

INTERPRETER



A JOURNAL OF LATTER-DAY SAINT
FAITH AND SCHOLARSHIP

Volume 34 · 2019 · Pages 1 - 16

An American Indian Language Family with Middle Eastern Loanwords: Responding to A Recent Critique

John Robertson

Offprint Series

AN AMERICAN INDIAN LANGUAGE FAMILY WITH MIDDLE EASTERN LOANWORDS: RESPONDING TO A RECENT CRITIQUE

John Robertson

Abstract: *In 2015 Brian Stubbs published a landmark book, demonstrating that Uto-Aztecan, an American Indian language family, contains a vast number of Northwest Semitic and Egyptian loanwords spoken in the first millennium bc. Unlike other similar claims — absurd, eccentric, and without substance — Stubbs’s book is a serious, linguistically based study that deserves serious consideration. In the scholarly world, any claim of Old World influence in the New World languages is met with critical, often hostile skepticism. This essay is written in response to one such criticism.*

The most recent issue of the *Journal of Book of Mormon Studies* included a review of two books by Brian Stubbs.¹ It gives me no pleasure writing this response to Chris Rogers’s review of Stubbs. I sat on Chris’s dissertation committee (University of Utah) directed by Lyle Campbell, and I wrote a positive letter of recommendation for his candidacy for the Linguistics Department at BYU. When I heard about his strong objection to Brian Stubbs’s books,² it piqued my curiosity because I had

1. Chris Rogers, “A Review of the Afro-Asiatic:Uto-Aztecan Proposal,” *Journal of Book of Mormon Studies* 28 (2019), 258–67. The books reviewed by Rogers were Brian D. Stubbs, *Exploring the Explanatory Power of Semitic and Egyptian in Uto-Aztecan* (Provo, UT: Grover Publications, 2015), hereafter referred to as “Stubbs 2015,” and Brian D. Stubbs, *Changes in Languages from Nephi to Now* (Blanding, UT: Four Corners Digital Design, 2016), hereafter referred to as “Stubbs 2016.”

2. This paper treats Rogers’s critique of Stubbs 2015. I do not address Rogers’s critique of Stubbs 2016 because that book treats his perception of his earlier works’ relevance to the Book of Mormon.

written a favorable article regarding Stubbs's 2015 book.³ I understood that Rogers's displeasure had to do with (a) Stubbs's shoddy scholarship and (b) the apparent impossibility of Middle Eastern languages effecting changes in Proto-Uto-Aztecan (PUA), a reconstructed American Indian ancestral language.

I immediately emailed Rogers, trying to get some clarification regarding his strong antipathetic attitude toward Stubbs's works. In his response, Rogers mentioned that it had something to do with "joiners and splitters."⁴ I was acquainted with "lumpers and splitters" (explained below), but I could not imagine how the concept applied to Stubbs's work. Rogers was kind enough to send a preliminary copy of an article, which was since revised, now appearing in the 2019 issue of *Journal of Book of Mormon Studies*.

When I read the draft, I saw what Rogers's view of lumpers and splitters meant as applied to Stubbs's work, and given what he meant, I was deeply disappointed for the reasons outlined below. He sees Stubbs's work as "so replete with disorganization, numerous assumptions, mistaken definitions or incorrect characterizations of linguistic concepts, inexact methods, pedantry, and apologetic rhetoric that the idea seems dubious, *even without careful scrutiny*" (260, emphasis added). He went on to say, "the content of the book ... suffer[s] from significant analytical and methodological issues" (261).

On reading the final publication, I now understand why Rogers used such derogation in describing Stubbs's work — he totally misunderstood the very foundation of Stubbs's work. What Rogers mistakenly concluded led him down a rabbit hole of confusion, misunderstanding, and mischaracterization, which he is now publicly sharing with others. Ironically, Rogers lumps together Stubbs 2015 and Stubbs 2016 in his review, which should have focused solely on Stubbs 2015.

In what follows, I will accomplish three things: I will first provide some background information, then show what Rogers understood Stubbs to have said, and finally give hard evidence of what Stubbs actually said.

