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## Language Findings Relevant to The Book of Mormon

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by Brian Stubbs

One Native American language family called Uto-Aztecan (some 30 related languages from the Utes in the north to the Aztecs in the south) appears to partially descend from a substantial Near-East infusion of Hebrew, Aramaic, and Egyptian. That claim has sparked the expected controversy. A brief review of the proposed evidence, then the positive and negative responses to the evidence, and then answers to the critics will provide some basics and directions to more information for those interested.

**The proposed evidence** and its backstory include an M.A. in linguistics, the coursework and comprehensive exams toward a PhD (ABD) in Semitic (Hebrew, Arabic, Aramaic), 30 years of research, several articles published in peer-reviewed journals, and three books on Uto-Aztecan. The first was a large reference book, *Uto-Aztecan: A Comparative Vocabulary* (Stubbs 2011), a new standard for comparative Uto-Aztecan linguistics, receiving a positive review (Hill 2012) and praises from specialists in Uto-Aztecan; the second, *Exploring the Explanatory Power of Semitic and Egyptian in Uto-Aztecan* (Stubbs 2015) unsurprisingly received a much cooler reception than the first, though both adhered to the proper linguistic comparative method. The third, *Changes in Languages from Nephi to Now* (Stubbs 2020, 2<sup>nd</sup> edition) summarizes some evidence and explains its relevance to the Book of Mormon, intended for LDS lay-readers, in contrast to the first two books full of linguistic detail for linguists and scholars.

**Responses** have spanned the spectrum. Positive reviews have come from linguists/scholars (30 or so) who have carefully scrutinized all the data, while the two negative responders skipped/missed much. Being well acquainted with Uto-Aztecan (UA) specialists after attending and presenting at the annual UA conferences for 30 years, I gave the first two books to 20 of the best. They all loved the first book (2011), but were mostly struck speechless by the second (2015); however, five of those top specialists in Uto-Aztecan, PhDs in linguistics, said things like “a strong case, sound, correspondences in order,” etcetera, but privately, not publicly. Most were silent, did not want to say anything; one said “no, couldn’t be,” but did not look at it much and offered no specifics to refute it. The first published reviews were positive, by Dirk Elzinga (2016), BYU linguistics professor, and John Robertson twice (2017, 2019), Harvard PhD in historical linguistics and retired professor and chair of the BYU Linguistics Department. People often ask what the non-LDS linguists are saying (also listed in *Changes in Languages*, Appendix J, and “Answering the Critics,” number 40 and note 37). Besides the five UA specialists above, Roger William Wescott, President of the Linguistic Association of Canada and the U.S., author of 500 articles and 40 books, spoke well of the work, called it good evidence for a UA and Near-East language tie. David H. Kelley, Harvard PhD who published in anthropology, linguistics, UA, and contributed to the decipherment of the Mayan glyphs, said upon receiving an early draft sent him by John Sorenson: “The thick thing came in the mail and I did not want to tackle it, but dutifully opened it, intending to look at a page or two. However, I started to read and ended up reading the whole book. It is the most interesting and significant piece of research I have seen in years.” Mary Ritchie Key, linguistics professor at the University of California at Irvine and specialist in Native American languages, lauded the work. Four years later, two linguists tried to discount the tie (Rogers 2019; Hansen 2019), but the article “Answering the Critics” (Stubbs 2020) lists 44 reasons

why their objections are flawed, except that Hansen was right on one point. Of course, adjustments will continue, but no one has made a substantive argument against the case as a whole.

“**Answering the Critics** in 44 Rebuttal Points” (Stubbs 2020, available online with the Interpreter Foundation) addresses the objections of Rogers (2019) and Hansen (2019). Concerns from another reviewer were expressed more mildly and civilly, and deserve to be clarified with comparable kindness; furthermore, the answers will add more information than has been included in previous publications.

In *Times and Seasons*, January 6, 2019, Jonathan Green published a post “Uto-Aztecan and Semitic: Too Much of a Good Thing.” A commenter, Steve J, asked: “I hope Stubbs will at some point address the concerns expressed in the post.” Steve’s hope is justified and a response is rightfully due.

Green is kind and fair in his opening paragraphs on my background and credentials. Later in the comments, he is again more than decent in my defense. So this is nothing against Green, only a clarification that he and many readers may appreciate.

