A review of Chris Rogers' "A Review of the Afro-Asiatic: Uto-Aztecan Proposal" in the *Journal of Book of Mormon Studies*, Vol. 28 (2019), 258-267, wherein he reviews *Exploring the Explanatory Power of Semitic and Egyptian in Uto-Aztecan* (Stubbs 2015b) and *Changes in Languages from Nephi to Now* (Stubbs 2016).

Brian D. Stubbs, 2019

The purpose of this paper is to grant a clear view of Chris Rogers' review. By way of background, Uto-Aztecan is a family of some 30 related languages in western Mexico, the U.S. Southwest, and the several Nahua dialects from Mexico to El Salvador. After 30 years of research and publishing several articles on Uto-Aztecan (UA) in peer-reviewed journals, I published *Uto-Aztecan: A Comparative Vocabulary* (2011), the new standard in comparative UA and well received among specialists in the field. After 35 years of research, I published *Exploring the Explanatory Power of Semitic and Egyptian in Uto-Aztecan* (2015b), which linguistically establishes a Northwest Semitic (Hebrew/Aramaic) and an Egyptian infusion, language mix, or massive borrowing into UA. Though skepticism was always the initial reaction, the LDS and non-LDS linguists, Uto-Aztecanists, and Semitists actually examining the data, offered favorable assessments, silence, or dismay, but none refuted it. Following those reference works, the smaller *Changes in Languages from Nephi to Now* (2016) emerged for LDS lay-readers, addressing the relevance of the research to *The Book of Mormon*.

While many BYU linguists, Semitists, and other scholars have followed the work with interest or approbation the last 35 years, I began hearing that one Chris Rogers, a new hire among BYU linguists, was disparaging the work. I was curious what fault he found, so I sent him a friendly email 4/23/18: "I'd like to amicably answer what questions you might have." I also explained that I had sent 2015b to the best 15 Uto-Aztecan specialists, all non-LDS PhDs in linguistics. Ten were silent, but five found it sound, with valid correspondences, etc.; "but none of the 15 refuted it with specifics. So maybe you are the first, as soon as I know the specifics you have in mind." However, Mr. Rogers never answered me. He seemed not interested in answers or discussion, but preferred a public forum for his attempt to discredit. So we can do it that way. A major problem in fairness is that the *Journal of Book of Mormon Studies* comes out only once a year now. Other linguists have expressed a desire to rebut his review in next year's issue. For my part, such a flawed denigration deserves a more immediate response.

Rogers' first error is found in the title and in several pages throughout, claiming that I am proposing a long-distance relationship between Afro-Asiatic and Uto-Aztecan (UA). Not at all! That would involve a time-depth of more than 7,000 years. The thousands of coherent sound correspondences suggest, rather, that UA received a substantial infusion or mixing or borrowing from something resembling a pre-exilic stage (before 600 BC) of something Northwest Semitic with early forms specific to Hebrew and Aramaic, along with much Late Egyptian (not Middle Egyptian nor Old Egyptian, nor Proto-Afro-Asiatic). The data point to a time-depth of perhaps 2500-3000 years, not much more than Proto-Romance or Germanic. The infusion / mixing / borrowing of these two Near-East elements into UA is mentioned at least 21 times in the two books (Stubbs 2015, 26, 35, 80, 158, 237, 320, 354, 356, 360-362; and Stubbs 2016, 64, 86, 89, 96, 104, 112, 114, 154, 161, 170), and such an infusion / mixing is very different from common descent from something ancestral to Afro-Asiatic and UA, as many paragraphs below will show.

In fact, I cannot understand what Rogers read or saw to make him assume this work deals with common genetic descent from something pre-Afro-Asiatic. An electronic search of 2015b shows Hebrew is mentioned 3132 times, Aramaic 1370, (Late) Egyptian 2136, Akkadian 140 times, and Ethiopic 27 times. Even within Semitic, Hebrew and Aramaic (4,502) are the topics of discussion, as the fewer mentions of Akkadian (140) and Ethiopic (27) are incidental to a few of the phonological or semantic discussions. So the focus is not even general Semitic, let alone Afro-Asiatic. In fact, Afro-Asiatic occurs only 6 times: once in a bibliographic title, once in a misphrased compliment of my work by the late Roger William Wescott (President of the Linguistic Association of Canada and U.S.), and 4 times in discussing phonological details. Furthermore, other branches of Afro-Asiatic (Berber, Chadic, Cushitic, etc) are not mentioned at all; and never was Afro-Asiatic mentioned as part of the comparison. How does a focus on two Semitic languages (out of many) and Egyptian, but no other branches, and practically no mention of Afro-Asiatic, become construed as a proposal having anything to do with Proto-Afro-Asiatic?

Second, Rogers frequently misrepresents my work. An example among many is his claim that the Nahuan or Aztecan languages "are *systematically* ignored in the comparisons" (Rogers 2019, 266, *my emphasis*). A search of 2015b revealed over 800 references to the Nahuan or Aztecan languages! Might he have missed such details as CN being an abbreviation for Classical Nahuatl, for which there are over 400 occurrences of that alone?

Third, Rogers' choice of quotes are often lifted out of context to turn positive comments into negative. For example, John S. Robertson, Harvard PhD, leading Mayanist, former chair of the BYU Linguistics Department, who has been following the progress of the work for 35 years, in his positive review more fully said, "I cannot find an easy way to challenge the breadth and depth of the data [in Stubbs 2015b]", yet Rogers claims, "there is ample reason to 'challenge the breadth and depth of the data'," citing Dr. Robertson 2017:114 at the end of the clause, to appear as if Robertson said that, when Robertson said the opposite. He does similarly with the Elzinga quote immediately afterwards in the same sentence (Rogers 2019, 259).