3. John S. Robertson, "Exploring Semitic and Egyptian in Uto-Aztecan Languages," *Interpreter: A Journal of Mormon Scripture* 25 (2017), 103–16, <https://journal.interpreterfoundation.org/exploring-semitic-and-egyptian-in-uto-aztecan-languages/>.

4. In his published critique, Rogers uses "lumpers" instead of "joiners." I will use "lumper" because this is the terminology he uses in his published version.

Lumpers, Splitters, and the Comparative Historical Method

Prior to examining Rogers's critique, it is important to understand two fundamental concepts: What the Comparative Historical Method is and what the terms "lumper" and "splitter" mean.

When establishing linguistic relationships — particularly relationships between a language and any derivatives or "daughter languages" — the researcher typically relies on a methodology known as the Comparative Historical Method. According to the renowned Calvert Watkins, the Comparative Historical Method "is one of the most powerful theories about human language that has ever been proposed — and the one most consistently validated and verified over the longest period of time."⁵ In applying this method, here are the steps that a successful comparatist follows:

1. In two or more languages, the researcher scouts out as many words as he can find, which resemble each other both in sound and meaning. Such words are *cognates* — etymologically 'born together' from a given ancestral form.
2. Based on a series of such cognates, the linguist then reconstructs the unique ancestral forms from which each daughter language descends.
3. Starting with each reconstructed proto form, the comparatist develops a set of rules for each daughter language.

To be successful, the application of such rules to the unique, reconstructed forms must produce the same forms that are attested in each daughter language.⁶ It is the rules alone, as applied to the ancestral forms, that validate the reconstructed forms. Without the effective application of the rules, the reconstructed forms remain purely hypothetical. This last step is the only way of telling if the reconstructed forms truly gave rise to the appropriate forms that exist in the daughter languages.

This brings us to the concept of "lumpers" and "splitters." These terms designate competing approaches to how languages and language families

5. Calvert Watkins, "New Parameters in Historical Linguistics, Philology and Culture History," *Language* 65/4 (1989), 783. Unknown to me until recently, Stubbs and I have a connection with the late Calvert Watkins, whose academic genealogy goes back to Ferdinand de Saussure, Antoine Meillet, and Émile Benveniste, all giants in the field of historical linguistics. Watkins was on my dissertation committee at Harvard, for which I am grateful. After retirement, Watkins went to UCLA where Brian Stubbs read one of his papers on comparative Uto-Aztecan. Watkins spoke highly of Stubbs's lecture to Uto-Aztecan scholar Pamela Munro.

6. If the rules do not properly derive the attested forms, the reconstructed ancestor form or the rules themselves must be revised.

should be viewed, particularly those in the Americas.⁷ An example of a lumper is found in the work of Joseph Greenberg, who contended that before the Spanish Conquest there were three language groups that populated the Americas: Eskimo-Aleut, Na-Dené, and the remaining thousands of other American Indian languages spoken in North, Central, and South America. These thousands form a Greenbergian macro-group, called “Amerind.”⁸ His Amerind macro-grouping was not based on the traditional Comparative Historical Method. Greenberg’s “lumper” grouping has since been rejected by the majority of scholars. In fact, Lyle Campbell, Rogers’s dissertation chair, said that Greenberg “should be shouted down.”⁹ Conversely, “splitters” should presumably not be “shouted down.” They generally seek to apply the Comparative Historical Method to determine how languages evolve and diverge (or *split*) over time.

Rogers’s Mistaken View of the Core of Stubbs’s Comparisons

Rogers claims that Stubbs’s work catastrophically fails because, like Greenberg’s work, “Stubbs’s proposal falls into the ‘lumper’ camp” (259). According to Rogers, by wrongly lumping Semitic, Egyptian, and Uto-Aztecan together, saying they came from some sort of proto-language, Stubbs is, like Greenberg, a misguided lumper.