Under his number 1, Green’s concern is that Semitic’s morphological variety in verb forms make finding purported matches easier. Nouns are simple, but verbs are indeed more complex. However, even in that complexity, certain forms are more apt to survive or be borrowed than others, and it is those very forms that we find in Uto-Aztecan (UA). Verbal morphology is typically simplified in language mixes (Velupillai 2015, 29), such that participles and 3<sup>rd</sup> person forms (he/she) are the more frequently borrowed, and that is what we find in UA. No 1<sup>st</sup> or 2<sup>nd</sup> person (I, you) verb forms are found. Hebrew *ya’amin-o* ‘he believes it’ becomes UA *\*yavamino* ‘believe it’ and with the object (-o) in Hebrew, UA, and both translations. The regular sound change of ’ (glottal stop) becoming ‘w’ is established by some 20 other instances. The pairs’ length puts its probability of matching by chance at one in 17 million (given the sound correspondences and 1/5 for 4 vowels and 1/13 for 4 consonants all multiplied together; vowels are pronounced like Latin, Spanish, Polynesian, and most languages, but not English).

Green suggests that drawing on Aramaic and other Semitic forms increases the possibilities of random correlations. Perhaps slightly, but not near enough to annul proper methodology (the comparative method). Whenever a new Semitic language or Indo-European language is discovered, comparisons with the related languages are looked at to see how this new discovery best fits the known languages (“Answering” number 5), and the newcomer is often a combination of similarities: some things are like this language, others like that, etcetera, which is what *Exploring* (2015) explores. Furthermore, when many forms fit Aramaic, and not Hebrew, we are reminded that Aramaic was the language of Abraham, Isaac, Rebekah, Jacob, Leah, Rachel, and Laban, the Aramean (Genesis 25:20) before the later Israelites adopted Canaanite (Hebrew being Israel’s dialect of it). The Aramaic forms likely came from Israel’s original language, which was Aramaic, not Hebrew. Several Semitic scholars argue that northern Israel maintained Aramaic, while adding Hebrew / Canaanite to result in varying degrees of bilingualism in the north, and note Manasseh’s northeast quadrant next to the Arameans (discussed more in *Changes in Languages*, pp. 44–46; and “Answering” number 14). Various Semitic forms are discussed as probable cognates to a few unattested Hebrew or Aramaic forms, but those two are Israel’s Semitic languages.

Green suggests that with so much to choose from, it would seem “impossible not to find cognates.” Actually, early in my career I looked for Hebrew / Aramaic / Semitic or Egyptian in several language

families (each with few to dozens of languages): Athapaskan, 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, but I did not find such an array of similarities in dozens of other language families, like I found in Uto-Aztecan, so then I specialized in UA. If I were prone to imagine or see what was not there, it would have happened long before UA. Furthermore, Aramaic is so prominent in the data that we can hardly pretend the similarities do not exist. Note the forms below that match Aramaic wonderfully, but not Hebrew:

Aramaic *kookb-aa*(?) ‘star-the’ > UA \**kuppaa*’ (but not Hebrew *hak-kokab* ‘the-star’; ‘the’ is a suffix in Aramaic (-aa), but a prefix (*ha-*) in Hebrew; and UA shows the Aramaic suffix and vowelizing; the symbol > means ‘became, changed to’)

Aramaic *rikb-aa* ‘upper millstone-the’ > UA \**tippa* ‘mortar, pestle’ (initial *r-* > UA *t-* is the established sound correspondence; and note that both of the above, as well as others, show the same cluster *-kb-* > \**-pp-* in UA; in English also two adjacent consonants often absorb the first to double the second: *in-legal* > *il-legal*; *in-regular* > *irregular*)

Aramaic *di’b-aa* ‘wolf-the’ > UA \**ti’pa* ‘wolf’

(so also UA ‘wolf’ matches Aramaic, not Hebrew *haz-zə’eb* ‘the-wolf’)

Aramaic *diqn-aa* ‘beard-the, chin-the’ > UA \**ti’na* ‘mouth’ (as Aramaic, not Hebrew *haz-zaaqaan* ‘the-chin’; also note in the three items above that \**-i-a* > \**-i-a* is consistent and natural)

Aramaic *dakar* ‘male’ > UA \**taka* ‘man, male, person, self’ (like the two above, this is another of many examples of the sound change Aramaic *d* > UA *t*, but not from Hebrew *z*, or *zakar*)

Above are only five items. Hundreds of other Aramaic cognates are also found. Because they obey attested principles of sound change, they cannot be dismissed. Green also states: “the numerous proposed Egyptian cognates are troubling, as they increase the suspicion that a similar list could be found for almost any language.” The comparative method would not allow that, and a similar list was not found for the other hundreds of languages looked at.

