

The Reading of Sinai 115's Caption: An Open Response to Thomas Schneider

The Word 'Hebrews of' Certainly Can Be Read on Sinai 115

by Douglas Petrovich (12 December 2016)

In a recent posting on Christopher Rollston's website, of which I just now became aware, Dr. Thomas Schneider of the University of British Columbia responded to part of my proposal that the earliest alphabetic inscriptions are Hebrew. He specifically discussed my rendering of the caption of Sinai 115, an otherwise typical ME (Middle Egyptian) stele of Dynasty 12 that recounts an Egyptian-led expedition to Serâbîṭ el-Khâdim for seasonal mining of turquoise. I am uncertain whether Dr. Schneider was present at my ASOR (2016) presentation, but my guess is that he would have rendered his criticisms differently if he had been. I am honored that he chose to interact with my proposal, and I am delighted to respond. His critique is copied below. I will use Schneider's own breakdown of subpoints, with necessary comments in reply.

(1) The correct reading of the second half of the first horizontal line (4 pictographs) and the second (4 pictographs) and third (1 pictograph) lines is "JBr n gbytw mr" or "iBr n gbitw mr" (or any combination of these two options), not "Jpn, son of Jrw" (on two lines), as he suggests. First, as I demonstrated in my ASOR presentation, and discuss in much greater depth in my book, the "rectangular square" (the second pictograph on this part of the caption) simply cannot be a /p/ stool, as Schneider suggests. This can be proven from various uses of /p/ and the B-house pictograph in the other stelae inscribed by Ḥebeded at Serâbîṭ. Evidently Dr. Schneider never considered the need to study the possibility of this contrast for himself. The reading of a *p*-stool here is simply untenable (on this, please consult my book for detailed argumentation). The assertion of the presence of a Hebrew-Semitic B-house here on a ME caption is bolstered significantly by the presence of the Canaanite syllabic *-wi* (the third pictograph in the first word in the caption, which Gardiner brilliantly understood to be part of the word *Itn-wi* "Levantines"), which loudly signals to the reader of Middle Egyptian that this caption is absolutely no typical ME text.

Schneider has followed Gardiner et al. (1952) in failing to read an *r*-mouth (hieroglyph) to the left of the "rectangular square" (called as such here, since the vertical sides extend slightly farther than the horizontal sides, as is typical of many of Ḥebeded's converted *pr*-house pictographs). A careful look at the drawing of Gardiner et al. (1952) reveals the conspicuous "blank spot" above the *n*-wave(-of-water glyph). This "blank spot" is crying out for a glyph to fill the space. In Černý's article, "Semites in Egyptian Mining Expeditions" (1935), this scholar perceptively recognized (and drew) the *r*-mouth above the *n*-wave. I confirmed Černý's reading by studying (under varying degrees of magnification) the black-and-white photo of the Egypt Exploration Society, which simply must be consulted before attempting to read these lines with confidence.

To begin the second line, Schneider evidently takes the bird to be *s3* (G39, the pintail duck). However, by consulting other inscriptions of Ḥebeded at Serâbîṭ where this very bird appears (see my book for a discussion), their contexts reveal that the bird must be taken as a *Gb*-goose (G38, the white-fronted goose), not the pintail duck that produces *s3* "son". Moreover, this word simply makes no logical sense whatsoever in the context of the caption, which is an extremely crucial issue where deciphering an inscription or literary text. Thus, "son" cannot be read here on the caption of Sinai 115. Instead, *Geb* must be read, and the only way to understand it properly *in this context* is to take its appearance as a use of honorific transposition.

For some unknown reason, Schneider has taken Gardiner et al.'s (1952) *t*-bread-loaf, which precedes the *w*-chick, as an *r*-mouth. Gardiner clearly intended the *t*-bread-loaf to be read here, which is confirmed by Černý's drawing (1935) of the same pictograph/hieroglyph. A careful look at the EES photo makes the reading of both/all of these scholars indisputable. A *t* must be read here, not an *r*. Thus Schneider's comment that "Petrovich combines J and p from line one with r from line 2" is incorrect: there is no *r* in line 2, and his wrongly-attributed *p* in line 1 is a *B*-house proto-consonantal letter, not a ME hieroglyph.

