

## ON BEING CRITICAL

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In thinking over a suitable topic for this memorial to Alfred L. Kroeber, I felt that it would be presumptuous of me to prepare a research article, for this would imply that, like the other authors in this volume, I had received formal training from him. As a matter of fact, during the three undergraduate and two graduate semesters that I majored in anthropology on the Berkeley campus in 1935-1937, I had only one course from Professor Kroeber--on the Indians of California--as an undergraduate.

It was under different and rather unique circumstances that I was able to come into contact with Kroeber's immense learning, experience, and wisdom. As a senior and during my one graduate year in Berkeley, I worked part-time as Preparator in the Museum of Anthropology. My first assignment was to complete the cataloging of the Reisner collections from Egypt, a fascinating introduction to archaeology indeed. After that, Robert Heizer and I cataloged the archaeological collections he and I and others had excavated in Nevada and central California on week ends and on summer expeditions. Then, in the summer of 1937, just before I went to the University of Oregon, Kroeber put me to work on the analysis of all collections made during some thirty-five years in the shell middens around San Francisco Bay.

When Kroeber needed to get away briefly from his superhuman work program in the old Anthropology Building, he would come to the Museum to relax a bit. But no sooner would he see one of us at work there than he would immediately become interested in what we were doing. He would look over the artifacts and skeletal material, ask questions about our field data and ideas of interpretation, offer comments and insights, and then, in a relaxed, conversational manner inject aspects of his own philosophy toward basic problems, methods, and attitudes towards colleagues. Those who knew Kroeber will know what I mean by his instant readiness at all times to help the "fledglings" along, at least as often as he could find time for it. All told, I probably did not spend more than ten hours in these conversations with Kroeber during the two years I was employed in the Museum, yet the net result was of greater value to the shaping of my future career, and my thought processes, than all of my university courses put together.

Besides these conversations in the Museum there were four other occasions on which I became better acquainted with Kroeber: once when he and Mrs. Kroeber visited our field camp in the Sacramento Valley in the summer of 1936, two short visits to their summer home near St. Helena in 1946 and 1950, and in 1947 when Kroeber came to the University of Texas to give a public lecture and stayed two or three days to discuss various North American archaeological problems, shortly to be incorporated in his 1948 edition of Anthropology.

In this account, then, I will set forth some of the principles gained from these conversations which I remember best. They may reveal something of

Kroeber's thinking not fully appreciated even by those who knew him well; and to the present and future generations of anthropologists who have never known him at all these observations may serve a purpose, just as they did for me. I cannot, of course, guarantee that the wordings are exact. Neither can I make a clear separation between what he actually said and some of my own philosophies which gradually developed from his initial stimulation. But I believe that the quotations given below convey fairly accurately the intent that he had at the time as well as the informality of his phrasings--"off the cuff" and without thought of being quoted.

Kroeber's incredible output and range of interests need no comment: they are known around the world. To me, the greatest single aspect of his long and productive life was the possession of a completely open mind. We all like to talk of open minds and undoubtedly most of us think we have one; but to this day I can think of no one else in which this ideal was so fully realized in practice. It took the form of constant seeking of new information, new ideas, and particularly of welcoming criticism. If criticism was not forthcoming voluntarily, he had ways of teasing it out of those whose opinions he wanted--and these included beginners as well as renowned professionals. He once said, very emphatically, "I do not like to have people agree with me. There is no stimulation in that." No matter who you were, all he asked was that you have some reason for your stand. He realized, of course, that his tremendous reputation caused greater and greater hesitation among others to criticize him, but I feel confident in saying that he did not like this and that he tried to overcome it by launching on a friendly discussion of anything that seemed appropriate with the person in question. Kroeber cared nothing about age or standing in the profession, only about what went on in the heads of his listeners. Here was a man who not only welcomed differences of opinion but actively sought them. I think that under it all he believed that everyone needs criticism for his own ultimate good.

