

DISTANT GENETIC RELATIONSHIP AND THE MAYA-CHIPAYA HYPOTHESIS

Lyle Campbell
University of Missouri, Columbia

1. Methods in distant genetic research
2. The Maya-Chipaya hypothesis and the data
3. Conclusions

1.0. The purpose of this paper is to evaluate methods employed in distant genetic research in linguistics, and to use those methods which prove most reliable in evaluating the proposed distant affinity of Uru-Chipaya of Bolivia with the Mayan family of languages.¹

Like all linguistic relationships, distant ones too depend on similarities in the languages. The problem is in presenting a case which strongly supports a genetic explanation of the similarities, at the same time negating the possibilities of chance, diffusion, or universals as the explanation. Unsuccessful proposals fail, not because they lack similarities (usually), but because they fail to demonstrate that the genetic explanation is the only one probable.

1.1. One common method has been to scan word lists, equating items which have similar sound and meaning, and claiming genetic accounts for cases with sufficient similarities. Little effort is made to eliminate chance, diffusion, and universals as possible explanations, and therefore, relationships based on this method are unconvincing.

Many have hoped that quantification would reduce the subjectivity in examining lexical similarities. For example, Bender (1969) proposed a method designed to reduce the chance factor statistically. He attempted to establish empirically confidence levels based on an investigation of 21 non-related languages. His conclusions were:

(1) That two or more CVC correspondences in the 100 word list refute the nil [sic] hypothesis at a 94% confidence level (three or more at a 99% confidence level), if the CVC correspondences are such that: (a) the vowels are identical and one or both pairs of consonants are identical while the other pair differs by only one feature (occasionally two); or (b) the consonant pairs correspond identically, regardless of the intervening vowel.

(2) that three or more correspondences refute the nil hypothesis at a 95% confidence level (four or more at 99%) if: (a) CV standing alone corresponds with an identical CV (alone or in a longer item); or (b) not only the

first, but any CVC's in longer items correspond.

(3) that seven or more correspondences refute the nil hypothesis at a 95% confidence level in items with plausible phonological correspondences (p. 525-7).

The following 'plausible' CVC correspondences between Finnish, Cakchiquel (a Mayan language), and Quechua, which I assume to be unrelated in any significant way,² suggest that Bender's claims are perhaps overstated:

FINNISH	CAKCHIQUEL	QUECHUA	ENGLISH	(Bender's conclusion)
1 kala	kar	čal'wa	fish	1 and 3
2 kylmä	horom	čiri	cold	3
3 kuole	kan	(wañu-)	die	3
4 kaula	qul	kunka	neck	3
5 nenä	č'a?n	siŋa	nose	3
6 kieli	(aq')	qal'yu	tongue	3
7 kuuma	meq'en	q'uñi	warm	3
8 kärpä	kumač	činčay	snake	3
9 kauka-	(nax)	karu	far	3
10 läKte-	el/[loq']	lyuqsi	go	3
11 töni-	[ten-]	tanqa	push	1 and 3
12 vaimo/nainen	nana	warmi	woman	3 (1?)
13 kelta-	q'an	q'il'yu	yellow	3
14 muna	mo?l	(runtu)	egg	3
15 tule-	[ta:l]	(hanu-)	come	1 and 3
16 tuhka	čax	(učpa)	ash	3
17 pure-	pur-	(kani-)	bite	1
18 vatsa	(pam)	wiksa	belly	3
19 istu-	č'uy-	tiya-	sit	3
20 kaKte-	ka?i?	iskay	two	1
21 mi-	(ma)	ima/ma-	what	3 (2?)

It is clear from this list that there are well over seven plausible CVC correspondences between Finnish and Cakchiquel, and between Finnish and Quechua. This means that there are at least two cases not covered within Bender's 95% confidence level for his third conclusion. Also, there are more than three cases of CVC correspondences between Cakchiquel and Finnish not covered in Bender's confidence level for his first conclusion. My conclusion is that either these test cases are merely exceptions to Bender's claims, or that they fall within that small margin of possible cases outside of his levels of confidence (i. e. not part of the 95% of cases which conform to his claims). In either event, I remain skeptical. Had I put my trust in such quantifying techniques, I would need to believe that Finnish is genetically related (in some meaningful way) to two American Indian languages (actually families of languages); this to me is unreasonable.

Lexicostatistics, as a formalization of lexical inspection, has often been employed in attempted linguistic classifications. However, in face of the many valid criticisms leveled against it (in its guise as glottochronology), it can hardly be considered reliable. It has been attacked for its impractical results (cf. Teeter 1963, Campbell 1971, etc.), and all of its basic assumptions have

been forcefully challenged (cf. Hoijer 1956, Arndt 1959, Bergsland and Vogt 1962, Hymes 1960, Chretien 1962, Teeter 1963, etc.). There is a more pervasive difficulty, however, when the method is used in distant genetic research. Apart from the many weaknesses treated in the literature, the method at best presupposes genetic relationship and therefore cannot find it or prove it.³

I conclude, therefore, that no method of lexical scanning or counting in and of itself is valid for finding distant genetic relationships.

1.2. Sapir was fairly successful in the distant relationships he proposed. Many were strengthened in subsequent research, while few were abandoned (many, of course, remain controversial). Examining his method may help to establish reliable criteria in distant genetic research.

Sapir never stated his method very explicitly; probably the closest he ever came was in *The Hokan affinity of Subtiaba of Nicaragua* (1925), where he said:

When one passes from a language to another that is only remotely related to it, say from English to Irish or from Haida to Hupa or from Yana to Salinan, one is overwhelmed at first by the great and obvious differences of grammatical structure. As one probes more deeply, however, resemblances are discovered which weigh far more in a genetic sense than the discrepancies that lie on the surface and that so often prove to be merely secondary dialectic developments which yield no very remote historical perspective. In the upshot it may appear, and frequently does appear, that the most important grammatical features of a given language and perhaps the bulk of what is conventionally called its grammar are of little value for remoter comparison, which may rest largely on submerged features that are of only minor interest to descriptive analysis (491-2).

One suspects that Sapir's 'submerged features' are those which are so strikingly arbitrary that they deny chance as an explanation, and which are so deeply 'submerged' in the grammar that they deny diffusion as a possible explanation. In so far as this method does deny chance and diffusion, it is powerful for establishing genetic relationships. However, it is not foolproof, for some extremely arbitrary similarities may still be due to chance, diffusion, or universals.

An example of a seemingly submerged feature is found in Quechua and Mayan pronouns. Both have two sets of pronouns performing distinct functions, which are incorporated into the verb. For first person singular Proto-Mayan had *in/w-, while Cuzco Quechua has -ni/wa-. This similarity would seem to be so arbitrary as to deny chance. More investigation, however, reveals that Quechua -ni is from the empty morph which is automatically inserted between two morphemes the juxtaposition of which would otherwise create a consonant cluster in the Quechua A dialects. Quechua A dialects probably originally had *-y as the 1st per sg morpheme. (Cf. Parker 1969:150 for details.) The -wa- (of Quechua A dialects) is probably best reconstructed for Proto-Quechua as *-ma- on the basis of the Quechua B reflexes (cf Parker 1969:193). Thus the neat and arbitrary correspondence between Mayan and Quechua (of the Quechua A dialects) is merely accidental and not really genetically shared or submerged.

