

The First Alphabet is Hebrew, Not Canaanite: An Open Response to Christopher Rollston's Rebuttal of 10 December 2016

Hebrew Is the Language of the Proto-Consonantal Script, Not Canaanite
by Dr. Douglas Petrovich (20 December 2016)

In another recent posting, Christopher Rollston responded to my open response to him on 29 Nov 2016 ([https://www.academia.edu/30159613/ 2016 The Early History of the Alphabet An Open Response to Christopher Rollston](https://www.academia.edu/30159613/2016_The_Early_History_of_the_Alphabet_An_Open_Response_to_Christopher_Rollston)), all of which deal with my contention that Hebrew is the proper Semitic language of the proto-consonantal script (*The World's Oldest Alphabet: Hebrew as the Language of the Proto-Consonantal Script* [Jerusalem: Carta, 2016]), which universally is accepted as the world's oldest alphabet. Once again, I am honored that Rollston chose to reply to my response, and I am delighted to reply to him again. While Rollston did not enumerate his points this time, I will enumerate my responses to various points that he has made, which are 18 in number. Rollston's response of 10 Dec 2016 is at bottom.

I. Rollston claimed that my thesis will find few takers among those who actually know the scripts and languages of ancient Semitic inscriptions written in linear alphabets. While I cannot predict accurately whether Rollston is correct or not, I can only hope that those who know the languages of ancient Semitic (and Middle Egyptian!) inscriptions are more open minded than Rollston, and not predisposed to a view on the thesis without ever having cracked open a page of the book. If they are not open minded, then they will be prone to dismiss my thesis out of hand, *a priori*. If this were to happen, it would not be the type of scholarly protocol I aspire to emulate, nor the type that I encourage my students to emulate.

II. Rollston claims that the inscriptions from Serâbîṭ el-Khâdim (Sinai) and Wadi el-Ḥôl (Upper Egypt) can be dated to *ca.* the 18th century BC. While there is indeed one inscription from these sites—to which I add Lahun (Middle Egypt) and Wadi Naṣb (Sinai)—that can be dated to the 18th century BC (Sinai 376 [Wadi Naṣb], dating to *ca.* 1772 BC, and translated and discussed in my book), the earliest inscriptions actually date back to the 19th century BC: (1) Sinai 377 (Year 20 of Amenemḥat III = 1840 BC), (2) Wadi el-Ḥôl 1 (Year 26 of Amenemḥat III = 1834 BC, based on Darnell et al. 2005), (3) Wadi el-Ḥôl 2 (Year 26 of Amenemḥat III = 1834 BC, based on Darnell et al. 2005), and (4) the Lahun Bilingual Ostrakon (Year 29 of Amenemḥat III = 1831 BC, based on diagnostic ceramic evidence, and Petrie 1921). Given that Rollston specializes in the epigraphy of the Iron Age, and not the Bronze Age, his lack of precision in the dating of Semitic inscriptions of the MK/MBA (Middle Kingdom/Middle Bronze Age) is understandable.

III. Rollston claims that the earliest inscriptions written in the distinctive Old Hebrew script can be dated to the 9th century BC, and that the script of these so-called Old Hebrew inscriptions is quite different from the script of the (proto-consonantal) inscriptions from Serâbîṭ el-Khâdim and Wadi el-Ḥôl. His use of the term “Old Hebrew” is not only unnecessary here, but misleading and inaccurate. Rollston's main point certainly is spoken as a specialist of the Iron-Age script, rather than the Bronze-Age script. He is correct that the scripts appear different in various ways, certainly at first glance. However, there are many letters that are *quite* similar between the MK/MBA and the Iron Age: *B* (cf. WeḤ 1 with Ṣartah ostrakon), *G* (Lahun ostrakon with Gibeon jar handles), *Z* (Lahun ostrakon and Lachish dagger with Gibeon jar handles), *H* (Lahun ostrakon with Ṣartah ostrakon and Qeiyafa ostrakon), *Y* (Sinai 376 with Qeiyafa ostrakon), *L* (Sinai 377, WeḤ 2, and Lahun ostrakon with Qeiyafa ostrakon and Ophel pithos inscription), *M* (Sinai 377 and

WeḤ 2 with Šartah ostrakon), [◌] (Sinai 376 and Lachish dagger with Šartah ostrakon), *Q* (Sinai 376 with Šartah ostrakon and Qeiyafa ostrakon), *T* (WeḤ 2 and Sinai 376 with Šartah ostrakon and Qeiyafa ostrakon). All of these examples, along with those in the paragraph below, can be found in my Figure 1, which can be accessed/downloaded here: https://www.academia.edu/30450681/Proto-Consonantal_Hebrew_Alphabet.

In most of these cases, the letters of the MK/MBA are virtually carbon copies of those from the Iron Age, but in every case the similarities—producing the conclusion that they are the same letters—is unmistakable. The similarities are even more numerous and pronounced between inscriptions of LBA I and the Iron Age: [◌] (cf. Sinai 375c with Šartah ostrakon and Qeiyafa ostrakon), *B* (Sinai 364 with Šartah ostrakon), *G* (Lachish prism and Sinai 357 with Gibeon jar handles), *D* (Sinai 353 with Šartah ostrakon and Qeiyafa ostrakon), *H* (Sinai 374 with Šartah ostrakon, which evolves into the form on the Siloam inscription and Gibeon jar handles), *Z* (Sinai 346a, Sinai 349, and Lachish prism with Gibeon jar handles), *Ḥ* (Sinai 353 with Šartah ostrakon and Qeiyafa ostrakon), *Ṭ* (Sinai 357 with Šartah ostrakon and Qeiyafa ostrakon), *Y* (Sinai 345a and Sinai 353 with Qeiyafa ostrakon), *K* (Sinai 364 with Šartah ostrakon and Qeiyafa ostrakon), *L* (Sinai 349 and Sinai 361 with Qeiyafa ostrakon and Ophel pithos inscription; Sinai 353 with Šartah ostrakon), *M* (Sinai 349 and Sinai 375a with Qeiyafa ostrakon and Ophel pithos inscription), *Š* (Sinai 351 with Lachish jar), [◌] (Sinai 353 with Šartah ostrakon), *Q* (Sinai 349, Sinai 351, and Sinai 353 with Šartah ostrakon and Qeiyafa ostrakon), *Š* (Sinai 349 and Sinai 357 with Šartah ostrakon and Qeiyafa ostrakon), *T* (Sinai 349, Sinai 351, and Lachish prism with Šartah ostrakon and Qeiyafa ostrakon).

