Book Reviews

The Boas Canon: A Posthumous Addition

Kwakiutl Ethnography. FRANZ BOAS. Edited and abridged, with an introduction, by HELEN CODERE. University of Chicago Press, Chicago, 1967. 477 pp., illus. $12.50.

Even today, 25 years after his death, Franz Uri Boas remains one of the most controversial figures in American anthropology. Assessments of his work run from adulation to almost utter denigration. One of his most famous students, Alfred L. Kroeber, wrote that Boas "was literally worshipped by some of those that came under his influence" (*1*). In an obituary for Boas in *Science*, Ruth Benedict concluded, "He found anthropology a collection of wild guesses and a happy hunting ground for the romantic lover of primitive things; he left it a discipline in which theories could be tested . . ." (*2*)—a theme later reiterated by Kroeber (*3*). Another of Boas's students rhapsodized, "To anthropology in this country Franz Boas, the 'Man,' came as . . . a culture-hero" (*4*). More recently, Abram Kardiner and Edward Preble tell us, "In his struggle to understand men he founded a disciplined anthropology—and never allowed himself one comfortable generalization about man" and "For his accomplishments in every major field of anthropology and for his unfailing integrity, Boas must be put down as one of the giants of modern anthropology. The problems that he discovered and the methods and attitudes that he developed to deal with them have, to an important extent, shaped the course that modern anthropology has taken, especially in this country" (*5*).

On the other hand, Leslie White, Boas's most outspoken critic, says, "Boas came fairly close to leaving the 'chaos of beliefs and customs' [in the ethnological enterprise] just about where he found it. . . . The belief that anthropology as a science was the creation of Boas is, of course, unwarranted (grotesque)" (*6*). Murray Wax claims that Boas's convictions on a variety of ethnological subjects "formed, when linked together, a chain that constricted creative research in cultural anthropology" (*7*). Someone else is alleged to have said that Boas set American anthropology back by 30 years. The full range of issues provoking these assertions and counterassertions is much too complex to describe here, but several of them revolve around the perceived inadequacies in Boas's ethnographic research and writing on the Indians of the Northwest Coast.

Boas worked with Northwest Coast materials for almost 60 years, and during this time (from 1886 to 1931) he made 13 field trips to the North Pacific Coast. Altogether he spent about two and one-half years on the coast conducting fieldwork. When he was not in the field he worked intensively by correspondence with several Indian informants, most notably with his major Kwakiutl informant, George Hunt, with whom he collaborated for more than 40 years. Even after his last field trip, at the age of 72, Boas brought informants to New York from time to time. He worked continually with his North Pacific Coast data until his death in 1942. The resulting ethnographic output was monumental. He published over 21,000 pages during his lifetime, almost half of which relate explicitly to the Indians of the Northwest Coast. More than 5000 of these pages represent major works on the Kwakiutl Indians alone. His bibliography contains more than 175 items dealing either totally or partially with the Kwakiutl.

But in all of these publications Boas never wrote an integrated, synthesized ethnographic description of any Northwest Coast group, not even his favorite people, the Kwakiutl. The greatest part of his North Pacific Coast publications are in the form of texts—verbatim ethnographic reports in the language of the Indians themselves. They include interlinear translations but no explanation, interpretation, or analysis to make the material more comprehensible to the reader. Boas did this deliberately, because he felt the most important task in ethnography was to present the culture of a people as perceived by the natives themselves. Only through verbatim texts, he believed, is it possible to avoid the inevitable (culture-bound) distortions occurring in the descriptions of outside investigators. Resulting from this belief in the importance of texts, however, are several thousand pages of ethnographic description which are exceedingly difficult to use.

Also, Boas was most interested in the strictly symbolic aspects of culture, such as folklore, mythology, art, and language, and he gave proprotionately less attention to structural features of Northwest Coast social systems such as social organization, economic organization (including the exact nature of the potlatch), and social stratification. Throughout his ethnographic reports he is sharply devoted to precise details, so much so, in fact that White has written, "Boas could not see the forest for the trees and could scarcely see a tree because of the multiplicity and 'complexity' of its boughs, branches and twigs" (*6*). Elsewhere (*8*), G. P. Murdock criticized Boas: "Despite Boas' 'five-foot shelf' of monographs on the Kwakiutl, this tribe falls into the quartile of those whose social structure and related practices are least adequately described among the 250 covered in the present study [*Social Structure*]."

These observations and criticisms form part of the polemic context in which the significance of Boas's posthumous *Kwakiutl Ethnography* must be viewed. That is, the ethnography comprises an additional source of evidence for evaluating the results and importance of Boas's fieldwork, and it contributes valuable supplementary information helping anthropology to formulate its ultimate verdict about the significance of Boas's work.

History of the Work

Boas began preparing the manuscript in 1936 while he was professor emeritus at Columbia, and he was still working on it when he died. The incomplete

manuscript was given to Ruth Benedict in 1943, and it was ultimately turned over to Helen Codere in 1958 to be prepared for publication. Influenced by its incompleteness and by the hostile criticisms raging against Boas's ethnographic work, Codere searched Boas's Kwakiutl publications for material that would round out the manuscript and make it a synthetic whole. Thus the volume reflects Boas's final assessment as well as (about one-quarter) Codere's editorial additions.