Another seemingly submerged, but circumstantial similarity, involves negation in Quechua and Mayan. Negation in Cuzco Quechua is mana...ču, a discontinuous construction. In Cákchiquel (and several other closely related

languages) negation is *man...ta*, also discontinuous. This would suggest a connection, but closer inspection reveals that Cakchiquel has undergone secondary developments which account for the strange similarity to Quechua. Proto-Mayan seems to have had only **ma*; Cakchiquel developed the discontinuous construction by obligatorily associating older **tah* (optional conditional particle, would) with negative forms. The seemingly arbitrary similarity is stripped of much of its potency.⁴

Quechua also provides an example which would indeed defy chance, but which, though seemingly arbitrary, does not deny universals as a possible explanation. Quechua and Finnish correspond in the following:

FINNISH	QUECHUA	
-ko	-ču	Question morpheme
-ko (with imperatives)	-ču	Negative morpheme
-k(ä)	-ču-(3rd per sg)	Imperative morpheme

Such a sound correspondence in grammatical morphemes (which are not overly common in the world's languages) would seem indeed striking. But, as it turns out, there is a universal tendency for negation and interrogation to be closely related processes. Some have proposed deep structure similarities and substantive universals to account for this. Even in English *yes-no* questions seem related to negation as shown in the paraphrase relation of sentences like 1 and 2:

- (1) Is John a celibate?
- (2) Is John a celibate, or isn't he a celibate?

Many other languages have negation and *yes-no* questions related morphologically (e.g. Somali, Mandarin, and others). Thus there is, in some semantic sense, an underlying association between negation and *yes-no* questions. Imperative and interrogative structures were equated in Katz and Postal (1964) who derived English imperatives, such as 'shut the door' from an underlying question, 'will you shut the door?'. Though the analysis is not widely held today, it seems perfectly plausible that languages, such as Quechua and Finnish, could develop the association of imperatives and questions independently, perhaps as many Indo-European languages did from optatives and subjunctives. Thus, where on the surface it would seem that Finnish and Quechua share a correspondence in several grammatical morphemes, that similarity is probably the result of universal tendencies.

Finally, there are examples of arbitrary similarities, which seem submerged in the grammar, but which are due to diffusion. Areal influences are generally well-known (cf. Haas 1969); it is also well-known that Sapir was generally skeptical about syntactic borrowing. Nevertheless, syntactic borrowing does take place as part of the phenomena of areal linguistics. Some examples in the Mesoamerican linguistic area are the following. Yucatec Mayan has borrowed a limited kind of noun incorporation from Aztec (most other Mayan languages lack noun incorporation into the verb). Many Mesoamerican languages have directional morphemes in the verb (e.g. Mayan languages, Aztec, Tarascan, some Otomanguean, etc.) which appears to be an areal feature. Many of the same languages show a relationship between body parts and positional morphemes, e.g.:

CAKCHIQUEL	TARASCAN	AZTEC
-pan <u>stomach, in</u>	-wa- <u>stomach, in</u>	-tlan <u>tooth, place of</u>
-či? <u>mouth, at</u>	-mu- <u>mouth, at</u>	-ten <u>lip, edge</u>
-ix <u>back, behind</u>	-ču <u>buttocks, underneath</u>	-is <u>eye, face, front</u>
		-tson <u>head-hair, top</u>

It would seem that some of these similarities are arbitrary enough to deny chance, but not to deny diffusion.

Thus, Sapir's method (or what I suppose to have been his method) was to support proposed relationships with both regular recurring sound correspondences and submerged features of the grammar. This is a valid method, although caution must be taken to insure against chance, diffusion, and universal factors entering into consideration.

1.3. Joseph Greenberg has been imaginative as a practitioner of distant affinities. His methods (1966) are simple. The first is to consider relevant only resemblances involving both sound and meaning. Resemblances involving sound alone (e. g. the presence of a tonal system) or meaning alone (e. g. morphemes of sex gender) are irrelevant. Such similarities can be (and quite often are) independent of genetic relations. Greenberg's second method is that of mass comparison, i. e. the simultaneous comparison of many languages, not just two or three isolated ones. Greenberg's third method is reliance on linguistic evidence alone for conclusions about classification. The first of Greenberg's methods is not new, of course. It is the comparative method of historical linguistics. The second is not so much a method as it is a heuristic principle for practice. If one does not happen to have a mass of languages to test, he may still compare the few he does wish to test. The third method also is more an instruction in procedure than a real method. It merely says don't pay attention to non-linguistic things. Thus, Greenberg has interesting and instructive strategies, but no fundamentally different methods.

1.4. Greenberg's method is the sound-meaning isomorphism of the comparative method, which is a valid method. However, Greenberg has been criticized for not requiring stricter regularity and for counting correspondences in short (CV or VC) sequences (see below). One can nearly always find two lexical items which both have some segment x in language A and two similar lexical items in language B which have some corresponding segment y — i. e. a 'matching'. A matching is a tentative sound correspondence which becomes a real correspondence only when one is able to reconstruct a valid proto form. Correspondences are assumed to descend from a common source genetically, whereas matchings may in fact be due to a number of other things as well, borrowing, onomatopoeia, etc. not excluded.

It is important to suggest criteria whereby one may be reasonably sure that his matchings are significant genetically. One criterion involves basic vocabulary. One is suspicious of proposed relations which lack a significant amount of basic vocabulary supporting them. Though glottochronology with its notion of a universal or culture-free basic vocabulary is to be discounted, there is something to the notion that common vocabulary tends to be preserved more than other domains. This is not the same as the claim that a culture free list

exists, or that the basic vocabulary cannot suffer severe changes; it is only a statement of a general tendency. I would require a significant number of matchings in basic vocabulary for any worthwhile proposal of relationship.

A second criterion involves the length of items matched. The recurrence of a matching in several CV forms is not nearly so chance-defying as a few good matchings in CVC or longer forms in which all the segments correspond. This notion of length has been discussed both by Swadesh (1954) and Bender (1969). I would discredit matchings in longer items which matched, say, only the first consonant but no other segments. If all the consonants of a trisyllabic form were matched, it would be impressive, but for only one to be matched would be suspicious. It is possible to find many exact recurring matchings if one considers only one matched segment for each proposed cognate set tested, but such a chance occurrence is reduced when more than one segment is matched for each tested set.

Just as one can always find some recurring matchings in short items, one can always match lots more things for items that do not mean the same thing. That is, once one relaxes constraints on how similar semantically items must be to be considered, then automatically the number of matchings he finds increases. Matchings are more persuasive in semantically equivalent forms. I would suggest for a third criterion that semantic equivalence be maintained for proposed cognate sets.

I should try to anticipate objections to the criteria just suggested. It is true that requiring CVC or longer forms in which all the segments match is unnecessarily strong in the long run. The details of Indo-European and other well-studied families abound in instances of short forms which have been reduced from longer forms by regular changes. Even though this is true, I would maintain that only clear matchings in CVC or longer forms should be admitted as direct evidence for a proposal of distant genetic relationship, because until the hypothesis is demonstrated we may not know whether the similarities in short forms are due to chance or to common history. Even though we may not be able to say that short forms are not true cognates, I would maintain that any relationship worthy of investigation will have enough cases of matchings in CVC or longer forms to establish the hypothesis as reasonable. Once established as reasonable, we can go on to seek the evasive cases of correspondences in shorter forms. Short forms, however, do not help much to overcome the possibilities of chance in the initial stages of a proposal of distant relationship.⁵

Similarly, semantic shifts are a fact of linguistic life, and many bizarre semantic shifts are on record. Nevertheless, I maintain that enough clear cases will remain in any true genetic relationship (which have not undergone shifts) that it can be established by considering only semantic equivalences. In principle, we may never know whether semantic non-equivalences which have similar sound are accidental or genetically transmitted similarities until we know the exact history. Once a proposal has been shown to have merit based on semantically equivalent forms, then we can go on to seek the bizarre and less clear cases. However, initially such cases offer no real support to

proposals of genetic affinity.

A final consideration involves onomatopoetic forms, which have too often been included as evidence in distant genetic proposals. Such forms (where the sound is not totally independent of the meaning) do little to support genetic relationships, since the similarity in sound may owe its origin to attributes of the referent rather than to a prior common history.

In conclusion, I suggest common sense as a general methodology. Common sense advises that constraints on the number of segments matched is needed if chance is to be negated. Common sense suggests semantic equivalence best denies chance factors; common sense advocates that regularity of recurrent matchings in a significant number of basic vocabulary items is most likely to negate alternate explanations from chance, diffusion, and universals. Common sense speaks for consideration of 'submerged features' and grammatical concerns outside the lexicon. I will try to employ these common-sense criteria in evaluating the proposed affinity of Uru-Chipayan with Mayan below.