Since Schneider stated that the correct reading "is in two lines", he apparently did not notice the *mr*-glyph "beloved" that clearly is written far enough below the bread loaf that it only can be taken as a single glyph in a third line (i.e. consisting of no other glyph[s] than this one). Reading the transcription from Gardiner et al. (i.e. looking at and depending on their hand-drawing), as Dr. Schneider apparently has done, simply is not a sufficient means for attempting to refute the claims of one who has examined Sinai 115 carefully.

(2) I agree with Dr. Schneider that ME /j/ (the reed leaf), which also can be transliterated /i/, is not known to be rendered for Semitic *ayin*. As Hoch (1997: 8) has pointed out, there are two recognized consonantal values for the reed leaf, and one vocalic value: [1] the glottal stop, [2] the value of *y*, and [3] the vowel *i*. He also notes that the vocalic pronunciation *i* is almost exclusively used with foreign names. As Seow (1993: 3) has pointed out, *aleph* is the glottal stop in BH (Biblical Hebrew). Therefore, I see two primary possibilities for the reed leaf before *Br* on the caption of Sinai 115:

[1] The initial reed leaf in the caption of Sinai 115 is consonantal, and thus probably a glottal stop, rather than *y*. With this option, I assume as valid that the original spoken-Hebrew word for "Hebrew", whatever its initial consonant must have been, is synonymous with Akkadian *ḥa-bi-ru* and Egyptian *ḥpir(u)*, which used to be the time-honored view in scholarship. Egyptian-language specialist James Hoch (1994: 63) said that Egyptian *ḥpiru* and Akkadian *ḥa-bi-ru* is "very likely related to the Biblical term/name עִבְרִי 'Hebrew'", while Waterhouse (2001: 31) stated that "it is now agreed upon that indeed there is a valid etymological relationship between the term 'Habiru' and the biblical name 'Hebrew' (*ḥibri*).” The potential validity of this view simply cannot be swept under a rug. However, the range of meaning for *Habiru/Apiru* is a matter that I will not take up here.

In this first option for the reed leaf in Sinai 115, Ḥebeded would have departed from rendering into ME writing the same sound as the initial *ḥ* consonant of the Akkadian noun *ḥa-bi-ru*. However, neither was the Akkadian consonant rendered into ME with phonetic precision. Please allow me to explain. To my knowledge, no scholar disputes that *ḥpir(u)* "Apiru" and *ḥa-bi-ru/Habiru* are synonymous. To my knowledge, no scholar would dispute that the term and the people originally came from Mesopotamia, and that their appearance in Egypt and its appearance in Egyptian literature/inscriptions was a later phenomenon. If this is true, the Akkadian form is the earlier one.

According to Heuhnergard (2011: 2), Caplice (2002), and Marcus (1978: 1), Akkadian *ḥ* is pronounced as *ch* in the Scottish word *loch*. Yet in ME, Hoch (1997: 8) notes that the unvoiced velar *ḥ* (Aa1, unclassified sign [a circle with horizontal lines interspersed within]) is the consonant pronounced as *ch* in Scottish *loch*, not the *ḥ* (the forearm glyph, D36) of *ḥpir(u)*. Akkadian *ḥ* is unvoiced, whereas ME *ḥ* is "produced with a restriction in the pharynx and with voicing" (Hoch 1997:8). Even in proto-NW Semitic, the consonant *ḥ* is listed as an unvoiced velar fricative, whereas *ayin* is traditionally considered a voiced pharyngeal fricative. Plus, proto-NW Semitic *ḥ* merges into *ḥ* in Hebrew (preserved in Ugaritic and Arabic),

and ME *h* (V28, wick thread or twisted flax: a pharyngeal aspirate) is the unvoiced counterpart of *ayin* (Hoch 1997: 8).