And this does not even account for the similarities between the PCH (proto-consonantal Hebrew) letters of the MK/MBA and those of LBA I. What those who are not as familiar with the proto-consonantal script need to understand is that the script began as pure pictographs taken from the ME sign list—just as Gardiner suggested 100 years ago—and evolved over time into a less and less pictographic alphabet. This predictable phenomenon accounts for the script's movement from complexity to simplicity, ease of writing, and the visually abstract. In my book, I refer to RPV (= residual pictographic value), which lessens naturally over the centuries. Nonetheless, there are—to varying degrees—clear and definable remnants of pictographic value from the original Hebrew letters in *every* one of the Hebrew inscriptions from the Iron Age. What must be understood at the beginning is the exact Hebrew word behind each letter, defining its acrophonic origin and nature. All of this is visually represented in my alphabetic chart (Fig. 1), which documents the lessening of the RPV from the MBA to LBA I, and from the LBA to the Iron Age.

IV. Rollston states that nothing at all is new about my proposal, citing Grimme's book of 1923, where he proposed the same thesis that I have presented. For Grimme's initial attribution of the script as Hebrew, my book is dedicated to him, because he should be credited with being the first to acknowledge this connection. What's absolutely new in my book is that I have benefitted from the scholarship and work of many scholars from Grimme's time until mine, as many advancements in the identification of PC letters have been made. Grimme's shortcomings are that he did not identify each of the PCH letters correctly, and that paleographically he did not always draw the letters accurately, which flaws are *vital*ly important for deciphering accurately.

The post-Grimme discovery of Ugaritic's alphabet actually has nothing whatsoever to do with the decipherment of the PC script. The fact is that nearly 500 years elapsed between the advent of the pictographic alphabet in Egypt—*not* the wedge-shaped signs borrowed from the cuneiform script(s)—and Ugarit's inscriptional attestation to a consonantal script, so the mysteries of the PC script's *origins* have

nothing remotely to do with Ugaritic or the site of Ugarit. Moreover, Byblos was Egypt's primary training partner during the MBA and deep into the LBA, not Ugarit. Nor do the existence of consonants at Ugarit that number beyond the 22 of the original alphabet have any relevance to the question of the script's *origins*. These are apples and oranges.

My book also was dedicated to all of those underappreciated scholars on whose efforts my contributions are founded. While they go unnamed in the dedication, this includes—but is not limited to—scholars such as Butin, Černý, Leibovitch, Albright, Rainey, Beit Arieh, Sass, Colless, Hamilton, Puech, Darnell, and Wilson-Wright. Deserving of special merit is Brian Colless, whose work probably is more underappreciated than anyone's, and whose contributions are numerous and were critical to the unlocking of this 150-year-old mystery. My work stands on the collective shoulders of all of these scholars.

What specifically is new with my work that was not true of Grimme's? First is that, due to the scholarly dialogue over the identification of the original 22 (not 27!) letters of the PCH alphabet over the decades, I benefitted from these advancements, which made it easier to identify every letter properly. So, what I offer is the confident resolution of the disputed letters. Until now, all of these dedicated scholars still have not agreed on the identification of all of the letters. Having all of their views in hand, combined with the living laboratory of the PCH inscriptions that were waiting to be translated after I knew they are written in Hebrew, I was able to resolve every one of the disputed letters. The inscriptions, themselves, aided me in this process. Plus, I was able to find the acrophonic root of every letter, thanks to a knowledge of the Egyptian language and sign list. All of this also was dependent on the willingness to draw *every* letter in *every* inscription (of the 16 in the book) slowly, carefully, and accurately. This required countless hours and the utmost patience.

V. As for Naveh's 2011 statement that there were scholars who tried to relate this script to the Israelites, who after the exodus lived for a generation in the Sinai Peninsula, and that nowadays these romantic views are no longer accepted, I will agree that—to my knowledge—there is no conclusive material cultural evidence from Sinai that attests to the presence of Israelites in the desert during the Bible's 40 years of wandering, post-exodus. However, where Naveh's statement is now outdated is his claim that the script cannot be related to the Israelites *before* the biblical exodus. While I cannot speak for any scholars who have gone before me, my claims have nothing to do with romanticism.

VI. According to Rollston, the evidence demonstrates that the inscriptions from Serâbîṭ el-Khâdim and Wadi el-Ḥôl are written in "Canaanite." However, there is no evidence whatsoever to make the case that the originators of the alphabet were Canaanite. Therefore, I ask Rollston to present his evidence from both epigraphy and material culture. He also states that there is nothing distinctively Phoenician, or Hebrew, or Aramaic, or Moabite, or Ammonite, or Edomite about the words in these inscriptions that would reasonably allow someone to call the language of these inscriptions by one of those terms. Yet Rollston failed to address *any* of the uniquely Hebrew terms that are found in the 16 inscriptions, as discussed in my last post. Certainly Rollston mentioned that 1 or 2 distinctly Hebrew words can be read differently, but that is a completely different issue, one to which I will return below. For now, it must be stated that Rollston completely failed to discuss the vocabulary words, and simply dismissed the proper names out of hand. As any lawyer will admit, a defense attorney who completely ignores key evidence presented in a trial risks the damaging effects that it will have for his/her client when the impartial jury is asked to render a verdict.

