at low position [see B. E. F. Lindblom, “Phonetics and the Description of Language,” Proceedings of the Seventh International Congress of Phonetic Sciences (The Hague 1971), pp. 86–87; I thank Ms. Malcah Yaeger for this reference]. Note also that a similar merger has taken place in the Palestinian system, as well as in Eastern Syriac, as argued above (see Morag, “Hebrew Language Tradition,” p. 103).


72 Ibid.
compare the near total absence of *miqpaš pumma* in precisely that environment in HP. Although Garbell claims these dialects are not direct descendants of BJA, they must at very least be descended from closely related ones. These parallels, then, are more than typological and strengthen considerably the present theory.

3.4.1. This hypothesis affords a great simplification of the diachronic picture (see sec. 2 above). First, we need no longer posit an unmotivated and synchronically superfluous length distinction between ⟨a⟩ and ⟨ā⟩. Secondly, there is no paradoxical shortening of vowels under stress. Where I have posited stress lengthening (in word final position and the infinitive absolute), the increased duration has the plausible effect of allophonically backing the vowel (i.e., increasing its peripherality vis-à-vis the vowel envelope), with concomitant rounding and perhaps raising. Thirdly, there are no ad hoc analogies required (see n. 20 above), and finally, it accounts for the unexplained cases of *miṯaḥ pumma* for historical *ā* and *miqpaš pumma* for historical *a*, e.g., *šaṙu*, “they said” < *šaṙu* (p. 236).

3.4.2 Assuming that my hypotheses are correct up to this point, there were two systems of vowels in use in Babylonia in the Geonic period: the archaic six vowel system used for Biblical Hebrew, Targum, and liturgy, and the colloquial five vowel system. A great deal of effort would have been required of liturgical readers and scholars and the like not to merge the /ə/ with /a/ or /o/.

Students and the relatively unlettered would very likely have heard the /ə/ as /o/. This explains the total confusion of /ə/ and /o/ manifested by the two Hebrew texts discussed above (sec. 3.3.2). Were /ə/ retained as a separate phoneme in the speech of the scribes of those documents, the inverted spellings of *miqpaš pumma* for /o/ and w for /ə/ would of course be inconceivable. Since there is no evidence of a general /ə/-/o/ merger in BJA, the only remaining explanation is the one I have proposed—loss of /ə/ in speech and its identification with /o/ in liturgical use by the nonsophisticated. Of course the allophonic /ə/ of BJA colloquial speech in this period, which has been argued for in the previous section, could also have been identified with /o/.

Turning, however, to the magic bowls, we find some problems. It may be fairly stated that most of the cases of *waw* for *ā* are where they would be predicted by my hypothesis, i.e., in words whose form or context proves them Hebrew or Targumic. Thus we find:

a) *waḏun*, “lord”
b) *wliṭem*, “and forever”

3. I cite these forms in transliteration, grapheme for grapheme.


76 It might be noted that this view would tend to support Morag’s explanation of another name for *miqpaš pumma*, namely *mesaf pumma*. Morag explains this name as referring to the “special attention” required for the correct pronunciation of this vowel. If his etymology is correct, it would seem to be best explained if the vowel in question did not occur in the vernacular.

75 It may be remembered that this view would tend to support Morag’s explanation of another name for *miqpaš pumma*, namely *mesaf pumma*. Morag explains this name as referring to the “special attention” required for the correct pronunciation of this vowel. If his etymology is correct, it would seem to be best explained if the vowel in question did not occur in the vernacular.

76 I cite these forms in transliteration, grapheme for grapheme.


78 Ibid.
c) h'wrg, "the earth"79

d) hurwdr, "spirits"80

e) hurwdr, "spirit"81

f) gw'wr, "exorcised"82

g) bywrywlyyn, "in Jerusalem"83

h) bstwvny, "with Satans"84

i) burwch, "blessed"85

j) hw'wm, "the world"86

k) 'wtk, "on you"87

These forms are clearly Hebrew. We find also clear Targumic forms:

l) hwdyn, "this"88

m) gdvrmhyn, "before him"89

n) ywth, "him"90

Of the forms in the last group, Gordon writes, "Ginsberg is inclined to attribute the occurrences of such official Aramaic forms to imitating the Targums," and this without reference to the w spelling but only morphological features.91