The point is that NK (New Kingdom) ME's choice of *ʿ* to represent Akkadian *ḥ* is anything but a phonetic match, yet the connection between the *Habiru* and the *Apiru* has gone unchallenged, despite the linguistic incongruity. Evidently the congruity between the third consonant of each word + the second consonant of each word (if the need for a *b* → *p* shift can be overcome, and this is something addressed in my book), has been enough to prevent scholars from contending that the association of the *Habiru* with the *Apiru* is flawed. In like manner, I am merely claiming the same, namely that the congruity in the second two consonants (i.e. the *br* of *iBr/jBr* in Sinai 115 and the *br* in *ḥa-bi-ru*) is enough to establish the validity of the connection. The phonetic incongruity in the first consonant of *iBr/jBr* with the *ḥ* of *ḥa-bi-ru* is no more objectionable than ME's rendering of the Akkadian unvoiced velar with the voiced pharyngeal. It also must not be forgotten that Sinai 115 dates to 1842 BC, whereas the consistent use of *ʿ* in *ʿpir(u)* dates no earlier than the 15th century BC. What was true in the rendering of foreign words at one time in history cannot be expected to have been exactly the same 400 years earlier. Perhaps the use of *ayin* in ME and later Hebrew arose (i.e. between 1842 and 1500 BC) for some unknown, but intricately-connected, reason.

Kogan (2001: 291) even stated that *ḥapiru* is obviously a non-Akkadian term, while CAD calls it a "foreign word". When discussing the lack of consistent correspondence between Akkadian *ḥ* and Western Semitic consonants, Huehnergard (2003: 112) said this: "When confronted with such a situation, where two co-equal branches of a language family exhibit a large set of cognates in which one of the consonants differs consistently in the two branches, and yet no conditioning factors can be found to account for the difference, the historical linguist is justified in suggesting that the cognates reflect mergers in the two branches of an earlier, now lost, third consonant." Given all of this, it seems that a number of consonants can actualize in Akkadian as *ḥ*, and that the reed leaf on Sinai 115 may preserve some hint of that third, now lost, consonant.

Why do I assume as valid that the original spoken-Hebrew word for "Hebrew" is synonymous with Akkadian *ḥa-bi-ru* and Egyptian *ʿpir(u)*? Among the texts from southern Mesopotamia of about 1850 BC is one that departs from the typical use of the Sumerian logogram SA.GAZ by supplying the Akkadian cuneiform *ḥapiri*. According to biblical history, Abram ventured from southern Mesopotamia in *ca.* 2091 BC (if OT chronology is determined/taken literally). Therefore, if Abram was a historical figure as described in Genesis (with "of the Chaldeans" safely understood to be an anachronism), he undoubtedly would have spoken Akkadian as a resident of southern Mesopotamia, plus his own native Semitic tongue. If this was the case, why would we expect an initial voiced pharyngeal on the Semitic (Hebrew?) term that he used of his own ancestry (Eberite/Heberite → Hebrews, Habiru, Hapiru, Hapiri, Apiru, etc.), and before his offspring had been in Egypt for any considerable length of time? I discuss this matter at greater length in the book, but for now the key point is that the second and third Hebrew consonants on Sinai 115 match perfectly with the *br* of *ḥa-bi-ru/ḥa-pi-ru*, which is no less than the match between *Habiru/Hapiru* & *Apiru*.

[2] The second option is that the initial reed leaf in the caption of Sinai 115 is vocalic, represented by the *i* vowel in ME. The reed leaf sometimes is used with the vocalic pronunciation of *i* (Hoch 1997: 8), almost exclusively in foreign names. In this scenario, Hebeded may have been so impacted by the lack of an exact equivalent for the sound of the original Hebrew pronunciation of the consonant (perhaps lost to us today), just as is true of the initial *ḥ* consonant in Akkadian *ḥa-bi-ru*, that he chose to begin his mostly-ME rendering of "Hebrews of" only with the *i* vowel that seems to have passed down to BH (e.g. Gen 39:14) as the vowel that follows immediately after the initial consonant: *i* in *ʿibrî*.