VIII. Rollston states that it is absolutely and empirically wrong to suggest that the script of the inscriptions from Serâbîṭ el-Khâdim and Wadi el-Ḥôl is the Hebrew script. How Rollston states this unequivocally, without reading my book or carefully studying and processing the evidence that supports my thesis, is difficult to comprehend. Once again, there are vocabulary words and uniquely biblical names—which, incidentally, fit biblical chronology perfectly—that Rollston conveniently has avoided. Secondly, every single grammatical structure and syntactical formation in the corpus of 16 inscriptions I have translated is perfectly compatible with the grammar and syntax of epigraphical and biblical Hebrew (e.g. a *min*-comparative [WeḤ 1]; *qal* verbs and verbals of various persons and number [Sinai 353, Sinai 357, Sinai 360, Sinai 377, etc.]; *piel* verbs [Sinai 349] and *pual* verbs of various numbers and persons, occasionally with pronominal suffixes [Sinai 349, Sinai 357]; there are even *poal* [Sinai 360] and *poel* [Sinai 361] verbs, plus much more. The truth is that the evidence passes the empirical test up, down, around, inside, and outside. It even passes tests that go beyond Rollston’s areas of expertise, such as those of material culture, biblical chronology, biblical exegesis, and historical geography. Of course, much of the evidence from these fields is relegated to the unfinished companion volume to my book, which will be published in the future.

VII. Rollston states that this is the precise derivation of the alphabetic scripts: first the Early Alphabetic script (e.g. from Serâbîṭ el-Khâdim and Wadi el-Ḥôl), and from this the Phoenician script, and from this the Old Hebrew script and the Aramaic script. He also suggests that the Hebrew script was developed in the homeland: Israel, which he claims to be attested there first. Firstly, Rollston is correct in summing up conventional thinking up to the present time. Conventional thought within scholarship used to be that “the Bible’s Hittites” were a fabrication of overambitious biblical writers. Yet when Hittite material culture and textual records were found, conventional scholarly opinion changed. Conventional thought within scholarship in Egyptology was that Tanis was the site of *pr*-Ramesses of the 19th Dynasty, until further excavations at Tanis and Qantir made it clear that Qantir is the proper site for *pr*-Ramesses. Whether it takes 1 year, 5 years, 10 years, 20 years, or 40 years, scholarship eventually will change its view on the alphabet’s historical development, at least in relation to Hebrew and the PC script. Hebrew is attested first at Serâbîṭ el-Khâdim (1842 BC) in the form of a single PCH letter, then at Wadi Naṣb (1840 BC) in the form of the oldest fully Hebrew inscription, then at Wadi el-Ḥôl (1834 BC) in the form of the two PCH inscriptions composed alongside the ME inscription known as Wadi el-Ḥôl Inscription 3, and then at Lahun (1831 BC) in the form of a “bilingual” inscription discovered by Petrie over a century ago.

IX. Rollston states that the script of the inscriptions from Serâbîṭ el-Khâdim, Wadi el-Ḥôl, etc. is not one of the distinctive national scripts (such as Phoenician or Hebrew or Aramaic, etc.), but is the early ancestor of all of these scripts, earning it the title of Early Alphabetic. This perspective is problematic on numerous levels, the first of which is that Rollston assumes many things that he does not know. Without having determined the language of the PC script or deciphered any of the inscriptions, on what basis can Rollston even assert that the PC script is not a known (or “national”) script, or that because of its earliness the script must necessarily be different than that of a known, or national, script?

If Rollston first were to identify the language of the script and decipher some of the inscriptions, then he would put himself in the position to make such a statement with sound reasoning and substantive supporting evidence. Yet this is not the case. Rollston’s lack of in-depth experience with the PCH script and writings of the Bronze Age serves as blinders to prevent him from seeing the clear and quantifiable evolution that the script underwent from *ca.* 1845 BC (or so) until the Iron Age (*ca.* 1000 BC or so). The impact of the lengthiness of these 850-odd years simply cannot be ignored, including the potential for a script to undergo

such a gradual and discernable transformation, particularly with its derivation from the ME sign list, making for cumbersome pictographic origins and a complicated way of forming every letter early in its history.

The time and artistic skill that was required of ME scribes to practice their trade was enormous. These fundamental elements inherent within the writing of a fully pictographic script do not naturally dissipate when the writing transforms from hieroglyphics to alphabetic letters. Nothing changes but the shift from over 800 signs that must be mastered to only 22 letters to learn and know, along with the shift to a simplified system of the use of the pictographs, called acrophony. Moreover, ME was practiced by a scribal class that primarily was tied to the administrative machine of the Egyptian monarchy. On the contrary, the beauty of the simplicity of writing only 22 pictographs and using the rudimentary system of acrophony is that the average person on the street now gained access to the ability to write with ease and accuracy, even if not with the same stylistic flare. The *only* inhibiting factor is the artistic element required for writing.

It is for this reason that (1) even the beginnings of the pictographic alphabet's implementation reveal its artistic inferiority to the writing of the same pictographs by the professional Egyptian scribes who wrote in hieroglyphic ME, and (2) alphabetic inscriptions written on the walls of the turquoise mines in Sinai during the NK were composed by shepherds and miners, as the external and internal evidence from the inscriptions, themselves, clearly reveals. All of this flies in the face of Rollston's hollow demand that the PC script could not have been a preexisting national script. This *a priori* assumption simply does not stand up against the preponderance of evidence, which needs to be the primary factor for shaping one's view on any matter.