Structurally the ethnography is divided into 11 chapters plus Boas's introduction, five appendixes, and Codere's introduction. These chapters cover a range of topics, from the setting and background of the Kwakiutl to technology and economic organization, social organization, the potlatch, war, religion, two chapters on the winter ceremonial, mythology, the arts, and life-cycle materials. About half of Boas's original manuscript was devoted to the Kwakiutl winter ceremonial.

But what are some of the broader characteristics of the ethnography, and, more particularly, what contribution does the ethnography make as Boas's final, if incomplete, summary of the Kwakiutl? In the first place, the volume is not a conventional ethnography, in that it is not a descriptive statement of a single community in a definable time period. Second, a large part of the ethnography is in the form of what appear to be organized field notes with very few generalizations or overall conclusions. As he had always done, Boas draws heavily from the informants' reports and from verbatim excerpts of textual material expressing an informant's own viewpoint. Clearly more of the ethnography draws from these sources (especially from correspondence with George Hunt) than from Boas's own observations. Furthermore, the ethnography reflects Boas's minimal—from the standpoint of modern fieldwork—participation or active involvement in community life.

The style and content of *Kwakiutl Ethnography* are entirely consistent with Boas's earlier work, but they pose several basic problems that make the ethnography awkward to use. At the most general level it is often impossible to tell which Kwakiutl group Boas is talking about. (The term *Kwakiutl* designates all Kwakwala-speaking groups, even though no such term is recognized by the Indians themselves—except as introduced by whites. The term comes from early trading and missionary attempts to spell *Kwa'g·uł*, the name of one specific tribe residing at Fort Rupert, and the people with whom Boas worked most intensively.) Sometimes he refers to the "*Kwa'g·uł* proper," by which he means the Kwakiutl at Fort Rupert. At other times he simply writes "*Kwa'g·uł*," and except occasionally from context the reader cannot tell if he is referring to the people of Fort Rupert specifically or to all the Kwakiutl tribes collectively. Sufficient variation exists among the individual Kwakiutl groups to make it essential that the reader know at what level of generality Boas is writing. One can *usually* assume, however, that he is talking about the Kwakiutl at Fort Rupert unless the context makes it clear that he is not.

A second and equally pervasive problem is that Boas usually does not specify the time period about which he is writing, that is, whether he is describing events in the 19th century or in 1931. This is a serious flaw because of the fundamental transformations that took place among the Kwakiutl during the half century that Boas worked with these people.

Ethnological Issues

Other, more specific problems are also apparent in *Kwakiutl Ethnography*—problems that Boas's critics will not bypass without additional polemics. Boas and some of his students have been bluntly attacked for inadequately describing the nature of class-rank stratification on the North Pacific Coast, the potlatch system, and numimots (Kwakiutl ambilateral descent groups). These comprise three of the most hotly debated issues in Northwest Coast ethnology. *Kwakiutl Ethnography* would stand as an all-time classic if Boas had resolved or even clarified these issues. Unfortunately, little substantive amplification is provided for any of them.

Regarding numimots, for example, Boas writes, "It might seem that the numayma [numimots] as here described are analogous to the sibs, clans, or gentes of other tribes, but their peculiar constitution makes these terms inapplicable. The numayma is neither strictly patrilineal nor matrilineal, and within certain limits, a child may be assigned to any one of the lines from which he or she is descended, by bequest even to unrelated lines" (p. 51). Even though the passage shows that he perceived something of the ambilateral structure of Kwakiutl descent groups, he chose to use the noncommittal native term *numayma* to designate this form of social structure. Apparently he was not able to analyze the structure of these groups any further.

Furthermore, Boas introduces some curious inconsistencies in his description of Kwakiutl social organization. After expressly saying that such terms as *clan*, *sib*, or *gens* are inapplicable in relation to Kwakiutl descent groups, he later writes, "The ordinary form of blood revenge, which required equality of losses of the hostile family groups, was not foreign to the Kwakiutl. In cases in which a clan member had been killed by a member of another clan, the honor of the bereaved clan demanded that a man of equal rank or several people of lower rank belonging to the offending clan should be killed" (p. 109). Similar inconsistencies appear elsewhere.

As editor of the volume, Codere supplies additional information in her notes prefacing several chapters. She cites sources in other of Boas's publications where information on a given topic can be found—for example, sources on material culture, religion, and mythology. In her general introduction to the volume she favorably appraises the aims, content, and character of Boas's ethnographic research. She relates part of this to the growing anthropological subfield ethnoscience, where the attempt is made to discover and parsimoniously describe native principles of conceptualization and classification in some specific domain.