1.5. In recent times others have proposed methods which stem from newer linguistic theories. Teeter (1964) proposed a method involving his 'depth hypothesis', or 'contextualization'. 'Depth' in Teeter's sense means relative earliness in some subset of rules of the grammar of a language. He suggests that much work in genetic relationship has been wasted on the manipulation of lexical items, which are inserted rather late in a grammar, and are not very 'contextualized' in terms of position in the grammar. He presented two examples relating 'deep' rules. The first is Proto-Central-Algonkian's: Possessive + V → Possessive + t + V, which is said to be rather early in the morphophonemic component. This is compared to Wiyot's: Possessive + h + V → Possessive + t + V (Wiyot has no vowel-initial stems). This correspondence of a 'deep' rule is said to provide qualitatively much of the evidence for the relationship between Algonkian and Wiyot.

The second example is Proto-Central Algonkian's rule of pronominal prefix ordering. Stems tolerate only a single prefix. The second person prefix is employed if either of the persons involved is a second person; if no second person is involved and a first person is, then the first person prefix is chosen; otherwise a third person prefix is chosen. This ordering is correlated with a similar phenomena in Wiyot. Wiyot does not employ prefixes to express persons with verbs. Negative preverbs, however, have three forms for the three pronominal persons, and when more than one person is to be related to the verb, only one negative preverb is used and its choice is governed by a rule identical to the Proto-Central Algonkian rule just mentioned.

These two deep rule correspondences are calculated to illustrate how position early in some component of a generative grammar can be used to establish genetic relationship. Teeter's method is not new, however. It is really Sapir's 'submerged features' method. It is dependent upon correspondences at points in the grammars which defy chance and diffusion, but reference to a formal grammar does not, I think, make the method any more potent than Sapir's original employment of it.

King (1969:152) has claimed that transformational syntax has 'potentially great usefulness in establishing genetic relationships among languages'. He refers to Harms (1966) paper relating Yukaghir and Uralic as a case in point. Since this is an important claim, it is appropriate to examine briefly Harms' use of transformational syntax in dealing with the controversial Uralo-Yukaghir hypothesis.

Harms points out that a survey of lexical items from these languages is only suggestive of a relationship, but that no strong case can be made from the formal-semantic similarities. His argument, then, is that 'the far-reaching formal-semantic similarities in the focus system (or "logical stress") of Yukaghir and the intricate subject and object case determinations in Balto-Finnic as well as various of the focus phenomena in all Uralic languages' is strong enough to rule out explanations based on chance, universals, or borrowing, and to establish the genetic relationship. In short, Harms' method is to account for the Yukaghir focus system in a short transformational grammar, and then to reconstruct internally an earlier Pre-Yukaghir stage. It is not clear to me why a transformational account of Yukaghir must be written in order to arrive at Harms' conclusions. It is perfectly plausible, beginning with surface data, to reconstruct internally Pre-Yukaghir without appeal to transformational syntax. In the end Harms' method is clear: the Yukaghir and Uralic relationship depends on sound correspondences discovered by Harms in the focus-case systems. This is by no means a new method of establishing genetic relationship, but rather is the comparative method.

Though there probably is something to the Yukaghir-Uralic hypothesis, it is important to point out as a caution to practitioners of distant relationships that Quechua shares a very similar system with much the same sound correspondences, as shown in the following (the first two columns are from Harms, p. 15):

YUKAGHIR	PROTO-BALTO-FINNIC	QUECHUA
me-, -m, -me-	*-mä, *-m	-mi, -m focus/inf. comp., acc./comment focus
-le	*-tä	-ta focus/partitive Obj/ Obj focus
-l	*-tä-k	-kti- inf/inf/complementizer
-pul, -pe	*-t (*pätä)	-kuna pl./pl./pl.
-l	*-tä	-ta adj. fromant/adj. form./derives adv. from adj.
-k	*-kä	-qa pron. focus/emph. clitic/topic focus
-k	*-kö	-ču interrogative
-k	*-k(ä)	-ču- imp./imp./3rd pers. imp.

I do not believe Quechua to be meaningfully related to either Yukaghir or Uralic, so that this chart should point out the strong need for caution even if valid methods are used.

2. Think of almost any American Indian language family (or Old World family too, for that matter) and probably someone has suggested that it is genetically related to the Mayan family. More or less respectable linguists have suggested Mayan affinities with such languages as Mixe-Zoquean, Totonacan, Huave, Xinca and Lenca, Natchez and Muskogean, Tarascan, Zuni, Uto-Aztecan, California Penutian, Arawakan, Araucanian, Yunga and Chipaya-Uru. Obviously an exhaustive evaluation of all these proposals is beyond the practical limitations of this paper. The Maya-Chipayan hypothesis is more or less widely accepted, and of the above it is the most fully articulated with the most explicit supporting data. Several of the other proposals (all the South American connections) were stimulated by it.⁶ Because of its impact and testability it is important to consider the evidence for Maya-Chipayan in some detail.

Ronald Olson (1964) presented the evidence for the proposed affinity between Mayan and Uru-Chipayan. His hypothesis was based on 121 supposed cognate sets. I will consider these sets first individually in some detail, evaluating the strength of each, and then collectively, evaluating the overall strength of the hypothesis. Olson's original numbers will be used for easy reference. The only change in Olson's data is q to represent his k, a post-velar or uvular stop.

(1) Chipaya (henceforth Cp) ahk- to take to pasture/Proto-Mayan (henceforth PM) *ahk', *ahk to shepherd, pasture. Olson's PM reconstruction is based on several unrelated etyma; Tzeltal ak- and Maya ak- have nothing to do with Huastec ak' (all would have to be glottalized or plain, but not mixed). Furthermore, the correspondence of Tzeltal k and Maya k reflect PM *q, and Olson has no proposed correspondence of Cp k/PM *q.

(2) Cp ax^w/PM *č'ax to wash. There is no CVC correspondence in this proposed cognate set; Olson offers no explanation for why PM č' corresponds to ø in Cp in this one set. The similarity may be purely chance.

(3) Cp ax-/PM *k'ax to harvest. There is no CVC correspondence in this set either (with no explanation for why PM *k' has no corresponding segment in Cp).

(5) Cp aš to be bashful/PM *k'eš bashful. Again, there is no CVC correspondence. Also, the Mayan forms seem to mean primarily shame.

(6) Cp ata/PM *ti? mouth. There is no CVC correspondence, and though these could conceivably be real cognates, the forms are too short to deny the possibility of chance accounting for their similarity.

(7) Cp ay(in)- to hurt/PM *ya? pain. Again, there is no CVC agreement to reduce the chance similarity. Olson suggested that both Cp and Tzotzil (ay-) have undergone metathesis, but Tzotzil ay- to be born is a different etymon for the other Mayan languages' ya(h) pain. This makes Cp look even less like Mayan, since no Mayan forms have ay.

(8) Cp čawk old, čah^wk important/PM *čawq. In reality there is no such PM form. Olson's reconstruction is based on Cakchiquel čaq old (really fallen), Mam čak big, large, and Mam čawak high. These Mayan forms have nothing to do with each other, either semantically or phonologically. The

Mam forms reflect *tak and *tawak (Mam \check{x} < PM *t; which corresponds to Cakchiquel \underline{t} (not \underline{t})). Mam \underline{k} corresponds to Cakchiquel \underline{k} ; Cakchiquel \underline{q} corresponds to Mam \underline{q} . There are simply two separate etyma involved, not reflecting anything like Olson's reconstructed PM form.

(9) Cp $\check{c}ew(\check{sin})$ - to provide shade/PM *te?w cold, frozen. There are two problems in these proposed cognates. Semantically, they are less than likely. Phonologically Proto-Maya-Chipayan (henceforth PMC) **t ($**te?w$)⁷ is supposed to have the regular Chipaya reflex of \underline{t} , not the \check{x} which is given here. That is to say, Cp \check{c} /PM *t is not a regular, recurrent correspondence, and hence there is no support for this example.