X. Rollston contends that the PC script could be written from left-to-right (i.e. dextrograde), right-to-left (i.e. sinistrograde), boustrophedon (left-to-right, then right-to-left, and back-and-forth), and columnar (top to bottom, as in a column). He is correct that PCH was written either dextrograde, sinistrograde, or columnar. However, he wrongly relied on those who have published on the PC script who have argued that one or more of the inscriptions was written boustrophedon. This simply is untrue. Here are three questions that must be asked: (1) Which inscription(s) is/are written boustrophedon? (2) How does Rollston know this? and (3) What is the language and translation of the inscription(s) written boustrophedon?

PC Hebrew of the MK follows ME in being written only dextrograde, sinistrograde, or columnar. There are no other options for ME, and there are no other options for PCH. This is borne out in all 16 PCH inscriptions that I have translated. Therefore, the fact that PCH strictly follows ME in its options for the direction of writing is another of many factors that tie its origins to Egypt, not Sinai, as many scholars have been assuming wrongly for over a century (with Goldwasser possibly being the most recent one).

XI. Rollston also contends that the letters of the PC script "face" one way if you were writing left-to-right but face the other way if you were writing right-to-left. He has erred here, but his mistake is understandable because what he describes is indeed true of ME: the text on ME inscriptions reads from the direction toward which the people, deities, and animals are facing. One *could* expect that the Hebrews would have followed ME conventions here, but the fact is that they did not, for reasons known only to them. This is certain because in the PCH inscriptions, the direction from which the text reads *may or may not be* the direction toward which the people and animals are facing.

Since Rollston agrees that the PCH inscriptions may follow the direction consistent with ME inscriptions, I will cite only examples of inscriptions [using letter-codes according to my book] where PCH breaks with

ME practice for the direction of reading: WeH 1 (horizontal line, going right to left [H1, H8, H12, H13, and H17]), Sinai 345a (horizontal line, going left to right [H2 and H3]), Sinai 345b (horizontal line, going left to right [H8 and H11, contra H9]), Sinai 346b (columnar, with columns going left to right [V29, contra V26, V32, and V34]), Sinai 349 (7 horizontal lines, going right to left [many humans and animals, all facing away from the right-to-left direction of reading]), Sinai 351 (columnar, with columns going right to left [V4, V7 and V13]), Sinai 353 (columnar, with columns going right to left [V25 and V35]), Sinai 357 (with humans and animals on the right-to-left horizontal line facing the opposite direction from those in the vertical column [H3, H4, H5, H7, and H10, contra V1, V2, V6, and V11]), Sinai 361 (columnar, with columns going right to left [V6 and V9, contra V15 and V19]), Sinai 375a (horizontal line and vertical column [H3 and V1, contra H6 and V3]), Sinai 377 (columnar, with columns going right to left [V3]).

XII. Rollston listed a number of the most important scholarly works on the PC script to date (Gardiner, Albright, etc.), virtually all of which I have both cited and interacted with extensively. Rollston also stated that the PC script has been studied heavily through the years. In reality, while the topic has been studied over an exceedingly lengthy period of about 150 years, relatively speaking the topic has been studied only sparsely, with *far fewer* scholars working on it than on other scripts or other topics related to ANE historical studies. I would venture to guess that if Rollston consulted the small number of living scholars who have invested serious and extensive time into the topic of the PC script, they would agree with my assessment completely. It is unfortunate that so few scholars have invested themselves into this amazing field.

XIII. In Rollston's critique of 17 November 2016, he used several examples from my ASOR 2016 presentation, including *RB* "many, abundant" and *YN* "wine", to declare that since these terms are common to a number of Semitic languages, we cannot say that the language of the PC script is Hebrew. I replied in agreement that they are common to various Semitic languages, but that in the case of (in this example) Wadi el-Hôl 1 they are definitely Hebrew words. Rollston now states, "That's fascinating, as he is conceding that these words are Common Semitic and he is saying that they are Hebrew in the Serabit el-Khadem inscriptions because he says that these inscriptions are Hebrew. That is, of course, a textbook case of circular reasoning."

The charge of circular reasoning is invalid. If Rollston is expecting that every Semitic word in the PCH inscriptions must be *exclusively* Hebrew in order for the inscription to be Hebrew, then there would not be enough words left—when removing the words that are common to more than one Semitic language—to make any sense of any of the inscriptions. Perhaps Rollston should attempt this with known Hebrew inscriptions of the ninth, eighth, or seventh century BC. If he were to remove all of the common Semitic words, he could illegitimize most of the so-considered "Hebrew" inscriptions, and probably create for himself the right to call these Phoenician, Aramaic, or inscriptions of some other Semitic tongue. Rollston has heaped on the PC inscriptions demands that they should not be required to bear.

My reply to him was something completely different than circular reasoning. Instead, I was saying that one cannot illegitimize words on PCH inscriptions as being Hebrew simply because they have cognate forms in one or more other Semitic language. The 16 inscriptions I translated form a sort of corpus of texts, and within that corpus there are distinctively Hebrew words, including proper names found not only in the HB (Hebrew Bible) alone, but attested only exceedingly rarely in the HB. The combination of those distinctively Hebrew words, spread among several inscriptions, legitimize the entire corpus as being Hebrew. This is

not circular reasoning; rather, it is innocence—against accusations of illegitimacy—by “corpusorial” association. To put it in biblical terms, the little bit of leaven leavens the entire loaf.

XIV. Rollston cited one of my examples of a distinctively Hebrew word in the “corpus” of the 16 inscriptions from my book: *TL(Y)* “quiver” (Sinai 349). Rollston next claimed I was suggesting that “those two letters must mean ‘quiver’.” He then pointed out other uses of *TL* in Semitic languages, such as (1) the noun “ruins”, (2) the verb “to attach, or hang” and (3) the verb “to scheme, flirt”. Subsequently, he claimed I was assuming that the only possible meaning for *TL* in Sinai 349 is “quiver”. If he had asked me a question or two instead of assuming the worst, he would have learned that this simply is a false claim on his part, based on a wrong assumption.

