

# The Early History of the Alphabet: An Open Response to Christopher Rollston

Hebrew as the Language of the Proto-Consonantal Script  
by Douglas Petrovich (29 November 2016)

In a recent posting, Christopher Rollston responded to my assertion in a presentation at the annual ASOR meeting (17 November 2016) that Hebrew is the proper Semitic language of the proto-consonantal script, which universally is accepted as the world's oldest alphabet. I feel honored that Rollston chose to reply to my thesis, especially since I know of several who have chosen to ignore it entirely, and I am extremely delighted that he kept his disagreements to the issues related to the thesis, since in other forums my scholarly abilities have been attacked rather viciously, simply due to the substance of the thesis. I humbly ask the readers to allow me to interact with Rollston's comments, which is the heart and soul of healthy, scholarly discussion. I will use Rollston's own breakdown of subpoints into Roman numerals, followed by a few comments of my own.

I. Rollston is correct that in my reading of one inscription (Wadi el-Hol Inscription 1, to be specific), I assert the reading of RB “many, abundant” and YN “wine” to be Hebrew words in the context of the inscription. It should be noted, though, that I did not assert that these words can be read only as Hebrew. One of my slides included WN as the Old South Arabic reading for “wine”, YN as a typical Semitic reading for “wine” (including Israelite Hebrew), and YYN as the Judahite Hebrew spelling for “wine” (cf. Petrovich 2015: 139, based on Galil 2013: 17–18). Rollston then stated confidently that “there is no word that can be considered distinctively Hebrew in the Early Alphabetic Inscriptions from Serabit el-Khadem, Wadi el-Hol, or any other Early Alphabetic Inscription”, and that “to say that these inscriptions from Serabit el-Khadem and Wadi el-Hol are somehow distinctively Hebrew is just not going to work.” Here I must disagree with Rollston, and ask why he did not wait to read the book—complete with transliterations and translations of 16 inscriptions—before making this unproven declaration.

As one example, in Sinai 349 the Hebrew word TL(Y) “quiver” (no final Y used, since *matres lectionis* do not occur in epigraphical Hebrew until at least 850 BC) appears, which—according to Lawson Younger's analysis (1997: 298)—has no ANE cognate forms (with this meaning of TLY). As another example in Sinai 349, the Hebrew word 'M(H) “terror, dread” (no final H used) appears. This word occurs only in BH and Aramaic (Van Pelt and Kaiser 1997: 381), the latter being a language that no one (at least to my knowledge) is proposing as a possible language behind the proto-consonantal script. Aramaic may have used the BH word as a later loan word. At this point, one might suggest that Semitic words attested only in Hebrew could have been in use in other Semitic languages, but simply are unattested thus far in extant writings.

While this option is theoretically possible, its potential force is blunted exceedingly by the presence of three BH proper names that appear in the PCH (proto-consonantal Hebrew) inscriptions of the MBA and LBA I. On Sinai 376 from Wadi Nasb, which can be dated to the reign of Sekhemre Khutawy Khabaw (*ca.* 1775–1772 BC) on account of its connection to the outlines of a rectangular panel with a rounded corner that contains his regnal cartouche, the author inscribed the name 'SNT “Asenath”. This is the name of Joseph's wife (Gen 41:45), and as far as I know it is unattested in any other Semitic language. Certainly it was used of only one personage in the HB, and using proper chronological calculations, she probably would have been over 130 years old at the time. Given that the inscription describes the engraving of the house of the

vineyard of Asenath and its innermost room (with three of the same words that are used of the later First [Solomonic] Temple, as found in 1 Kgs 6), the sense of the inscription suggests strongly that her name was used honorifically, and thus posthumously. I am at a loss to understand how the presence of Asenath's name on an inscription of 1772 BC can be construed as anything but Hebrew.