Let me be more specific regarding what Rogers alleges Stubbs said:

The purpose of this piece is to review the *long-distance genetic linguistic relationship* between languages of the Afro-Asiatic language family and the Uto-Aztecan language family suggested in Stubbs’s *Exploring the Explanatory Power of Semitic and Egyptian in Uto-Aztecan* and *Changes in Languages from Nephi to Now*. (258, emphasis added)

Note the emphasized phrase: *long-distance genetic linguistic relationship*. This phrase has a special meaning when it comes to Rogers’s assessment of Stubbs’s work. Later in his critique, Rogers begins to clarify his use of the phrase:

A proposal for a genetic relationship between two or more languages must be supported by two types of evidence: (1) evidence that the languages discussed are in fact genetically

8. Joseph H. Greenberg, *Language in the Americas* (Stanford: Stanford University Press, 1987).

9. Lyle Campbell, Comment on Greenberg, Turner, and Zegura, *Current Anthropology* 27/5 (December 1986), 488–89.

related, and (2) evidence for the reconstruction of the common linguistic ancestor. (261)

Rogers left out an important step of the comparative historical method. Review, again, the three steps utilized under the Comparative Historical Method. Of those three steps, it is the third step — the crucial step — that Rogers left out. Only after this is done can the comparatist speak of a *genetic* relationship among the derived sister languages.¹⁰

The quintessential example of relations due to common ancestry would be Proto-Indo-European (PIE), the reconstructed ancestor of almost all European languages, certain languages typically found in Northern India, as well as the languages of Iran and Afghanistan. Here we consider two branches of PIE, the Italic and the Germanic branch. The Italic branch preserved the speech sounds *p*, *t*, and *k*, which were present probably around 6,000 years ago, in PIE times. On the other hand, the Germanic branch innovated by changing the original *p* to *f* (compare Latin *pater* to English *father*, both from PIE **pH₂tér*), *t* to *th* (compare Latin *trēs* to English *three*, both from PIE **tréyes*), and *k* to *h* (compare Latin *cornu* to English *horn*, both from PIE **kr₂no*).¹¹ It is the application of many more rules like *p* > *f*, *t* > *th*, and *k* > *h* that help validate the reconstruction of ancient Indo-European spoken all those millennia ago.

However, any person who understands the Comparative Historical Method would never surreptitiously put in people’s minds that Stubbs should have used the method (1) to compare Near Eastern and the New World languages, (2) to postulate an ersatz proto-language, and (3) to devise a set of rules that would predict the cognate forms in each daughter language.

Specifically, the theory that Rogers attributes to Stubbs is that somehow, perhaps 7,000 years ago, in an unknown continent, there existed an ancestor of PUA *and* Proto-Afro-Asiatic (PAA), namely Proto-PUA-PAA. This ancestral language would have been an even more remote ancestor of what today we know as Uto-Aztecan (New World) and Semitic, Egyptian, Berber and other languages (Old World). According to Rogers, Stubbs 2015 has the duty of giving evidence of *genetically related* languages, descendants of some remote, ur-language.

This, however, is an unfortunate strawman; it is no wonder that Rogers assesses the content of Stubbs’s book as suffering from “disorganization, numerous assumptions, mistaken definitions or incorrect characterizations of linguistic concepts, inexact methods,

10. French, Spanish, Portuguese, Dalmatian, etc. come from the common ancestor, Latin (or, to be more precise, “that come from the emergent dialects of Vulgar Latin”).

11. This is the celebrated Grimm’s Law, named after one of the Brothers Grimm.

pedantry, and apologetic rhetoric” (259). His mischaracterization of Stubbs’s work and the strawman he constructs makes it easy to make Stubbs look like a fool — but as we shall see, Stubbs is no fool.

Rogers further asserts that “one of the main methodological issues of Stubbs’s proposal is the omission of an explanation for why the Uto-Aztecan and Afro-Asiatic languages are being compared in the first place” (261). The only reason I can possibly imagine for Rogers’s statement is owing to his determination that Stubbs is a lumpner. According to Rogers, Stubbs omits the reason for comparing Uto-Aztecan and Afro-Asiatic because he should have, but could not, reconstruct a 7,000+ year old ancestral language existing on two separate continents. So, like Greenberg, Stubbs had to “lump” them, without reconstructing an ancestor language, and without providing the rules that would correctly predict the forms of each daughter language. This makes Stubbs, like Greenberg, a lumpner.

The Core of Stubbs’s Actual Comparisons

The truth, though, is that Stubbs *does* make a comparison — but on what grounds? Throughout Stubbs 2015 the comparisons come from *borrowing* and not from ancestral descent. To be clear, what Stubbs asserts is that at a given time in the past, in the environs of Uto-Aztecan, and in an intimate relationship, *borrowing* effectively brought Uto-Aztecan and the Near Eastern languages together. Therein lies the grounds of comparison; there was no way that he ever imagined that the relationship was genetic, despite Rogers’s strawman allegations.