Instead, in my previous response I merely told Rollston what is the correct option for the meaning of *TL* in the context of Sinai 349, rather than enumerating a grocery list of options for the combination of *T + L*. Rollston previously had accused me of not producing any distinctly Hebrew words, so I merely obliged him by producing them. I most certainly considered other options during the process of translating Sinai 349, as I did throughout the entire process of the decipherment of the “corpus” of 16 inscriptions.

What Rollston also has failed to do is study carefully the context in which *TL* is found, in order to determine whether any of his three options would work in Sinai 349. Let’s explore the possibilities. The context requires that *TL* be a noun or substantive, as it begins a dependent clause with a verb (“thoroughly despised”) that follows, thus making for an emphatic S-V-O word order. This means that a verb will not work here, unless in participial form. The word *TL* also is used in a bound construction, with “of our brothers” following it and intricately bound to it, denoting possession. The author of Sinai 349 states that “they (i.e. his Hebrew brethren) yearned for Hathor, but the ??? of our brothers was thoroughly despised.”

Those opposing his Hebrew brethren could not have been saying that the ruins of his brothers were despised, because those in opposition already were making life miserable for them. It would make no sense if the opposers were thoroughly despising the ruins of the writer’s Hebrew brethren. Why? Because they would have been rejoicing over the Hebrews’ ruins, not despising the Hebrews for “their ruins” (whatever that would mean here, anyway). The next option is this: “they (i.e. his Hebrew brethren) yearned for Hathor, but the attaching/hanging of our brothers was thoroughly despised.” The word “attaching” here would be silly. Even if the “hanging” could be construed as hanging by noose, for example, once again this option has the inherent flaw that the opposers would not be despising the action of many Hebrews being hanged, but instead—based on the greater context of the inscription(!)—they would have rejoiced over it.

Rollston’s third option is this: “they (i.e. his Hebrew brethren) yearned for Hathor, but the scheming/flirting of our brothers was thoroughly despised.” It could be suggested that the act of scheming on the part of the Hebrews was despised by the opposers, because the Hebrews were being accused of scheming against those in authority over them. While this is logically possible out of any particular context, Sinai 349 does offer a particular context. Immediately below, the text states that as a result of this action, “he performed terror against their quiver and brought about a cry of wailing.” Here, the Hebrew writer used the other Hebrew word for “quiver”, *šP*, which removes all doubt about the meaning of *TL(Y)* above, because the individual in power performed terror against the very thing that was what he and his people despised: the Hebrew quiver, which signifies their child-bearing and tendency to multiply effectively. As hard as Rollston may want to try, he simply cannot force any of these words into the context of the inscription where *TL* is found,

and in so doing he neglects the three most important rules in exegesis and hermeneutics: (1) context, (2) context, and (3) context.

XV. Rollston repeats the same drill with my discussion of the uniquely Hebrew word ³M “terror, dread”, implying that since ³M can mean “mother” (a term common to many Semitic languages), (1) I must not have realized this, and (2) the use of those two letters in succession in Sinai 349 could be referring to a mother. This analysis is incorrect on both counts. With the latter, he again did not make the effort to evaluate the potential of “mother” in the context of the inscription at hand, forgetting the rules of (1) context, (2) context, and (3) context. Let’s see how ³M would work as “mother” in Sinai 349.

Here’s how the text would read with “mother” inserted for the contextually-appropriate word “terror”: “They yearned for Hathor, but the quiver of our brothers was thoroughly despised, [so] he performed (or ‘did’ or ‘made’) a mother against their quiver and brought about a cry of wailing.” This construction would lead us to believe that the leader of those who opposed the Hebrews despised their offspring so thoroughly that his way of causing them great pain was to perform a mother against them. Clearly this option fails, because it cannot pass the test of logical feasibility within the context of the inscription.

All good epigraphers and exegetes of ancient texts understand the vital role of context in eliminating non-feasible options. Rollston cannot simply find two random consonants, pull them out of the context of an inscription, and claim to have found a legitimate option for translation. This is not how careful deciphering takes place. The same contextual failures apply to his other options, including “forearm/cubit, tribe/people”.

XVI. Since Rollston is unable to challenge my reading of the caption on Sinai 115, a mostly ME (Middle Egyptian) inscription, he enlisted the aid of an Egyptologist, Thomas Schneider, to challenge my reading, which Schneider uploaded to Rollston’s website. Schneider’s attempted refutation reveals several vital deficiencies: (1) He evidently was not at my ASOR presentation, which would have caused him to prepare much differently for his attempted refutation. (2) He relied completely on the 1952 drawing of Gardiner et al., which is insufficient for the task of proper evaluation. (3) He failed to consult Černý’s vitally important article in *Archív orientální* (1935), “Semites in Egyptian Mining Expeditions”, which includes both crucial discussion and his own transcribed rendering of the caption. (4) Of even greater importance, Schneider failed to consult any photographs, three of which I have secured (two color and one black and white, the latter one taken by the Egypt Exploration Society). I have rebutted each of Schneider’s critiques here: https://www.academia.edu/30408017/2016_The_Reading_of_Sinai_115s_Caption_An_Open_Response_to_Thomas_Schneider. The reading of “Hebrews of” in Sinai 115’s caption is absolutely plausible.

XVII. Rollston then declared *a priori* that my identification of several biblical personages in several of these inscriptions is invalid. In his own words, “They are simply not there. I wish that they were. I would really like to see some references to more biblical people in the epigraphic record.” Yet he has neither invested himself fully into the study of the PC script, nor claimed to have deciphered even one of these inscriptions in which the names of these biblical personages are inscribed. Therefore, I must ask, on what rational basis not only *would* Rollston make this declaration, but *can* Rollston make this declaration, . . . if indeed he is an objective scholar impartially pursuing the truth. And if Rollston truly wished to see more biblical people in the epigraphical record, he would wait until the evidence from my book is in his hands before merely dismissing my associations out of hand. Thus Rollston’s stated wish is contradicted by his

non-objective act of discarding my thesis without examining the empirical evidence that should lead to his own measured conclusions.