Another concern Green mentions is that “the long list of cognates is actually a problem,” calling for a shorter list of stricter criteria. Identifying an established system of sound correspondences/changes (each phoneme/sound subject to a consistent/predicted change) is the ultimate strictness required by the linguistic comparative method, which eliminates general similarities or non-cognates, and the longer the list of cognates the stronger the case. If the sound changes are not strictly identified, then it is not considered a cognate set. Sound correspondences and cognate status eliminate chance

similarities. I have never heard of objecting to a long list of cognates, except by one who totally misunderstood what was being compared (“Answering” numbers 1, 2, 5, 6, 7, 15, 18).

Under his number 2, Green also wonders how a language’s basic vocabulary is found in the language mix, but not its accompanying grammar. Actually, that result is common in language mixtures. Media Lingua or Chaupi Quichua has almost exclusively Spanish vocabulary but Ecuadorian Quichua grammar (Velupillai 2015, 402); Ma’a/Mbugu has Cushitic basic vocabulary and primarily Bantu grammar; and Angloromani adopted largely English grammar and Romani lexicon. Velupillai (2015, 71) calls these G-L mixed languages, the grammar coming mostly from one language and the Lexicon (words) mostly from the other. The 3rd paragraph of Wikipedia’s statement on “Creole Language” describes what may have happened in UA: “The lexicon of a creole language is largely supplied by the parent languages, particularly that of the most dominant group in the social context ... On the other hand, the grammar that has evolved often has new or unique features that differ substantially from those of the parent languages.” If some Lehites were dominant culturally in their successive contact situations, we might expect a good amount of their vocabulary to be incorporated or retained, but little of their language’s grammar to persevere through contacts with the other populations. Then it would not be a matter of adopting pronouns (I, you, he, etc) as Green surmised, but that the Lehites kept their own pronouns and let the less dominant adopt and adapt. Grammar happens more subconsciously than lexical choices (see *Changes in Languages*, Appendix B “The Subconscious Mind’s Role in Language Acquisition”), so developments in grammar are essentially out of everyone’s control, and the grammar of the larger population(s) may be more influential. Reduplication (for plurals, repetition, intensification) is also more common in mixed languages than others (Velupillai 2015, 332), and that is exactly what we find in UA, that the fairly minimal reduplication in both Egyptian and Semitic is multiplied in UA.

In the second paragraph under his number 2, Green raises a valid issue that has puzzled me too—significant amounts of both Semitic and Egyptian vocabulary in the same semantic spheres. It does not invalidate the reality of the cognates, nor would it necessarily suggest that they were “fully bilingual in both Egyptian and a form of Hebrew” as Green stated; but it does make one wonder how it happened. Whether regularly reading records in Egyptian gradually brought some of that vocabulary into their language (as reading Latin brought Latin vocabulary into English via Latin’s dominance in academia for centuries) or whether other matters merit consideration, like facts that the leaders in the house of Joseph (Manasseh, Ephraim, and early descendants) were native speakers of Egyptian and their family records may have started (in Egyptian) before Hebrew was a written language, etcetera (more fully discussed in *Changes in Languages*, pp. 32–43). Also consider that UA preserves the phonology of ancient Egyptian better than Coptic does (a later form of Egyptian; *Exploring*, p. 343).

Also under number 2, Green asks another good question: “are they [reconstructions] merely schematic representations, a type of intellectual game, or do they represent a linguistic and historical reality?” The answer is both to some degree, which is why it is a good question. Reconstructed forms are linguists’ best guesses at what the ancient form looked like, yet they are right or nearly right, most of the time, because of an understanding of how sounds change and which directions of change are most likely, etcetera, after taking hundreds of language histories into account. On the other hand, new information does and should change opinions. In Indo-European, for example, reconstructions have changed over the years, often due to new information from newly discovered Indo-European

languages. Most of the reconstructed items discussed in *Exploring* (2015) have been scrutinized and refined by scholars over the years and accepted as reliable, and the few which I adjusted contain internal evidence justifying the adjustments and were deemed noteworthy in the 2011 work.

One curiosity is that Green mentions the single parallel of Quechua kumar ‘sweet potato’ and Polynesian kumara / kumala ‘sweet potato’ as evidence for contact (with which I agree), but sees UA’s 1500 Near-East parallels as not likely to “pan out.” Some scholars see the Quechua-Polynesian term as a coincidence, but may accept a dozen or so parallels. Yet progressively more scholars are accepting the ‘sweet potato’ pair as a genuine loan. Regardless the present state of DNA evidence for that contact situation, some DNA evidence between Uto-Aztecans and Near-East Arab populations has emerged consistent with the UA-Near-East language tie (see “Answering” number 27).