As another onomastic example, the two horizontal lines on Sinai 375a present a typical Egyptian administrative formula beginning with the word "overseer" (F20). Leibovitch (1940) understood the initial two glyphs' identification correctly, but Semiticists untrained in Middle Egyptian have largely ignored his reading ever since. The standard formula goes like this: the word "overseer" + the office held + the overseer's name. In this "bilingual" inscription, the first two parts of the formula are written in perfect Middle Egyptian, while the third part is written in PCH. Here, the last six pictographs of these two lines contain the clear rendering of ḥySMK "Ahisamach". Just as with Asenath, there is only one person in the HB with this name (Exod 31:6), the father of Oholiab, one of the two men appointed to build the tabernacle in the desert, after the Israelite exodus from Egypt. Once again, I am unaware of any other Semitic language that attests to ḥySMK. As only those with proper training in Middle Egyptian will know, the "y" in his name is represented by the dual sign, which produces phonetic long-*i*, in this case taking the place of the 1 cs pronominal suffix *hireq yod* of BH (i.e. "MY brother [who] supports"). In the exceedingly rare times that PCH inscriptions include hieroglyphs, they are NEVER used as determinatives, but always read phonetically. The PCH inscriptions attest to two other uses of the dual sign for the 1 cs pronominal suffix, the earliest one of the three dating to the Middle Kingdom (the Lahun Bilingual Ostrakon; see Petrie 1921: 1–2). Once again, I am at a loss to understand how the presence of Ahisamach's name on an inscription dating to the reign of Thutmose III or Amenhotep II (based on ceramic evidence) can be construed as anything but Hebrew. I will consider this as enough examples and move to Rollston's next point.

II. For Rollston's second point, he states that the initial alphabet contained 27 letters, which is five more than the 22 consonants of Hebrew. It is true that the proto-consonantal (PC) script long has been dubbed as having 27 letters, and was then reduced to 22 by the 13th century BC (Naveh 1982: 42; Rollston 2012: 34–35). The articulation of these alleged 27 PC letters is best presented by Hamilton (2006). Probably this long-held assumption is due to both the earliness and the large number of consonants of the Ugaritic alphabet. However, in Appendix 1 of my book, I spent 25 pages of painstaking analysis to discuss the Middle Egyptian hieroglyphic precursor to every one of the 22 original PCH letters. In every case, a proper Hebrew word perfectly fits each and every pictograph representing that consonant. Additionally, in Appendix 2, I demonstrated how each one of the alleged five additional letters is not actually part of the original PC alphabet. Moreover, I prove this paleographically, not with theory or erudite argumentation. The evidence in these two appendices simply needs to be studied and evaluated empirically, and not dismissed *a priori*. A better scenario is that the Hebrew alphabetic script "began things" with a 22-letter alphabet in the middle of the 19th century BC, then several centuries later Ugaritic expanded that count due to its greater number of consonants, then the 22-letter alphabet resurfaced in inscriptions of the late 2nd millennium BC.

III. In this argument, Rollston referred to my analysis of the caption on Sinai 115, which (caption) will yield long, blank stares from any reader of Middle Egyptian when attempting to read it for the first time. I know, because I was one of them. The first thing that one observes is the presence of two non-Middle-Egyptian pictographs in an otherwise Middle Egyptian caption. In a stroke of sheer genius, Gardiner (1955) must have read the first one as the Canaanite syllabic *-wi* (in the first word of the caption), which derives from the Canaanite noun *wiru* "copper" (Colless 1998: 32, 34), because Gardiner correctly understood the noun

as “Itchenwi”, the Levantine way of referring to Levantines, in contradistinction to the Egyptian way of calling them “Retjenu”. Gardiner knew this from his vast knowledge of Middle Egyptian literature, because in *Sinuhe*, the autobiographer—who spent an extended period of time in the Levant—referred to the inhabitants there as *Itchenwi*, not *Retjenu*. Gardiner stopped translating after “6 Retjenu” (= Levantines). Why would the polyglottal Levantine author include a Canaanite syllabic in an otherwise Middle Egyptian caption? Evidently “out of the ordinary” was par for the course on this potentially last of Hēbeded’s numerous inscribed stelae with similar but slightly different drawings and captions at their bases, because he then added the oldest attested PC pictographic/alphabetic letter (*ca.* 1842 BC) in the middle of the third word. (Note: the first PCH inscription [Sinai 377] appears a mere two years later, at Wadi Nasb, Serabit’s water source.) In my ASOR presentation, and to a greater extent in my book, I articulated why the middle pictograph only can be taken as a PC “B”, and not a Middle Egyptian *p*-stool. Rollston must have mis-remembered my spelling of this word, because I spelled it iBR, not ‘BR. Therefore, the rest of his argument is of no consequence to the case that I have presented in the book or at ASOR. And yes, I am well aware of the absence of the *ayin* in iBR. There is no reason to rehash all of the argumentation from my book here.