XVIII. I appreciate greatly that Rollston believes that I, like Grimme and van den Branden, am well intentioned. I believe that he has good intentions, as well. Just as I do, he wants to learn as much about the ancient world as we can learn, and probably we both believe that the singularly most effective way to learn about it is to study what the ancients, themselves, had to say. At the same time, Rollston stated, “But the difference between (another scholar)’s work and Petrovich’s is that (the other scholar) is really reading inscriptions carefully and accurately, considers all of the options, and draws reasonable conclusions.”

The truth is that Rollston has seen nothing of my methodology, nor has he asked me any questions, which would help him to know whether or not I am careful and accurate. One place where he can evaluate the care and accuracy of my scholarship related to Hebrew/Semitic epigraphy is my 2015 *PEQ* article on the Ophel Pithos Inscription. There, in agreement with Gershon Galil, I concluded that the inscription must be read sinistrograde (p. 132, 141), citing—as one reason—Rollston (2013) for correctly stating that sinistrograde is the normal direction for reading a Hebrew text of the Iron Age IIA and beyond, because the direction seemingly became fixed as sinistrograde during the terminal phase of the 2nd millennium BC. Yet Rollston, himself, has concluded—at least in his preliminary study of the inscription—that it reads dextrograde. Other more compelling reasons make it clear that the Ophel Pithos Inscription should be read sinistrograde, and that it should be dated to the 10th century BC, not the 11th century BC, as Rollston has suggested.

The Proto-Sinaitic Inscriptions 2.0: Canaanite Language and Canaanite Script, Not Hebrew.
Dr. Christopher Rollston (rollston@gwu.edu), George Washington University (10 December 2016)
<http://www.rollstonepigraphy.com/?p=779>

Doug Petrovich has proposed in a paper presented on November 17th, 2016 at the annual meeting of the American Schools of Oriental Research and in a forthcoming volume (Jerusalem: Carta) that the Proto-Sinaitic Inscriptions from Serabit el-Khadem and Wadi el-Hol are Hebrew. This is quite a claim, but it will find few takers among those who actually know the scripts and languages of ancient Semitic inscriptions written in linear alphabets.

After all, the inscriptions from Serabit el-Khadem and Wadi el-Hol can be dated to ca. the 18th century BCE. But the earliest inscriptions written in the distinctive Old Hebrew script can be dated with substantial certitude to the 9th century BCE. And the script of the actual Old Hebrew inscriptions is very, very different from the script of the inscriptions from Serabit el-Khadem and Wadi el-Hol. In fact, no one who has formal training in these different scripts would ever confuse them or suggest that they were the same. I'll get back to this point in a moment, but first a little history.

There is nothing all that new about Petrovich's proposal. After all, more than ninety years ago, H. Grimme wrote a volume entitled *_Althebräische Inschriften vom Sinai_* (Hannover, 1923) in which he argued that the inscriptions from Serabit el-Khadem were Hebrew. Grimme can almost be forgiven, because he wrote before the discovery of Ugaritic. Almost. But even then, his views didn't find many takers, as he was manipulating the evidence to the point that his proposal had more twists and turns than a dirt road in a Tennessee holler.

In fact, here is what the great epigrapher Joseph Naveh (d. 2011) of Hebrew University (Jerusalem) said about Grimme's proposal: "There were scholars who tried to relate this script to the Israelites, who after the Exodus lived for a generation in the Sinai Peninsula. Nowadays these romantic views are no longer accepted" (Naveh, *_Early History of the Alphabet_* 1987, 26). Similarly, A. van den Branden argued (in an article entitled "Les inscriptions protosinaitiques," published in 1962 in *_Oriens Antiquus_* 1, pp. 197-214) that the inscriptions from Serabit el-Khadem were written in a South Semitic language that is attested on various South Arabian monuments. Neither of these proposals gained any traction with people who knew multiple Semitic languages and scripts...because these proposals were based on erroneous and partial understandings of the actual inscriptional evidence.

I'm sure that Grimme and van den Branden were well intentioned, and I believe that Petrovich is as well. But the evidence is weighty, and the evidence demonstrates the following: the inscriptions from Serabit el-Khadem and Wadi el-Hol (and various other second millennium sites, etc.) are written in a Northwest Semitic dialect of the early Second Millennium BCE. In terms of the name for this language, the most apt term is "Canaanite." After all, there is nothing distinctively Phoenician, or Hebrew, or Aramaic, or Moabite, or Ammonite, or Edomite about the words in these inscriptions that would reasonably allow someone to call the language of these inscriptions by one of those terms. Indeed, the words in the inscriptions from Serabit el-Khadem and Wadi el-Hol are found in lots of Semitic languages, not just one. Thus, the best term of the language of these inscriptions is "Canaanite."

And as for the script of these inscriptions from Serabit el-Khadem and Wadi el-Hol, the best terms are "Early Alphabetic," or "Canaanite." Some prefer the term "Proto-Sinaitic Script." Any of these terms is

acceptable. But it is absolutely and empirically wrong to suggest that the script of the inscriptions from Serabit el-Khadem and Wadi el-Hol is the Hebrew script, or the Phoenician script, or the Aramaic script, or the Moabite script, or the Ammonite script, or the Edomite script. The script of these inscriptions from Serabit el-Khadem and Wadi el-Hol (etc.) is not one of the distinctive national scripts (such as Phoenician or Hebrew or Aramaic, etc.), but rather it is the early ancestor of all of these scripts and we term that early ancestor: Early Alphanumeric. Here is the precise derivation: first came the Early Alphanumeric script (e.g., of the inscriptions from Serabit el-Khadem and Wadi el-Hol, dating to ca. 18th century BCE), and from this Early Alphanumeric script (which continued to be used in the Levantine world for several centuries) came the Phoenician script (in ca. the late 11th century BCE and the early 10th century BCE). And from the Phoenician script came the Old Hebrew script (in the 9th century BCE) and the Aramaic script (8th century BCE). And it should be emphasized that the Hebrew script was developed in the homeland, that is, Israel (and that explains why it is first attested there).