Under Green’s number 3, I do not understand his objection to English as a parallel example, because English retains most of its core vocabulary from the original Old English/Germanic. Exactly! Similarly, the Lehite element in UA retained much of its core Semitic vocabulary, while absorbing from neighboring / intruding languages. He likely sees Lehite as a foreign entity coming in to mix with an already established language, while I see it the other way, as other languages successively influencing an established Lehite language. Thus, Lehitese kept/retained their own pronouns and much vocabulary, while contact influences regularly diminished the percentages. Green is correct that Yiddish is basically a Germanic language with Semitic and other elements contributing some vocabulary, so his objection to Yiddish might be understandable, though again the original language of those Mediterranean/European Jews was Hebrew and some Aramaic, of which they kept some, but not a lot. While modern English retains only 15% of the Old English vocabulary and Yiddish shows a similarly low percentage of Semitic, the UA vocabulary may be 35–40% from the Semitic and Egyptian components (double the other two), and in the pervasive cognates (that are in 25 or more of the 30 UA languages) it is 60% (*Exploring*, pp. 344–345). More to the point, only 11 UA cognates appear in all 30 UA languages and 11/11 or 100% of those are of the Semitic-Egyptian data. I merely mentioned (for lay readers) that English and Yiddish are examples of language mixing wherein a small percentage of the original language is kept, yet UA retains more than either of those languages of whatever component we are highlighting in each.

It is worth responding to a few comments as well. Steve J’s first comment is cited above. In his second comment, he gently counters what Franklin had said in the previous comment: that I had reached a conclusion and then looked for evidence. As stated earlier, a search in dozens of language families typically turned up nothing, but caused a focus on UA, and as Steve J noted, I was totally surprised to see as much Aramaic and Egyptian as Hebrew (explained in the works cited).

Clark Goble also hits the nail on the head regularly: even with “a completely compelling argument, the implications of saying something good about it would be damaging career wise.” He also addresses the transoceanic evidence for the chicken and attending controversialities, even when done by non-Latter-day Saint scholars, and the magnification of opposition when done by Latter-day Saint scholars.

The many probabilities of the UA tie are tremendously in its favor. For example, semantic combinations, like Egyptian ‘serpent, partner’ to UA ‘snake, twin’ (*Exploring*, number 332; *Changes in Languages*, p. 68). What is the chance that two meanings so different (serpent and partner?) would also be found in the corresponding UA word, meaning ‘snake, twin’? And there are many more.

Most impressive is that nine unanswered phonological questions in UA have never been explained in the 100 years since UA was demonstrated by Edward Sapir (1913, 1915). Then after the underlying Semitic and Egyptian components were recognized, the Semitic-Egyptian data explain seven of the nine (*Exploring*, pp. 306–322; and “Answering” number 23). That is astounding! For example, one of those is the solution to why Tarahumara reflects UA \*t- as both r- and t-, about half in each category for some 40 items. Curiously, the previously unexplained variation is explained by the UA data’s alignment with Semitic and Egyptian r- vs. t-/d-. The probability of such an alignment by chance is the same as guessing 40 coin tosses in a row, or 1 in a trillion (1/2 multiplied to the 40<sup>th</sup> power). That kind of explanatory power is impossible, unless the case is valid.

Jonathan Green’s second large comment astutely summarizes publication challenges. He is right, that the audience for UA materials is very small. Five of us research in the whole language family of comparative UA; maybe 30–40 others work on a UA language; and maybe a few hundred others care or would be interested in reading anything on UA. Indeed, publishers must consider what will make them money; UA will not. Therefore, I self-publish. If I were to wait for a publisher to be willing to lose the money, I may expire first.

Regarding Clark Goble’s astute observations on transoceanic and Book-of-Mormon-friendly research, there is no doubt that if a reviewer is “working back from a presumed conclusion” (that it couldn’t be), or is biased, any solid work can be framed to sound bad, and given that option, what chance is there for anything with implications favoring the Book of Mormon in secular academia? I know of only 15 linguists (7 LDS and 8 non-LDS) who have considered the whole case (no small task) and all of them came to positive conclusions. The two negative reviewers, by their statements, obviously missed much and did not consider the whole work, so they can hardly be counted among the scrutinizers.

In everyone’s defense, comprehending the case requires a huge investment, and few have the time or interest or adequate background, and I admit that some dimensions could be presented more clearly, which future work is in the planning stages. Yet in my defense, a multi-dimensional case can be presented in multiple ways and still not cover all the links/networks between features that portray the various strengths of the case. Nevertheless, when enough other linguists consider the whole case and are willing to admit in print that Semitic and Egyptian do explain what has been otherwise unexplainable in UA through the intervening 100 years from Sapir (1915) to *Exploring* (2015), then those voices and the several dimensions that defy chance probabilities may raise the future question: “Why did it take so long?” Until then, if it is truth, more truth will continue emerging and only strengthen the case, as has been happening since publishing those previous works.

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