To bring Stubbs’s notion of borrowing closer to home, let us consider a hypothetical scenario. Let us suppose that a Martian has accessed thousands of pages of English as well as voluminous pages of French, doing this for the purposes of careful research. His investigation reveals two classes of words. On the one hand he finds words like *hand*, *foot*, *tooth*, *child*, and *sun* that he cannot find in French. On the other hand, he finds words like *disorganization*, *numerous*, *assumption*, *definition*, *incorrect*, *inexact*, *method*, and *pedantry*¹² that he finds in both languages. With no English words in French and nearly half of English words of French origin, our Martian friend rightly concludes that English borrowed from French, which makes our Martian researcher a splitter,

12. For the record, he also finds affixes that are attached to French words, like *dis-*, *mis-*, *de-*, *in-*, *-ous*, *-tion*, *-ment*, *-able*, *-ain*, *ism*, which are compelling evidence of French loans into English.

not a lumpner — a splitter because he separates French loanwords from what was originally Anglo-Saxon English!

And so it is with Stubbs, who has assiduously documented a vast reservoir — literally thousands — of Near Eastern words as well as fossilized grammatical affixes presently found in the sinews of Uto-Aztecan. This makes Stubbs a splitter by the very act of identifying borrowed words, separating Near Eastern loanwords from what was originally Uto-Aztecan. But there are no Uto-Aztecan words in the Near Eastern languages.

The Relationship between Genetic Relatedness and Borrowing

Here I must point out that borrowing does implicate the Comparative Historical Method. First, though, I must note that when one language borrows from another, there is a compulsory transformation of the loanword to accommodate the shared speech habits of the speakers of the receiving language. For example, contrast the donor language Spanish with the receiving language English in these three examples:

- Spanish: *galeón* [galeon] > English *galleon* [gal-ee-uhn, gal-yuhn]¹³
- Spanish *tomato* [tomato] > English *tomato* [tuh-mey-doh]
- Spanish *bonanza* [bonansa] > English *bonanza* [buh-năn-zuh]

The point is that phonological rules describe the transformation of words of the donor language wherein sounds and meanings are adapted into the ingrained sound patterns of the receiving language. From the examples above, the rule would be, “In English, any vowel of the syllable next to a syllable with a stressed vowel, is reduced to ‘uh’ as in, for example, *buh-năn-zuh*.”

Note that borrowing is comparable to the three rules of the Comparative Historical Method:

1. Finding Similarities

Genetic Relatedness: Words that are cognates in sister languages are equivalent to the ancestral words from which they descend, having similar sounds and meanings.

Borrowing: Words in the donor language are equivalent to words in the receiving language, having similar sounds and meanings.

2. Reconstructing

Genetic Relatedness: Based on cognates, ancestral

13. The “phoneticism” representing English pronunciation was borrowed from <https://www.dictionary.com>.

forms are postulated from which the words of the daughter languages can be derived.

Borrowing: The words of the donor language are the words from which the receiving language can be derived.

3. Derivational Rules

Genetic Relatedness: If results turn out to be valid, rules applied to the words of the ancestral form must correctly predict the sounds and the meanings of the words attested in the daughter languages.

Borrowing: If results turn out to be valid, rules applied to the words of the donor language must correctly predict the sounds and the meanings of the borrowed words of the receiving language.

A Word About Rules

Rules take the form “if x, then y.” Rules are indispensable to scientific inquiry because their application predicts repeatedly true results. *If* I heat water to 212° Fahrenheit, at sea level, *then* the water will boil, every time. The reason Watkins says that the Comparative Historical Method “is one of the most powerful theories about human language” is because the method takes a form that is largely predictive and replicable. A Spanish word borrowed into English almost always follows the rule outlined in the previous section.

