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TEXAS MEMORIAL MUSEUM

THE UNIVERSITY OF TEXAS

the Ethnography and Ethnology of
FRANZ BOAS



By

LESLIE A. WHITE

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FIG. 1. Franz Boas on the *Germania*, en route to Baffinland, 1883. *Courtesy of Helene Boas Yampolsky.*

The Ethnography and Ethnology of Franz Boas*

by LESLIE A. WHITE

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Foreword

In the preparation of the following paper I have tried conscientiously to address myself to Boas' own publications and to ignore, insofar as possible, the commentaries of others, so that my conception of his work could be formed as independently as possible. I have, however, drawn upon essays about Boas in instances where quotations would illuminate his work or contribute to a better appreciation of it.

Before beginning the actual writing of this paper, I read or reviewed almost everything listed in Boas' complete bibliography as published in Kroeber, *et al.*, 1943, pp. 67-109, covering the years 1884 to 1943, inclusive, pertaining to the ethnographic field work and ethnological interpretations of Boas. I did not read any of the material published in German; almost all, if not all, of this has its counterpart in English publication. The material covered by me was very voluminous, numbering in the thousands of pages.

On some field trips Boas occupied himself with physical anthropology and linguistics as well as with conventional ethnographic work. I have endeavored to include in my study all of his field trips during which ethnographic work was done. I have, however, excluded from my review his technical work in linguistics.

Biographic note

Franz Boas was born in Minden, Westphalia in 1858. His father was a prosperous business man. He studied at Heidelberg, Bonn, and Kiel, taking his doctorate at the latter institution in 1881. He majored in physics and mathematics, but shifted toward physical, and later, cultural geography. In 1883-84 he was in Baffinland on a scientific expedition. His first field trip to the North Pacific Coast was made in 1886. He taught anthropology at Clark University from 1888 to 1892. He served as Chief Assistant in anthropology at the World Columbian Exposition in Chicago, in 1893. A year later he was appointed Curator (anthropology) in the Field Museum, Chicago. In 1896,

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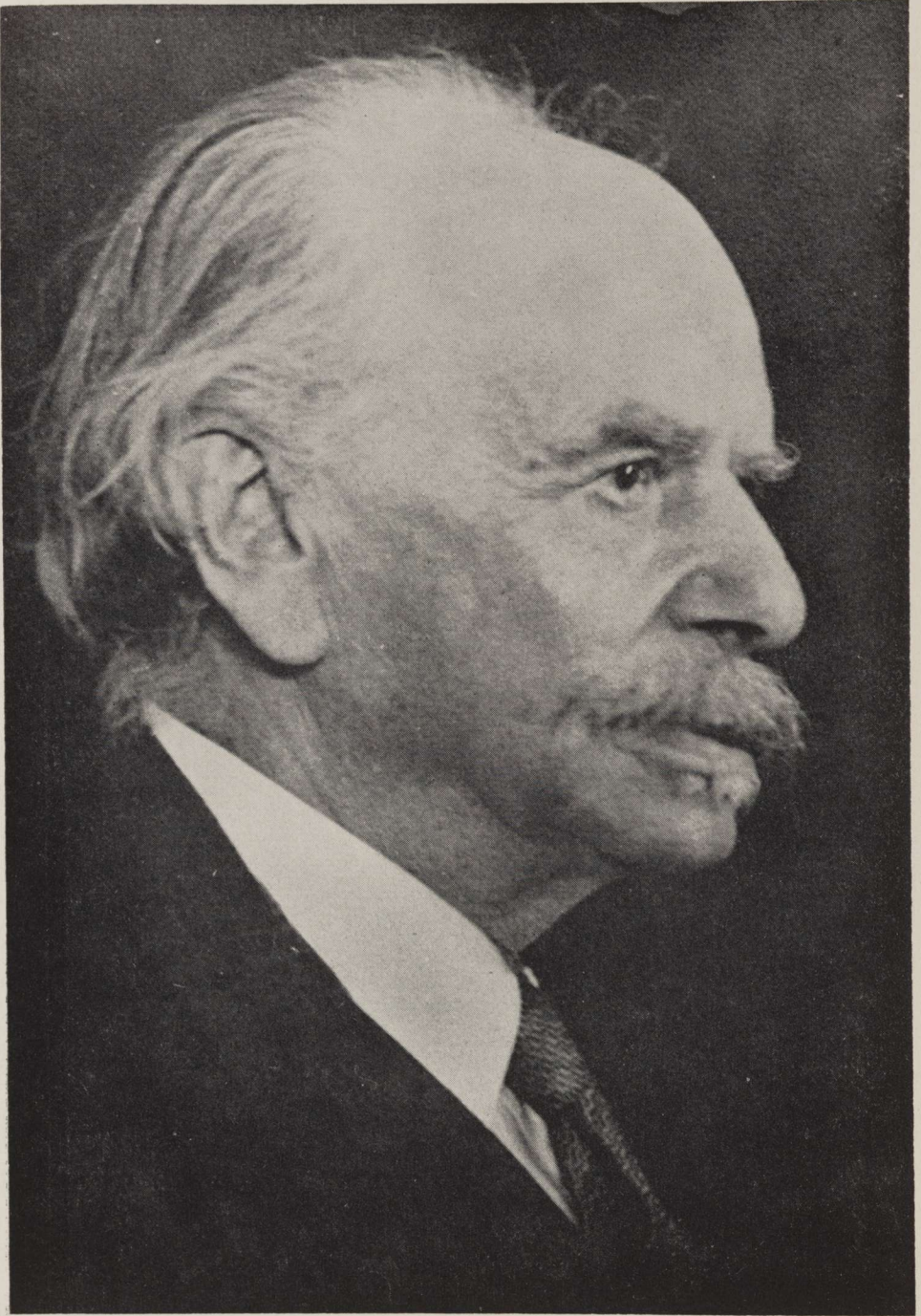


FIG. 2. Franz Boas. *Courtesy of Columbia University.*

he was appointed Assistant Curator in ethnology and somatology at the American Museum of Natural History, New York. In the same year he became a lecturer in physical anthropology at Columbia University; in 1899 he was appointed Professor of Anthropology at Columbia. He directed the series of Jesup North Pacific Expeditions from their inception in 1897. In 1905, he resigned from the staff of the American Museum of Natural History. He was elected to the National Academy of Sciences, and, in 1931, served as President of the American Association for the Advancement of Science. In 1937 he retired from his professorship at Columbia. He died in New York in 1942 (Kroeber, 1943; Herskovits, 1953, pp. 127-28).