(10) Cp $\check{c}hep$ /PM *ošeb three. The -eb of Olson's PM form is the plural morpheme which occurs with numerals in their elicitation form in many Mayan languages. The root is oš three, which has nothing to do with Cp $\check{c}hep$ since $\check{c}h$ and \check{s} are not suggested to correspond. In this example, then, there is not one single correspondence of \underline{C} or \underline{V} (not to mention \underline{CVC}). Of course, the hypothesis can be supported only by regular sound correspondences, not by the irregularity of this and the preceding example.

(11) Cp $\check{c}hip$ -/PM *č'ihb to fill (PMC **č'ihp). There are a number of problems with this example. First, PMC **č'h is supposed to have the regular Cp reflex of \check{c} , so that there is no regular sound correspondence in this case. Also, there is no such PM form, since its reconstruction is based on non-cognates. Cakchiquel č'ihb(a) and Mam $\check{c}up$ cannot be equated. The Cakchiquel form is actually č'ix to contain plus -b'a (instrumental causative of positional verbs). It cannot be related to Mam $\check{c}up$ since Cakchiquel \underline{p} corresponds to Mam \underline{p} , Cakchiquel $\underline{b'}$ to Mam $\underline{b'}$; Mam \check{c}' can correspond only to Cakchiquel $\underline{t'}$ (from PM *t'); finally, Mam \underline{u} and Cakchiquel \underline{i} do not regularly correspond.

(14) Cp $\check{c}hohq$ - untie, loose/PM *č'ohq/*č'ohk to pull out. These have only a vague semantic connection, and neither is part of the very basic vocabulary.

(15) Cp $\check{c}huht(u)$ small, undersized/PM *č'ut small, short, undersized (PMC **čhut). The regular sound correspondences Olson posited would demand Cp to have a retroflexed consonant in this case; therefore, this is based on an irregular correspondence.

(16) Cp $\check{c}hul$ -/PM *č'ub to kiss. There is no CVC correspondence in this case (\underline{l} is not said to correspond to \underline{b}). Furthermore, kiss is onomatopoeic, having irregular correspondences even within the Mayan family.

(17) Cp $\check{c}ip$ to be happy/PM *č'eb to laugh. The PM form should be *č'e?--; the -b is a separate morpheme which occurs with this form only in a very few of the languages. There is no CVC correspondence, and the semantic connection could be closer.

(18) Cp - $\check{c}uk$ towards (with directions)/PM *č'i(h) towards, in. There is no CVC correspondence in this example. Also, the PM form really means at (i. e. location with no special implication of motion). The PM form is derived from mouth, just as most other Mayan positional particles are derived from body parts (cf. the discussion above). For that reason, Cp in

no. 6 above and no. 18 here cannot both be cognates to this single Mayan form, since they have quite different forms.

(20) Cp čahk- to hit, throw/PM *čahq' to throw. In reality the PM form should be *kaq' (Olson's reconstruction is based on Western Mayan languages which have č; Eastern Mayan languages have k reflexes, which demands *k for PM). This, of course, is not one of Olson's proposed correspondences. The Pokomchi form čahq to push is simply a separate etymon, not related to to throw.

(21) Cp čara rock/PM *č'en rock, cave. This is the only example presented of this affricate correspondence, and is therefore not really a recurring, regular sound correspondence. The correspondence of r/n is not well supported either. It hinges on getting an l into the picture through Yucatec Maya čal(tun) rock. But this is really a separate etymon (PM *ton) rock; Yucatec has č'e'en well as cognate with Olson's č'en cave of the other Mayan languages. (Even if one could find a spurious l/n correspondence within the Mayan family, Yucatec's č would have to be glottalized in order for it to participate in the regular correspondence set.)

(22) Cp čhaq to finish, complete/PM *č'aq to complete. Olson says that "the reason for the retroflexion in the Cp č is obscure," meaning that the correspondence is irregular. Also, the PM form is again based on unrelated etyma (e.g. Maya čuk pahal and Cakchiquel č'aq(at), where č/č' is never a correspondence in Mayan, nor is u/a).

(23) Cp čhap- to pierce, nail/PM *č'ab. Olson's PM form is erroneously reconstructed on the basis of unrelated etyma. Huastec čup(iš) to penetrate cannot be related to Cakchiquel č'ab(iš) to be pierced (-iš is the passive morpheme) or č'ap(uh) to drive wedges in a log, since the regular correspondence would require both to have glottalized or both non-glottalized affricates. Chol č'ub to penetrate, puncture has the wrong vowel to correspond to either Cakchiquel or Huastec.

(24) Cp čhe·t- to stretch out/PM *č'aht bed. These are not semantically equivalent, nor is there any a priori reason to believe that to stretch out is related to bed (or sleeping) in any non-arbitrary way.

(26) Cp čhoh-/PM *č'uh or *č'uh to drip. Olson admits that the "retroflexion in the Cp form is ... obscure" (p. 31); i.e., there is no regular sound correspondence in this example. Furthermore, to drip is onomatopoeic in Mayan, as witnessed by the irregular reflexes involving both č' and č' (and t').

(27) čhulu what/PM *čVʔbV what, that which. There is no CVC correspondence in this example, and there is no such PM form. It is based on Chol čubi that which and Pokomchi čahaʔ wilik whatever, but these are in no way cognates. (To be cognates both would have to agree in their consonant and vowel correspondences, but these do not.)

(28) Cp čhuš/i/PM *č'iʔš thorn. The similarity in this proposed example of cognates is striking. However, it is not a case of support for the hypothesis because Olson's PM form is erroneously reconstructed. It should be PM *k'iʔš since the Eastern Mayan languages (which Olson did not consider) have

reflexes with k' (e.g. Quiche k'i:š, etc.). There is, of course, no correspondence of Cp č to PM *k' in Olson's work, and therefore this example has no regular recurring sound correspondence to support it.

(29) Cp ek- to leave/PM *ik' to leave, pass by. This proposed cognate has several problems. First, there is no CVC correspondence to reduce the possibility that it is merely a chance similarity. Second, the PM reconstruction has several problems. It is based upon Pokomchi ik' to pass by and Tzeltal ik(tay), Tzotzil ik(ta) to leave, which are not cognates. The Tzeltal-Tzotzil to leave is the leave of abandon, leave it (dejar), not the leave of a person's going. It cannot be cognate with Pokomchi ik' because the regular correspondences require either glottalization or non-glottalization of k in both, and Pokomchi k' corresponds to Tzeltal-Tzotzil č', not k (Tzeltal-Tzotzil k corresponds to Pokomchi q).

(30) Cp ewi last year/PM *ewi(r) yesterday. There is no CVC correspondence in this example, and the semantic association is a bit vague. (The Mayan forms are also bimorphemic, ew-ir).

(31) Cp hača/PM *ek'al, *hek'al tomorrow. The PM form is based on several different etyma, and even Olson says, "Some of the developments within the Mayan languages are obscure" (p. 32).

(32) Cp hat-/PM *huš to sharpen. The Mayan forms really means whetstone, and Olson has no proposed t/š correspondence. Therefore, this example is both vague in its semantic connection, and has no regular sound correspondences (except h/h) to support it (a/u and t/š do not correspond).

(33) Cp heh- to breathe, rest/PM *heʔel to rest. The Cp form is onomatopoeic, as the PM form may be.

(34) Cp ho-/PM *hoč to drag. These forms are too short, and there is no CVC correspondence; nothing explains the absence of a Cp reflex corresponding to PM *č. The similarity may well be due to nothing more than chance.

(35) Cp h^wer to untie/PM *h^wel to undo, untie. The PM form is based on separate etyma which are non-cognate. Cakchiquel wer(er) to undo cannot be cognate with Yucatec Maya hil(mok) to tie with a knot that is easily undone since there is neither a w/h nor a r/l correspondence set. Cakchiquel r corresponds to Maya y (never Maya l) and Cakchiquel w corresponds to Maya w (Cakchiquel changed *h > w before round vowels, but that has no bearing here).