Fourth, it is ironic that Rogers accuses me of "numerous assumptions" (p. 260) without clarifying, but then wildly assumes that "The only motivation for comparing Semitic languages and Egyptian to the Uto-Aztecan languages seems to be Stubbs' personal investment in Uto-Aztecan languages and linguistics" (p. 262). Nothing could be further from the truth. Navajo and its Athapaskan affiliation were my first language exposure, but my own three-day investigation into Athapaskan and various East Asian languages convinced me that Athapaskan came from East Asia, which other linguists later provided evidence for, receiving considerable, but not universal, acceptance (e.g., Vajda 2010, Rice 2011, Campbell 2011, Kiparsky 2015). After Athapaskan, I looked into Yuman, Pomoan, Wintuan, Maiduan, Shastan, Yana, Kiowa-Tanoan, Keresan, Zuni, Salishan, Karuk, Algic, Siouan, Caddoan, Iroquoian, Muskogean, and Uto-Aztecan in North America; and Mayan, Totonacan, Mixe-Zoquean, Otomanguean, and a few isolates in Central America; and Chibchan, Cariban, Tupian, Paez, Arawakan, Aymaran, Witotoan, Quechuan, Matacoan, Pano-Tacanan, Guahiboan, Barbacoan, Macro-Je, Jivaroan, Movima, Zaparoan, and others in South America. An MA in linguistics and studies in Semitic (Hebrew, Arabic, Aramaic, PhD/ABD) preceded the perusing and enabled me to see a substantial infusion / borrowing of Northwest Semitic and Late Egyptian in UA. So for Rogers to assume that my investment in UA was the motivation for seeking Semitic similarities in it is astonishing, when it was the opposite: years of investigating dozens of language families throughout the Americas led to or motivated my 40-year investment in UA.

Fifth, Rogers insists that "Linguistic comparisons require like systems" and that "the similarities identified must come from like systems, such as families, languages, or dialects" (p. 262). Does he mean that one can only compare proto-language family to proto-language family, or language to language? Discoveries often call for a language (or two) to be compared with a language family, as when Tocharian A and B were discovered and then proven to belong to the Proto-Indo-European (PIE) language family (Sieg and Siegling 1908), and as when Hittite was discovered and was shown to belong to PIE (Hrozny 1917), and as when the Cochimi language was united to the Yuman language family by my former professor Mauricio Mixco (1978). Additional obstacles to his ideal are that:

(1) There is not yet consensus on a system for reconstructing UA. We UA specialists agree on many points, but other matters are still under discussion.
(2) Semitic reconstructions are further along than UA, yet reconstructions of Proto-Semitic are not entirely agreed upon either. Interestingly, UA contains evidence relevant to one Semitic question: whether the so-called Semitic velar fricative x was velar or uvular. The UA evidence suggests uvular (Stubbs 2015b, 313-6; Stubbs 2019, 33-37).
(3) So much remains unknown about ancient Semitic languages; for example, ancient written Hebrew is only a fraction of what was in the spoken language. That is why Semitists in comparative work often list several relevant related forms for a broader perspective or bigger picture for comparison, as I did also on occasion. For example, Rogers (pp. 262-3) listed set 13 as a flawed set:

> Arabic **snw**; Ethiopic **snw**; Hebrew šaani; Akkadian sinitu; and Hopi **soniwa** 'beautiful, bright' all sharing common meanings. Rogers shows it as Arabic snw; Ethiopic snw; Hebrew šaani; Akkadian sinitu > Hopi soniwa, as if Hopi soniwa descends from all of them, but that is not how I had it. In Stubbs 2015b, the key forms to consider are in bold, as in two sentences above. From these terms we can see that the original Semitic root consonants are s-n-w (clear in Arabic and Ethiopic), which three consonants are clear in the Hopi form as well.

(4) Because Semitic forms are based on consonantal roots of usually 3 consonants (sometimes 2 or 4 or 5), Semitists do not see vowel variations as invalidating what identical consonants offer. For example, the root ђrm 'to be sacred, forbidden' is foundation to many vowelings of words for 'woman, wives'—Arabic *ђuram, ђurm, ђurma, ђaram, ђarama, ђariim, ђirma*; plurals: *ђaraamaa, ђuraamaa, ђiraamaa*, and ma-prefixes: *maђrama, maђruma*—and all 12 vowelings mean 'woman, female(s)'. For this reason, consonantal roots, not vowel variation, anchor cognate relationships in comparative historical work in Semitic, especially since only fractions of the ancient languages are attested. So to suppose, for example, that the UA forms, Warihio *oerume / oorume* 'woman', do not reflect Semitic *ђrm* 'woman' for lack of an attested voweling would be ignoring the comparative historical method, especially in light of the fact that pharyngeal ђ always shows rounding w/o/u in UA. Leonid Kogan (2011, 119-123), a prominent Semitist, says it well in noting a "wide variety of unpredictable deviations in

the vocalic domain in glaring contrast to the full regularity of the consonantal skeleton." So in the Semitists' tradition, I often bring in various Semitic forms to the discussion, but UA is not being compared to Proto-Semitic, but to something of a Hebrew-Aramaic mix.

Sixth, he states, "long-distance relationships are less likely to include a large number of similarities. The sheer number of similarities in Stubbs' proposal is not likely for the type of linguistic scenario presented" (p. 263). So too many similarities invalidates the case? Of course, again he is mistakenly assuming that I am lumping Afro-Asiatic and UA in a long-distance relationship. True, a time-depth of 7,000-10,000 years would yield fewer similarities. However, the bulk of 2015b identifies much vocabulary, fitting a system of sound correspondences, that accords with a language or dialect of Northwest Semitic (Aramaic-Hebrew, 700 sets) mixed with a substantial amount of Late Egyptian (400 sets), not Middle Egyptian, nor Old Egyptian, nor Afro-Asiatic, but specifically Late Egyptian, exhibiting the Late Egyptian definite article prefixes, which had not yet developed in Middle Egyptian (Stubbs 2015, 137-8). For non-linguists, I might clarify that a long-distance relationship means a deep time-depth, usually connecting language families; so UA compared with a Hebrew-Aramaic dialect is geographically long-distance, but not linguistically a long-distance relationship.

Seventh, he says "lexical similarities are often used as evidence for genetic relationships between languages" (p. 263), then in footnote 17 he adds, "but these are far from convincing; see Campbell and Poser 2008, 165-172." Lexical similarities are a lesser or larger part of every demonstration of language relatedness, though morphology and other factors are important too. In the Campbell-and-Poser book that my friend Lyle Campbell sent me, pages 165-72 refer to lexical similarities (1) of limited amounts (because any two languages can have some accidental sound-alikes), (2) without additional supporting evidence like sound correspondences, and refer to (3) long-range comparative linguistics, the primary example cited being Greenberg 1987, who is a lumper (unlike me) and lumps language families on lexical similarities without sound correspondences. So none of the three applies to my work! My lexical similarities are based on a system of sound correspondences, and they are many, and they do not involve a long-distance relationship, but show one language family with considerable language contribution from specific languages, at a fairly shallow time-depth of 2500-3000 years.

