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DISCUSSION AND CORRESPONDENCE

KINSHIP AND HISTORY

I hasten to admit the contention of Radcliffe-Brown, made in "Kinship Terminologies in California" in a recent number of this journal,¹ that I claimed too much in stating that the time for an attack on the problem of the relation of kin terminologies to coexistent institutions was on the whole after some insight into their history had been attained rather than before. Actually, of course, the factual problem of what the correlation is, especially in the norm or average, is not intrinsically affected by historical considerations. Brown is quite within his rights in attempting to solve the problem as he sees it, which is unhistorically, without reference to complicating antecedents. If one seeks to isolate constants, simplification of the issue and its extrication from the chaos of apparent phenomena are important, and anterior stages may be irrelevant. What I should have expressed was my conviction that the factors at work in the phenomena in question are numerous and variable enough to make it seem highly questionable whether determinations of constants other than of narrow range or vague nature can be made, or at any rate have yet been made, while historical considerations are omitted. I cheerfully make this correction. That I was not trying to say that probabilities or inferences or hypotheses should be allowed to supersede facts, will probably be believed without explicit reaffirmation.

Among "historical considerations" in this connection I reckon language, of which kin terminologies are part. Hupa and Tolowa are Athabascan, and this fact therefore must enter ultimately into the problem of terminological-institutional correlation in northwest California. One can of course refrain from ultimates and limit the problem to the question of whether or not correlation exists in these particular tribes. This is perfectly legitimate, but seems rather narrow, and I do not believe Brown would wish to draw such a limit. In fact I admit without hesitation that there must expectably be some correlation: both because speech is not an independent universe, and because in other cases hitherto we have always found some accord between terminologies and institutions. A much more real problem is how much correlation there is, and what factors have made it stronger at some points and weaker at others. Here I believe language cannot be left out, in the sense that Hupa being Athabascan, and Yurok being if not Algonkin at least non-Athabascan, the speakers of the two languages must at one time have come into contact and into acquisition of a highly similar culture, not only with differently pronounced words, but—if all precedent holds—with kinship words of somewhat or considerably different meaning. The situation, in short, is characterized by the impingement on each other of a set of social institutions and usages at least highly uniform in the area and of several languages which are thoroughly different—so different that their contained terminologies still remain extremely diverse in plan or system of concepts. I do not doubt that detailed investigation will also reveal a number of differences of

¹ Vol. 37, 1935, pp. 530-35.

social usage corresponding to differences in the terminologies, and that there will be value in knowing all such cases. I shall be happy to assist Brown, or anyone like-minded, in planning field investigation directed at these very matters. Nevertheless, however many fine points of this sort may have been overlooked by those of us who have studied the tribes in question, enough work has been done to make it clear that the great mass of the social system of the five tribes is similar, so similar as to be fairly designable as one in plan and pattern, whereas their kinship terminologies are of two types or patterns. This fact seems to me of more significance than the expectable one that there has also been a certain amount of adjustment between the impinging institutional and terminological patterns. It seems doubly significant in view of the Hupa and Tolowa being Athabascans, and the Wiyot and Yurok proto-Algonkins, if Sapir is right, and at any rate having a kin system of the type of the Salish and Wakash, irrespective of whether or not the Salish and Wakash are also proto-Algonkin. Perhaps I should say "of greater interest to myself" rather than "of greater significance," for significances change with interests.

The problem has by no means been exhausted even within the frame of the north-west California area. For instance, I have pointed out that each of the five terminologies shows certain assimilations to the others, contrary to, or on top of, its basic type or pattern. I have assumed that these assimilations were due to inter-influencings of the terminological systems, which are of course also systems of thought or unconscious semantic logic. It is however equally possible, theoretically, that the assimilations are due to the leveling influence of the more uniform institutions. Quite likely both factors have been at work; and it would be interesting to know to what degree and at what points and circumstances. Again, therefore, I renew my invitation to more fieldwork.

As regards the generic problem of term-usage correlation, I am ready to retract some of my intransigence of earlier years, which I now construe as a reaction to the once-prevalent abuse of seeing in kinship systems chiefly instruments for reconstructing systems of social structure; and I suggest the following basis for a permanent and productive peace.

The relation of kinship-term systems to institutions and practices seems analogous to the relation of dress to the human body. One expects normally a considerable degree of fit; but it would be dogmatic and futile to say that body conformation "determines" dress, or that dress "reflects" the body. Sometimes it does, sometimes it does not. The real problem obviously is when it does and when it does not, and how and why. Styles have a way of traveling their own course, sometimes to the point of requisitioning mechanical inventions to preserve even adhesion to the body. Fit may be loose and cool or snug and warm, comfortable or uncomfortable whether loose or tight. Similarly with the fit of kin terminology to social usage: it may be close or wide. Expectably there will always be some fit, and there may be a great deal, but it may also be remarkably partial. Every kinship system is also a little system of classificatory thought, and unconscious peoples sometimes are as ingenious in their logical productions as ethnologists in their analyses. There is no reason why such systems should not have a history of their own: not of course wholly cut

off and self-determined, but partially so, with their own novelties, growths, diffusions, and contact modifications. They are styles of logic in a limited field of universal occurrence.