Sadly, Rogers chose to ignore whether Stubbs’s rules are predictive, and this is a glaring omission of his review. Had he turned to page 2 of Stubbs 2015, he would have been able to comment on 31 instances of the rules $b > p$, $d > t$, and $g > k$, where there is an obvious similarity in both sound and meaning between Semitic and PUA words. Had he looked at the next page he would have been able to comment on 18 instances where $? > w/o/u$, where the similarities between the donor and borrower words are again transparent. Each of the following pages (4 through 9) is full of rules that show a plethora of predicted relationships of similarity between Near Eastern donor and PUA borrower. These data are included in the 1,528 well-documented instances of the same relationships that can hardly be attributed to a chance happening, to onomatopoeic similarity, to universal traits, or to genetic descent. These are all examples of borrowing — Uto-Aztecan borrowings from languages of the Near East, fully ensconced in the Uto-Aztecan language family.

Rogers pokes at Stubbs by paraphrasing his words: “Yet gullible may better describe those accepting the (assumptions) in the book than those

digging in to find the facts” (261). Those same words ironically apply to Rogers’s critique — he obviously does not accept the assumptions of the book he reviews and fails to dig in to find facts that Stubbs provided; he ignores making reference to the rules themselves. In other words, he never interacts with the data. Recall that it is the application of the rules to the donor words which, if successful, validates the proposed relationship between the Near Eastern words found in Uto-Aztec. More than anything, this omission deprives Rogers’s readers of a genuine understanding of the intellectual merit of Stubbs 2015.

E Pluribus Unum

One consequence of ignoring rules shows up in Table 6 (265). The table wrongly assumes that a multitude of correspondences between sound and meaning magically occur by chance without taking account of rules. Let us look more closely at the role rules play in Stubbs 2015.

A single, proper application of a rule belonging to any of the three languages would have only a 2% or 3% chance of showing a meaningful correspondence of sound and meaning. Another proper application would slightly diminish the possibility of chance. However, what happens if there is a multitude of proper applications of all the rules, each having the same consistent results? In the end, the cumulate effect of direct hits makes the appeal to happenstance so diminished as to rule out even the possibility of chance. Out of many proper applications of the rules, there can be only one, cumulative result for each rule.

Criticism of What Stubbs Did Not Do

Here, let us pick up with Rogers’s discussion on “other explanations of the similarities” (264).

Languages do not have to be genetically related to share similarities (as Stubbs correctly points out). Language similarities can be a consequence of accidents/change, borrowing, onomatopoeia and sound symbolism (or ideophones), universal traits, and genetic inheritance (or a combination of these). (264)

Let us consider each type that Rogers mentions:

- **Accident:** Lowland Mayan has *ay* and Spanish has (*h*) *ay* (the initial *h* was never pronounced). Both mean

‘there is/are. (*H*)ay comes from Latin.¹⁴ Obviously, Stubbs’s work is not founded on forms that are purely accidental.

- **Sound Symbolism:** “moo” English; “moo” Hindi; “mø” Norwegian; “*mu, muh*” Uropi.¹⁵ Again, Stubbs’s work is not driven by sound symbolism. Rogers’s attempts to use sound symbolism to diminish Stubbs’s work is pointless, to say the least. Rogers says, “at least 100 of the 1,528 suggested similarities in the proposal are likely due to sound symbolism ... This leaves conservatively 1,328 [sic] similarities as evidence for the proposal” (264). I do not know how Rogers came up with the round number 100, but I do know that 1,428 data points are left to be accounted for.
- **Universal Traits:** All languages have speech sounds that convey meanings. Obviously, this is not the basis of Stubbs’s work.
- **Borrowing vs. Genetic Inheritance:** I find it conspicuously damning that Rogers ignores borrowing as one of the explanations for seeing similarities among languages. He goes on to say, “in a proposal of genetic relatedness, these other possibilities [including borrowing] should also be considered, but are not presented in either of Stubbs’s publications” (264). Clearly, borrowing is Stubbs’s *only* consideration, not the “genetic relatedness” that Rogers misattributes to Stubbs’s thinking. Why should Rogers even think that Stubbs was obligated to consider any of the other reasons for similarities between Semitic and Uto-Aztecan?