Eighth, Rogers objects to my straying from the usual focus on Proto-Uto-Aztecan or the whole language family to intermittent focus on certain UA languages, which he claims results in "cherry picking the data to fit the proposal" (p. 262). No, there is no cherry picking. What happens (and it happens in every language family) is that some ancient words leave related cognates in most of the descendant languages, while other ancient words survive in only a few languages or only one. I list all cognate / descendant forms available for each established UA cognate set in 2015b (as in 2011), sometimes many and other times a few or one, along with the Semitic/Egyptian form or root aligning with the sound correspondences. For example,
Hopi soniwa 'beautiful, bright' (< Semitic snw 'gleam, be beautiful'), and
Hopi hoonaqa 'drunkard' (< Egyptian ḫnqt 'beer'; n'-ḫnqt 'the-drinkers'; no vowels provided in Egyptian, but the round vowel for the initial pharyngeal is exactly as expected in UA)
These two exist only in Hopi, and in no other UA languages. Yet such impressive matches deserve to be listed. Does Rogers think I should leave out single language matches?

Furthermore, not many UA cognate sets appear in all 30 UA languages, but from among the two dozen or so that do, a high percentage of them (ca. 70%) belong to the Near-East contribution. That is significant, as it suggests that the Near-East language component was part of Proto-UA. Some might contend that such could not be the case given UA's supposed glottochronological time-depth of 5,000 years or so, but Campbell and Poser (2008, 167), in the same book that both Rogers and I cite above, also say "It [glottochronology] has been rejected by most linguists, since all its basic assumptions have been challenged."

Ninth, Rogers claims to see "mistaken definitions or incorrect characterizations of linguistic concepts" (p. 260). That is odd, because the best 35 Uto-Aztecanists in the world, most being PhDs in Linguistics, have all received the work by now, and none of them spoke of incorrect characterizations of linguistic concepts. Through 40 years of presenting at professional linguistic conferences and publishing in several journals, I have never been accused of mischaracterizing linguistic concepts. Two different editors of the *International Journal of American Linguistics* (the most prestigious journal for publishing comparative Native American work, in which I've published four articles) both said (20 years apart) that I do good work. The late Jane Hill at an annual UA conference said, "Brian is the only one of us who does a comparative paper every year" (because a grammatical aspect of one language is easier than dealing with 30). I was invited to give a lecture at UCLA on comparative Uto-Aztecan, and Calvert Watkins, Harvard's internationally renowned Indo-European scholar, happened to attend, and afterwards he told Dr. Munro (the prominent UCLA linguist, accomplished in Uto-Aztecan, Yuman, Muskogean, and Zapotecan) that "we need more lectures like that one." When MIT decided to publish a volume on UA, the other Uto-Aztecanists voted me to do the first article to introduce the language family with a comparative overview (Stubbs 2003). When the *Society for the Study of Indigenous Languages of the Americas* decided to do a special session on UA to celebrate the centennial since Sapir's establishment of UA in 1915, the other Uto-Aztecan specialists selected me to present the lead paper to begin the session (Stubbs 2015a). That is an unusual list of honors for one mischaracterizing linguistic concepts.

Tenth, he calls my work "so replete with disorganization" (p. 260). Organization may be in the eye of the beholder. The organization proceeds from an introduction, then systematically addresses the sound correspondences, then shows how Semitic or Egyptian provides the underlying forms that explain seven of nine phonological puzzles that UAnists have not been able to solve in the last 100 years, since Sapir's establishment of the language family in 1913 / 1915 (interestingly my work was published exactly a century later 2015), then vowel correspondences, then addresses medial consonant clusters, then shows grammatical and morphological parallels, and then discusses unusual semantic combinations preserved in UA. He may prefer a different organization, but I see nothing radically awry in my organization.

Eleventh, he says that my two books under review are not substantially different (p. 260). Most who examine the two would disagree. The larger work (2015b), with 20 times more detail, is for linguists, Semitists, and other scholars, and establishes the tie linguistically. The smaller work (2016, *Changes in Languages from Nephi to Now*) is greatly simplified for lay-readers, is one-fifth the size, and contains in reduced / simplified form 1/20th of the data, and addresses the data's relevance to the Book of Mormon.

Twelfth, Rogers' condescending attitude and derogatory language are apparent throughout: e.g., "it is so replete with disorganization, numerous assumptions, mistaken definitions or incorrect characterizations of linguistic concepts, inexact methods, pedantry, and apologetic rhetoric that the idea [of the language tie] seems dubious, even without careful scrutiny" (p. 260). Wow, that could leave one with a very negative impression, yet the non-LDS linguists, who are the best UA specialists in the world, and Semitic scholars said no such thing, but responded with favorable comments or "no comment." Like Robertson (2017) says in his review of 2015b, that it includes many of the same cognate sets and the same quality of work as my 2011 *Uto-Aztecan Comparative Vocabulary*, which was praised in the *International Journal of American Linguistics* review (Hill 2012).

Thirteenth, he even hints at disdain for the appendices: "Other information of varying usefulness to the proposal itself, but which seems personally significant to Stubbs, is presented in the remainder of both books through a number of appendices" (p. 260). In the 2015b book the appendices are:

A sound correspondences (very important),
B English index to the sets,
C Semitic index to the sets,
D Egyptian index to the sets

It should be obvious that the appendices are indices, helpful in finding forms in the massive 435-page, 365,000-word work. Each of the appendices to *Changes in Languages from Nephi to Now* (2016) is also relevant, either to a particular chapter, a group of chapters, or to the whole book.