IV. Rollston never actually presented a new argument for IV, so there is no response to offer here.

Not one of Rollston’s initial arguments stands up under scrutiny. Yet as a result of his posts, some have told me that they find it difficult or impossible to accept my thesis. This is perplexing to me on a number of levels. For one, neither they nor Rollston has yet to read the book (*The World’s Oldest Alphabet: Hebrew as the Language of the Proto-Consonantal Script*), so how can any of his arguments be persuasive, whether or not I have had the chance to respond? In my early 30s, the Simpson trial dominated the news where I was living in Southern California. Imagine if after hearing the opening arguments of either the defense attorney or the prosecution, the judge simply declared Simpson’s guilt or innocence, with no opportunity for a theoretically impartial jury to hear both sides. That is about how my thesis is being treated by some. I ask, does it not deserve its fair day in court? Should the thousands and thousands of hours I have poured into the research, writing, and editing of this book be dismissed *a priori*, simply because Rollston’s negative pre-conclusions as to its merits were offered before he even read the book? My hope is that, as fellow academics, each of you would read the arguments on your own, and give the evidence its fair day in court before writing off my thesis as impossible, improbable, or unacceptable. I truly hope I would do the same with the work that any of you has/will publish. For my final thought, I will state that I would never put my entire career at risk if I were not 100% certain of the veracity of my thesis. Thank you for hearing my reply.

The Early History of the Alphabet and the Recent Claim that the Northwest Semitic Inscriptions from Serabit el-Khadem and Wadi el-Hol are Hebrew: Spoiler Alert, They're Not.

By Christopher Rollston (23 November 2016)

Recently, it has been claimed by Douglas Petrovich that the Early Alphabetic Inscriptions from Serabit el-Khadem and Wadi el-Hol are Hebrew. His volume on these inscriptions will be published by Carta (Jerusalem) in the coming months. Petrovich also presented a paper on this subject at the annual meeting of the American Schools of Oriental Research on Thursday, November 17th, 2016. I was present for his presentation. Alas, I am confident that his proposal for these inscriptions will not get traction with scholars who work in the field, that is, scholars who work in the fields known as Palaeography and Comparative Semitics. I will write a much longer discussion of his proposal fairly soon (around the time the book comes out). But I wish to put pen to paper at this moment as well, so as to help bring some important empirical evidence to the fore.

But before discussing some of the serious problems with the Petrovich proposal, I should like to emphasize first that the core inscriptions that form the basis of Petrovich's claim are not recent discoveries. (1) In fact, in the case of the Early Alphabetic inscriptions from Serabit el-Khadem (in the Sinai), these have been known for more than a century (with publications on them going back, for example, to 1906 and 1916 by Gardiner). (2) And as for the Early Alphabetic Inscriptions from Wadi el-Hol (also in Egypt), these were published more than a decade ago (Darnell, Dobbs-Allsopp, Lundberg, McCarter, Zuckerman, 2005) in a volume published by the American Schools of Oriental Research for which I served as an external reviewer. Now to some of the salient details about Petrovich's proposal and some of the empirical problems with it (there are a rather large number of problems, but I'll focus on some of the most salient ones).