But they are also more than this: they are parts of languages which always have a long history and ordinarily change slowly. They therefore tend to contain precipitates of greater or less age: old elements with changed function, also elements with unchanged function inconsistent in a new pattern. The Navaho system for instance is almost certainly more than a mirror of Navaho clan system, marriage avoidance, obligations, familiarities, etc., though it will undoubtedly fit these usages in part. It is also still an Athabascan system, presumably not only in its sounds but partially in its concepts. If these concepts have been made over completely to fit the institutions of the Navaho in the Southwest, or Pueblo ideology, it would be a surprising fact. Expectably the Navaho system is an adjustment between functioning Navaho social usages, no longer functioning ones, an ancient northern Athabascan system, and Pueblo ideology, with the two latter in turn the resultant of adjustments between practice systems and thought systems.

A normally large amount of play or give in fit is evident. Portions of a naming system can be indifferent from the point of view of social structure, or vice versa. Grandparents as compared with uncles, siblings with cousins, more often lie in these areas of indifference; but they are no less important, in the conceptual system, or in life, except on the premise that fit to social structure is the most important aspect of the subject. To me it is not: the ideologies as such possess at least equal interest and significance. More, in fact, as long as they continue to be under-weighted in pursuit of the social-fit theory. Can we not all meet on the common ground that the determinants are multiple and variable, and then amicably follow the ways of our respective bents as these most profitably lead us, with tolerance also of other approaches? It does seem a symptom of immaturity in Anthropology that we should still divide up into militant camps like eighteenth century Vulcanists and Neptunists. Perhaps I threw the first stone, but herewith I extend the olive branch.

On the broader question of the relation of sociology and history, Brown and I have expressed ourselves at greater length in articles in the same and following numbers of the AMERICAN ANTHROPOLOGIST, and a few comments must suffice. In common with most historians I hold the essential and characteristic thing about history to be neither documentation nor time sequences, but an attitude of mind, a particular approach in trying to understand phenomena. The distinction between the "detailed and documented history of the historian" and "the hypothetical history of the ethnologist" is valid enough but hardly seems to go to the root of things. Neither does the distinction between the "evidence of eye-witnesses" as distinct from inferences "based on circumstantial evidence." The most documented history that limited itself to eye-witness testimony and refused to infer from circumstantial evidence would be only skin deep. Brown's double characterization sounds like a scientist's conception of history. As I have said before, all historians reconstruct. If they do not reconstruct, they are accomplishing nothing, because historical interpretation *is* reconstruction. And the values of all historiography lie precisely

in its being hypothetical, if a categorical paradox may be pardoned. Brown's distinction of ethnology, which he sees as historical though apparently condemned to being an inferior kind of history, from social anthropology or comparative sociology, which investigates "the nature of human society," I would accept, with reservation as to the inferiority if that was implied, and with reservation also as to the emphasis on society instead of culture. And I would accept his distinction as referring to two currents within Anthropology rather than to two disciplines. We agree that their aims, methods, interests, and I think values, are different. I take it that the investigation of the nature of human society, or culture, has for its end the determination of constants, in other words of abstractions extricated from phenomena as they occur in space, time, and variety of character. This is a genuinely and wholly scientific objective, evidently very difficult to attain from social or cultural material, but certainly important and significant. Any method which will really bring us there is a good method. Whether the better procedure is to dissociate as much as possible from the historic approach, as Brown seems to want, or on the other hand to envisage and emphasize the historicity of phenomena, as Lesser advocates, I do not know. Brown's course seems the purer, more drastic, and to date more sterile. But it may in the end carry us farther into new concepts. Good speed on his journey.

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NOTES ON THE TÜBATULABAL LANGUAGE

The language of the Tübatulabal of California, belonging to the Uto-Aztecan stock, has been ably presented by C. F. Voegelin in two recent monographs.¹ It is an extraordinarily interesting language to any one interested in curious linguistic phenomena, with a remarkable word-structure that is not emphasized in the grammar and which I describe below. It has a process that the author calls initial reduplication but which I should like to call "exfixation," in which the word is postfixed to its own first vowel (*wic*: *iwíc*; *lik*: *ilik*; *ma'g*: *amak*). Sometimes the exfixed vowel disappears from within the stem when it appears in the exfix (*no'in*- [phonetically *no'in*]: *o'nin*; *po'han*: *ophan*). A nasal after the stem-vowel is also exfixed and assimilated (*pa'n*: *amban*). The category of true past tense is developed in nouns (my house: what used to be my house) but not in verbs, which have a different, two-tense system: realized and future. In contrast to the usual verb technique of suffixation to the verb stem, certain modalities can be expressed by using a naked stem along with a detached auxiliary verb to which suffixes may be added. There is a stylistic distinction between the two techniques: the naked stem is "strong talk," piling of suffixes on the stem is "high language."

Voegelin has not yet worked out to its last details the complete phonemics of Tübatulabal but he has given a good general account of the phonemes.

¹ Tübatulabal Grammar (University of California Publications in American Archaeology and Ethnology, Vol. 34, No. 2, 1935); Tübatulabal Texts (*idem*, No. 3, 1935).