Fourteenth, on p. 261, Rogers says, "A proposal for a genetic relationship … must be supported by two types of evidence: (1) evidence that the languages discussed are in fact genetically related." That is exactly what the 435-page work (Stubbs 2015b) does: it establishes that a significant amount of Proto-UA descends from the specified Near-East infusion, with sound correspondences, morphological parallels, unusual semantic combinations preserved, and other parallel patterns. It is not genetically related at a bi-family level, but is descended from a sizable Near-East infusion mixed with other. He continues, "and (2) evidence for the reconstruction of the common linguistic ancestor." His footnote 14 lists a couple of books on historical linguistics, both of which I have and have read, in addition to others. Again, he is insisting on the reconstruction of a non-existent ancestor of Proto-Afro-Asiatic and UA, but that is not what I am proposing, but rather, Aramaic often provides the needed reconstructed form, given the devoicing of voiced stops (b > p, d > t, g > k):

- (1274) Aramaic kookb-aa(') 'star-the' > UA *kuppaa': Sr kupaa' 'to shine (as of the stars)'
  (a denominalized verb, all vowels as expected; Sr v < *-p-, so Sr p < *-pp- or cluster)
- (889) Aramaic rikb-aa(') 'upper millstone-the' > UA *tïppa 'mortar, pestle' (initial r- > UA t- is well demonstrated in 2015b, 100-101, 173-174, 221)
  (note that both of the above show the same cluster -kb- > *-pp- in UA, common in UA)
- (618) Aramaic di'b-aa(') 'wolf-the' > UA *tï'pa 'wolf'
  (not from Hebrew haz-zə'eb 'the-wolf')

- (617) Aramaic diqn-aa(') 'beard-the, chin-the' > UA *tï'na / *tï'ni 'mouth'
  (consonants and vowels align with Aramaic, not from Hebrew zaaqaan 'beard, chin')
  (also note in the 3 above, the vowel assimilation *-i-a > UA -ï-a is natural and common)
- (616) Aramaic dəkar 'male' > UA *taka 'man, male, person, self, body'
  (aligns with initial d of Aramaic, but vowels of Hebrew zaakaar 'male, man')
  (note the 3 above and several others all suggest Aramaic d > UA t, not from Hebrew z)
- (1130) Aramaic pagr-aa(') 'corpse-the' > Hp pïïkya 'skin, fur'
  (not from Hebrew hap-pɛgɛr 'the-corpse')
- (1403) Aramaic šigr-aa(') 'drain, ditch, gutter-the' > Hp sikya 'ravine, canyon of sloped sides'
- (743) Aramaic tuumr-aa(') 'palm-the / date-palm-the' > UA *tu'ya 'type of palm tree':
  (aligns with Aramaic, but not Hebrew taamaar)
  (note in the 3 above that -r- as 2nd consonant in a cluster > -y-: *-Craa > -Cyaa)
- (967) Aramaic qušṭ-aa(') 'bow-the' > UA *kuCta-pi 'bow'
  (usual loss of s in a cluster, again from Aramaic, not from Hebrew qešet / qašt- 'bow')
- (1409) Aramaic kuuky-aa(') 'spiderweb' > UA kukyaC: Hopi kookyaŋw 'spider'; Cp kúka-t 'blackwidow spider'
  (note 9 of the 10 nouns above show Aramaic suffix: -aa 'the')
- (559) Hebrew bky/ baakaa^y 'cry, weep' (perf stem); Aramaic bakaa / baka' > Hopi pak- 'cry';
  Tb pahaa'at / 'apahaa' 'cry, bawl, howl' (Tb h < *k); Ktn paka' 'ceremonial yeller, clown who shouts all day to announce a fiesta'.
  (Northern UA (Tb, Ktn, Sr, Hp) sometimes shows the glottal stop of written Aramaic -aa', which suffix Hebrew does not have. The Aramaic article suffix -aa(') 'the' has a written glottal stop, but debates continue whether it was pronounced or simply signifies the long vowel of the suffix. Northern UA languages often show that glottal stop, whereas Southern UA languages do not.)

So because the UA forms usually aligned so well with the Northwest Semitic form or Egyptian form, there seldom seemed a need for an identical or near identical reconstruction.

The number of matches with specific Aramaic forms means that the UA infusion was after Aramaic and Hebrew were clearly defined as separate Northwest Semitic languages. And Hebrew did not exist as a language until after Jacob's reentrance or Moses' entrance into Canaan to begin adopting the Canaanite language, for Hebrew is the Israelites' dialect of Canaanite after their adoption of Canaanite. And several UA terms specific to Israeli culture (ephod, Yahwe, etc.) suggest that the infusion included Israelite Hebrew. As for Aramaic, note that Abraham, Jacob, and Laban the Aramean (Genesis 25:20) and his daughters Leah and Rachel and maids (the mothers of future Israel) came from Aramaic-speaking areas. In addition, the Manasseh land grant in the Northeast corner of Northern Israel borders Aramaic regions, and Lehi, being of Manasseh, was likely descended from Josephite refugees to Jerusalem fleeing the destruction of Northern Israel a century earlier (Stubbs 2016, 75). Furthermore, Semitists like Young (1993, 54-62, 85-86) and Rendsburg (1997, 2003, 2006) believe that many northern Israelites may have been bilingual, never losing their Aramaic, if they even added Hebrew / Canaanite to their repertoire. Or even if they lost Aramaic at some point, reacquiring the international lingua franca in their proximity to neighboring Arameans is probable for a percentage of the population. For all those reasons, we should not be surprised if Lehi's and Ishmael's language(s) included a substantial amount of Aramaic, as the UA data suggest it did. Yet UA's preservation of some archaic phonology and old Hebrew and Aramaic forms points to at least pre-exilic. So all factors taken together suggest an infusion of language forms like the Hebrew or Aramaic of 1300-600 BC, which also approximates the Late Egyptian period. Thus, nothing as far back as Proto-Afro-Asiatic is suggested or possible, which should be apparent from reading either book (2015b, 11-12, 34-35, 66, 320-322, 343-4, 357-9; or 2016, 64, 71-73, 125-27).

Fifteenth, Rogers says, "One of the main methodological issues of Stubbs' proposal is the omission of an explanation for why the UA and Afro-Asiatic languages are being compared in the first place" (pp. 261-2). Again, I am not lumping UA and Afro-Asiatic as related language families, but am dealing with an infusion or substantial borrowing from Northwest Semitic (Hebrew/Aramaic) and Late Egyptian into UA. In the next paragraph, he repeats his concern, "Stubbs' proposal sidesteps this issue and suggests that the putative similarities are the evidence that these are related languages, but without explanation for why specific languages are named and used in the comparison." Is he expecting me to depend on archaeology or other extra-linguistic evidence to point to the language selections? The languages themselves are the best source for finding whether languages are related or not! Sir William Jones noticed the similarities among key Indo-European languages (Sanskrit, Greek, Latin, Germanic, Celtic) simply because he was familiar with the languages (Campbell and Poser 2008, 5-6), not because something else told him: those are the languages you need to look at.

