

## DISCUSSION AND CORRESPONDENCE

### HISTORY AND SCIENCE IN ANTHROPOLOGY: A REPLY

It was interesting to me to read Dr Kroeber's analysis not only of my scientific work but also of my personality.<sup>1</sup> I may perhaps misinterpret both. Nevertheless I wish to express my complete disagreement with his interpretation. It is quite true that as a young man I devoted my time to the study of physics and geography. In 1887 I tried to define my position in regard to these subjects,<sup>2</sup> giving expression to my consciousness of the diversity of their fundamental viewpoints. I aligned myself clearly with those who are motivated by the affective appeal of a phenomenon that impresses us as a unit, although its elements may be irreducible to a common cause. In other words the problem that attracted me primarily was the intelligent understanding of a complex phenomenon. When from geography my interest was directed to ethnology, the same interest prevailed. To understand a phenomenon we have to know not only what it is, but also how it came into being. Our problem is historical. Dr Kroeber suggests as

the distinctive feature of the historical approach, in any field, not the dealing with time sequences, though that almost inevitably crops out when historical impulses are genuine and strong; but an endeavor at descriptive integration. . . . Process in history is a nexus among phenomena treated as phenomena, not a thing to be sought out and extracted from phenomena.

I confess that to me this does not give any sense. We have descriptions of culture more or less adequately understood. These are valuable material. They yield, if well done, most illuminating material in regard to the working of the culture, by which I mean the life of the individual as controlled by culture and the effect of the individual upon culture. But they are not history. For historical interpretation the descriptive material has to be handled in other ways. For this work archaeological, biological, linguistic, and ethnographic comparisons furnish more or less adequate leads.

If Dr Kroeber calls my first piece of ethnological work, "The Central Eskimo," (written in 1885), historical, I fail to understand him. It is a description based on intimate knowledge of the daily life of the people, with bad gaps, due to my ignorance of problems. The only historical points made are based on a comparison of the tribe studied with other Eskimo tribes and with the Indians of the Mackenzie basin, on a careful study of evidences of earlier habitations of the Eskimo, and a guess as to the course of their early migrations. The rest is description pure and simple. If in later writings I did not stress geographical conditions the reason must be sought in an exaggerated belief in the importance of geographical determinants with which I started on my expedition in 1883-84 and the thorough disillusionment in regard to their significance as creative elements in cultural life. I shall

<sup>1</sup> American Anthropologist, Vol. 37, pp. 539-69, 1935.

<sup>2</sup> The Study of Geography (Science, Vol. 9, pp. 137-41, 1887).

always continue to consider them as relevant in limiting and modifying existing cultures, but it so happened that in my later field work this question has never come to the fore as particularly enlightening.

May I remind Dr Kroeber of one little incident that illustrates my interest in the sociological or psychological interpretation of cultures, an aspect that is now-a-days called by the new term functionalism. I had asked him to collect Arapaho traditions without regard to the "true" forms of ancient tales and customs, the discovery of which dominated, at that time, the ideas of many ethnologists. The result was a collection of stories some of which were extremely gross. This excited the wrath of Alice C. Fletcher who wanted to know only the ideal Indian, and hated what she called the "stable boy" manners of an inferior social group. Since she tried to discredit Dr Kroeber's work on this basis I wrote a little article on "The Ethnological Significance of Esoteric Doctrines"<sup>3</sup> in which I tried to show the "functional" interrelation between exoteric and esoteric knowledge, and emphasized the necessity of knowing the habits of thought of the common people as expressed in story telling. Similar considerations regarding the inner structural relations between various cultural phenomena are contained in a contribution on the secret societies of the Kwakiutl in the Anniversary Volume for Adolf Bastian (1896) and from another angle in a discussion of the same subject in the reports on the Fourteenth Congress of Americanists, 1904 (published 1906); the latter more from the angle of the establishment of a pattern of cultural behavior. These I should call contributions to cultural history dealing with the ways in which the whole of an indigenous culture in its setting among neighboring cultures builds up its own fabric.