I. Douglas Petrovich argued (in his presentation at ASOR) that root-words such as *rb* ("great," "big"), *'l* ("God"), *wyn* ("wine") are present in the Early Alphabetic inscriptions and that these are Hebrew words. These words are attested in Hebrew, but that is only part of the story. Namely, these words are actually not just Hebrew but rather they appear in many Semitic languages. For example, this root for "big," "great," occurs not just in Hebrew, but also in Phoenician, Ugaritic, Aramaic, Akkadian, Old South Arabic (among others). Similarly, the word *'l* for God is also Common Semitic and it occurs in Ugaritic, Phoenician, Akkadian, Aramaic, Old South Arabic (along with some others). Or again, the word for "wine" (sometimes spelled with two yods and sometimes with one) occurs not just in the Semitic languages (such as Ugaritic, Phoenician, Arabic), but even in Indo-European languages such as Hittite! (and it comes into Greek and Latin, and ultimately even into the Romance Languages and even English). In short, there is no word that can be considered distinctively Hebrew in the Early Alphabetic Inscriptions from Serabit el-Khadem, Wadi el-Hol, or any other Early Alphabetic Inscription. Rather, for more than a century, the best that we can say is that these Early Alphabetic inscriptions are written in Canaanite, that is, the language of the Ancient Canaanites (who lived in ancient Canaan and often traveled down into Egypt, a tradition also reflected in Genesis). Thus, to say that these inscriptions from Serabit el-Khadem and Wadi el-Hol are somehow distinctively Hebrew is just not going to work. After all, if a word occurs in lots of different languages, that word cannot be considered diagnostic for one language. For that to be the case, that word would have to be present ONLY in one language. And that's just not what we have with these inscriptions.

II. We have hundreds of Hebrew inscriptions and the earliest of these dates to ca. 900 BCE (see my article in *Biblical Archaeology Review* on "The Oldest Hebrew Inscription" for some of the most relevant inscriptions) and the alphabet of these Hebrew inscriptions has twenty-two consonantal letters. The Early

Alphabetic Inscriptions (e.g., from Serabit el-Khadem and Wadi el-Hol), however, have twenty-seven consonantal letters. Thus, to claim that these inscriptions from Serabit el-Khadem and Wadi el-Hol are Hebrew is an argument that is strained well beyond the breaking point.

III. In one of the Egyptian inscriptions from Serabit el-Khadem (i.e., many inscriptions are present at this site, most of them are written in Egyptian, not Northwest Semitic...and as for one of them written in Egyptian...), Petrovich attempts to read the word 'br, a word that is the root for the word "to cross over," and when used as an ethnicon is the basis for the word "Hebrew." The first thing that I would say is that this is a very, very strained reading of the Egyptian inscription. I am confident that few trained Egyptologists will embrace Petrovich's reading of this text as having the root 'br. Furthermore, even if this root were to be present, the fact of the matter is that even that root word (ayin, bet, resh) is attested in lots of different Semitic languages, including Phoenician, Aramaic, Arabic, Old South Arabic, Akkadian, and Ugaritic. And in these cases, the word can and normally does mean "to cross over," "to overstep." In short, one must be careful to understand and remember that if a word occurs in multiple Semitic languages, it cannot be considered diagnostic or reflective of just one of those languages...and that holds true for all words, including the root 'br.

IV. In short, the things that Douglas Petrovich considers to be markers of Hebrew are, in fact, just markers of the Semitic languages in general. We even have a term of these sorts of words that occur in multiple Semitic languages. We call them "Common Semitic," because they are attested in so many languages. In short, the only thing that can reasonably be said about the Early Alphabetic inscriptions from Serabit el-Khadem and Wadi el-Hol (etc.) is that they are written in a Northwest Semitic language and script. There is nothing in these inscriptions that is diagnostic for Hebrew. It would be interesting if there were features that could be considered distinctively Hebrew, but there are not. So, as has been the case for a very long time, we refer to these inscriptions as Canaanite or Early Alphabetic. And from this script called Canaanite or Early Alphabetic, the Phoenician script will later develop during the final decades of the 2nd millennium (and that's an interesting story in and of itself). And from the Phoenician script, the Hebrew and Aramaic scripts will come in the early First Millennium (and that's another interesting story). But that's a long ways down the line in terms of time...long after the scribes of Serabit el-Khadem and Wadi el-Hol penned there inscriptions. Thus, the Early Alphabetic inscriptions from Serabit el-Khadem and Wadi el-Hol are definitely not Hebrew.