PART I: ETHNOGRAPHY

"Boas must be understood, first of all, as a field worker," Robert H. Lowie (*The History of Ethnological Theory*, p. 131).

Boas embarked upon his first field trip in 1883; his last venture into the field was in 1930, when he was seventy-two years old. And when he could no longer go to the field himself, he brought a Kwakiutl Indian to New York in 1937 in order to continue his researches (Benedict, 1943, p. 60). His publications upon his explorations and researches amount to many thousands of pages and cover a period of six decades: from letters on his trip to the arctic in 1883 to *Kwakiutl Tales* in 1943. In terms of time spent in field researches and in magnitude of literary output, his record is impressive. But, to his achievement in cultural anthropology one must add his extensive work in linguistics and physical anthropology. And all this paralleled a long and distinguished career as a professor at Columbia University.

The main outlines of the history of Boas' ethnographic researches are easily summarized. He spent the year 1883-84 in arctic North America among the Eskimo. His investigations of the cultures of the Northwest Coast began in Berlin in 1885 and ended in New York in 1937, with many field trips to the actual region between these dates. He collected some folk-tales in Mexico in 1912 while he "was engaged in work for the International School of American Archaeology and Ethnology" (Boas, 1912a, p. 204). Between 1919 and 1921 he worked at some pueblos—at Laguna, Zuñi, and Cochiti—in New Mexico. He also took advantage, on a few occasions, to obtain data from members of non-Western societies whom he chanced to encounter in Chicago and New York. "During the past spring," he wrote in "Notes on the Eskimos of Port Clarence" (1894a, p. 205), "I had the good fortune to fall in with a party of Eskimo from Port Clarence, Alaska, who stopped in Chicago [where Boas was engaged in the World's Fair] on their way to Washington . . . Follow are the results of my fragmentary inquiries."

In the winter of 1914, Louis Shotridge, a full-blood Chilkat (Tlingit) In-

dian, born on the Chilkat River, and who had been a member of the staff of the University Museum, University of Pennsylvania since 1912, went to New York to study linguistics with Dr. Boas, according to the Museum's director, G. B. Gordon. In a prefatory note to "Grammatical Notes on the Language of the Tlingit Indians" (1917, p. 7) Boas said: "the following notes on the Tlingit language were obtained from Mr. Louis Shotridge, who spent about six weeks in New York during the winter of 1914-15 . . ."

We learn from a report of the Chief of the Bureau of American Ethnology (Holmes, 1916, p. 18) that "field work required in completing the Handbook [of American Indian Languages] was limited to a brief visit by Doctor Boas to the Carlisle Indian School in Pennsylvania."

Working with Mr. C. K. Simango, a Mundau from Portuguese South Africa (Herskovits, 1953, pp. 47-48), Boas published four papers on the Vandau: the concept of the soul among the Vandau, the avunculate, the relationship system, and tales in proverbs (the first was published in 1921, the latter three in 1922, see our bibliography). Ella Deloria was a Plains Indian studying at Teachers' College, Columbia. With her Boas produced the *Dakota Grammar*, published in 1941.

It is curious, in view of the fact that Boas' field work has been emphasized so much by those who have written about him, that no one has taken the trouble to bring together the most elementary facts about his field trips, such as the number of trips, the places where he worked, and the dates of the investigations. Boas supplies some information on some of these points in some—but far from all—of his publications, and even when he does, his information is sometimes indefinite and incomplete. In more than one instance, in his reports and monographs on the Northwest Coast, he says "in the winter of" such and such years, mentioning two consecutive years, when, as he states in other places, he was actually in the field only in the fall and up to and including December. Thus, in four places (Boas, 1888a, p. 47; 1888b, p. 49; 1889a, p. 233; 1890, p. 801), he tells us that he was in British Columbia "in the winter of 1886-87." But, as we shall see below, he arrived in the field in September and returned to New York in December. In one publication (Boas, 1896a, p. 257) he states that he was in the field "in the winter of 1894-95," but in another place he says "during the months from September to December, 1894, I revisited British Columbia . . ." (Boas, 1895a, p. 523).

In the preface to *Kwakiutl Tales* (1910, p. v) he says merely that the tales had been collected "on various journeys to British Columbia." And in the preface to *Keresan Texts* (Pt. I, p. VIII), he states that the material was collected "during the years 1919-1921," but he does not specify what portions of these years were spent in the field or even indicate at which of the seven Keresan pueblos the data were obtained. Apparently Boas did not feel that it was necessary, or important, to provide information of this sort.

I present herewith a list of dates and places of Boas' ethnographic field trips. It is based upon data obtained for me by a research assistant, Miss Meredith Jean Black, from Boas' diaries and correspondence in the archives of the American Philosophical Society,* from a microfilm of abstracts of Boas' correspondence made by his daughter, Mrs. Helene Boas Yampolsky, and from information provided by Boas himself or by one or two of his co-workers in the field. This is the first time, I believe, that such a list has ever been compiled. It is not perfect and complete, however; on the contrary, we know that some of the dates are only approximate.

