CURRENT ANTHROPOLOGY Vol. 14, No. 4, October 1973

DISCUSSION AND CRITICISM

On the Idea of Sumerian–Ural-Altaic Affinities

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Zakar's paper "Sumerian–Ural-Altaic Affinities" (CA 12:215) relates primarily to the Sumerian and Hungarian languages, and I am not a specialist in either; but as a Turcologist in a modest way of some 60 years' standing, I should like to make some observations on points raised in his paper, the comments on it, and his reply.

The only direct reference to Turkish in the paper is in the list of seven words for "head." If one were to consider a phonetic resemblance between a word in one language which means "the chief of an organization" and a word in another language which means primarily the physical "head" and only metaphorically "the chief of an organization" as proof of a genetic relationship between the two languages, and if the words in Zakar's list were all native words in the languages enumerated, then his list might carry some conviction. But the first condition is, I think, unacceptable, and the second is unfulfilled. Jacobsen has already pointed out (p. 218) that the Sumerian character PA when used ideographically to convey the meaning "overseer" was pronounced *ugula*. Pierce has pointed out (p. 221) that the word for "head" in all Turkish languages is *baş;* I would add that the word has some phonetic variations, of which the most extreme is Chuvash *puś*. He might usefully have added, for reasons to be stated below, that the other "Altaic" words for "head" are Mongolian *teri'ün* and Manchu *uǰu*. He did not realize that Zakar's *bi, bij, bej* (quoted from Vambéry, of all preposterous authorities) are merely modern pronunciations of the old (8th-century) Turkish word *beg*, "the chief of a clan." Most early Turkish words for rulers and officials are loan-words inherited from earlier steppe empires or borrowed from the Chinese, and there is a more than even chance that this is no exception. Having regard to the fact that there is no unvoiced initial *p-* in early Turkish, *beg* is practically identical phonetically with the Chinese word for "the chief of a hundred men" (a character made up of the character for "a hundred" and the radical for "man"), now pronounced *po* or *bo* but in northwest China round about A.D. 600 pronounced something like *peg*. Obviously a Chinese loan-word is out of place in this list.

On p. 223 Zakar refers to a Mongolian word *eki(n)* as further evidence for the existence of a word meaning "head" and beginning with a labial plosive in the "Altaic" languages. *Eki(n)* originally meant "skull, cranium," but came at a fairly early date to be used more generally for "head"; there are references in the 13th-century *Secret History of the Mongols* to an *eki* being cut off (Haenisch 1939:43). In this text the word is spelt *heki* once and *eki* half a dozen times. In the Classical period (16th century and later) it had lost its physical meaning and come to mean "beginning, source (of a river), seed, primary cause" (Kowalewski 1844–49:236). It is one of a fairly small group of words beginning with vowels which were sometimes pronounced with an initial *h-* in the 13th century; some are still pronounced with a velar aspirate (x) in one or two modern aberrant dialects. A great deal has been written about this initial aspirate, and there are two schools of thought. Some scholars contend that there was a well-established initial *h-* in Proto-Mongolian which was beginning to disappear in the 13th century and was itself an attenuation of an initial **p-* in "Altaic" (e.g., Aalto 1955). Others, myself included, contend that this latter is a delusion and that there never was a Proto-Mongolian *h-* but that this sound developed in about the 13th century as a vulgarism comparable to the English cockney prosthetic *h-* heard, for example, in the name *hAlbert* and now more or less obsolete. I have pointed out (Clauson 1961) that in the Turkish spoken in Sinkiang, an area contiguous to Mongolia, an exactly similar practice arose, probably quite recently, of adding a prosthetic *h-* to about a dozen words which begin, and quite certainly have always begun, with vowels in all other Turkish languages, ancient and modern. Be that as it may, everyone is agreed that when such a Mongolian word became a loan-word in the Tungus languages the initial *h-* persisted in Lamut and became *f-* in most other languages, and that this *f-* became *p-* in one small group—Goldi, Olcha, and Orok. All the Tungus words in Zakar's list are loan-words from Mongolian *(h)eki(n)* and are an almost classical example of the evolution (zero)→*h*→*f*→*p*. Zakar's "*peki(n)*, so far from being the first of the series, is the theoretical form of Goldi *pejé*, the last one.