Rogers clearly confuses the relationship between borrowing and genetic inheritance. I cannot begin to understand what he means by the following: “Lastly, when similarities due to borrowing are extensive, the result can be a mistaken conclusion of linguistic relatedness” (265). I am guessing that Rogers is using “linguistic relatedness” as equivalent to “genetic relatedness,” as defined above. But who, if anyone, confuses extensive borrowing with “linguistic relatedness?” Is Rogers saying that because similarities due to borrowing are so extensive that Stubbs confused genetic heritage with borrowing? Or was it that Rogers knew all along that Stubbs’s work was based on extensive borrowing, but the

14. “From Old Spanish *ha i* (“it has there”) (compare Catalan *hi ha* and French *il y a*), from *ha*, third-person singular present of *haber* (“to have”), + *i*, enclitic form of *ahí*, from Latin *ibi* (“there”),” <https://en.wiktionary.org/wiki/hay#Etymology>.

15. See https://en.wikipedia.org/wiki/Cross-linguistic_onomatopoeias.

“confusion” allowed him to allege Stubbs to be a lumper? Or is this somehow political theater, a kabuki dance?

Rogers goes even deeper into the weeds. His language in the following is so opaque, so difficult to follow, that I include here a bracketed translation from my attempt to get at what he means:

The potential of borrowing resulting from a scenario of contact is not systematically considered as an explanation for the similarities presented in Stubbs’s proposal. [Translation: There might be languages other than Near Eastern languages responsible for the loanwords found in Uto-Aztec, which Stubbs did not take into account.] That is, the similarities are not put into the context of the other languages spoken all around the Uto-Aztec languages. [Translation: The loanwords found in Uto-Aztec might be due to languages “spoken all around Uto-Aztec.”] Without such a comparison, it is not possible to rule out the scenario that the Uto-Aztec similarities to Near-Eastern languages are a result of borrowing these features from other languages or from Near-Eastern languages themselves. [Translation: Since there were other languages in the neighborhood of Uto-Aztec, it is impossible to tell whether the loanwords in Uto-Aztec came from Near Eastern languages or other languages contiguous with Uto-Aztec]. (266)

First, it is perplexing that after all that Rogers said about genetic relatedness, out of nowhere he suddenly recognizes that there really are “similarities presented in Stubbs’s proposal” *owing to borrowing*. However, I am puzzled by the fact that Rogers could even imagine that the loanwords cited by Stubbs as present in Uto-Aztec could have been borrowed from “other languages spoken all around the Uto-Aztec languages.” It should go without saying that every sentient human knows that two mutually unintelligible languages are not the same, which is certainly the case regarding Near Eastern languages and languages neighboring Uto-Aztec. It is unimaginable that Rogers would formally state that the characteristics unique to Near Eastern languages, which are clearly found in Uto-Aztec, could possibly be the same as the characteristics inherent in the American Indian languages “spoken all around Uto-Aztec languages.” This makes just as much sense as saying apples are identical to oranges in every possible way — taste, texture, smell, peelability — including all the other features that distinguish apples from oranges.

So, following Rogers, there are two possible explanations for the loanwords found in Uto-Aztecan. They either come from Near Eastern languages or from “the other languages spoken all around the Uto-Aztecan languages.” For Rogers, both explanations are on equal footing. But it is clearly impossible to know what the other languages “spoken all around Uto-Aztecan” were. Rogers even calls Stubbs to task for failing to note that “the potential of borrowing resulting from a scenario of contact [was] not systematically considered as an explanation for the similarities presented in Stubbs’s proposal” (266).

In light of Stubbs’s “failure,” let us do what Rogers omitted doing: choose between Near Eastern languages and his curiously odd alternative. Ockham’s Razor makes the choice easy: “*Entia non sunt multiplicanda praeter necessitatem*, that is, a hypothesis ought not to introduce complications not requisite to explain the facts.”¹⁶ Near Eastern languages are responsible for the loanwords found in Uto-Aztecan because no other language or combination of languages could have features identical to those belonging to Uto-Aztecan. Surely, Rogers’s alternative suggestion that “the other languages spoken all around the Uto-Aztecan languages” is responsible for the similarities between the two languages is a complication “not requisite to explain the facts.”