1. To Baffinland, August, 1883 to August, 1884 (Boas, 1884, pp. 248, 270).
2. 1886, to the North Pacific Coast, September 18 to December 16.
3. 1888, to the North Pacific Coast, from the latter part of May to the latter part of July (see Boas, 1890, p. 801).
4. 1889, to the North Pacific Coast from about the middle of July until the early part of October (see, also, Boas, 1891a, p. 604).
5. 1890, to the North Pacific Coast. It appears, from letters, that he was in Portland on June 9, and in Oakland, California, on July 24; a letter from Horatio Hale, dated October 10, indicates that Boas had recently returned to the east. "In 1890 I fell in with a number of Bella Coola who were fishing for salmon in Fraser River" (Boas, 1898a, p. 27); he refers, also, to his summer's work in Boas, 1894b, p. 454.
6. 1891, to the North Pacific Coast from approximately June to latter part of October. The *Chinook Texts* were recorded in the summers of 1890 and 1891 (Boas, 1894c, p. 5).
7. 1894-95. "During the months from September to December, 1894, I revisited British Columbia. . . ." (Boas, 1895a, p. 523); in the preface to *The Kwakiutl of Vancouver Island* (1909b, p. 308) Boas writes "In 1895 I revisited British Columbia. . . ." But from material at the American Philosophical Society it would appear that he went to California about the end of December or early in January, where he gave a lecture at Stanford University. He did some field work also: in "Anthropometrical Observations on the Mission Indians of Southern California" (1896g, p. 261) he states that the work was done "in the winter of 1894-95." He returned to New York in January, 1895.
8. 1897, to the North Pacific Coast, from June 2 until the "middle of September" (Boas, 1898b, pp. 8, 11; also, Boas, 1905a, p. 93, where he says he worked with the Tsimshian, Bella Coola, and Thompson Indians).
9. 1900, from June to September. Work with the Thompson River and the Vancouver Indians of British Columbia (Boas, 1905a, p. 93).
10. 1912, in Mexico where, as we have already noted, he collected some folktales while "engaged in work for the International School of American Archaeology and Ethnology. . . ." (Boas, 1912a, p. 204).
11. 1919, at Laguna Pueblo, New Mexico. He left New York late in May or

* I wish to acknowledge my indebtedness to Dr. John F. Freeman, Research Associate at the American Philosophical Society, for his generous assistance in acquainting me with this material.

early in June. He was in Laguna by June 16, and had returned to New York by July 10.

12. 1920, to Pueblos in New Mexico: San Felipe and Santo Domingo and perhaps others. He left New York the latter part of May or early in June; he had returned to New York by July 13.

13. 1921, to Cochiti, and possibly other pueblos. He left New York on September 9 and returned by the middle of October. Goldfrank (1927, p. 5) tells us that she "accompanied Professor Franz Boas, who was making a special [*sic*] study of the Keres language," to Cochiti Pueblo "in the fall of 1921 and the spring of 1922."

14. 1923, to the North Pacific Coast. His expense account gives the dates of November 4 to December 24. *Bella Bella Tales* (1932a) were collected in the fall of 1923 (Boas, 1932a, p. VI).

15. 1930-31, to the North Pacific Coast. He arrived in Vancouver on October 21 and returned to New York the latter part of January. Mead (1959b) has published letters from Boas to Ruth Benedict, written in October and November, 1930. In the preface to *Kwakiutl Tales* (new series, 1935, p. VII) Boas states that they were obtained at Fort Rupert in the winter of 1930-31.

Boas spent the summer of 1892 in Germany. In 1893 he was engaged in work with the Columbian Exposition in Chicago. In 1896 he was appointed Lecturer at Columbia University for the period July 1, 1896 to June 30, 1897. He wrote to G. M. Dawson on June 4, 1896, that he would give up his plans for field work during that summer, but would be back in the field in the summer of 1897. In "The Jesup North Pacific Expedition" (Boas, 1905a, p. 93) Boas has a list of men who did field work and the years during which it was done. For himself, he lists 1897 and 1900 as being in the North Pacific Coast, but mentions no field work for 1898, 1899, 1901, and 1902.

In the preface to *The Religion of the Kwakiutl Indians* (1930a, Pt. I, p. x) Boas wrote: "In 1903 Mr. Hunt visited New York. . . . I had no opportunity to revisit the native villages on the coast of British Columbia until 1923 when I went to Bella Bella. . . ." But, on the same page, he said: "Twice more, in 1912 and 1927, I met Mr. Hunt in Victoria, British Columbia." I do not know what to make of these reports.

If we omit his trip to Baffinland, which was undertaken primarily as a geographer, and the year in Mexico, where his ethnographic researches appear to have been very limited, the amount of time spent by Boas in the field is approximately as follows:

1886	3 months	1900	3 months
1888	2 months	1919	1½ months
1889	3 months	1920	1½ months
1890	2 months	1921	1 month
1891	4 months	1923	2 months
1894-5	4 months	1930-1	3 months
1897	3½ months	Total.....	33½ months

Of the total, 33½ months, 4 were spent in New Mexico; 29½ on the North Pacific Coast.

If our knowledge of the number and dates of Boas' field trips is insufficient, our information on the way in which these trips were financed and the auspices under which they were undertaken is even more meager and inadequate. In 1882, when he was planning an expedition to Arctic America he wrote "I have drawn up a budget, and I find that for from 500-600 dollars everything can be cared for" (Boas, 1882, p. 114). He hoped that he might obtain "the sponsorship of the American Geographical Society. . . . [for] through this organization, Hall in a very short time received from private individuals nearly everything he needed for his trip. . . ." (*ibid.*). As we shall note shortly, the German Polar Commission provided him with passage to America and with supplies and equipment for the field. And, according to Kroeber (1943, p. 8), Boas, "his own funds being insufficient, arranged for additional financing from a newspaper—the *Berliner Tageblatt*, which published a series of his letters and travel reports from 1883 to 1885."