Another group of forms, occurring in one bowl, cannot be accounted for in this way however:

o) rwhyn byšwfd, "evil spirits"92

p) "ynwsh (5 x), "man"93

q) dsmwl, "of the left"94

r) dsmwl", "of the left"95

One notices here a tendency to hyper or pseudo-correction in this bowl, as shown by the Hebraizing spelling with š in (p) and the barbarism rwhyn byšwfd of (o). (We should expect either ruhin bišan or ruhe bišada, of course).96 Perhaps this Hebraizing or hyper-correcting tendency could explain the w's here. We are still left with a residue of unexplained w's however.

80 Ibid.
81 Ibid.
82 Ibid.
83 Ibid.
84 Ibid., text 11.
86 Ibid.
87 Isabell, Aramaic Incantation Bowls, p. 152.
88 Gordon, "Incantation Bowls," text g.
89 Ibid.
90 Ibid.
91 Ibid., p. 346, n. 1. See also Rossell's remark: "Note also that ą > ó (as shown by mater lectiones ą) goes with the ד form (i.e., נרמשנ versus הראגנ), where ą remains." (W. Rossell, Aramaic Magical Texts, 22.) Also: "This usage of ד is a literary affectation from Biblical Aramaic or still more likely the Targumim ..." (ibid., p. 37). See also J. Blau, On Pseudo-Corrections in Some Semitic Languages (Jerusalem, 1970), pp. 53-54, citing Rossell approvingly here.
93 Ibid.
94 Ibid.
95 Ibid.
96 See Blau, Pseudo-Corrections, pp. 24-25 for a hypercorrection in Medieval Hebrew. I interpret rwhyn here as the effort of a scribe used only to the Eastern plural ruhe attempting to write "correct" Aramaic and misusing the -in ending in the emphatic state—a classic case of "over-self-denial" in Blau's terminology (ibid., p. 13). For another sort of hyper-correction see below sec. 3.5, esp. the paragraph beginning "The most dramatic . . ."
If I wished to maintain my explanation of all of these spellings as owing to lack of ą in the scribe's speech and his consequent merger of the Hebrew and Targumic phoneme with ą, I would be hard pressed to account for this last group. However, another explanation may be offered for all these spellings (including perhaps those of the previous groups). They all occur in environments where allophonic miqṣaṣ pumma occurs in HP. They would then, nevertheless, support my general hypothesis. Such spellings of conditioned [p] are well known from Geonic writings as well and even from the Talmud and therefore may be plausibly supposed here. A pseudo-correcting explanation may not be completely excluded, however, even for this last group.

All in all, our conclusion of a merger of /ą/ and /ā/ in the BJA of the Geonic period seems quite justified in the light of the documentation of miqṣaṣ pumma and miṣṭać pumma explicitly to regard the final ā as apocopated. A further possibility (not very convincing) would be to regard these forms as Hebraizing. The most likely explanation in my opinion is that ą here is to be read y (a possibility considered by Gordon ad loc. as palaeographically possible) and the forms would then represent the substitution of masculine -ēh for feminine -āh, well documented for BJA in S. Friedman, "Three Studies in Babylonian Aramaic Grammar: C. Ṽ as Feminine Possessive Suffix," Tarbiz 43 (1973): 64–69. The least likely alternative is that in the BJA dialect of the writer the feminine suffix was -āh (as claimed by Rossell). There is simply no evidence for such a form in BJA (see J. Malone, "The Isolation of Schematisierung: A Service of Linguistics to Philology," JAOS 94 [1974]: 396–98).

Finally the form byṣmāḥ, "in your name" (see Gordon, "Aramaic Magic Bowls," text G and pasim), must be dealt with. If this is to be read with w, it is in any case Hebrew (or Hebraizing) as the context shows: bīṣmāḥ 'ami 'odē, "in your name I do." Although, to be sure, it does not always occur in this context, it does so often enough that it may be regarded as always Hebrew. It is not impossible, however, that the correct reading is byṣmik, in which case the form would be bīṣmēz or bīṣmīz, by no means a surprising form in BJA.