On one occasion that I remember well, Kroeber stated that "There is too much talk about truth and scientific method. Actually there are few real truths, and these are the result of constant testing by different people with different points of view and living in different ages. There are always fads in thinking and when these wear off others take their place. There is almost nothing that does not need to be done over again sooner or later and perhaps many times. As to 'scientific method,' there are many methods which are equally scientific if they are sincere and attempt to be objective. Only time and repeated testing will tell which methods are best; but different methods are suitable for different purposes, so we have to know what the objectives are before any method can be called more 'scientific' than another. On the other hand, there is a scientific attitude, which is not a method but a state of mind, and is not subject to changing fads in thinking. The scientific attitude is simply a matter of never being satisfied, either with one's own methods or with those of others. In other words, no matter how well we think we are doing, we can always do better after we realize the flaws in our past attempts."

And again: "No one can really examine all the evidence on any major problem; at least not at first hand. This puts us in a nice fix when, as so often happens, we claim to have reached valid conclusions after having examined and weighed 'all the evidence.' I don't know any way out of this dilemma except

to put my trust in those people who seem to know best what they are doing. Mistakes can be made this way, too; but I am always more impressed by those who know their own limitations and the limitations of their information than I am by those who reach too-pat answers. I am usually suspicious of a supposedly revolutionary discovery or interpretation, not because I feel more comfortable with the status quo, but because the startling 'new' discovery is often not very new at all: It may be made by someone only half educated in the problem or by someone who is anxious to become famous in a hurry. Here again we have fads in thinking and it becomes fashionable to jump on the bandwagon and go hurrying off in the new direction. As for myself, I would rather wait and see how the experts react over a period of time."

Once, after I had been at the University of Oregon for a year, Kroeber asked if I had reached any opinions about a survey of Oregon tribal boundaries which he had recommended for publication "without having the information to criticize it." I had not, but he went on to say that he had often recommended manuscripts for publication if they seemed substantial. "No matter how much one person may know of a subject, there is always someone, somewhere, who knows more. This may not always be true, but it is well to allow for it because there are scholars who spend their lives collecting information but are too shy or too lazy to publish much of it. Or perhaps they labor under the mistaken idea that if they work long enough the report will be above criticism. Sometimes, however, these people who sit forever on their unpublished notes can be teased into speaking up if they see what they think are mistakes in someone else's work. So by urging publication of anything that seems essentially reasonable, I always hope to stir up comments that would not have been forthcoming otherwise. The trouble with this policy is that so few people understand its purpose, so that instead of getting corrections and additional information, we often get unnecessarily bitter attacks.

"I would rather publish as fast as I can feel reasonably sure of my material, even at the risk of errors and uncertainties, so that it will be out where others can shoot at it. Nothing is worth much in any profession until it is published. . . . Controversies are healthy and necessary; progress is impossible without them. . . . Even the wildest ideas often serve a purpose by pointing out questions that need to be answered."

In the 1930's and 1940's, a considerable amount of classroom time and publication space was devoted all over the country to the discussion of "schools" of anthropological thought: "historical anthropology" versus "functionalism"; extreme "diffusionism" versus "environmental determinism," and so on. There was (and perhaps there still is, in some quarters) a good deal of bitterness and snobbery over what was the "right" way to approach anthropology. Kroeber, as one of the world's great anthropologists, was naturally in the midst of many of these arguments and was often criticized as too much this or too little that. I heard him remark on this situation only once, when he said "There isn't any right way or wrong way to advance anthropology: we need them all."

Kroeber's one insistence was independence of thought and action. "Don't believe too strongly in what anyone else says or writes," he would say. "Pick a problem and work at it to see where it leads. If it doesn't seem to be working out, try another approach and see where it leads. Work and experiment and

try to think for yourself. Your professors can help but in the long run it is up to you to learn how to get the most out of any problem you tackle."

This insistence on experimenting and independence is worth mentioning here in connection with the problem of the antiquity of man in America, to mention only one example. In recent years it has become the fashion to open any discussion of this problem with a biting indictment of the hyperconservatism of Holmes and Hrdlicka, a dogma which supposedly dominated thought so completely for some forty years that all progress came to a halt. Furthermore, the argument goes, even the present generations of anthropologists are by and large still captives of this dogma because they have been taught by those who were more directly influenced. I have no doubt that the Holmes-Hrdlicka dogma did influence some anthropologists--and perhaps still does--but I think the situation has been greatly exaggerated and I shall have more to say about it in another place. Just now I wish to state that it is preposterous to claim that thought-control of this kind would have been brought to bear on anthropology students on the Berkeley campus while Kroeber and his colleagues were teaching there. In some of his writings, Kroeber does touch briefly on the subject of man's antiquity in America, but only to emphasize how little was (and is) known about it: not enough, in fact, for any real conclusions. This was just another of the many problems to which students would be urged to apply themselves with energy and independence, with critical questioning of previous interpretations. If we look for causes of extreme conservatism, scholars like Kroeber must be among the first of all possible causes to be eliminated.