(37) Cp iks womb, ik- to be pregnant/PM *(C)ik' intestine, pregnant. This example has several problems. As it stands, there is no CVC correspondence to prevent chance. The forms listed in support of the PM reconstruction are not cognates. Pokomchi eq(saʔ) intestine is not cognate with Tzeltal yik' pregnant (since these do not agree in the glottalization of k, and in the presence or absence of y). Neither of these is cognate with Tzotzil bik(il) intestine, where b must correspond to b (not to ∅). Also, the Cp form looks suspiciously like Quechua wiksa belly, womb, which may indicate a loan from Quechumaran languages.

(38) Cp is(ñi) ingernail, iš(qe) tooth/PM *iš- ingernail. Since there is no CVC correspondence, the similarity could be due to chance.

(40) Cp kahl-/PM *kal to pull out. If the PM form is real (based on Huastec kal(θal) and Maya kol pull out), then it is erroneously reconstructed. If these Mayan forms are real cognates, then they reflect PM *q, not *k. (PM *k became č in most environments in Maya, and it became ǰ in Huastec.) The k of Huastec and most of the k's in Yucatec Maya can come from only one source, PM *q (cf. Olson's p. 318). There is no proposed correspondence of CP k to PM *q in Olson's work.

(41) Cp kaw- to squat/PM *k'aw to lie with face up, to prostrate oneself. The semantic connection is not equivalent.

(42) Cp ker- male (of animals)/PM *kel, *qel male of birds, boy. The PM form is suspicious. Pokomchi per(en) male of birds has nothing to do with Tzeltal kele- male of birds (p/k is not a correspondence set), and Tzeltal-Tzotzil kerem boy is not obviously related to either of these forms. Boy is a suspicious form in that r is extremely rare in Tzotzil-Tzeltal, and it is not obvious what the semantic connection between boy and males of animals is. Furthermore, the Cp form ker looks suspiciously like q(h)ari male of Quechua.

(43) Cp khat- to fight/PM *k'aq. The PM form is spurious. Cakchiquel k'aq to deprecate another, melancholy is not cognate with Pokomchi k'a angry, nor with Maya k'at(um) to fight (q/θ/t is not a correspondence set).

(44) Cp kher- to burp/PM *k'eb to burp. Burping in Mayan languages is onomatopoeic. Furthermore Cakchiquel k'ab to gasp is not cognate with Tzeltal keb(ah) to burp (if it were then Tzeltal would have to have č' corresponding to Cakchiquel k', or Cakchiquel would have to have q corresponding to Tzeltal k). In any case, Cp r and Mayan *b are not said to correspond, so there is no CVC correspondence in this case.

(45) Cp khew- to become fat/PM *keh**b** robust, stout. The PM form is not well founded. Cakchiquel kub(aš) robust is not the same etymon as that reflected by Tzeltal-Tzotzil hu(h)b to become fat (since there is no known k/h correspondence between these languages).

(46) Cp khuñ- to remember/PM *k(')an to know, remember, learn. Again the PM form is spurious. Cakchiquel kan(ay) to know and Quiche ul(ik) to remember have nothing to do with each other (both languages have both etyma). Chol kñ and Maya kan may be cognate with each other, but if they do descend from PM they must reflect PM *q (cf. p. 318), and Cp kh does not correspond to PM *q in Olson's scheme.

(47) Cp kiht to nail/PM *k'iš thorn. This set has but a very weak semantic connection, and there is no CVC correspondence (Cp t and PM *š are not said to correspond). Furthermore, the PM form is the same as in 28 above, but the same PM form cannot be cognate to these two distinct Cp forms.

(48) Cp kokhi skunk/PM *ku(h)k, *ku(h)k squirrel. Though these are not implausible, they are not semantic equivalents.

(49) Cp kol- to stoop/PM *k'oŋ to stoop. Since l/ŋ is not a regular correspondence in Olson's work, there is no CVC agreement to deny chance similarities in this case. Furthermore Olson's PM form is not well founded.

Jacalteco k'oŋ(ba) to stoop comes from PM *k'oŋ to leave (dejar), and Jacalteco n does not correspond to Tzeltal h (koht' (ah)), but rather only to Tzeltal n. Also Jacalteco k' and Tzeltal k do not correspond (either k'/c' or q/k, not k'/k).

(51) Cp kon(a) sickness/PM *k'oŋ to cough up phlegm, to clear the throat. The semantic connection is a bit vague, and the PM form is not supported. It is based only on Maya kol(kal) to clear the throat (kal is throat) and Cakchiquel kon(on) to cough up phlegm, which are not cognates. There is no l/n correspondence set between these two languages. (The real cognate is Cakchiquel qul throat.)

(52) Cp qah to ask for on credit/PM *qah to ask to borrow, beg. Although the semantic connection is a little vague in this example, the PM form is much worse. It is based on Cakchiquel qah to ask to borrow (which really means to lower, sink, descend) and Pokomchi (pah)qax to ask, beg (which is really the Pokomchi verb pahq to ask plus -ax (transitive verb ending)). These are in no way cognates.

(53) Cp qaši/PM *qaš, *qal neck. Actually the common cognates in the Mayan languages reflect *qul. Olson's *qaš is based solely on Kekchi kuš, but this form reflects PM *k (not *q), and Cp q/PM *k is not one of Olson's correspondences.

(55) Cp qhara/PM *q'ab hand. Since r/b is not said to be a correspondence, there is no CVC agreement and this could be a similarity due to chance.

(56) Cp qhaw- to shout, txawun- to cause to shout/PM *aw to shout. These are onomatopoeic forms, and there is no CVC correspondence. The Cp form may be a loan from Quechua qhaway.

(57) Cp qhay- buy/PM *k'ay to sell. Although the correspondence of Cp qh to Mayan k' is said to hold before PMC **y, this is the only example, and hence it is not a regular recurring sound correspondence.

(59) Cp quš heart, will/PM *k'uš heart, will, chest. Though the superficial similarity is impressive, this example has difficulties. Cp q is said to correspond to PM *k' only when it is before an š in the same form, but this is the only example so that this also is not a regular recurring sound correspondence.

(60) Cp quš/PM *quč to carry. Cp š is said to correspond to PM *č in final position, but this is the only example of such so that this also is not a regular sound correspondence.

(61) Cp qut- to heat/PM *k'at to burn. There is no proposed correspondence of q/k' in Olson's scheme (an 'obscurity' which Olson admits).

(65) Cp lop- to stain, dirty/PM *lob to stain. There is no such PM form. Olson's PM reconstruction is based on Cakchiquel (ɬ'i)lob to stain (really ɬ'il dirty plus -ob' (instrumental causative)) and Tzeltal bolob to stain (bol- plus -ob). These are in no way cognate, nor related to anything like *lob.

(66) Cp luko husband, luktaqa man, lukmahč boy/PM *winaq man. Olson has no proposed k/q correspondence (l/n correspond in only two

examples). For this set to be considered, one would have to disregard the initial PM *wi

(68) Cp mahk- to put inside/PM *ma(h)q' to bury, close the top. The semantics are vague in this example, and Olson has confused several etyma in setting up his PM form. One is -muq- to bury; another is -maq- to close. The correspondence of Cp k to PM *q' is supposed to hold only after PMC **ah-, but in this case there is no real h. The h which occurs in Olson's Mayan examples is the h which is the infix passive morpheme in Tzeltal.

(72) Cp mur(mursi) quicksand-like/PM *mul to sink. The semantic association is a bit vague, and Olson admits that this is a 'comparatively weak set' (based on separate Mayan etyma).

(73) Cp nuk(ta) big/PM *niwak big. Actually the PM form is *nim. The *-ak (really *-aq) is the plural morpheme which occurs with some adjectives. (The w of Tzeltal is a dissimilation of nasals in sequence and is only a secondary development in that language (niwak from *nim-aq).) Also, Olson confused a separate etymon, *naqat near, which has nothing to do with big.