In 1885-86, when Boas was in Berlin—and presumably after he had "spent two strenuous weeks with the [Bella Coola] Indians" who were being "exhibited" there (Boas' letter to M. K., his fiancée, dated February 5, 1886, quoted by his daughter, Mrs. Helene Boas Yampolsky, 1958, p. 312)—he tried, but without success to obtain funds for a field trip to Canada (*ibid.*). Bastian, to whom Boas applied for assistance, was rude to him (*ibid.*).

When Boas was in New York in the summer of 1886, on his way to the Northwest Coast, he addressed a letter to Charles P. Daly, the president of the American Geographical Society, asking him to "lend him his valuable assistance." "Most probably I shall get a free passage on the Canadian Pacific Railroad by the help of my Canadian friends," he wrote Daly (Boas, 1886a, p. 525), and "instruments will be lent to me by my friends in Washington." But he needed money: "I estimate that I must have a credit of \$1,000 for the purpose. Though I suppose that only \$750 will be needed I ought to be allowed to spend the above amount as I cannot judge now how much traveling there will have to be done" (*ibid.*). He adds: "I consider this amount a small one as compared to the importance of the object in view and beg to have your support for my plans." But, according to Benison (1949, p. 525), "Boas was unable to get any financial aid from the Geographical Society."

In the letter to Daly, cited above, Boas says that he had applied to the Canadian Geological Survey for financial support, but "on account of the small appropriations of that institution they could not give me any material help."

Mrs. Yampolsky (1958, p. 312) reports that Boas "tried to interest the Ethnographic Bureau of Canada and his efforts finally bore fruit—his three months' field trip to Vancouver Island in the fall of 1886." Boas, himself, states in the preface to *The Kwakiutl of Vancouver Island* (1909b, p. 307) that "with the financial aid of personal friends, I was enabled to visit the coast of

British Columbia in the fall of 1886." Whether the "personal friends" were officers of the Ethnographic Bureau of Canada, or whether assistance came from these two different sources, are questions that we cannot answer. He did, however, obtain a free pass on the railroad to the west coast (Yampolsky, 1958, p. 313).

Intermittently between 1888 and 1898, Boas conducted anthropological researches on the Northwest Coast under the auspices of the British Association for the Advancement of Science. There has been some difference of opinion as to how and when this working arrangement was effected. Benedict (1931, p. 279) states that it began in 1886, which is definitely an error. De Laguna (1960, p. 102) states that "in 1888 Franz Boas was sent by E. B. Tylor to the Northwest Coast for his first summer's investigations for the British Association for the Advancement of Science." Kroeber (1943, pp. 11-12) writes: "In the summer of 1888 he [Boas] revisited the Indians of British Columbia, this time under the auspices of the British Association Committee on the Northwestern Tribes of Canada, of which Tylor was chairman and Bloxam secretary. Whether he applied for the work, or how he came to be selected, is not clear; nor when he met Tylor." We have Boas' own statement of how it came about:

"The meagre results of my first journey [in 1886] brought me the opportunity to revisit British Columbia in 1888, following an invitation of Horatio Hale, then editor of the Committee of the British Association for the Advancement of Science for the Study of the Northwestern Tribes of Canada, which had been appointed at the Montreal meeting in 1884" (Boas, 1909b, p. 308).

We learn from the fourth Report of the Committee, consisting of E. B. Tylor, chairman, and others, appointed by the British Association for the Advancement of Science "to investigate the Physical Characters, Languages, and Industrial and Social Conditions of the Northwestern Tribes of the Dominion of Canada" that they had "been enabled to secure the services of Dr. Franz Boas . . . who has consented to return to that province [British Columbia] for the purpose of continuing his researches there on behalf of the Committee. . . ." (Report of the British Association for Advancement of Science; London, 1889, p. 233). Contained in this report were a letter from Boas addressed to Horatio Hale and "Preliminary Notes on the Indians of British Columbia" (pp. 233-242). Boas submitted, and the Committee published, reports of his researches in subsequent reports of the Committee: Fifth Report, pp. 801-893, 1890; Sixth Report, pp. 562-715, 1891; Seventh Report, pp. 408-449, 1892; none in the Eighth Report; Ninth Report, pp. 454-463, 1894; Tenth Report, pp. 523-592, 1895; Eleventh Report, pp. 569-591, 1896; Twelfth and Final Report, pp. 628-644 (with Livingston Farrand), 648-682, 1889. These dates are for the publication of the Reports of the British Association; the field trips were usually, but not always, a year earlier.

Our information concerning the extent to which the British Association supported Boas' field work on the Northwest Coast is rather meager. A letter from Horatio Hale to Boas, dated May 14, 1888, enclosed a letter of credit and railroad tickets (archives of American Philosophical Society). A sum of \$600 was specified in correspondence between Boas and the Committee on North Western Tribes prior to the field trip of 1889. Letters from Horatio Hale (June 27, 1889) and Daniel Wilson (July 12, 1889) tell Boas that the Committee had approved the sum of \$1,000 for three months work in the summer of 1889. The Sixth Report of the Committee (Boas, 1891a, p. 553) states that "the grant made to the Committee was supplemented by \$500 from the Canadian Government. . . . The Committee ask for reappointment, and for a grant of 200 l." A letter from D. Wilson, a member of the Committee, to Boas on June 3, 1891, enclosed a draft for \$280 for the summer—and best wishes for the trip (American Philosophical Society). In 1894 a sum of \$500 was placed at the disposal of Boas and Dawson for three months in the field. The Tenth Report of the Committee (Boas, 1895a, p. 522) alludes to funds provided by the British Association and mentions a "large contribution by the Canadian government to the funds at the disposal of the Committee."