Sixteenth, Rogers (p. 262) also says, "this [Semitic speakers coming to the Americas] does not limit their contact to the UA languages, perhaps they intermingled with speakers of the Chibchan languages in South America (among other possibilities)." I never suggested that the UA case means that Lehite posterity did not also intermingle with other language families. In fact, in Stubbs 2016, I say the opposite several times, that they probably did mix with many language families, and the appendices (of varying usefulness) D, E, and F are included for the very purpose of showing how easily and widely an ethnic infusion can mix far and wide. Rogers has me saying things that I never said nor suggested.

Seventeenth, a lot of double-speak may sound academic, but "unravels with scrutiny." For example, he claims, "each similarity must be rigorously proven to be both valid and reliable. Many, if not most, similarities in this proposal are not accompanied by the necessary explanations to make them either valid or reliable" (p. 263). Valid **AND** reliable? Can it be valid but unreliable, or reliable and not valid? If it's one, it's both. But doubling up and then saying it is neither helps make it sound very bad. Furthermore, this criticism is somewhere between hyper and unfounded. When needed, explanations are provided. Though a few spots might benefit by more, it was my explanations that have pleased the UA specialists: as Hill (2012) said, "Each set is discussed in some detail and the serious comparativist will delight in the discussions." After explaining that Semitic b > UA *p, how much explanation is needed to show that:

- Hebrew boo' 'way to' parallels UA *pooC 'road' (C means unknown consonant)
- Semitic baraq 'lightning' parallels UA pïrok 'lightning' (and the vowel changes are explained)

- Semitic baka' 'cry, he cried' parallels UA *paka' 'cry'
- Hebrew batt 'daughter' parallels UA *pattï 'daughter'
- Aramaic bəquuraa 'livestock' parallels UA *pukuC 'domestic animal' (and vowel changes are explained); etcetera for more than 1,000 parallels.

Eighteenth, he arranges my data to suggest things I never said. In addition to set 13 addressed above, he also presents at table 3, set 1, the plural suffix as Semitic *-iima > Hebrew -iim > UA *-ima, and then says that an explanation is needed for why the final -a disappeared in Hebrew but then was reinserted in UA (p. 264). The book explains that the Hebrew Bible was voweled by the Masoretes AD 700 or so, nearly a millennium and a half after Lehi. UA did not reinsert -a, but Northwest Semitic *-iima > UA *-ima, and Semitic *-iima > Hebrew -iim. So both derive from the older Northwest Semitic form, not one from the other. In fact, items like this point only to Northwest Semitic *-iima, because Arabic -uuna, Akkadian -uu, etc, exclude other Semitic languages, removing it far from Proto-Afro-Asiatic (see Stubbs 2015b, 66). The great UAnist, Wick Miller, my professor, agreed with my reconstruction of PUA *-ima. Most before me had reconstructed UA *-mï, but they all neglected to consider that five UA languages have a high-front vowel (i or e) before -m and other matters. Though Wick Miller did not like and refused to consider my proposed Near-East tie, he could not refute it, and he agreed with various points that I brought to his attention, as long as I did not mention the Semitic source of my insights. Miller was kind to me, valued my abilities, and was pleased with and encouraged my comparative work in UA.

In Table 4, set 3, Rogers calls for explanations why š > s (p. 264). I explain elsewhere that all three kinds of Semitic s (š, ś, s) merged to PUA *s. If I explain it in every case that occurs, the book would have been even larger and too repetitive. The merger of those three is also apparent in the appendix of "varying usefulness" that lists the sound correspondences. This set also points specifically to Hebrew yšb, and not to Aramaic ytb nor Arabic/Proto-Semitic wθb, again far from Afro-Asiatic. The most interesting aspect of this set is that Masoretic Hebrew yaašab has been determined to be from an earlier pre-Masoretic Hebrew *yašiba, another older voweling found in UA. In addition, yašiba 'he sat, dwelt' is 3rd person singular perfect, while yašibuu 'they sat, dwelt' is plural; and in the Piman branch of UA we see the plural voweling and the plural meaning *yasipu 'they sit/dwell'. Rogers also says that changes in vowel length need explanation. That would be nice, but vowel length has not yet been figured out for PUA, as various layers of changes in stress patterns in the various branches and languages caused the lengthening of stressed vowels and the shortening or loss of unstressed vowels, but the sorting through those multiple and changing layers has not been accomplished yet. So only vowel quality is reconstructed for UA, not vowel quantity, explained twice (in Stubbs 2015b, 12, 37).

Nineteenth, Rogers asserts, "while the UA language family is one of the most studied language families in the Americas, as is the Mesoamerican cultural area, the fact that very little is done to connect the proposal back to this previous scholarship is thus odd" (p. 266). That ranks among the most rank of his misstatements. I wrote the book on comparative UA. It includes and builds on the viable previous linguistic scholarship. If he has in mind cultural, archaeological factors, etc., the other major linguistic works on UA have not included that either. Miller (1967) wrote *Uto-Aztecan Cognate Sets* with 514 sets. Miller later collected others, and Kenneth Hill added another 400 sets to total some 1200 sets on a UA computer file. The next publication was my 2011 *Uto-Aztecan Comparative Vocabulary* with 2700 cognate sets. Kenneth Hill, another great Uto-Aztecanist, wrote a positive review in the *International Journal of American Linguistics* (Hill 2012); and UAnists have spoken highly of that work ever since its first preliminary edition in 2006. Besides cognate sets, it has sizable sections treating UA phonology, from PUA phonology to the branches and some individual languages, discussing the previous research, verifying or adjusting what the past UA scholars have proposed. I cite the literature or the "previous scholarship" with a new look at a lot more data, thus enabling me to further verify some previous views and improve others. True, the 2015b work does not include a lot of the comparative detail / findings of the 2011 work, except when helpful or applicable.

Twentieth, his math and statistics on page 265 are creative (wrong). If a set's match is a coincidence, it does not matter whether the UA cognates in that set number 30, 15, or 2, the one set might be subtracted from 1528 (i.e., 1528-1 = 1527), but not 30 or 90 subtracted for each set. Even if the whole book were wrong, the total of valid sets would be 0, not negative / — 2,598. Furthermore, when the vocabulary are consistent with an established system of sound correspondences, those within that framework are not counted as accidental matches.