Various strategies may be adopted to explain these forms. Gordon transcribes the w with ą as games and calls these "double matres lectionis" (see ibid., p. 126). I deduce from this designation that he considers the original consonantal /h/ as apocopated. That being the case, one could regard this phenomenon as the word final allophonic raising of /ą/ which I have posited. (See also ibid., p. 358, where Gordon seems observably to regard the final h as apocopated.) A further possibility (not very convincing) would be to regard these forms as Hebraizing. The most likely explanation in my opinion is that w here is to be read y (a possibility considered by Gordon ad loc. as palaeographically possible) and the forms would then represent the substitution of masculine -ēh for feminine -āh, well documented for BJA in S. Friedman, "Three Studies in Babylonian Aramaic Grammar: C. Ṽ as Feminine Possessive Suffix," Tarbiz 43 (1973): 64–69. The least likely alternative is that in the BJA dialect of the writer the feminine suffix was -āh (as claimed by Rossell). There is simply no evidence for such a form in BJA (see J. Malone, "The Isolation of Schematisierung: A Service of Linguistics to Philology," JAOS 94 [1974]: 396–98).

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in HP. The conclusion seems well supported in addition by the /ã/-/o/ confusions of certain uneducated scribes in writing Hebrew and “high” Aramaic. According to my hypothesis, the scribe of HP himself identified the allophonic [ã] of his dialect with the phonemic /ã/ of Targum and Hebrew and thereby adopted mîqpaš pumma to spell his allophone. Thus in Babylonia at that time there was a kind of Aramaic diglossia, with a and ą as allophones in colloquial speech and in reading the living literary language but phonemically distributed in Targumic reading. This would have been the situation in the last centuries of living Aramaic speech and literary productivity among Babylonian Jewry.

3.5 Yemenite Re-creation of the Opposition

The Yemenite Jews received their literary and linguistic traditions from Babylonia at just about this time. This readily explains their realization of qames as ą, as well as the closeness of their Talmudic reading to many aspects of Halakhot Pasuqot. However, in their reading of the Talmud, ą and ă are differently distributed in part from the situation in HP. All of the differences consist of incidences of ą where HP has a.104 The opposition is phonemic as shown by Morag’s minimal pair /păreš/ “he leaves,” /păreš/, “he explained.”105 In my opinion the most satisfactory explanation of this phenomenon is to assume interference between the liturgical reading tradition of the Targum and that of the Talmud.

Morag himself allows for a certain degree of interference between Targumic and Talmudic phonology in Y.106 I believe that he underestimates the extent of interference and that precisely in this area of /ã/-/ă/ patterning it can be demonstrated. Examining the environments in which Y consistently shows /ã/ but HP has mîltah pumma, we find that they may be categorized grammatically rather than phonetically. In short, they are all in situations where merger of the two phonemes resulted in the loss of a morphological opposition, otherwise well established in the language. Thus, for instance, the merger of /ã/ and /ă/ in the feminine plural suffix led to the loss of contrast between that morpheme and the first person plural, ha6ran meaning both “they (fem.) return” and “we returned.” The hypothesized restoration of /ã/ in the feminine form (i.e., its reimportation from TOJ) re-created the morphological opposition. It seems to me that the assumption of analogical restoration of iconicity is much more satisfying than that of analogical destruction thereof.

The most dramatic confirmation of this thesis comes, however, when we consider the forms of the participle. In HP, of course, all forms of the participle have only mîltah pumma. In Y all participial forms in which there would be homophony with other verb forms if /ă/ were retained have /ã/ and only in those. Where no loss of grammatical contrast was present, the original /a/ = [ã] resulting from the merger has been retained. Thus the Yemenites restored /ã/ in the feminine participle qâṭlā to distinguish it from the past tense form, which is qatlā in their tradition. It is perhaps superfluous to note that these are a minimal pair, since shva is an allophone of 0 conditioned by the /ã/.

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105 See idem, “Vowel System,” p. 221, as well as others.
for this schematisierung\textsuperscript{107} is the extension of this morphophonological opposition to an area where it demonstrably never existed in the language. The Yemenite tradition shows two variants of the plural participle qatle and qâtsle, differentiated semantically. The first is used when the context requires a past tense and the second in more properly participial contexts.\textsuperscript{108} This distinction—paralleling the distinction in the feminine singular—can have no historical basis. The old past tense form was qatalu, and of course, there are no phonetic conditions that would lead to such a split. Unless we adopt a completely new set of assumptions about the nature of sound change, I believe we are forced to conclude that the Yemenite sages restored the qâtsle participle as well as the qâtslā form for the feminine, following the Targumic model.