The field trip of 1894–95 was supported "through an arrangement with the Committee of the British Association. . . the U. S. National Museum, and the American Museum of Natural History" (Boas, 1909b, p. 308).

The field trip to the Mission Indians of Southern California was financed by a grant from the American Association for the Advancement of Science (Boas, 1896g, p. 261).

Boas had a close working relationship with the Bureau of American Ethnology for many years. A letter from Major J. W. Powell, director of the Bureau, to Boas, dated December 21, 1888, informs the latter that the Bureau would support his linguistic (Salish) researches during the summer of 1889 to the extent of \$450.

"Toward the close of the calendar year," wrote Major Powell, in his report for 1894–95, "Dr. Franz Boas . . . communicated with the Bureau suggesting that he be given authority and means for visiting the Kathlamet Indians . . . for the purpose of collecting texts . . . he was authorized to carry out his plan of operations, the material to be conveyed to the Bureau for a stipulated sum. Dr. Boas devoted several weeks to the work, and after his return to the United States prepared the material for publication" (Powell, 1897, p. LXV).

In his Director's Report for 1898–99, Major Powell wrote: ". . . working under a small allotment, Dr. Franz Boas continued the preparation of linguistic material collected among the tribes of Northwestern United States and contiguous Canadian territory" [adding that the material would be published] (Powell, 1903, p. XXI).

"At the opening of the year [1901–02] Professor Franz Boas, of Columbia

University, received an honorary appointment as philologist [from the Bureau of American Ethnology] and was intrusted with the supervision of a considerable part of the linguistic researches in which the Bureau is engaged" (Powell, 1904, p. XXXIV).

We do not know the extent to which Boas' linguistic or ethnographic researches were supported financially by the Bureau of American Ethnology, but it would appear that such aid was rather meager.

The field researches made by Boas and others under the auspices of the Jesup North Pacific Expedition were financed by Mr. Morris K. Jesup, president of the American Museum of Natural History. It was organized in 1897 "at my suggestion" says Boas (1909b, p. 309). The Jesup North Pacific Expe-

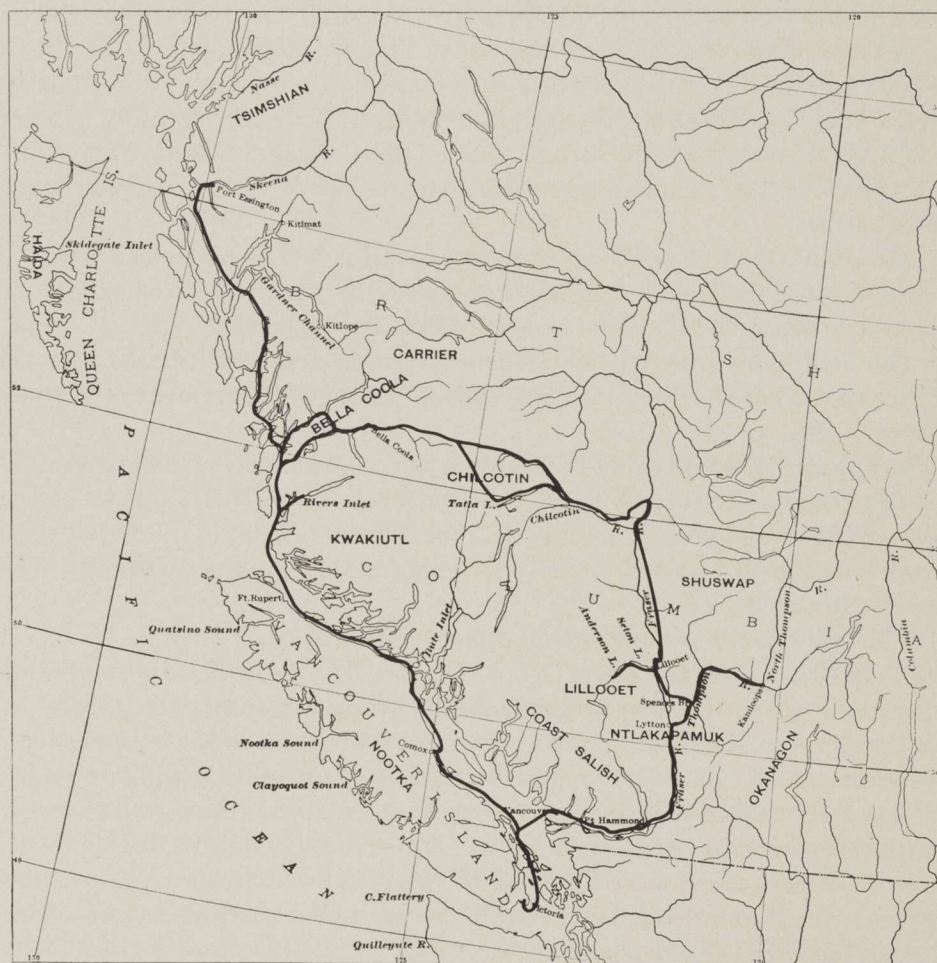


FIG. 3. Sketch map of British Columbia showing the field of operations of the Jesup North Pacific Expedition in 1897. Courtesy of American Museum of Natural History.

dition was not a group enterprise as was the Torres Straits Expedition of 1898, for example. Rather, it consisted of a number of field trips by a number of individuals—John R. Swanton, Harlan I. Smith, Livingston Farrand, James A. Teit, and Franz Boas on the west coast of North America, and Waldemar Jochelson and Waldemar Bogoras in northeastern Asia.