Running through the paper, comments, and reply are constant references to the "Ural-Altaic languages" and the like, with the implication that they are all genetically related. No one would deny that the Uralian languages are genetically related to one another, but so far as the proposed marriage between Uralian and Altaic is concerned all the wooing has come from the Uralian side. I do not think that there is a single qualified Turcologist who now accepts the theory that Turkish is genetically related to this group. There are admittedly a good many Turkish loan-words in some Uralian languages, including a most interesting series of loan-words in Hungarian. The earliest were borrowed perhaps as early as the 8th century, the latest in the Middle Ages. They reflect the contact of the Magyars with various Turkish tribes at different stages in their long and colourful history. There are also some Finno-Ugrian loan-words in one or two modern Turkish languages spoken in the middle Volga basin, but that is all. The relationship between the Turkish and Uralian languages is not a genetic one, it is merely one of a mutual exchange of vocabulary material over the last 1,000 years or perhaps a little longer.

The Ural-Altaic theory may therefore be regarded as pretty much of a dead duck. The Altaic theory, that is, the theory that the Turkish, Mongolian, and Tungus language groups are genetically related, is still alive, but it is now fighting its last losing battle. A number of older scholars still believe that these language groups are related, mainly on the ground that they have similar grammatical structures. Most younger scholars, among whom quite incongruously I number myself, are firmly convinced that they are not, that the grammatical similarities between them are irrelevant, and that the words which are common to any two groups (there are almost none which are common to all three) are loan-words in one group or the other. There are no Mongolian loan-words in Turkish before the emergence of the Mongols under Chinggis Khan in about A.D. 1200, but earlier languages in the Mongolian group, notably Kitan, seem to have been borrowing Turkish words, the first perhaps as early as the 5th century, and they continued to do so at any rate until the 13th century and perhaps later.

In a recent paper (Clauson 1969) I published parallel lists of the 8th- to 11th-century Turkish, 13th-century Mongolian (with a few gaps filled from later periods), and 18th-century Manchu equivalents of the latest 100-plus-100 word list produced by the glottochronologists (Hymes 1960). Manchu is not a very satisfactory representative of the Tungus group, but it is the only one from which the full material is at present readily available. A typical entry is that of "head" quoted above. The results can be summarized as follows. After eliminating the entries of "I" and "we," where the resemblances can easily be explained as coincidences—for example, the Mongolian genitive of "I," *minö*, is more like "mine" in English than *mening* in Turkish—there are no words at all which are found in all three columns and no words at all common to the Turkish and Manchu columns. The Turkish and Tungus language groups cannot therefore be genetically related, and in its classical form the Altaic theory is dead. There are perhaps 14 words, excluding "I" and "we," which are more or less identical in the Turkish and Mongolian columns. Of these, three words are loan-words (one Iranian, one Hsiung-nu [Hun], and one Chinese) in the Turkish column and reappear as loan-words, presumably from Turkish, in the Mongolian column. Another eight words are demonstrably or almost certainly native in the Turkish and loan-words in the Mongolian column. This leaves four words (2%): *er–ere*, "man, husband"; *kara–qara*, "black"; *karangu–garangguy*, "dark"; *sarig–šira* (*''sira*), "yellow." It is incredible that these words should have survived in practically identical form as legacies from a common ancestor so remote that 93% of the vocabularies of these languages are now entirely different. The conclusion that these too are loan-words and that the Turkish and Mongolian language groups are not genetically related seems to me irresistible.

Comparison of the Mongolian and Manchu columns is complicated by the fact that Manchu is more or less saturated with Mongolian and Chinese loan-words; two words in the list—"wood, tree" and "dirty"—are Chinese loan-words. After eliminating "I" and "we," there are 13 words which are more or less identical in the two columns. Of these, for the reasons stated in the paper, four are certainly Mongolian loan-words in Manchu, and the resemblances between the words for "he" and "that" are almost certainly due to coincidence. This leaves seven words (3-1/2%): "belly," "good," "hot," "red," "to walk," "to pull," and "to suck," which are practically identical in both columns. The argument that the Turkish hard core of four words must be loan-words and not legacies seems to be equally applicable here, and the conclusion that the Mongolian and Tungus language groups also are not genetically related seems irresistible.