(74) Cp ohk- to go, walk, qxohča foot/PM *oq foot, leg. There is no CVC correspondence to reduce the chance of accidental similarity.

(75) Cp oka vicuña/PM *ok' coyote, fox. These are too vaguely connected semantically, and have no CVC correspondence.

(76) Cp oq- to have compassion on/PM *oq' to cry. These are vaguely connected semantically, and there is no CVC correspondence.

(77) Cp paki rib/PM *bak bone. These are not equivalent semantically, and the PM form should be *baq (Quiche, Mam b'aq) and there is no k/q in Olson's correspondences.

(78) Cp paq grow, paqhi big/PM *paq', *baq' great, big, fat. Olson again confused several Mayan forms which are not cognates. Cakchiquel baq (said to mean great) actually means only bone, and by extension skinny. Pokomchi -aq (a suffix) does not mean big, but rather is the plural marker on some adjectives (including the adjective for big). Jacaltec paq' (ič) fat and Huastec pak(θa?) big do not correspond (there is no q'/k correspondence, but only q'/k' or q/k).

(79) Cp paqu/PM *peq' dog. The forms pek' in Maya and Mopan, and pik' (o?) in Huastec are borrowed from Zapotec (Proto-Zapotec *pe?kku). The native Mayan form in all the other languages is *ç'i?.

(80) Cp pari string/PM *bal to make rope, twist cords, roll up, twist, etc. I believe the actual meaning of these Mayan forms is closer to roll, ball, twist with no special reference to the object of twisting, etc. In any case, the connection is vague semantically, and the Mayan forms are onomatopoeic (if they were not, the examples cited would not have bal, but p'al by the regular rule which changes b to p' before sonorants and fricatives in these languages). The PM form is *b'ol, *b'al round, ball.

(81) Cp pat- to make a basket/PM *paç' granary, basket. Again the PM form is unfounded. It is based on Cakchiquel paç'an granary of corn stalks and Huastec peç' (eç') basket, but these cannot be cognates because

Cakchiquel ζ' corresponds to Huastec t' (Huastec ζ' corresponds to Cakchiquel k'). Also, the meaning between these proposed items is not very close.

(82) Cp phik- to fill up with food (person)/PM *bihq' to swallow. These are not semantic equivalences, though not implausible.

(83) Cp phit straw/PM *hiṭ to make petates, to tie reeds. The semantic association between the two is a bit weak (since petates are not necessarily made of straw).

(84) Cp phoma well (with plenty of water)/PM *hom hole. Actually there is no such PM form, since Olson's reconstruction is again based on several different etyma. Cakchiquel hob(e) to make a hole in the ground is not related to Tzeltal hom hole (there is no such b/m correspondence, rather only b/b and m/m (and rarely p'/b)). Huastec mom well has m 's that do not correspond with anything in the examples from the other languages.

(85) Cp phur- to pin, to fasten with a pin/PM *hul hole. The semantic association is too vague. (Although Olson lists other meanings for hul in Mayan languages, its only real meaning is hole. His suggested prick, sting is probably from hup(el), not hul.)

(86) Cp phurna peak, ridge/PM *holna ridge, head. Actually the Mayan forms reflect only *xol(om) head. The Tzeltal form holna, said to mean ridge, really means ridge of the house, and is bimorphemic from hol head plus na house. Thus the semantic association as well as the phonetic similarity between the Cp and the PM forms is less suggestive.

(87) Cp pokulʸ(či) blister, puku(či) bladder/PM *poq'V(1) to blister, smallpox. Olson confused several unrelated Mayan forms again in this case. Cakchiquel poq'i(sah) does not mean so much to blister as stated, but rather to swell (which includes blisters). The Yucatec Maya p'o·lak to blister is unrelated to this Cakchiquel form. The Maya form is from *b'ol round. A more common Yucatec form for blister is t'aham, not associated with round.

(88) Cp sami louse/PM *samik ant. Actually the PM form should be *sanik, as reflected in all the Mayan languages. Olson's Tzeltal form ζ' isim warrior ant is not cognate, rather Tzeltal šanič ant. Thus, no forms show the m which Olson reconstructs for this item. Thus there is no CVC, and the semantic association is not one of equivalence.

(89) Cp sqara crotch/PM *šak(an) fork, to prop with a forked stick, to straddle the path. There is no regular sound correspondence involved in these examples, since Cp s is said to correspond to PM ξ only before nasals; also the q/k is not one of Olson's regular sets. Olson says, "The fronting of the s and backing of the k occur under obscure conditions in Chipaya" (p. 35).

(90) Cp sqo· - white salty residue on the ground/PM *saq white. The semantic association is too distant; presumably the Cp form means something like alkali, where white and salty are merely descriptive terms employed in the translation equivalent.

(91) Cp smew-/PM *šub to whistle. These are both onomatopoeic forms. The correspondence s/ξ is said to occur when a nasal follows, but since this is the only example of it, it cannot be considered regular. Also, Olson does

not explain why in this one instance -me-/-u- correspond.

(92) Cp spaht/PM *bač' to spin thread. The correspondence t/č' is said to occur in final position, but this is its only example and hence it is not regular.

(94) Cp šehq- to calm (rain)/PM *šehk(al) to wet, rainbow. The semantic connection is vague (wetting and calming need not be related, and rainbows and calming may be related only biblically). In any case, Olson again confused several Mayan etyma. Cakchiquel šek'aba to wet has nothing to do with Pokomchi riškohl (from r- 3rd pers sg -iš- diminutive -ko(h)l to hang (-h- is the infix passive morpheme)). Neither has any connection with Tzeltal sehkara (Cakchiquel š corresponds to Tzeltal š, not s, k' to č', not k, and b to b, not r (r occurs only in onomatopoeic and rare forms in Tzeltal)).

(95) Cp šihl- to scrape off, flake off/PM *šil to make pieces, not complete, make an opening. The semantics is vague, and the Mayan examples seem to confuse several non-cognate forms.

(96) Cp šik- to part, to pick out with the fingernails/PM *siq' to pick up, to get, to select, search for, to part. The semantic association is vague, and seems even vaguer when it is realized that Cakchiquel sik'(a) (said to mean to pick up, to get) really means to lay (of chickens), to collect. The Jacaltec example, seq'(tox) to part, cannot be cognate with the Cakchiquel form because there is no k'/q' correspondence set (only k'/k' or q'/q').

(97) Cp sika green and dry (leaf)/PM *sik', *šik', *šik' tobacco, leaf. Olson confused two Mayan etyma, *sik' tobacco, cigarette (Pokomchi, Cakchiquel, and Jacaltec sik' in Olson's examples), and *šaq leaf (Mam šaq Huastec šek(lek)). The semantic connection is vague in either case.

(98) Cp šip beard/PM *šob chin, to bristle. Though not implausible, these are not semantic equivalents.

(99) Cp šqala field/PM *q'al, *k'al field, property. Olson's Mayan examples are from two separate etyma, *kol milpa (Maya kol field) and *ak'al land (Tzeltal k'al field, Pokomchi ak'al land, Jacaltec ak'al plain). Huastec k'a·l(a·b) property has to be from some other source, since it reflects PM *q'.

(100) Cp šqoqa tadpole/PM šukuk toad. Although there is a tendency for the several terms for toads in Mayan languages to be onomatopoeic (through croaking), that is not the only problem with this set. Cp q is said to correspond to PM *k only when š precedes it in Cp; however, this is the only example of such a correspondence and hence it is not regular.

(103) Cp taqu word, speech/PM *taq' to answer. These are not semantic equivalences, and the correspondences are irregular, as Olson says, "the absence of the expected h reflex in the Cp form is obscure" (p. 36).