The project was placed under the direction of Boas, and publication of its results was executed under his editorship. Mr. Jesup “donated to the Museum the means” to carry on the work of the expedition (Boas, 1905a, p. 92). In a letter to George Hurlbut, librarian of the American Geographical Society, written in 1900, Boas said “we are about to send an expedition to Siberia for the purpose of ethnological investigations . . . Mr. Jesup, who is defraying the expenses of the expedition. . . .” (Benison, 1949, p. 526).

Mr. Homer E. Sargent, of Chicago—later, of Pasadena—“for many years defrayed all the very considerable expenses of Mr. [James A.] Teit’s work (Boas, 1930b, p. 25). We find other mention of Mr. Sargent’s “generous financial support” of, at least Teit’s work, and possibly that of another, or others (Hodge, 1916, pp. 18–19; Fewkes, 1925, p. 12). We do not know whether or not any of Boas’ ethnographic field researches were supported by Mr. Sargent.

In the preface to *Ethnology of the Kwakiutl* (1921a, p. 45), Boas wrote: “The material contained in the following pages was collected partly in connection with the work of the Jesup North Pacific Expedition, partly after the close of the expedition, largely with funds provided by friends interested in the scientific work of the Department of Anthropology in Columbia University.”

Boas unquestionably obtained funds for research from foundations in later years of his field work—“research funds rolled in on him for the asking” (Kroeber, 1956, p. 153)—but we have no data on this point.

We turn now to his field researches and the publications resulting therefrom.

Field Work in Arctic America

From a letter written by Boas to his uncle, Dr. Abraham Jacobi, in the fall of 1882 we learn that he (Boas) had applied for a “Fellowship in Baltimore” in order to “learn the things which, as a geographer, I will later need to know. . . . I wanted to prepare myself in Baltimore . . . to go on scientific trips for a few years, to see and to learn. . . . I very much hoped to find in Baltimore . . . the opportunity to join an expedition, to find a place for geographical research, or better still be sent somewhere all by myself. My research interest is completely oriented toward the American polar area. I am, so far as is possible, well qualified for this. I am entirely familiar with the literature on the region . . . the principal objective of my work would concern the mi-

grations of the Eskimo" (Boas, 1882, p. 114). Elsewhere in the letter he writes, "I have in mind a geographical investigation . . . I am making studies of the Eskimo and their knowledge of the land they inhabit, as well as of the entire region, in the hope of demonstrating a certain relationship between the number of people in a tribe, the distribution of food, and the . . . [word undecipherable]. . . . I am learning here the things that are needed for making a scientific expedition, and finally, most important of all, I am learning and already know the Eskimo language . . ." (ibid.).

Boas hoped to obtain the sponsorship of a geographical society, especially that of the American Geographical Society, and hoped, also, to get free passage to America on a whaling vessel or on the "ship that calls for supplies at the [German meteorological] station in Cumberland Sound" (ibid.).

Boas' efforts to find support for his expedition were successful. "On my request," he reported later, "the German Polar Commission gave me every assistance for the work I intended to do, granting me a passage on board the *Germania*, the use of the houses of the station [in Baffinland] and a good supply of provisions, hunting-gear and some instruments" (Boas, 1884, pp. 246-47).

On the 20th of June, 1883, Boas left Hamburg on the German schooner, *Germania*, for Arctic America. With him was Wilhelm Weike, his servant. This poor fellow was not to accompany his master on much of his exploration, however, as his (Weike's) feet were frozen on a journey in December, and he was incapacitated at the station at Kikkerton for the rest of the winter. The *Germania* entered Davis Strait on July 9, but did not reach the station at Kikkerton until about the last of August, due to ice and weather (Boas, 1884, pp. 247-48, 260-61).

"Dr. Boas spent about twelve months from August, 1883 till the 25th of August, 1884, in exploring from his headquarters at the Kikkerton Islands Whaling-station, the coasts of Cumberland Sound and Davis Strait. . . . as far as Cape Raper in latitude 69° 50' north, traversing in all his journeys nearly 2,400 miles of country most of which had previously been unexplored" (editor's note to Boas, 1885, p. 768).

"Besides the mere geographical work, which took most of my time," Boas wrote in 1884, "I made ethnographical collections and observations, which give many new points of view referring to the religious ideas and traditions of the Esquimaux. As I learned the language of this people, I was able to understand the old songs and tales . . . as I lived amongst them as one of them, I learned their habits and ways, I saw their customs referring to birth and death, their feasts, etc." (Boas, 1884, p. 271).

The impression that the first sight of Eskimos made upon this young cultured German scholar is vividly recorded by Boas:

"When our ship . . . was about to enter the port of Kikkerton . . . there came

a boat-load of Eskimos to offer us their help. I had not formed a good opinion of the appearance of these people, but I was really astonished at the figures I saw. The little bandy-legged fellows who ran laughing and chewing over the deck of the vessel, with their long black hair, flat faces, and dripping eyes, made an extremely repulsive impression; and when we were visited by a boat-load of women, among whom were a few antiquated matrons, my aversion toward my fellow residents reached its highest point. It really seemed as if the ugliest of the ugly had been selected to receive us, for I was afterward surprised by many a cheerful and pleasant face, or a strong, well-built figure" (Boas, 1885, pp. 771-72).

Before he left Germany in 1883, Boas published three articles on the Eskimos, and his letters on his trip began to appear in the *Berliner Tageblatt* in August, 1883. His publications on his year in the arctic were numerous (see bibliography in Kroeber *et al.*, 1943, pp. 67-72). "His purely geographical articles on the Arctic continued to appear until 1888," says Kroeber (1943, p. 9), "but even by then they had been surpassed in number and volume by ethnological ones on the Eskimo."