A Knotty Problem

Of course, Stubbs 2015 raises a knotty problem. It is academic dogma that any prehistoric migration from the Middle East to the Americas never happened, nor could it ever have happened. Any scholar’s work would be anathema if it made such a claim. Some say Stubbs’s work is anathema — but only at the expense of ignoring the breadth and depth of the actual data. There is actually existing evidence that favors such a migration — not an archeological artifact, nor a recorded manuscript — but evidence in the form of factual, predictive, lawful linguistic data found in Stubbs 2015. Such evidence of borrowing exists in abundance, available for proper review and criticism. And certainly, factual linguistic data should carry more weight for professional linguists than for anyone else.

16. C.P. Peirce, *Collected Papers of Charles Sanders Peirce*, Vol. IV (*The Simplest Mathematics*), edited by C. Hartshorne and P. Weiss (Cambridge, MA: Belknap Press, 1933), 4.

Stubbs's *Bona Fides*

Does Stubbs's professionalism, his *modus operandi* as a comparatist, measure up to Comparative Historical Method standards? I believe so, and below I explain why Stubbs's 2015 data meet every expectation of a good comparatist.

Among the many that have contributed to the study of PUA, there are three major players that stand out: Wick Miller, Kenneth Hill, and Brian Stubbs. Miller produced 700 correspondence sets (that is, reconstructions of PUA). Hill added 500 more, for a total of 1,200 correspondence sets. In 2011 Stubbs published a landmark book, *Uto-Aztecan: A Comparative Vocabulary*,¹⁷ which added 1,500 more, more than doubling Miller/Hill's 1,200 to 2,700 correspondence sets, without addressing any Near-Eastern contributions. Written for other Uto-Aztecanists, *Uto-Aztecan: A Comparative Vocabulary* treats Uto-Aztecan reconstructions of vocabulary and grammar of PUA. Stubbs's careful use of the Comparative Historical Method resulted in Hill saying, "All in all, this is a monumental contribution, raising comparative Uto-Aztecan to a new level."¹⁸ This work has become a standard, which every Uto-Aztecan comparatist must take into account.¹⁹

The methodology that all competent comparatists use is the Comparative Historical Method, which is, as already mentioned, "one of the most powerful theories about human language that has ever been proposed."²⁰ As a student of Watkins, as a comparatist who has studied the Mayan language family from 1967 to 2019, and as one who has 60+ refereed articles in prestigious journals, not to mention books, I feel that I might be somewhat competent to judge Stubbs's comparative work. My take is that Stubbs's work of both 2011 and 2015 is spot on, that Stubbs has always been an exacting practitioner of the Comparative Historical Method. In this regard, I would challenge anyone to demonstrate any significant drop off of methodology between Stubbs's 2011 and 2015 books. Both meet every standard required by the Comparative Historical Method.²¹ Furthermore,

17. Brian D. Stubbs, *Uto-Aztecan: A Comparative Vocabulary* (Flower Mound, TX: Shumway, 2011).

18. Kenneth C. Hill, "Uto-Aztecan: A Comparative Vocabulary by Stubbs," *International Journal of American Linguistics* 78/4 (October 2012), 591–92.

19. Hill told Stubbs (personal communication), "I must tell you Brian, whenever I think I have found a new Uto-Aztecan cognate set, I go to your Uto-Aztecan book, and nearly every time it's already there" (personal communication from Stubbs).

20. Calvert Watkins, "New Parameters in Historical Linguistics, Philology and Culture History," *Language* 65/4 (1989), 783.

21. In some sense, Uto-Aztecan comparatists, all of whom received the 2015 book, are in a bind because, on the one hand, they accept Stubbs's methods and

his publications in refereed journals and his presentations at Uto-Aztecan conferences also attest to his professionalism.

In evaluating *Uto-Aztecan: A Comparative Vocabulary*, Hill goes on to say,

Part III (pp. 47–420) is the core of the work, the comparative vocabulary. Stubbs numbers the sets 1–2703, but in reality there are many more than 2,703 sets because many subsets are given with numbers like 7a, 7b, 7c, for vocabulary that may or may not be groupable into a single more inclusive set. Each set is discussed in some detail and the serious comparativist *will delight in the discussions*.²²

It must be agreed on all hands that Stubbs, or anyone who understands scholarly methods and practices, has the right (if not the responsibility) to object to Rogers's criticisms. Regarding "splitters" and "lumpers" we can happily say that Stubbs is a splitter because he carefully separates the native aspects of Uto-Aztecan from the borrowed Near Eastern features that are now ensconced, part and parcel, in the fabric of Uto-Aztecan.