Here are some of the features of the Early Alphanumeric script. It could be written from left-to-right (for which we sometimes use the fancy word “dextrograde”), right-to-left (for which we sometimes use the fancy word “sinistrograde”), boustrophedon (which means the first line was written from left-to-right and the one below it was written from right-to-left, and so on...interestingly, the term “boustrophedon literally means “as the ox plows”), and columnar (which means: written from top to bottom, as in a column, rather than on a horizontal plane). In addition, in the Early Alphanumeric script, the letters were very pictographic in nature and they would “face” one way if you were writing left-to-right and they’d face the other way if you were writing right-to-left. In addition, the Early Alphanumeric script had twenty-seven letters. I’ve included here a scan of W.F. Albright’s chart of those letters.

However, Phoenician, Hebrew, and Aramaic (etc.) are consistently written from right-to-left (not in any of the other options, listed above). And Phoenician, Hebrew, and Aramaic always “face” the same direction (because they are always written in the same direction, that is, from right-to-left). And Phoenician, Hebrew, and Aramaic (etc.) all have twenty-two letters in their alphabet (as demonstrated by Hebrew inscriptions that list the Hebrew “abc’s” in order, and also by Biblical alphanumeric acrostics, such as Psalm 119, etc.), not twenty-seven, of course. And, on top of all this, the shapes (“morphology”) of the letters of the Phoenician, Hebrew, and Aramaic letters are often quite different from the shapes of the Early Alphanumeric letters. In short, the difference between the Early Alphanumeric script and the Old Hebrew script is night and day. Even a novice can be trained to tell the difference in a matter of minutes. In fact, I have actually tried this in class at times and even undergraduates can rapidly be taught which is which. It’s not rocket science.

So....let’s now continue with a little more history and a little more data...

The oldest inscriptions written in a distinctive Old Hebrew script can be dated with certitude to the 9th century BCE (some have suggested perhaps the 10th century...that too is fine), as the great Joseph Naveh of Hebrew University argued (1982, Early History of the Alphabet, 65-66), and as I too have argued (e.g., in my “Northwest Semitic Cursive Scripts” article in the Frank Moore Cross memorial volume entitled An Eye for Form, 202-234). We know an enormous amount about the history and development of the Hebrew script through time, as we have hundreds and hundreds of Hebrew inscriptions from the 9th through 6th centuries BCE (less for the earlier periods than the later periods, but still, many, many inscriptions and lots of evidence, therefore).

These inscriptions from Serabit el-Khadem and Wadi el-Hol arguably date to ca. the 18th century BCE. The Serabit el-Khadem inscriptions have been known for more than a century (Petrie, *Researches in Sinai*, 1906), and a decade after their discovery, A. Gardiner discerned that these inscriptions were written in a very early form of the Semitic Alphabet. Indeed the title of his seminal article on the subject is “The Egyptian Origin of the Semitic Alphabet” (*JEA* 3, 1916, 1-16). The Wadi el-Hol inscriptions had been discovered some time ago, but were studied and formally published just around a decade ago (Darnell, Dobbs-Allsopp, Lundberg, McCarter, Zuckerman, 2005). Inscriptions written in this same Early Semitic Alphabetic script (though dating to later centuries) have been found at additional sites, including Lachish and Gezer (i.e., sites in the Levant).

These inscriptions have been studied heavily through the years, with the publications by scholars such as W.F. Albright (*The Proto-Sinaitic Inscriptions and Their Decipherment*, 1966), F. M. Cross (written primarily in the 1960s-1990s and all collected in his volume entitled *Leaves from an Epigrapher’s Notebook*, Eisenbrauns, 2003), P. K. McCarter’s published dissertation (*The Antiquity of the Greek Alphabet and the Early Phoenician Scripts*, 1975), B. Sass’s published dissertation (entitled *The Genesis of the Alphabet and Its Development in the Second Millennium B.C.*, 1988), G. Hamilton’s 1985 Harvard dissertation and its augmented published form (entitled *The Origins of the West Semitic Alphabet in Egyptian Scripts*, 2006), various substantive and important articles by O. Goldwasser (referenced or archived at <https://huji.academia.edu/orlygoldwasser>), the volume by S. Sanders (entitled *The Invention of Hebrew*, 2009), as well as some publications by me on the subject of the Early Semitic Alphabet, the Phoenician alphabet, and the Old Hebrew alphabet (a number of these are referenced or archived at <https://gwu.academia.edu/ChristopherRollston>). And this is only a very partial list. The bibliography is really quite vast.

And now on to say a little more about Petrovich’s proposal, keeping the above background in mind... I noted in my previous post on this subject (<http://www.rollstonepigraphy.com/?p=763>) that various words that Petrovich was stating were distinctively Hebrew were actually Common Semitic (a term that means that those words are found in multiple Semitic languages, not just one or two). Here is Petrovich’s response to me: “I assert the reading rb “many, abundant” and yn “wine” to be Hebrew words in the context of the [Serabit el-Khadem] inscription.” That’s fascinating, as he is conceding that these words are Common Semitic and he is saying that they are Hebrew in the Serabit el-Khadem inscriptions because he says that these inscriptions are Hebrew. That is, of course, a textbook case of circular reasoning. After all, the script is definitively *NOT* the Old Hebrew script and the roots rb “many, abundant,” and yn “wine” are attested in multiple Semitic languages (and in the case of “wine” even in non-Semitic languages!), thus, there is no evidence forming the basis for a declaration that the inscriptions are Hebrew.