Twenty-first, moving ever further from probabilities of coincidence are lengthy matches: the longer a match, the less likely it could be by chance, and this tie exhibits many lengthy matches. An eight-segment match is:

(567) Hebrew ya'amiin-o 'he believes him/it' > UA *yawamin-(o) 'believe (him/it)' (the sound change ' > w is established; given 13 consonants and 5 vowels in UA, probabilities of such a match by chance are less than one in 17 million: 1/13 x 1/5 x 1/13 x 1/5 x 1/13 x 1/5 x 1/13 x 1/5.)

A few among other lengthy matches which are 6- and 7-segments long include:

- (853) Aramaic ђippušit 'beetle' > UA *wippusi 'stink beetle' (both have geminated -pp-; and both pharyngeals (ђ and ʕ below) result in UA rounding (w), as also Greek o < ʕ of Phoenician)
- (87) Arabic ʕgz / ʕagaza 'to age, grow old (of women)' > Tr wegaca- 'grow old (of women)'
- (57) Semitic singaab 'squirrel' = Hebrew *siggoob 'squirrel' > UA *sikkuC 'squirrel' (vowel changes are explained in the book and devoicing of g > k)
- (88) ʕalaqat 'leech' > UA *walaka 'snail'
- (892) ṣanawbar 'stone pine' (type of pine) > UA *sanawap 'pine tree'
- (832) *sarṭoon 'scratcher, crab' > *saCtun > siCtun / *suCtun 'claw, nail, crab'
- (1274) kookb-aa(') 'star-the' > UA *kuppaa' 'to shine (as of the stars)' (-kb- > -pp-)
- (614) makteš 'mortar' > UA *maCta 'mortar'; Ca *mattaš 'crush, squash, vt' (with *-tt- and -š)

Twenty-second, he thinks onomatopoeia (sound imitation) explains items like:

Arabic ṣurṣur / ṣurṣuur 'cricket'; Aramaic ṣarṣuur 'cricket'; Akkadian ṣarṣaar 'cricket'; Syriac ṣiṣr-aa / ṣiiṣr-aa 'cricket' and UA *corcor (or tsortsor) 'cricket' (pp. 264-5).

Possibly, but it is an impressive parallel with Arabic or Aramaic (after vowel-leveling) or an unattested ancient Hebrew form (cannot always specify a single language): it is six segments long, and I explain the change of ṣ > ts. It seems to me that sound imitation of a cricket would sound more like English ‘cricket’ or chichi with high-front vowels. While ṣ or ts fits cricket sounds, neither round vowels (o/u) nor r sound very cricket-like to me. Does he think the Semitic forms are also due to onomatopoeia? He might even disqualify the Semitic terms as a Semitic cognate set: the vowels do not match; there is no standard correspondence of u:a:i for these Semitic languages, but with the consonants corresponding, no Semitist doubts their relatedness.

Mr. Rogers once came close to saying well when he said, “Stubbs purports to provide some insight into the unknowns of Uto-Aztecan grammar” (p. 260)—if he had only left out ‘purports’. For this language tie does some provide profound insights into UA; in fact, perhaps the most impressive contribution of this tie to comparative UA linguistics is its being able to explain 7 of 9 puzzles that UAnists have not been able to solve over the last 100 years. For example, UAnists suppose that PUA initial *t- remained t- in all UA languages, except in Tarahumara (Tr), in which Tr *r- corresponds to UA *t- of the other languages. However, there are as many instances of initial Tr t- also corresponding to PUA *t- of the other languages. No one could explain the split or discrepancy through four generations of linguists. Yet the underlying Semitic and Egyptian provide the solution. Initial r- in Semitic or Egyptian became PUA *t-, probably because of contact / mixture with a people who did not have initial r- in their language’s phonological repertoire, only initial t-. However, Tr did keep the r-. So Tr’s showing both r-/t- corresponding to PUA *t is explained by the fact that Semitic and Egyptian t, ṯ, d > Tr t-, while Semitic r- and Egyptian r- > Tr r-. So simple! Some 40 Tr terms agree with and explain that distinction. Another matter is that PUA *w > Hopi l before low vowels a, e, ö much of the time, but not all the time. In many instances PUA *w remains Hopi w. Again, no one has been able to explain the dichotomy the last 100 years. Yet again, Semitic and Egyptian provide the solution. Many PUA *w are from Semitic / Egyptian pharyngeals / laryngeals ʕ, ḫ, ’. Those PUA *w from the Semitic / Egyptian pharyngeals / laryngeals became l before low vowels, while PUA *w from Semitic / Egyptian w, remain w in Hopi before those same vowels, as in Hopi soniwa < Semitic snw, mentioned previously. Pharyngeals’ becoming liquids (r, l) happens in some Arabic dialects also, as I’ve heard a native Syrian Arabic speaker say sabriina < sabʕiina ‘seventy’. The underlying Semitic and Egyptian clarify not only those two issues, but five other previously unresolved matters as well. That is huge—that Semitic and Egyptian explain seven of the nine, possibly eight of the nine, previously unresolved phonological puzzles of UA! How could that be, if the tie were not valid?