Boas published two "popular" articles upon his arctic adventure that are especially interesting: "A Journey in Cumberland Sound and on the West Shore of Davis Strait in 1883 and 1884," in the *Journal of the American Geographical Society* in 1884; and, "A Year Among the Eskimo," *Bulletin of the American Geographical Society*, 1887. They are vivid, well-written accounts of his travels and hardships, the Eskimos, their dog-teams, hunting, amusements, myths, and so on. "Cumberland Sound and its Eskimos" (*The Popular Science Monthly*, 1885), also describes his life among the Eskimos. Much of the material in one article is repeated in another: the myth of Sedna, the ceremony of the Kailertetang, the ritual slugging match in which visiting stranger and welcoming host engage, etc. One paragraph, for example, appears in identical wording in "Cumberland Sound and its Eskimos" and in *The Central Eskimo* (p. 603), and again, slightly different, in "A Year among the Eskimo" (p. 391):

"When late in the fall storms rage over the land and release the sea from the icy fetters by which it is as yet but slightly bound, when the loosened floes are driven one against the other and break up with loud crashes, when the cakes of ice are piled in wild disorder one upon another, the Eskimo believes he hears the voices of spirits which inhabit the mischief laden air."

In 1889 Boas published, with H. Rink, a short paper, "Eskimo Tales and Songs" (Boas, 1889b, pp. 123-131): the translations, the first excepted, and the linguistic notes were provided by Rink; the explanations of the songs, by Boas. Five years later he published "the texts and translations of a few more [Eskimo] tales, ditties, and songs which were collected by me in Cumberland Sound in the years 1883 and 1884" (Boas, 1894d, p. 45). We have already

noted his work, while in Chicago in 1893, with some Eskimos from Alaska (Boas, 1894a). More "tales, ditties and songs, collected by me [Boas] in Cumberland Sound" were published in 1897 (Boas, 1897a).

Boas also published the field observations of others—of sea captains and missionaries: 1) a short paper on "The Religious Beliefs of the Central Eskimo" (Boas, 1900); and 2) two lengthy papers on "The Eskimo of Baffin Land and Hudson Bay" (Boas, 1901a, 370 pp.; 1907, pp. 371–570).

Boas chose two of his articles on the Eskimo, "The Folk-Lore of the Eskimo" (1904) and "The Relationships of the Eskimo of East Greenland" (1909) for republication in his anthology, *Race, Language, and Culture* (1940).

Boas' major ethnographic publication was, however, *The Central Eskimo*, published in 1888 by the Bureau of American Ethnology. It was a noteworthy achievement, but hardly "monumental" as Kroeber (1943, p. 9) has termed it. It is a simple, straightforward, lucidly written monograph of 270 pages, of which 40 are devoted to distribution of tribes, 45 to hunting and fishing, 54 to social and religious life, 26 to tales and traditions, and 10 to poetry and music. Many aspects of Eskimo culture are treated superficially, or not at all. In one paragraph, for example, he (1) describes women doing housework in front of lamps; (2) women take care of puppies, if any; (3) children are carried in hoods, are suckled and weaned; (4) "when about twelve years old they [children] begin to help their parents, the girls sewing and preparing skins, the boys accompanying their fathers in hunting . . ." (5) parents treat their children kindly and the children are obedient (pp. 565–66). One of the subjects completely ignored by Boas is that of relationship terms: ". . . not only did [he] not collect kinship terms," says Eggan (1960, p. 180), "but for some reason did not utilize those published by Morgan a decade earlier from the same region." Boas drew upon many previously published works on the Eskimo and the Arctic in writing *The Central Eskimo*, but perhaps he was not acquainted with Morgan's *Systems of Consanguinity and Affinity* (1871).

Field Work on the Northwest Coast

Interestingly enough, Boas began his study of Northwest Coast culture in Berlin in 1885. A Captain Adrian Jacobsen had brought a group of Bella Coola Indians to Berlin where they were "exhibited" for about two weeks, during which time Boas made a brief study of their language and culture (Boas, 1886b, 1909b, p. 307). He published four articles on his investigations before he ever set foot on the Northwest Coast (Boas, 1886b, 1886c, 1886d, 1886e).

As we have already seen, Boas reached British Columbia in the fall of 1886, and immediately plunged into field researches and publication. In



FIG. 4a. Franz Boas on left, with Eskimos. *Courtesy of Helene Boas Yampolsky.*



b. Pencil sketch of Arctic village by Franz Boas. *Courtesy of the American Philosophical Society.*



FIG. 5a.



b. Bella Coola Indians in Berlin, 1885. Photographs by Carl Gunther. *Courtesy of the Provincial Museum of Natural History, Victoria, B.C.*

1887 he published 40 articles, eight of which dealt with the Northwest Coast; three of them were published in Germany.

According to Codere's tabulation (1959, p. 61), "Boas' Kwakiutl and Northwest Coast publications total more than 10,000 printed pages, written over a period of almost six decades"; his "major" works on the Kwakiutl alone ran to 5,255 pages. What does this extensive literature consist of?

It is not easy to devise a classification that will accommodate Boas' publications on the Northwest Coast, but the following breakdown will give a fair idea of their nature.