In nouns of the form *qatālā, where the /ā/-/ā/ merger had no semantic consequences, the two vowels were assigned more or less arbitrarily; /ā/ was retained in nouns which never had allophonic [ā] owing to their phonetic shape; and /ā/ was lexicalized in some nouns which often had [ā] in the spoken Babylonian language.\textsuperscript{109} Even here a semantic principle seems to have played a part as illustrated by the difference between the noun /šâlāmā/, “peace” and the adverb derived therefrom /biššālāmā/, “well and good.” In masculine singular participles where [ā] was extremely rare because of the front vowel in the second syllable and moreover no grammatical confusion resulted, /ā/ was lexicalized invariably. As far as I know, this criterion explains virtually all, if not all, cases where HP has miftah pumma but Y has ā, and therefore it is with a certain degree of confidence that we may characterize Y as a mixed tradition. It has a foundation of spoken Babylonian Aramaic similar to that of Halakhot Pessuqot, with important archaizing overlays derived from the Targumic tradition.

3.6 Less Important Geonic Traditions

The scope of this paper does not allow full analysis of the ā-a patterning of the less important Geonic texts, i.e., Halakhot Godolot and the Geniza material. I should like, however, to cite Morag’s conclusion:

The vocalization of HP and that of HG are not identical in the full meaning of the word. In the vocalizations of these two sources (and in that of the Geniza fragments) two layers are recognized in certain matters of phonology and morphology: one matching the general pattern of Aramaic and one different from it. In the first layer the influence of literary vocalization traditions which were known to the vocalizer (Biblical and Targumic Aramaic) reveals itself, whereas the second layer represents the reality of the spoken language of Babylonia in the Geonic period. In the vocalization of HP the scope of the second layer is much broader than in HG.\textsuperscript{110}

This (at least) typological parallel lends credence to my interpretation of the Yemenite system. It is not to be excluded even that there is a genetic connection between such late Geonic traditions of BJA and Y.

\textsuperscript{107} For the term and concept, see Malone, “Isolation of Schematisierung.”
\textsuperscript{109} See Morag’s list of forms with ā in Y (“Notes on the Vowel System of Babylonian Aramaic as Preserved in the Yemenite Tradition,” Phonetics 7 [1962]: 217–39, here esp. 223–24), e.g., qardva, qahīla, etc. All the examples have the expected conditioning consonants.
\textsuperscript{110} Idem, “Vocalization of the Babylonian Talmud,” p. 93.
Conclusions

The hypothesis which has been offered here represents a simplification of the history of Babylonian Jewish Aramaic from a linguistic point of view. Instead of conditioned mergers extended by complex analogies, we are now faced with an unconditioned merger of low vowels—a straightforward phonetic development. The linguistic simplification is at the cost of complication of the historical or socio-linguistic explanations. The concepts of “variable rules,” “diglossia,” and “languages in contact” have been introduced. I believe that this cost is in fact a gain for the theory, for all of these complexities are the well-known facts of language use today and in communities very similar to Geonic Babylonia.111

In the words of Nöldeke: “. . . ja so kann es gewesen sein, aber vielleicht war es doch ganz anders.”112 While I believe that the evidence points in the direction of the hypothesis proposed here, there is no evidence which proves the view of Morag wrong. The (at least) typological phonetic parallel from Modern Aramaic (see above) lends considerable plausibility to the reconstruction of the phonetics of the late BJA /a/ as attested in HP. In any case, the strong possibility that there is no linear relationship between a more archaic BJA behind the Yemenite tradition and a later development in Halakhot Päsugot but rather that Y is a later archaizing tradition, should give us pause. For the present, the safest approach would seem to be the conservative one of regarding Halakhot Päsugot as the closest we can come to the vocalism of a living dialect of Babylonian Jewish Aramaic.113

113 My student, Mr. Lee Paskind, is presently preparing a description of the vocalized BJA verb in the Halakhot Päsugot MS.