Another of Kroeber's remarks is worth repeating here, namely: "A generalization is not a law." He had said that too many anthropologists seemed to be deathly afraid of attempting generalizations because whenever they did so, an authority on some part of the world would be sure to leap forward to denounce it on the grounds that there were exceptions to it. "This kind of criticism misses the point," he would say, "because it confuses what may only be a general tendency in the data with an immutable law of some kind. Exceptions to a generalization are to be expected, but a law, on the other hand, is an ordering of phenomena which occurs invariably under certain stated conditions and any proved exception will destroy it. The discovery of underlying laws in any aspect of nature or culture is one of the basic aims of research everywhere and demands extreme caution; but anyone can draw generalizations from his studies as he sees fit."

On the subject of typology in archaeology and physical anthropology, I can recall only one occasion--lasting less than an hour--on which Kroeber expressed himself candidly. His comments struck me so forcibly at the time, however, that my own typological work has been largely based on them ever since. Heizer and I had laid out on tables the skulls and jaws, and associated artifacts, from some fifty burials excavated in a single Sacramento Valley site in 1936. In addition to analyzing the cultural material, we were ambitious to learn some of the techniques of physical anthropology as well, and had begun making standard measurements on the skulls. Like everyone else, including the physical anthropologists, we assumed that the way to discover "types" in skeletal material was painstakingly to make all the possible measurements, tabulate them, calculate means and standard deviations, etc., and then hope that somehow a series of "types" would be revealed by these statistics.

In the midst of this work, Kroeber made one of his visits to the Museum, and after watching us in silence for a time, began to pick up the skull-and-jaw assemblages one by one, holding them at arm's length, and gazing intently at the faces and profiles, then re-grouping them on the tables. As to our tabulations, he said "Those measurements are not the important thing. They have to be done, I suppose, but only as part of the description of the material after you have determined on groups or types some other way. Types will never emerge from the measurements and statistics alone. First you should experiment with groupings that might have some meaning; in other words, group them into skulls that look as if they belonged to people of similar appearance. Do the faces look long and narrow, or short and broad? Are the orbits round or oval or squarish? Are the foreheads high or low? Are the supraorbital ridges, mastoids, and other processes rugged or smooth? And so on."

After making groupings that seem to indicate generally similar appearance, Kroeber stated that we should examine the associated cultural material to see if there were any corresponding similarities or differences. After that, we should do the same with some other nearby sites to test the tentative results. Finally, and only after a lot of experimenting of this kind, we could make measurements to aid in describing to the reader the characteristics of each trial group. This is not the place to elaborate on Kroeber's recommendations: I wish only to point out that they emphasized the need for experimenting and searching for historical significance before "types" of any kind could be advanced. I followed these recommendations for years afterward with stone and bone artifacts, pottery, basketry, and so on.

I will end this account with one more of his remarks which may intrigue others as much as it did me. After the appearance of his Configurations of Culture Growth (University of California Press, 1944) and the second edition of Anthropology (Harcourt, Brace and Co., 1948), I once naively asked Kroeber how it was possible for anyone to get together such an immense amount of material in the short time he had spent, only about one year on each book when separated from the many other writings and duties he worked on at the same time. He said: "I depend pretty much on the Britannica for that sort of thing." After a pause he added: "The trick is to know when you can depend on the Britannica and when you have to go beyond it to dig up better information." Enough said!

There were, of course, many other comments on this and that during these all-too-brief conversations. I have tried to present in a very condensed form some aspects of Kroeber's philosophy which cannot be found in his voluminous writings--or at least not as candidly. Seldom indeed does the world produce such a combination of learning, insight, tolerance for others' views, modesty, and eagerness to be of help to all.