As so much emphasis is laid by some scholars on the importance of grammatical resemblances as a proof of genetic relationship, perhaps I might state the conclusions which I have reached as the result of many years of study of a good many languages regarding the time-honoured but now discredited trichotomy of agglutinating, flexional, and isolating languages. It seems to me that these are, at most, stages through which languages may, perhaps must, pass over the centuries, and that the way in which a language is categorized depends primarily on the characteristics which are selected as decisive. English is now, for example, regarded as an isolating language, but it is conceded that it was earlier a flexional language and that traces of this still survive in the conjugation of verbs. But if attention is concentrated on such groups of words as "parent, parenthood," "man, manly, manliness," and "rest, restless, restlessness," it is hard to deny it the status of an agglutinating language in the classical sense of that term. If it is the case, as I think it is, that it is impossible to place a particular language firmly in one of the three categories and be assured that it will remain there for a prolonged period, then I cannot see how this technique can usefully be employed as a basis for determining genetic relationships.

After I had finished my examination of the three columns for the purpose of appraising the validity of the Altaic theory, I thought that it would be interesting to examine the Turkish and Mongolian columns for the purpose of seeing whether the survival rate of the Turkish vocabulary of 1,000 or 1,200 years ago and the Mongolian vocabulary of 700 years ago conformed to the calculations of the glottochronologists. For this purpose I compared the Turkish column with the vocabularies of four modern Turkish languages—Tuvan, Uzbek, Osmanli, and Chuvash—and the Mongolian column with the written languages of the Mongolian People's Republic and Inner Mongolia. Chuvash is a special case; it is descended from the language of a Turkish tribe which got separated from the other Turkish tribes in the middle of the last millennium A.D. or even earlier and did not re-establish contact until some centuries later. The basis of comparison is therefore different and the survival rate markedly lower. So far as the other languages are concerned, Tuvan has been more exposed to the invasion of Mongolian loan-words than the rest, but on the average nearly 90% of the old vocabulary is still in current use in all the other languages. Of the old Mongolian vocabulary, only 1% has become entirely obsolete and nearly 95% is still in current use with its original meanings. This indicates much higher survival rates than those calculated by the glottochronologists. After a careful study of all the literature on glottochronology at my disposal, I concluded that it could never become an exact science for the following reasons:

1. Unless the whole vocabulary of a nuclear language is known (which in fact never occurs) it is impossible to determine how much of it has survived in the vocabularies of daughter languages, since some words may survive only in one language, and words are not counted as survivals unless they survive in all.

2. Languages do not all change at a uniform rate.

3. No language changes at an exactly uniform rate throughout the whole period of its existence.

4. Therefore, even if a particular language has survived in written documents sufficiently long to make it available as a "control case," so that its survival rate over that period can be calculated, this survival rate cannot safely be projected backwards to determine the date at which cognate languages parted from a common ancestor and became separate languages.

I was fascinated by Durbin's table of lexeme resemblances in Sumerian and Dravidian. Even assuming that the Sumerian column, based on Gordon, is reliable—and Jacobsen's remarks on Sumerian lexicography are not reassuring—surely only the indulgent eye of faith could discern a common origin for Sumerian *gal-la*, "vulva," and Malayalam *kantu*. Indeed, I can think of an English word which in these permissive days every schoolboy knows, but which the prim editors of the *Concise Oxford Dictionary* and other such publications exclude from their chaste pages, which is almost exactly synonymous and homophonous with the Malayalam word. This does not mean that I am proposing to launch a theory that English and Dravidian are genetically related. Dyakonov (1967:108), a leading authority on Elamite, a language which died at much the same date as Sumerian, give or take a century or two, recently stated very cautiously that the only theory which showed any sign of promise was that Elamite was genetically related to the Dravidian language group. I do not know what Dravidian scholars like Burrow and Emeneau think of this theory, but my impression is that, whether they like it or not, they do not at all like any other theory about the genetic relationship of Dravidian. They would have my complete sympathy. The case for an Elamite-Dravidian genetic relationship will be greatly strengthened if the Finnish team can prove their theory that the language of the Indus script was Proto-Dravidian (see Clauson and Chadwick 1969). There is no doubt that in the 3rd millennium B.C. there were close trading relations between Elam and the Indus Valley, and recent excavations in southern Persia are producing more and more evidence of the closeness of this relationship. Nothing is more probable than that the people who moved into the Indus Valley from the west and founded the Indus civilization were driven southward by the Aryan invasion in the 2nd millennium B.C. into the Dravidian-speaking areas of southern India, and it hardly less likely that the Indus people who moved east from an area more or less contiguous to Elam were ethnically related to the Elamites.