Dating the Field Trips

Codere makes several factual errors in her introduction that are surprising from such a competent Northwest Coast scholar. She writes, for example, "Boas made his first field trip to Bella Bella (Northern Kwakiutl) in 1885; his final Kwakiutl field trip was done in the fall and early winter of 1930 when he was seventy-two years old." In fact, Boas arrived on the North Pacific Coast for the first time on 18 September 1886, and he did not go to Bella Bella until 1888. Furthermore, his final field trip was terminated around 12 January 1931. She also writes that Boas "spent most of his time in the field at Fort Rupert." Again this is not accurate. Out of the approximately two and one-half years Boas spent on the coast (or approximately eight months in different Kwakiutl villages) he lived at Fort Rupert for less than three months.

Later Codere concludes, "The record shows Boas to have made twelve field trips to the Northwest Coast. During five of these trips, he was exclusively preoccupied with the Kwakiutl; and he worked in part with them on three further trips, bringing the Kwakiutl total to eight." As I have already mentioned, Boas made 13 trips to the coast. During only three of these trips, at most, can he be considered to have been (almost) "exclusively preoccupied with the Kwakiutl"; he worked in part with them on five other trips.

I comment on these inaccuracies not to disparage Codere's unquestionably valuable work but to correct the published record; until recently (*9*) almost nothing has been known about Boas's actual field work—how often he went to the field, what he did when he was there, how he felt about field work, the way in which he financed his field trips, and so forth.

Despite Codere's patient efforts, however, to develop an integrated, comprehensive ethnography from Boas's incomplete manuscript, anthropology is ultimately left with only a partial and inadequate insight into the rich cultural system of the traditional Kwakiutl. This must be the final verdict, even though the ethnography contains some valuable new information as well as amplifications on previously described issues. Disappointing though this is, scholars who are familiar with the general nature of Boas's Northwest Coast ethnographic output cannot be surprised.

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Chemistry to About 1700

The Origins of Chemistry. ROBERT P. MULTHAUF. Oldbourne, London, 1966, 70s; Watts, New York, 1967, $7.95. 412 pp., illus.

The writing of one-volume histories of chemistry remains a steady industry, but this latest product has more to commend it than most. Taking his title seriously, Multhauf devotes roughly equal portions of the book to the workers of antiquity, the medieval alchemists, and the new developments of the 16th and 17th centuries. He is content to end his tale "about 1700," when "within four generations . . . the basis of the science as we know it" would be constructed.

As an example of *haute vulgarisation* this book is outstandingly successful. Making few concessions to the general reader for whom he is avowedly writing, the author yet succeeds in providing a text which is lucid as well as detailed and scholarly. His writing is careful and his interpretations closely argued. With disarming ease he conjures up a wealth of names and dates to support his argument whenever he feels it necessary. The result is a work that is always readable and never dull. It will make an excellent replacement for J. M. Stillman's still-in-print but aging *Story of Early Chemistry*, to which it bears similarities that Multhauf himself notes.

Indeed one's major feeling of unease is that perhaps these two books are *too* similar. Surely we have obtained new historiographic insights into the periods in question, as well as new facts about them, since 1924. Multhauf is content to say that "perhaps I differ most with Stillman and other earlier historians in the attention here given to the history of medicine." His stress on the relation of medical and chemical thought is wholly admirable. But it is a pity that he does not draw on other recent scholarly developments, as seen in the writings of Pagel, Yates, Debus, and Rattansi. Then we would have had a high-level general work that truly reflected the findings of present-day scholarship.

Even so this is an impressive piece of work. Clear printing and adequate name and subject indices enhance its usefulness. If the argument is at times compressed almost to the point of meaninglessness (as in much of the discussion of the Pre-Socratics), this may be a penalty willingly paid by a writer determined to cram in so much. Indeed, at moments one is uncertain whether the author is informing the intelligent layman or lecturing his wayward colleagues. The wealth of footnotes and references adds to this uncertainty. Nonetheless this book can be confidently recommended to any intelligent chemist with a lively curiosity about how it all began. And no doubt many of the author's colleagues will purchase a copy, not least for its excellent 35-page bibliography.

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Archeological Report

Early Cultures and Human Ecology in South Coastal Guatemala. MICHAEL D. COE and KENT V. FLANNERY. Smithsonian Press, Washington, D.C., 1967. 179 pp., illus., plates. Available from the Government Printing Office, $3.50. Smithsonian Contributions to Anthropology, vol. 3.

Most archeological site reports are dreary affairs, rarely read except by a few specialists and rarely a pleasant experience even for them. Coe and Flannery have broken many of the rules. In fact the present synthesis of early material in the Ocos area of Guatemala is like a breath of fresh air in dusty library stacks.

The report succinctly presents the results of brief but effective test-pitting at Salinas la Blanca on the far northern Pacific coast of Guatemala (the title is geographically somewhat misleading). The artifacts discovered are clearly described and illustrated and are then related to Coe's previously published material from nearby La Victoria. This is followed by a detailed alignment of the local sequences with those of neighboring regions. Probably wisely, the question of very early maritime contacts with the north coast of South America, which Coe suggested in earlier publications, is not introduced.

Ecology is a term frequently used in titles these days, but a subject rarely set forth in any detail in the text. It is doubly important in early cultures, such as these (dated by radiocarbon about 1000 B.C.), when man's control of his surroundings was merely be-