But let’s look at more of Petrovich’s evidence. Thus, in his response to my previous blog post, he posited that “in Sinai 349 the Hebrew word tl(y) ‘quiver’” is used. He believes this word is distinctively Hebrew, that is, a smoking-gun demonstration of the fact that Sinai 349 is Hebrew. So, let’s look at the actual evidence, and I’ll even go with his reading (i.e., tav lamed) for the sake of argument. That is, he reads tl here and let’s see if those two letters must mean “quiver.” ...Well, a quick look at the lexicon reveals the following:well, there’s a word (as many people know, even if they don’t know any Semitic languages) that occurs in multiple ancient Semitic languages that means “ruins” and it is spelled tl. In addition, there is a word for “to attach, or hang” that occurs in multiple Semitic languages that would be spelled tl in this period (root tlh). Moreover, anyone trained in Semitic languages would know that tl could represent the geminate Semitic root tll (to scheme, flirt). And there are even more options. In any case, Petrovich

assumes that the only possible meaning for tl in Sinai 349 is “quiver,” but a quick look at the lexicon demonstrates that this assumption is erroneous. So much for those two letters demonstrating that Sinai 349 is Hebrew.

In Petrovich’s response to me he also states that “in Sinai 349 the Hebrew word ‘m (aleph mem) appears.” He also states that this is “the Hebrew word ‘m for “terror, dread” (and he also mentions that he believes that the final hey of the word ‘mh is a mater lectionis and that it would not be reflected in the early orthography....I could discuss that further, but this subject is well-known and so I’ll not delve into it here). And he states that “this word occurs only in Biblical Hebrew and Aramaic.” So, Petrovich reads the letters aleph and mem and he assumes that this must be the Hebrew word for “terror, or dread.” That is fascinating....but let’s look at a good dictionary again, as a diligent student would always do. And, lo and behold, there’s a word that jumps right off the page of the lexicon: the word “mother” is spelled aleph mem (and I suspect that a number of my readers already thought about the fact that the word “mother” is spelled aleph mem....and these readers, therefore, already knew that if you see the letters aleph + mem it isn’t necessarily the word for “dread, or terror”). And that word (i.e., aleph mem, “mother”) is attested across the Semitic languages, from Phoenician, and Ugaritic, to Hebrew and Aramaic, to Ethiopic (and beyond). And there’s more, if we keep looking...the word “if” is also spelled aleph mem. And the word for slave woman, maidservant is also spelled aleph mem (plus a hey mater) and it occurs in all kinds of Semitic languages, from Ugaritic, Phoenician, Aramaic, Hebrew, to Akkadian and Ethiopic. And the word for “forearm, cubit,” is also spelled aleph mem (and it’s attested in many ancient Semitic languages as well, including Ugaritic, Akkadian, Hebrew, Aramaic, and Ethiopic, etc). And aleph mem is also the way to spell “tribe, people” and this word is also attested in multiple Semitic languages. Again, the problem here is that Petrovich assumes that the letters he reads as aleph mem must be some word that he considers to be distinctively Hebrew (i.e., a word for “terror or dread”) and so he views that word as being a demonstration that Sinai 349 is using a rare Hebrew word. But the problem is that he is making all sorts of assumptions and a quick look at any dictionary proves that his assumptions are just not correct.

The same goes for his reading of the word “Hebrew” in Sinai 119. Egyptologist Thomas Schneider has demonstrated nicely (in a guest post on my blog) that the word “Hebrew” is just not there (<http://www.rollstonepigraphy.com/?p=771>).

And the same can be said for Petrovich’s attempts to read the names of biblical people in these inscriptions. They are simply not there. I wish that they were. I would really like to see some references to more biblical people in the epigraphic record. In fact, I very much like the work of Lawrence Mykytiuk (e.g., his *Identifying Biblical Persons in Northwest Semitic Inscriptions of 1200-539 BCE*, SBL 2004....and various really sterling articles by Mykytiuk since the publication of his book). But the difference between Mykytiuk’s work and Petrovich’s is that Mykytiuk is really reading inscriptions carefully and accurately, considers all of the options, and draws reasonable conclusions.

Finally, I should like to emphasize that the issue is not one of faith versus academia or “not believing in the biblical exodus from Egypt.” In my case, I do believe there was an exodus from Egypt, and things such as the Beni Hasan Tomb (a painting in the tomb of Rekhmire showing slaves making mudbricks), and Papyrus Anastasi 5, and the Instruction of Merikare (all of which are discussed so very well by Nahum Sarna in an article entitled “Israel in Egypt,” published in a volume edited by H. Shanks and entitled *Ancient Israel*, 2nd ed. 1999) are pretty important pieces of evidence regarding the fact that there was some sort of an exodus. In short, it is not my disinclination to believe in the exodus that causes me to not

embrace Petrovich's proposal. I do believe that the evidence (biblical and extrabiblical) are sufficient to believe there was an exodus. So...it's just Petrovich's proposal that I find unconvincing. The evidence just doesn't support his proposal.

Thus, in the end, the conclusion that must be drawn is that (1) the inscriptions from Serabit el-Khadem and Wadi el-Hol are written in the Early Alphabetic script (also called "Canaanite" and "Proto-Sinaitic"), not the Old Hebrew script (which we know so well from hundreds of inscriptions), and (2) the language of the inscriptions from Serabit el-Khadem and Wadi el-Hol is not the Hebrew, or Phoenician, or Aramaic language, but rather it is a West Semitic language or dialect that is best termed "Canaanite." That's it.