(104) Cp tan(qat)- to cause to burn (to cause ashes)/PM *taʔŋ ashes. These are not semantic equivalents. Also, one might wonder whether the Cp form could not be analysed as t- causative plus an burn (which Olson says

occurs only with the postposed causative element, qat). This is suggested by Olson's Cp examples qhaw- to shout, txawun cause to shout; qa· - to cry, txa·n cause to cry; and qut to heat, txutun cause to heat. Olson suggested that a causative morpheme tx- replaces initial q, but I would think rather that the morpheme was t-, which caused adjacent q to become x.

(105) Cp tha· - to give/PM *č'ah to give, to give a slap. These have no CVC correspondence.

(106) Cp thaxr(ki) - to cause to salivate/PM *tuhb saliva. These have no CVC correspondence (xr is not said to match hb). The Mayan forms are onomatopoeic (from the sound of spitting), as witnessed by their irregular correspondences within the family (i. e. *č/*t/*č). Finally, the Cp form looks like another t- causative (the root may be something like -haxr-), making it much different from the Mayan form.

(107) Cp thak- to hit (game when hunting)/PM *t'ahq to throw, shoot. The PM form is erroneous; Olson presented Cakchiquel č'aq(ih) to throw, but Cakchiquel has only -k'aq(ix) (or -k'yaq(ix) in some dialects). This form, of course, is not associated with Olson's correspondence set in which Cp th participates. The Tzeltal form tuk'(ay) to shoot (with a rifle) has nothing to do with the Cakchiquel form (t and k' do not correspond). It is perhaps to be derived from tuk' straight (with -h- infixes as passive and -ay as verbalizing suffix).

(108) Cp thap- to burn/PM *č'ab to heat, light a fire. These are not semantic equivalents (though not implausible). The PM form is not well supported, however. It is based on Pokomchi č'ab(sanik) to heat, and Maya t'ab- to light (a fire), but these are not cognates, since there is no regular correspondence of č'/t' (only Maya t'/Pokomchi t', or č'/č', or t'/č' (from PM *t')).

(111) Cp txa· - to laugh/PM *č'eʔn to laugh. There is no CVC correspondence in these forms. Furthermore, there is no real explanation for Cp x/PM *∅. It is said that Cp x corresponds to PM ∅ in prevocalic position, but this is the only example of such a correspondence, so it is not a regular one.

(112) Cp toh- to divide an inheritance/PM *toh to pay. The PM *h is supposed to correspond to Cp h only in word-final position, but this is the only example of such a correspondence and hence it is not regular. (Actually the PM form, even by Olson's correspondences, should have been reconstructed as *tox, and h/x do not correspond in Olson's work.)

(114) Cp tos(kara) a lie, falsehood/PM *tos to pronounce obscurely, to lisp, to lie. In reality both Cakchiquel tos to lisp, pronounce badly and Maya tus lie are loans from Aztec tos- voice (with its many derivative meanings).

(115) Cp tup(i) strong/PM *č'Vp strong. This PM form is not substantiated. It is based upon Cakchiquel č'up(up) and Huastec č'ap(ik) alone, but these are not cognates. Cakchiquel č' corresponds only to Huastec t'; Huastec č' only to Cakchiquel k; and the vowels u/a do not correspond.

(116) Cp č'hel(a) rip, tear/PM *č'el to rip, tear. The PM form is based on several unrelated etyma. Cakchiquel č'er cannot be related to Cholan-Tzotzilán č'il, sil, because Cakchiquel r corresponds only to y in those lan-

guages (their l corresponds only to Cakchiquel l); and the vowels do not correspond (either e/e or i/i, but not e/i).

(117) Cp ulan to go out/PM *el to go out. There is no CVC correspondence.

(118) Cp uš girl/PM *iř female, etc. There is no CVC form correspondence.

(119) Cp wel- to toast (flour)/PM *weʔ1 to eat, food. The semantic association is not overly close in these forms, but the PM form is not well supported anyway. It should be *waʔ food (also eat); the -el in some of Olson's Mayan examples is the adjectival, possessive morpheme, and is not reconstructable as part of the PM form. Thus, there is no CVC agreement, either.

(120) Cp wiy(a) dream/PM *war(an) to sleep. These forms contain the only example of Olson's proposed y/r correspondence, and hence it is not regular. Since there is no CVC agreement in this case, it could be similarity due to chance.

(121) Cp yap(an) - to wrap up, to cause to be wrapped up/PM *yab to bend, curve, have a cramp. The semantic connection is quite vague.

3. It has been seen that Olson's proposed cognate list of 121 items has several problems. Considered collectively, the list has more than thirty-five cases of irregular, non-recurring sound correspondences, which greatly weakens the hypothesis, since these were claimed to be regular. (Examples: 9, 10, 11, 15, 20, 21, 22, 26, 28, 31, 37, 40, 44, 46, 49, 53, 55, 57, 59, 60, 61, 66, 68, 77, 88, 89, 91, 92, 100, 103, 106, 107, 111, 112, 120.)

Multiple, non-related etyma were given in support of about forty erroneous PM reconstructions. (Examples: 1, 2, 11, 20, 21, 23, 27, 29, 35, 37, 40, 42, 43, 44, 45, 51, 52, 53, 56, 65, 68, 69, 73, 78, 81, 84, 85, 87, 88, 94, 95, 96, 97, 107, 108, 115, 116.) Since the PM reconstructions in these cases are not valid, they cannot be related to Cp, and this greatly weakens the hypothesis. It might be claimed that in some of these cases the non-cognate forms could be eliminated, still salvaging some viable Mayan form. Though this may be possible, I am quite certain that the majority of these cases will support no PM reconstruction.

In six purported cognate sets Olson related a single PM etymon to two separate Cp forms. Thus, at best, three of Olson's cases must be eliminated (perhaps all six?) (Examples: 6 and 18, 28 and 47, and 17 and 111).

At least two other sets must be eliminated because the forms are not truly Mayan, but rather were borrowed into Mayan languages from Zapotec (79) and Aztec (114). Three other cases might be suspected of involving Quechumaran loans in Cp (37, 41, and 42).

More than thirty of Olson's examples fail to have CVC correspondences, which means that they could be due to mere chance similarity. Also, the cases with irregular or non-recurring correspondences must automatically be considered as non-CVC sets. Together, these leave a rather large percentage of the 121 cognate sets which do not deny chance factors. (Examples of non-

CVC sets: 1, 2, 3, 5, 6, 7, 10, 16, 17, 18, 27, 29, 30, 32, 34, 37, 38, 44, 47, 55, 73, 76, 88(?), 105, 106, 111, 117, 119, 120.)

Nine of Olson's sets involve onomatopoeic forms which quite probably are similar due to the nature of their referents rather than their history. This may be a subjective matter, but nevertheless is a valid criticism. (Examples: 16, 26, 33, 44, 56, (80?), 91, 100, 106.)

In a few cases, Olson drew morpheme boundaries in the wrong places, creating a false appearance of similarity. (Examples: 17, 65, 73, 86, 104, 78.)

Finally, about forty cases involve a vague semantic connection. (Examples: (4), 9, 14, 18, 24, 29, 30, 32, 41, 42, 47, 48, 51, 52, 57, 68, 72, 75, 76, 77, 78, 80, 82, 83, 85, 86, 88, 90, 94, 95, 96, 98, 103, 104, 106, 112, 119, 121.) Some of these cases are not as implausible as others (and 'semantic vagueness' may be a matter of opinion). Nevertheless, if the hypothesis cannot find sufficient support from semantically equivalent examples, then I believe that these non-equivalent cases will not help much. The high percentage of the 121 proposed cognates with vague semantic connection greatly weakens the hypothesis.

So far I have been negative. On the basis of the negative information discussed I would eliminate most of the cases discussed from consideration in a genetic proposal. They have other possible explanations from chance, loans, onomatopoeia, etc. With these eliminated, very little indeed remains upon which to support a case for genetic relationship.

Now I would like to consider the hypothesis as positively as I can, again discussing Olson's examples.