Reply

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Let me point out that I said (p. 223, italics added), "Both the Uralic and the Altaic family *have many correspondences* to the Sumerian roots." This does not mean that " 'the Ural-Altaic languages' . . . are all genetically related." Csőke has gone on to demonstrate further these correspondences in two subsequent papers. My 1968 paper will soon be available in English to study by archaeologists, historians, and anthropologists as well as linguists. Meanwhile, I will be happy to send a copy of the 100-word list to anyone who requests it.

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On "Assumption and Inference on Human Origins"

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CURRENT ANTHROPOLOGY's publication of Quigley's "Assumption and Inference on Human Origins" (CA 12:519–40) may prove a signal service to paleanthropological thought. Certainly its fate should not be decided by the faulting it has undergone, whether well-founded or not. In final analysis, it touches what I believe to be the most crucial issue which that thinking should be facing today.

The paper has been presented, a panel has commented, the author has made a reply. Assumably, the topic is now opened to the audience; remarks from the floor are in order. What follows is so conceived; the remarks will be lean and somewhat abrupt; the subject itself is easily of book length.

The author is a scholar coming from another discipline, with which, however, ours has a measure of congeniality. He is among us not as an authority, but as a versed amateur and an *amicus curiae;* he hopes to be treated only as such.

Most of the panel has been accordingly receptive, though not uncritical; a minority has only frowned. The paper is indeed vulnerable—but how vulnerable? And how vulnerable, in turn, are some of the criticisms?

I am concerned only about those which would throw the entire case out of court on the premise that the paper does not meet the criteria of certain assumptions—when it is precisely those assumptions that are being questioned. And my concern is the more acute because of what transpired when St. Hilaire debated Cuvier. They were not matched well, prestigewise. St. Hilaire pled that man might well be a product of an evolution; Cuvier's reply was, "Il n'y a pas d'hommes fossiles." He was so very right.

Let me first summarise the gist of what is to follow. The *amicus curiae* has written about the assumptions from which paleanthropologists pass to their inferences about human origins. He suggests that a different set of assumptions would allow other inferences and might indeed be more fruitful; he illustrates, without insisting that the illustrations are truer than those already dominant in paleanthropological circles. He suggests that "holistic" assumptions, systems premises, would be more heuristic than "atomistic" ones (these are called by students of system "reductionistic"; I shall use both terms); that they would lead to a different investigative emphasis in the pursuit of "humanisation." Unfortunately, his own assumptions are not unambiguous, so his critics have had at least an excuse for missing his cardinal point. This being so, I too shall begin by criticising some of his assertions; then turn to certain of the more serious criticisms leveled by the panel; and finally state some of the distinctions between the reductionistic and the systemic approaches, particularly with regard to their bearing upon the investigation of "humanisation."

1. The final paragraph of Quigley's reply contains some surprising anachronisms, such as one would not expect from a historian, for they are matters of the history of biological thought. The thesis that man, or his ancestry (it is here immaterial), was a "failure" in nature for 12 million years, until he was "extruded into culture," making him a "success," is very hard for any scientist to salvage, with all the best intent in the world. Both Crawford and Holloway have indicated succinctly the unviability of the thesis, whatever may be its meaning; but Quigley's reply seems to indicate that he has not grasped the decisiveness of their disposal. His conception of "behavior" or "activities" belongs to an earlier period of thinking; the dichotomy of "inherited" versus "learned" behavior is obsolete for the very use to which he would