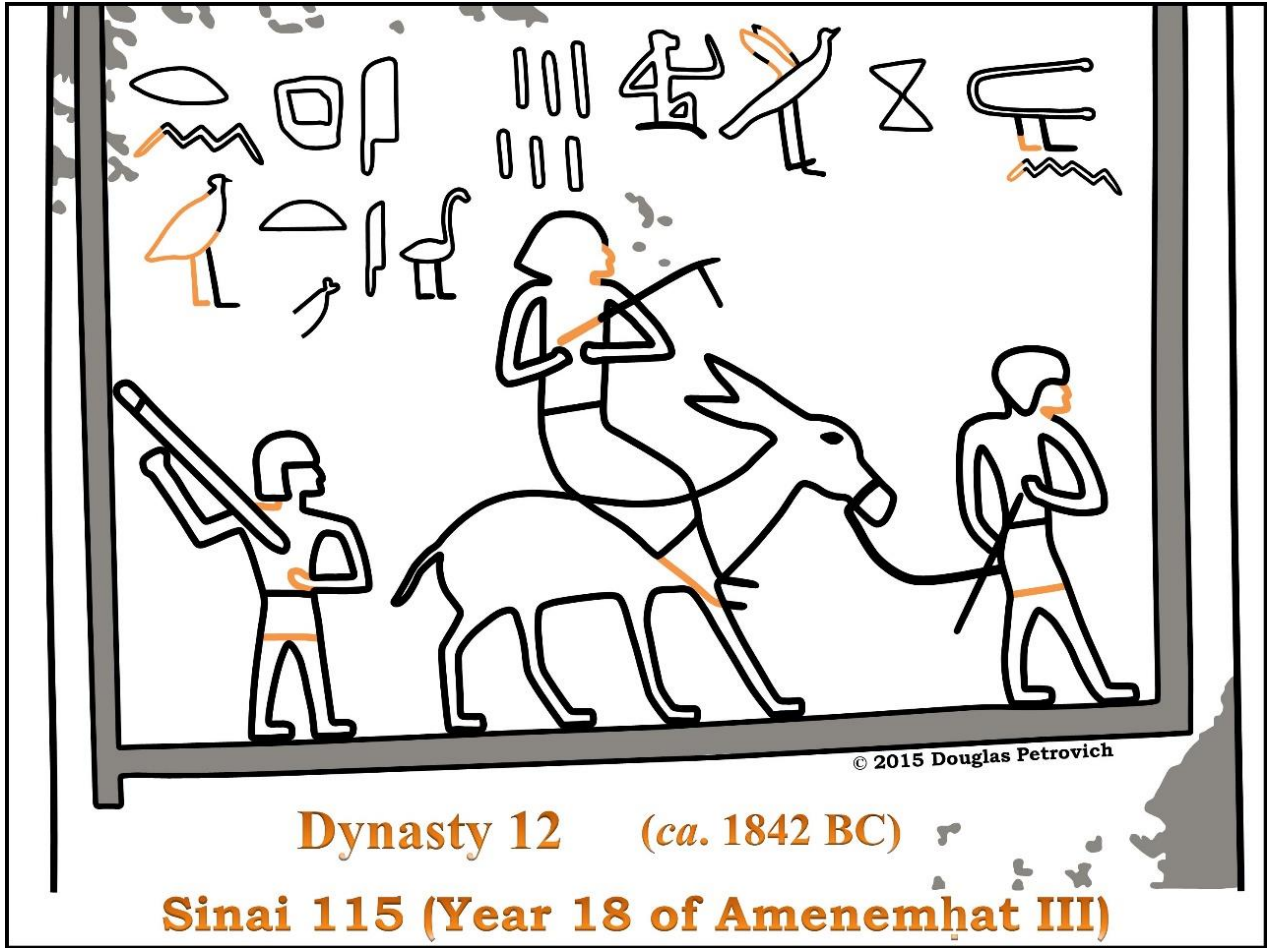
A possible variation of this option is that Ḥebeded chose a pictograph with consonantal and vocalic value, and that it was chosen primarily because its vocalic value is a precise match between its Hebrew and Egyptian pronunciation. If this variation is what Ḥebeded intended, it would follow the proposed “doubled” use of the *b* consonant in *Gb-(b)itu/Gb-(b)aytu* here in Sinai 115’s caption, as well as the many occurrences in PCH (proto-consonantal Hebrew) inscriptions where a pictograph/consonant was written once but spoken twice (Sinai 351 [V2, 21], 353 [V21], 357 [V2, V6], 361 [V2], 377 [V2]), including times when it functions as the last consonant in one word and the first consonant in another (Sinai 345a [H4], 360 [V10], 361 [V16]). Whether or not Ḥebeded would have intended a “double use” variation here, with option two his 1842-BC spelling of “Hebrews of” (mp, in construct form), which is *iBR(ê)*, neither follows (later) biblical Hebrew’s spelling with an initial *ayin* guttural nor (later) Egyptian’s consistently-rendered NK spelling of *ʿpir(u)* with the initial *ayin* (Hoch 1994: 61–63). I am unable to rule out option [1] or [2] for the role of the initial *i/j*.