Twenty-third, speaking of mischaracterizations, Rogers claims that “any connections between Mesoamerican languages and South American languages have been definitively disproved” (p. 266), and footnote 21 “For an overview, see Campbell 1997.” We will overlook the fact, as Rogers seems to have done, that both the Chibchan and the Arawakan language families are spread into both Central America and South America, though not all definitions of Mesoamerica include all of Central America. Nevertheless, disregarding those two language families, one can say that no such connections have yet been demonstrated to the satisfaction of a majority of linguists, but one cannot say that a viable proposal will never emerge from such a huge arena of far-from-fully-explored potential (190 language families), or that all pertaining to futurity must be automatically rejected, in all future proposals' being hereby / previously disproved. Rogers refers us to Campbell (1997), but Campbell says no such thing. I have read twice Campbell's (1997) *American Indian Languages: The Historical Linguistics of Native America*, and Campbell leaves open a few possibilities. First of all, most controversial proposals are on a spectrum, opinions ranging from—demonstrated, probable, maybe but not fully demonstrated, not likely, not close. Campbell provides his own assessments of several such proposals, giving a number within a 200-point range from +100 (definitely proven) to -100 (definitely not). Campbell (1997, 326) gives Misumalpan (in Nicaragua, Honduras, El Salvador) with Chibchan (South and Central America) a +20 (120/200 = 60% chance). He gives much lesser probabilities to Tarascan-Quechua (5%, p. 325) and Maya-Chipaya (10%, p. 324), the latter of which Campbell (1973) was the main critic after others had viewed the proposal favorably. I do not support any of the above, yet to none of the above does Campbell give a 0% chance, as he does to some other proposals; and thus his assessments, though not supportive, are far from saying that all such possibilities are "definitively disproved"; i.e., none is at 0%; nor does he say that all future proposals are "definitively disproved." In fact, in ways I am a stricter judge than Campbell (1997, 269-273), who gives the UA-Tanoan tie a 50% possibility. In addition to my 40 years in UA, I spent some years investigating the Kiowa-Tanoan (KT) language family, and had compiled the largest Tewa dictionary in existence. The tribe asked that I not publish it, so I dropped working on it; twenty years later another larger work appeared, whether with permission or not, I don't know; nevertheless, I am quite familiar with UA and KT, and with the UA-KT debate. Their grammars are very different, and the limited lexical similarities look much more like areal loans (among the Ancient Puebloans) than genetic affinity. In fact, in Stubbs (2011, no. 613) I track one such areal UA loan into Keresan: UA *posi 'bear' > Piman *wohi (*p > w, *s > h), which is borrowed into Tr/Wr wohi/gohi/ohi, which is a loan consideration for Keres guhaya 'bear' (< *gohi). I would give a possible UA-KT genetic tie 10- 20%, much less than Campbell's 50% possibility of a genetic tie. I respect Campbell as a foremost authority in Native American historical linguistics, as his publications demonstrate, and I agree with him most of the time, so this slight difference of opinion in areas in which I may be the more familiar, is hardly a criticism of him, but I simply give a possible UA-KT genetic tie less promise than he does.

What did surprise me was Rogers' use of Edward Sapir's (1925) "Hokan Affinity of Subtiaba in Nicaragua" to exemplify that "long-distance relationships are convincingly determined through submerged features" (p. 263), when Campbell (1997, 208 and 324-5) cites Rensch, Suarez, and Kaufman as superseding Sapir and says that "it is now clear that Tlapanec-Subtiaba is just one more branch of Otomanguean" and gives that tie a 95% probability. So not only is the Hokan-Subtiaba tie discounted by Campbell, but Hokan itself is a hypothesis "still undemonstrated and controversial" (Campbell 1997, 68).

Twenty-fourth, I disagree with most of Rogers' last two paragraphs, but let's address only his closing sentences: "We simply do not have any recorded information about the language(s) being used by the people in the Book of Mormon (other than a small amount of information about the class of priest-scribes). Without that information, any suggestions of linguistic affinities are wildly speculative and should be dismissed" (p. 267). While I agree that all branches of Lehite posterity were thrust into multilingual environments, which tend toward language change or loss, that does not mean that no trace could be left anywhere, or that actually finding a sizable body of similarities must be labeled as "wildly speculative" and should be automatically dismissed. It is better to consider the evidence before going with the assumption that it would be "wildly speculative and should be dismissed." Furthermore, we do have limited information about the languages of the people. At the beginning are mentioned Egyptian and the learning of the Jews (1 Nephi 1:2) and at the end of their history a millennium later, both writing systems—Hebrew and Egyptian—had been altered according to their manner of speech (Mormon 9:32-34). Only dead languages (if written) do not change (vs. all living / spoken languages which do change): the dead language Latin vs. spoken Italian, French, Spanish, etc; the liturgical Hebrew studied and read by Jews the world over, though recently revived in Israel in quite a different form; the Classical Middle Egyptian was often written in the fossilized former form with little change, while the people's later spoken forms of Egyptian changed according to their manner of speech. So in light of Moroni 9:32-34, we must leave open the possibility, if not probability, that elements of those ancient languages had continued into whatever language mixture(s) had developed by then, even if in ever-decreasing amounts. Whether only the educated priest-class or also the commoners could read, some kind of continuation from those languages seems to have been recognized among them. And surely they were a scripture-reading people during those centuries of peace and righteousness, for Christ told them to search the prophets (3 Nephi 23:5) and to search the words of Isaiah (3 Nephi 23:1); and only a century earlier, the rulers of Ammonihah burned both the righteous and their scriptures (Alma 14:8). So it seems that a number of common people were also able to read their scriptures and search the prophets (Isaiah in Hebrew) besides the so-called "class of priest-scribes." There I go again, in "apologetic rhetoric"—as if there is anything wrong with reason defending faith.

I searched Rogers' article for a valid criticism or helpful suggestion, not entirely in vain, for I did come away with the realization that his reference to "disorganization" may have been partly due to a general sense of 2015b seeming 'unfinished'; that's because it **IS** unfinished. As I say in (2016, 188), "Only when I die do all drafts become final drafts." Such massive reference works as 2011 and 2015b can hardly be finished in one lifetime. Though working on both for 30 years, I can look at any page of either, and see improvable wordings, a typo, or matters inviting further investigation. The UAnists at each annual conference from 2000 to 2011 heard me say that I hoped to finish the comparative vocabulary by next year. After three preliminary editions in 2006, 2007, 2008, finally in 2011 the hardbound published edition appeared. Likewise, many wondered for decades when I would have the full measure of the Semitic and Egyptian in UA out and available. Massive reference works always take years longer than expected, and I finally realized that it may take more years to complete than I have left—there is no end to unfinished trails and questions that many data lead to—but after 30 years of assembling, I decided I had to simply impose an arbitrary breaking point and call it a decent plateau. Yet even rounding out or finishing / presenting the content of that arbitrary cut-off took five years. If I were to attempt finishing the book to perfection, I'd expire first, and then nothing would be available. So what's better—an unfinished plateau with lots of data to fill decades of others figuring out other things, or having nothing? That's what I decided too. The published copies of 2011, 2015b, and 2016 are all but sold out and await 2nd editions. I shudder at the thought; and realizing that "only when I die do all drafts become final drafts" offers little hope for conclusion and rest.