In an attempt to follow the history of a culture back into earlier times we are confined to indirect evidence and it is our duty to use it with greatest circumspection. Dr Kroeber accuses me of not being interested in these questions. I do not know, then, why I should have used years of my life in trying to unravel the historical development of social organization, secret societies, the spread of art forms, of folktales on the Northwest Coast of America. I think that such a detailed study is worth while not only for its own sake but because it illuminates also general aspects of the history of mankind, for here we see the totality of cultural phenomena reflected in the individual culture. Is it that painstaking work of this kind does not seem to Dr Kroeber worth while, but that it requires the flight of an unbridled imagination to have his approval? I cannot understand in any other way his praise of a public lecture which I gave as President of the New York Academy of Sciences on "The History of the American Race,"<sup>4</sup> guarding my statement however, at the very beginning by saying that I should give my fancy freer rein than I ordinarily permit myself. When as early as 1895<sup>5</sup> I made a careful analysis of the then available material, showing the relations of Northwest Coast mythologies among them-

---

<sup>3</sup> Science, n.s., Vol. 16, pp. 872-74, 1902.

<sup>4</sup> Annals of the New York Academy of Sciences, Vol. 21, pp. 177-83, 1912.

<sup>5</sup> Indianische Sagen von der Nord-Pacifischen Küste Amerikas (Berlin, 1895).

selves and to other American and Old World areas, the object was to demonstrate historical relations. Perhaps I did not go far enough for Dr Kroeber in establishing the center of origin of each element; but there I balk, because I believe this can be done in exceptional cases only. The fact that a phenomenon has its highest development at a certain point does not prove that it had its origin there. The belief in this, which I consider an unjustified assumption, and a more lighthearted weighing of evidence differentiates our methods. In a conversation Dr Kroeber admitted that I wanted a high degree of probability for a conclusion, while he was satisfied with much less. That is an Epicurean position, not that of a modern scientist.

I am sorry that I cannot acknowledge as fair the summary of my work. It is true that I have done little archaeological work myself. My own only contribution was the establishment of the sequence of archaic, Teotihuacan type and Aztec in Mexico, I believe except Dall's work on the Aleutian Islands, the first stratigraphic work in North America; but in the plan of the Jesup Expedition I assigned an important part to archaeological work which in the careful hands of Harlan I. Smith gave important results on Fraser River showing the invasion of inland culture. If farther north it did not give any results the cause was not lack of interest but failure to find significant material. I may also claim to have kept before our scientific public year after year the necessity of careful archaeological work in northern Alaska, which has unfortunately been deviated from its main object by sensational artistic finds, although the main problem remains that of the occurrence or non-occurrence of pre-Eskimo types in the Bering Sea region.

In regard to linguistic work Dr Kroeber's criticism does not seem to me to hit the mark at all. Relationship of languages is a powerful means of historical research. It remains equally valid, whether we assume purely genetic relationship or whether we ask ourselves whether by contact languages may exert far reaching mutual influences. This question is important for the interpretation of relationships but has absolutely nothing to do with a historic or non-historic approach. If it can be settled we shall know how to interpret historically the linguistic data. That I am here as elsewhere opposed to ill substantiated guesses, goes without saying, but has nothing to do with the case. Here also a 40% possibility is no satisfactory proof for me.

Dr Kroeber's strictures on my book on "Primitive Art" are entirely unintelligible to me. He says style has not been treated. There is a whole chapter on style and one specific one on Northwest Coast style intended as a sample of treatment of the problem. Maybe Dr Kroeber has an idea of his own of what style is, as he has an idea of his own of what history is. He reproaches me for not having written on the history of Northwest Coast style. Unfortunately there are no data that throw any light on its development. It appears in full bloom and disappears under the onslaught of white contact. The slight local differences and the relation between the arts of the Eskimo and other neighboring tribes do not seem to me to throw any light on the subject. Does he want me to write its history without such data? Am I to repeat the wild guesses of Schurtz?

I have never made the statement that history is legitimate and proper, but his-

torical reconstruction unsound and sterile. As a matter of fact, all the history of primitive people that any ethnologist has ever developed is reconstruction and cannot be anything else. There is, however, a difference between cautious reconstruction based on ascertained data and sweeping generalizations that must remain more or less fanciful. I do recognize quite a number of very fundamental general historical problems in regard to which I have more or less decided opinions, such as the distribution and relationships of races, the relation of America to the Old World, that of Africa to Asia, and so on. It depends entirely upon the evidence how strongly I hold to these opinions. It has happened to me too often that a suggestion cautiously made has been repeated by others as though I had pronounced it as a set dogma.

Now as to the use of statistics in ethnology as a tool of research. Being somewhat familiar with the difficulties of statistical work I do not believe that it is a safe guide in ethnological inquiry. I believe I was the first after Tylor's discussion of 1888<sup>6</sup> to try it on the field of mythology, and if at that time the correlation method had been as much abused as it is now, and since I had not yet understood its dangers, I might have established some nice coefficients of correlation for elements of mythology.<sup>7</sup> The data of ethnology are not of such character that they can be expressed by mathematical formulas so that results are obtained which are in any way more convincing than those secured by simpler ways of numerical comparison. Behind these always loom the unanswered questions in how far the materials enumerated are really comparable, or in other types of problems, like Tylor's, in how far they are independent.