While some will dismiss my attribution of *iBr/jBr* to Hebrews on Sinai 115 simply because of the presence of a reed leaf glyph in lieu of a forearm glyph (despite the same linguistic incongruity between the initial consonant in *Habiru* and *Apiru*), perhaps the most compelling argument for overcoming the “linguistic problem” with my proposal relates to the ability to connect several pertinent historical figures with biblical figures, which is what brought me to Sinai 115’s caption in the first place. This includes the following: (1) Ḥebeded, who inscribed Sinai 115, (2) Ḥebeded’s son, who is depicted on many of these stelae with him, (3) Ḥebeded’s brother, who can be linked with him thanks both to their titles and to chronological/material cultural evidence, and (4) the exceedingly prominent individual who appointed Ḥebeded’s brother to his lofty position. All of this will be developed in great detail in the companion volume to my current book. Other compelling arguments also exist, such as how the first full blown PCH inscription dates to 1840 BC, only two years after Sinai 115 was inscribed.

(3) I never made a case for taking Egyptian */r/* as a transcription of Semitic */r/*. I made a case for taking the Egyptian *r*-mouth at the (left) end of horizontal line 1 as ME */r/*, its natural function in ME. There are only two non-hieroglyphs on the caption, and the *r*-mouth at the end of line 1 is not one of them. Schneider did not notice this hieroglyph, because he evidently only consulted Gardiner et al.’s drawing, rather than Černý’s article and/or the black-and-white EES photo. Plus, Schneider took the bread-loaf glyph as an *r*-mouth, so he did not even understand the proper context of where I was reading the *r*-mouth on Sinai 115.

(4) I did not take any hieroglyphic */p/* to be a Semitic */p/*, so there was no improper rendering of a *p*-stool as Semitic *B*. The reason I committed no violation is that Sinai 115’s caption contains no *p*-stool hieroglyphs whatsoever. The argument for this was made in (1) above, and to a much greater extent in my book, *The World’s Oldest Alphabet: Hebrew as the Language of the Proto-Consonantal Script* (Jerusalem: Carta). Careful study of this hieroglyph, both on Sinai 115 (pictograph H8 in the electronic drawing in my book) and on other stelae inscribed by Ḥebeded, will make this abundantly clear to any reader of Middle Egyptian.

With all due respect to Dr. Schneider, when he has read so much of Sinai 115’s caption incorrectly, I simply cannot understand how his concluding words, “Ultimately, therefore, Petrovich’s proposed reading of this word as ‘Hebrew’ is not possible”, can be accepted with any confidence or conviction by objective scholars. His reading of Sinai 115’s caption contains far too many impossibilities and inaccuracies. For the sake of clarity, I am adding my electronic drawing of the caption, which is a composite drawing from three photographs of the caption: two in color, and the other being the black-and-white photo of the EES.



The Problem with Reading the Word 'Hebrew' in Sinai 115: An Egyptologist's Response

by Thomas Schneider (23 November 2016)

<http://www.rollstonepigraphy.com/?p=771>

As part of Douglas Petrovich's proposal that the Early Alphabetic Inscriptions (e.g., Serabit el-Khadem, Wadi el-Hol) are Hebrew, he has proposed (as putatively supporting evidence) to read a word in one of the Egyptian inscriptions (namely: Sinai 115) from Serabit el-Khadem as the word for "Hebrew." This, however, is just not correct. Here are some reasons:

(1) the correct reading is in two lines: Jpn, son of Jrw. Petrovich combines J and p from line one with r from line 2.

(2) Egyptian /j/ is used to render Semitic aleph, never an ayin (and one would need an ayin here to have the word "Hebrew").

(3) Similarly, in the 12th dynasty, Egyptian /r/ is never a transcription of Semitic /r/, but of /l/ and /d/.

(4) Also, /p/ is a regular rendering of Semitic /p/ and not /b/, although this is less objectionable.

(5) Ultimately, therefore, Petrovich's proposed reading of this word as "Hebrew" is not possible.

Respectfully,

Dr. Thomas Schneider