(6) Cp ata/PM *t̥iʔ mouth. This set does not meet the CVC requirement, but it does involve basic vocabulary; it is suggestive, and if the matching of t/*t̥ can be found to occur in other cases, this example may lend supportive evidence.

(28) Cp čhuši/PM *k'iʔš (Olson's PM *č'iʔš) thorn. Even though these do not agree with Olson's suggested correspondences, if other examples with the matching of č/*k' (or š/š) can be found, then this example would support the hypothesis. It is striking in its similarity, and it is rather basic semantically.

(48) Cp kokhi skunk/PM *kuʔk squirrel. These have striking phonological similarities, even though they are not semantically equivalent. If the hypothesis could be supported from stronger examples, then this example might lend secondary support.

(50) Cp kon to kill/PM *kam to die. This is a possible cognate, since die and kill are morphologically related in Mayan, and it is easy to imagine a final m changing to n in Cp.

(57) Cp qhay- to buy/PM *k'ay- to sell. This is a suggestive example which would support the hypothesis if the recurring matchings could be found in other forms.

(59) Cp quš heart, will/PM *k'uš heart, chest, will. These are suggestive just as in the previous example, illustrating the same q/k' matching,

with the wrinkle that Cp has qh in one but q in the other. If more examples of either can be found in other forms, then they will lend support to the proposal.

(61) Cp qut- to heat/PM *k'at to burn. Though not semantically equivalent, these are not at all implausible, and they involve the matching of no. 59, q/k'.

(62) Cp lahč(a) long, tall/PM *naxt far. Though not semantically equivalent, these are plausible, and could provide secondary support if the matchings are found to recur.

(70) Cp moq- to knot, tie/PM *moq' fist, knot. This set is striking in its similarities; it would lend strong support if its matchings recur.

(71) Cp muk- to chew/PM *mak' to eat soft foods, to feed, nourish. These are not unlikely, though not semantically equivalent; the strength depends on whether the matchings recur.

(88) Cp sami louse/PM *sanik ant. This is weakly suggestive. The semantics are not equivalent, and the matchings (especially m/n) seem not to recur.

(99) Cp šqala field/PM *kol field, *k'al land (Olson's *q'al, *k'al). This example seems good. It illustrates the recurring matching of q/k', and semantically it is at least possible.

(112) Cp toh- to divide an inheritance/PM *tox to pay (Olson's *toh). This involves a striking phonological similarity, and although the semantic association seems a bit vague, it is not implausible. Such a case might lend secondary support if its matchings recur.

(120) Cp wiy(a) dream/PM *war- to sleep. These seem semantically plausible, although the y/r correspondence does not recur.

In conclusion, I find very few examples which suggest a possible relationship between Uru-Chipayan and the Mayan family. When Olson's evidence is considered in light of valid criteria for distant genetic research, it is found to have many shortcomings. I do not deny that there may be a genetic relationship between these two families, but Olson has not shown that his similarities are indeed to be attributed to such a relationship and not possibly to chance or other factors. Also, it has not been shown that the little bit of supporting evidence that there may be (e.g. a possible recurring matching of q/k', etc.) is to be attributed to some special relationship between Mayan and Uru-Chipayan, rather than to some wider classification, such as Macro-Macro-Penutian, or Macro-Amerind. That is, it is not clear that a test of most American Indian languages would not turn up as much evidence for relation to one or the other of these two families. Therefore, my general conclusion is that the Maya-Chipayan hypothesis may be worthy of more investigation, but that we cannot at present fully embrace it.

WORKS CITED

- Bender, Marvin L. 1969. Chance CVC Correspondences in Unrelated Languages, *Lg* 35. 519-31.
- Bergsland, K. and H. Vogt 1962. On the Validity of Glottochronology, *Current Anthropology* 3. 115-53.
- Campbell, Lyle 1971. Historical Linguistics and Quichean Linguistic Prehistory. Unpublished dissertation, UCLA.
- Greenberg, Joseph 1963. Languages of Africa. *RCPAFL* 25.
- Haas, Mary A. 1969. The Prehistory of Languages, *Janua Linguarum* 57. The Hague: Mouton.
- Hamp, Eric P. 1967. On Maya-Chipayan, *IJAL* 33. 74-6.
- _____. 1970. Maya-Chipaya and Typology of Labials. Papers from the 6th Regional Meeting of the Chicago Linguistics Society, 20-22.
- _____. 1971. On Maya-Araucanian Comparative Phonology, *IJAL* 37. 156-9.
- Harms, Robert 1966. Ugric Reflexes of the Uralo-Jukaghir Focus System. Unpublished paper, University of Texas. Published (1967) in Hungarian: *Az uráli-jukagír fókuszrendszer ugor megfelelői. A magyar nyelv története és rendszere (Nyelvtudományi értekezések, 58)*, ed. by S. Imre and I. Szathmári, 94-103. Budapest: Akadémiai Kiadó.
- Hoijer, Harry 1956. Lexicostatistics: A Critique, *Lg* 32. 49-60.
- Katz, K., and P. Postal 1964. *An Integrated Theory of Linguistic Description*. Cambridge, Mass.: MIT Press.
- King, Robert D. 1969. *Historical Linguistics and Generative Grammar*. Englewood Cliffs, N. J.: Prentice-Hall.
- Olson, Ronald D. 1964. Mayan Affinities with Chipaya of Bolivia, I: Correspondences, *IJAL* 30. 313-24.
- _____. 1965. Mayan Affinities with Chipaya of Bolivia, II: Cognates, *IJAL* 31. 29-38.
- Sapir, Edward 1925. The Hokan Affinity of Subtiaba, *AA* 27. 402-35; 491-527.
- Stark, Louisa R. 1970. Mayan Affinities with Araucanian. Papers from the 6th Regional Meeting of the Chicago Linguistics Society, 57-69.
- _____. 1972. Maya-Yunga-Chipayan: A New Linguistic Alignment, *IJAL* 38. 119-35.
- Swadesh, Morris 1954. Perspectives and Problems of Amerindian Comparative Linguistics, *Word* 10. 306-32.

Teeter, Karl 1963. Lexicostatistics and Genetic Relationship, Lg 39. 638-48.

_____. 1964. Algonquian Languages and Genetic Relationship. Proceedings of the 9th International Congress of Linguists, 1026-1033, ed. by Horace G. Lunt. The Hague: Mouton.

NOTES

1. The hypothesis was first presented by Ronald Olson (1964). Eric Hamp has further elaborated it (1967, 1970). I have benefitted from criticism of earlier drafts of this paper by Eric Hamp, Terrence Kaufman, Louisa Stark, Marvin Bender, and others. None of these individuals, however, necessarily holds the views I have presented.

2. I assume that Finnish is not meaningfully related to either Cakchiquel or Quechua, though the latter two may ultimately prove to be related. Of course, one can never in principle prove that any two languages are unrelated; nevertheless, I believe that any relationship Finnish may have with Latin American Indian languages has to be so remote as to put it beyond the powers of our linguistic methods to recover.

3. I thank Eric Hamp for this observation.

4. One could still maintain that negation with *ma for Proto-Mayan and mana for Quechua is significant, but such a similarity seems to lose its force when compared with Somali which also has ma for negation. A genetic relationship involving these American languages should not need to also involve Cushitic.

5. Some might object that for Sino-Tibetan and other such structurally simple languages which do not characteristically employ long forms that my criterion is too strong. This may be true, but even in Sino-Tibetan reasonable hypotheses can initially seek for CVC matchings, with reasonable CV matchings for secondary support. Obviously, the criterion is designed to facilitate valid comparisons. If the structure of the languages compared do not allow CVC comparisons, then one must be careful to work with what he has, requiring stricter matchings in CV, with tones, etc.

6. Others who have proposed affinities of Mayan with South American languages, and who have been stimulated by Olson's work, are: Stark (1970, 1972), Hamp (1967, 1970, 1971), etc.

7. I will use a single asterisk to represent PM forms; a double asterisk (**) will signal Olson's Proto-Maya-Chipayan forms.