More could be said of Rogers' review, but let that suffice for the moment, as I now feel for him. Whether or not he eventually realizes the value of what is actually quite sound and will stand the test of time, most others will … with time. No one knows everything, or gets everything right all the time; so those who associate with him, be nice to him.

References

Campbell, Lyle. 1973. "Distant Genetic Relationships and the Maya-Chipaya Hypothesis." *Anthropological Linguistics* 15(3):113-135.

Campbell, Lyle. 1997. *American Indian Languages: The Historical Linguistics of Native America.* Oxford: Oxford University Press.

Campbell, Lyle. 2011. "Review of the Dene-Yeniseian Connection, ed. James Kari and Ben A. Potter." *International Journal of American Linguistics* 77(3), 445-451.

Campbell, Lyle, and William John Poser. 2008. *Language Classification: History and Method*. Cambridge: Cambridge University Press.

Elzinga, Dirk. 2016. Review of Brian D. Stubbs, *Exploring the Explanatory Power of Semitic and Egyptian in Uto-Aztecan.* Provo: Jerry D Grover Publications, 2015. BYU Studies Quarterly 55:4, 172-176.

Hill, Kenneth C. 2012. Review of *Uto-Aztecan: A Comparative Vocabulary*, by Brian Stubbs. *International Journal of American Linguistics* 78/4 (2012), 591-3.

Hrozny, Bedrich. 1917. Die Sprache der Hethiter: Ihr Bau und ihre Zugehorigkeit zum indogermanischen Sprachstamm. Leipzig: Hinrichs.

Kiparsky, Paul. 2015. "New Perspectives in Historical Linguistics." In Bowern, C.; Evans, B. (eds), *The Routledge Handbook of Historical Linguistics*. London and New York: Routledge, 64-102.

Kogan, Leonid. 2011. "Proto-Semitic Phonetics and Phonology." In Weninger, Stefan, and Khan, Geoffrey, and Streck, Michael P., and Watson, Janet C.E. (eds.), *The Semitic Languages: An International Handbook*, 54-151. Berlin: De Guyter Mouton.

Miller, Wick R. 1967. *Uto-Aztecan Cognate Sets*. UCPL 48. Berkeley and Los Angeles: University of California Press.

Mixco, Mauricio J. 1978. *Cochimi and Proto-Yuman: Lexical and Syntactic Evidence for a New Language Family in Lower California*. University of Utah Anthropological Papers, No. 101.

Rendsburg, Gary A. 1997. Chapter 5 "Ancient Hebrew Phonology." Online offprint from *Phonologies of Asia and Africa*. Alan S. Kaye, ed. Eisenbrauns.

Rendsburg, Gary A. 2003. "A Comprehensive Guide to Israelian Hebrew: Grammar and Lexicon." Orient 38, pp. 5-35.

Rendsburg, Gary A. 2006. "Aramaic-like Features in the Pentateuch." Hebrew Studies 47, pp. 163-176.

Rice, Keren. 2011. "Review of the Dene-Yeniseian Connection, ed. James Kari and Ben A. Potter." *Diachronica* 28:2, 255-271.

Robertson, John. 2017. Review of Brian D. Stubbs, *Exploring the Explanatory Power of Semitic and Egyptian in Uto-Aztecan.* Provo: Jerry D Grover Publications, 2015. The Interpreter: A Journal of Mormon Scripture 25, 103-116.

Rogers, Chris. 2019. "A Review of the Afro-Asiatic: Uto-Aztecan Proposal."

Journal of Book of Mormon Studies, 2019.
Sapir, Edward. 1913, 1915. Southern Paiute and Nahuatl: a study in Uto-Aztecan, parts 1 and 2. Part 1, 1913 in *Journal de la Société des Américanistes de Paris* 10:379-425. Part 2, 1915 in *American Anthropologist* 17:98-120, 306-328, reprinted 1919 in JSAP 11: 443-88. Parts 1 and 2 reprinted 1990 in *The collected works of Edward Sapir 5: American Indian Languages*, William Bright, ed., 351-444. Berlin: Mouton de Gruyter.
Sapir, Edward. 1925. "The Hokan Affinity of Subtiaba in Nicaragua." In *American Anthropologist* 27/4: 402-435, 491-527.
Sieg, Emil, and Wilhelm Siegling. 1908. "Tocharisch, die Sprache der Indoskythen, Vorlaufige Bemerkungen uber eine bisher unbekannte indogermanische Literatursprach." In *Sitzungsberichte der koniglichen Preussischen Akademie der Wissenschaft*. Berlin, 915-932.
Stubbs, Brian D. 2003. "New Sets Yield New Perspectives for Uto-Aztecan Reconstructions." In *Studies in Uto-Aztecan*, Luis M. Barragan and Jason D. Haugen, eds., 1-20. MIT Working Papers on Endangered and Less Familiar Languages, no. 5.
Stubbs, Brian D. 2011. *Uto-Aztecan: A Comparative Vocabulary.* Flower Mound, Texas: Shumway Family History Services and Rocky Mountain Books.
Stubbs, Brian D. 2015a. "The Proto-Uto-Aztecan Lexicon: Distribution of Cognate Sets and Language Family Prehistory." Paper delivered at the annual meeting of the *Society for the Study of Indigenous Languages of the Americas*, January 6-11, 2015.
Stubbs, Brian D. 2015b. *Exploring the Explanatory Power of Semitic and Egyptian in Uto-Aztecan.* Provo: Jerry D Grover Publications.
Stubbs, Brian D. 2016. *Changes in Languages from Nephi to Now.* Blanding, Utah: Four Corners Digital Design.
Stubbs, Brian D. 2019. *Comparative Issues in Uto-Aztecan.* Paper presented at the 2017 Uto-Aztecan conference, Boise, Idaho, and later filled out to address all 7 of the 9 previously unresolved issues in UA that Northwest Semitic and Egyptian solve, and sent to 35 Uto-Aztecanists in 2019.
Vajda, Edward J. 2010. "A Siberian Link with Na-Dene Languages." In Kari, J.; Potter, B. (eds.) *The Dene-Yeniseian Connection*. Anthropological Papers of the University of Alaska, new series 5. Fairbanks: University of Alaska Fairbanks, Dept of Anthropology, pp. 33-99.
Young, Ian. *Diversity in Pre-Exilic Hebrew.* Tübingen: J.C.B. Mohr, 1993.