Conclusion

As I said initially, I am sorry that this whole thing came up. I am sorry that Rogers abused my words, wrongly giving the impression to the quick or casual reader that I agree with his harsh disavowal of Stubbs 2015:

The result is that when evidence and methods are considered carefully, there is ample reason to "*challenge the breadth and depth of the data*" ... (259, emphasis added)

I was saddened when I saw that Rogers had manipulated the intent of my words. These were the closing words of my favorable review of Stubbs, not conveyed by Rogers:

As a practitioner of the comparative historical method for 40+ years, I believe I can say what Stubbs's scholarship does and does not deserve: It does not deserve aprioristic dismissal given the extensive data he presents. It *does* deserve authoritative

practices in his 2011 book, dealing only with Uto-Aztecan, but on the other hand, they recognize the same professionalism found in the 2015 book, dealing with Middle Eastern languages and Uto-Aztecan.

22. Kenneth C. Hill, "Uto-Aztecan: A Comparative Vocabulary by Stubbs," *International Journal of American Linguistics* 78/4 (October 2012), 591–92, emphasis added.

consideration because, from my point of view, *I cannot find an easy way to challenge the breadth and depth of the data.*²³

I am sorry that Rogers similarly manipulated Dirk Elzinga's statement regarding Stubbs 2015. Rogers wrote:

... to remain unconvinced by the "*extensive accurate data, to back up his [Stubbs's] extraordinary claim.*" (259, emphasis added).

How can extensive, accurate data constitute a reason for remaining unconvinced? This is what Elzinga actually said:

Stubbs has something the language eccentrics do not have: the training and experience, together with *extensive accurate data, to back up his extraordinary claim* of significant Old World linguistic influence in Uto-Aztecan, a New World language family.²⁴

I am sorry that Rogers accused Stubbs of postulating a logically impossible theory of some contrived common ancestor of the Uto-Aztecan and the Afro-Asiatic languages, that he was so fixated on "lumpers and splitters" that it blinded him to Stubbs's obvious claim of borrowing, which functionally makes him a splitter.

I am sorry that Rogers became so overly zealous as to state that Stubbs was someone who made false assumptions, mistook definitions, mischaracterized linguistic concepts, used inexact methods, and was pedantic and apologetic.

I am sorry that Rogers accused Stubbs of wholly inept scholarship, below academic standards, in the face of Uto-Aztecanists' general acceptance of and acclaim for his past work.

I am sorry that Rogers was blind to Stubbs's genuine contributions to the field of comparative Uto-Aztecan — his 2011 book (crucial to the field), his publications in scholarly journals, as well as his lifetime of presentations at Uto-Aztecan conferences.

Most of all, I am sorry that his misleading representation of Stubbs's work may cause LDS, LDS dissenters, and non-LDS scholars not interested in the Book of Mormon to discount out-of-hand the 1,528 data points, the unyielding facts that alone give substance to the predicate of Stubbs's claim.

John S. Robertson is a Professor of Linguistics Emeritus, Brigham Young University. He received a BA in political science, an MA in linguistics,

23. Robertson, "Exploring Semitic and Egyptian," 114.

24. Dirk Elzinga, Review of "Brian D. Stubbs: Exploring the Explanatory Power of Semitic and Egyptian in Uto-Aztecan," *BYU Studies Quarterly* 55/4 (2016): 176.

both from BYU, and a PhD in linguistics from Harvard University. He has more than 60 publications, which include several books and many articles that treat the reconstruction of proto-Mayan, language change, and the grammar and sound system of the Mayan Hieroglyphs. He is also an inveterate student of the polymath C.S. Peirce, founder of American Pragmatics and Semiotics. His missionary service includes France Paris (1962–1965), England London, Family History (2011–2012), Pathway (2015–2017), Cody Historic Mural and Museum (May–September 2017), the latter three with his wife Barbara. Other Church service includes bishop, bishop’s counselor, branch president (Guatemala), high council, Young Men’s, and Primary. He is happily married to Barbara Clyde Robertson, with 23 grandchildren.