I regret that Dr Kroeber also does not see the aim I have in mind in physical anthropology. We talk all the time glibly of races and nobody can give us a definite answer to the question what constitutes a race. The first stimulus to my active participation in work in physical anthropology was due to G. Stanley Hall and to the atmosphere of Clark University, and had little to do with racial questions, rather with the influences of environment upon growth. When I turned to the consideration of racial problems I was shocked by the formalism of the work. Nobody had tried to answer the questions why certain measurements were taken, why they were considered significant, whether they were subject to outer influences; and my interest has since remained centered on these problems which must be solved before the data of physical anthropology can be used for the elucidation of historical problems. Equally important seems to me the question in how far the functioning of the body is dependent upon bodily structure. The answer to this problem is the necessary basis for any intelligent discussion of racial physiology and psychology.

Dr Kroeber refers to the discussion on anthropological methods at the time of the Americanist Congress held in New York in 1928. He does not quite completely

---

<sup>6</sup> Journal, [Royal] Anthropological Institute of Great Britain and Ireland, Vol. 18, pp. 245-72, 1889.

<sup>7</sup> *Indianische Sagen*, pp. 341 *et seq.*

tell the story of this incident. The discussion had centered entirely around Kulturkreise and other attempts at historical reconstruction. Finally I said that I had all through my life tried to understand the culture I was studying as the result of historical growth, but since the whole discussion had been devoted to historic sequences I had to arise as the *advocatus diaboli* and defend those who sought to understand the processes by which historical changes came about, knowledge of which is needed to give a deeper meaning to the picture. This was no new position of mine, as I think has become sufficiently clear from the preceding. It is true enough that in general the participants in the discussion did not want to have anything to do with the investigation of "processes" which seemed anathema but preferred to stick to their pet theories which they considered satisfactorily proven.

FRANZ BOAS

COLUMBIA UNIVERSITY  
NEW YORK CITY

### MAMMOTH OR "STIFF-LEGGED BEAR"

Dr W. D. Strong<sup>1</sup> gives a portion of a Naskapi tale about Djákabish including the adventure of his slaying the monster Kátcheetohúskw who had killed and eaten his parents. Dr Strong considers the monster (owing to his large ears, etc.) as reminiscent of the mammoth. Prof F. Speck<sup>2</sup> gives a Mistassini version of the same tale. The name of the hero is Tsaka'bec and that of the monster is Katci'to'-wack'w. According to Speck, among both the Naskapi and Montagnais the animal is referable to the *Ursidae*. He further notes that Katci'to'-wack'w is translatable as "Stiff-legged Bear," and cites pertinent words in support of this etymology.

Both of these authors seem to have overlooked the fact that Skinner,<sup>3</sup> had previously recorded versions from Rupert's House (Tcikápis, Katci'tos) and the Albany River: in the last variant note "the bears who killed our parents"—which lends support to Speck's contention. But Skinner as well as Strong and Speck seems not to have noted that in Le Jeune's Relation of 1637<sup>4</sup> a very old variant occurs: the hero is Tchakabeche, who is "a little Dwarf:" a bear devours his father but the "great Hare" ("Michtabouchiou") devours his mother, and hair is found in its belly.<sup>5</sup>

In this connection it may be pointed out that in an Ojibwa version concerning the same hero Tcakāpās ("The Gnome") given by the late Dr Jones,<sup>6</sup> "Bears-with-Heads-at-Both-Ends" (Ä'īdawa'kwag) are "the names of those that slew our parents."

<sup>1</sup> American Anthropologist, Vol. 36, 1934, pp. 83-84.

<sup>2</sup> *Ibid.*, Vol. 37, 1935, pp. 159-60.

<sup>3</sup> Anthropological Papers, American Museum of Natural History, Vol. 9, 1911, pp. 100-104.

<sup>4</sup> Jesuit Relations, ed. Thwaites, Vol. 12, p. 31 *et seq.*

<sup>5</sup> See also Ellen Russell Emerson, Indian Myths (Boston, 1884), p. 371, which work contains some valuable notes besides much trash.

<sup>6</sup> Ojibwa Texts (Publications, American Ethnological Society, Vol. 7, Part 2, 1919), p. 354 *et seq.*