Preliminary and General

- 1887 The Coast Tribes of British Columbia (Science)
- 1887 Notes on the Ethnology of British Columbia (Proceedings of the American Philosophical Society)
- 1888 Indians of British Columbia (Transactions of the Royal Society of Canada)
- 1888 Indians of British Columbia (Popular Science Monthly)
- 1888 The Development of the Culture of Northwest America (Science)
- 1889 Preliminary notes on the Indians of British Columbia (Fourth Report of the Committee to study North-Western Tribes of Canada; British Assn. for Advancement of Science)
- 1890 The Indians of British Columbia (Fifth Report)
- 1891 The Indians of British Columbia (Sixth Report)
- 1895 The Indians of British Columbia (physical; Tenth Report)
- 1896 The Indians of British Columbia (Bulletin, American Geographical Society)
- 1896 The Indians of British Columbia (Eleventh Report)
- 1899 Physical Characteristics of the Tribes of British Columbia, with Livingston Farrand (Twelfth Report)
- 1906 The Tribes of the North Pacific Coast (Annual Archaeological Report, Ontario)

All but one of these articles were descriptive and some were relatively brief. But it is significant to note that already in 1888, only a year or so after he first set foot in an Indian village, he undertakes a general statement on "The Development of the Culture of Northwest America."

Boas' first article on the Kwakiutl Indians, the tribe that was to occupy so much of his time for some fifty years, appeared in 1887, within a year after his first trip to British Columbia. A number of others appeared during the first decade of his work in this area:

Kwakiutl

- 1887 Census and Reservations of the Kwakiutl Nation (Bulletin of the American Geographical Society)



FIG.6. Nawiti, a Kwakiutl Village on Vancouver Island. *Courtesy of the American Museum of Natural History.*

- 1888 On Certain Songs and Dances of the Kwakiutl of British Columbia (Journal of American Folk-Lore)
- 1888 Dawson on the Kwakiutl (unsigned; Science)
- 1889 The Houses of the Kwakiutl Indians, British Columbia (Proceedings of the U. S. National Museum)
- 1892 Vocabulary of the Kwakiutl Language (Proceedings of the American Philosophical Society)
- 1896 Songs of the Kwakiutl Indians (Internationales Archiv für Ethnographie).

One of Boas' most distinguishing traits as a field worker was his emphasis upon the importance of obtaining and recording ethnographic data in the language of the natives themselves, i.e., in texts; he was the first anthropologist, as far as I know, to do this, and he followed this course more extensively than any other anthropologist that I know of. Boas felt that it was important to "present the culture as it appears to the Indian himself." "For this reason," he wrote in 1908 (Boas, 1909b, p. 309), "I have spared no trouble to collect descriptions of customs and beliefs in the language of the Indian, because in these the points that seem important to him are emphasized,

and the almost unavoidable distortion contained in the descriptions given by the casual visitor and student is eliminated. For many years I have advocated a more extended application of this method in our studies of the American aborigines . . .”

Not only did Boas record voluminous texts himself, he taught two Indians to write their own languages and persuaded them to record ethnographic material; we shall return to this point later.

Paul Radin, one of Boas’ early students, reports as follows upon Boas’ instructions regarding field work:

“To Professor Franz Boas he [Radin] is under especial obligations for directing him to the Winnebago, for the methods of research inculcated in him . . . and particularly for impressing upon him the necessity of obtaining as much information as possible in text” (Radin, 1923, p. 48).

We list herewith Boas’ principal publications in text:

Texts and Translations

1894 Chinook Texts	278 pp.
1901 Kathlamet Texts	261 pp.
1902 Tsimshian Texts	240 pp.
1905 Kwakiutl Texts	528 pp.
1906 Kwakiutl Texts, second series	267 pp.
1912 Tsimshian Texts, new series	220 pp.
1921 Ethnology of the Kwakiutl	1418 pp.
1925 Contributions to the Ethnology of the Kwakiutl	357 pp.
1928 Bella Bella Texts	291 pp.
1930 The Religion of the Kwakiutl Indians	572 pp.
Total	4,432 pp.

To these texts from the Northwest Coast should be added *Keresan Texts* (1925; 344 pp.) from the southwest and their translation (1928; 300 pp.).

What do these texts consist of? Myths and folk-tales for the most part. *Chinook Texts* contains 187 pages of myths, eight pages of historical tales, and 83 pages of “beliefs, customs [pregnancy, marriage, birth, death, whaling, elk hunting, etc.], and tales.” *Tsimshian Texts* are of folk-tales. The bulk of *Kwakiutl Texts* (1905) consists of “traditions”; there are some “miscellaneous texts,” and a few songs. Tales and miscellaneous material are recorded in *Bella Bella Texts*.

We turn next to collections of myths and tales, many of which were recorded both in text and translation:

Myths and Tales

1888 Myths and Legends of the Catloltq	19 pp.
1896 Traditions of the Tsetsaut	26 pp.

1898 Traditions of the Tillamook	34 pp.
1898 Mythology of the Bella Coola	103 pp.
1910 Kwakiutl Tales (texts and translations)	495 pp.
1916 Tsimshian Mythology (texts and translations)	335 pp.
1918 Kutenai Tales (texts and translations)	387 pp.
1932 Bella Bella Tales	178 pp.
1935 Kwakiutl Tales (translations)	230 pp.
1943 Kwakiutl Tales (texts for 1935)	228 pp.
Total	<u>2,035 pp.</u>

To which we may add:

1920 Spanish Tales from Laguna	17 pp.
1926 Ten Folk-tales in Modern Nahuatl	26 pp.
1928 <i>Keresan Texts</i> (translation)	300 pp.
Total	<u>343 pp.</u>
Grand Total	2,378 pp.

Boas' general ethnographic works from the Northwest Coast deal with the Kwakiutl Indians; some of them are recorded in both text and translation.

General

1897 The Social Organization and Secret Societies of the Kwakiutl Indians	428 pp.
1909 The Kwakiutl of Vancouver Island (some texts)	222 pp.
1921 Ethnology of the Kwakiutl, Pts. 1 and 2 (texts and translations)	1418 pp.
1925 Contributions to the Ethnology of the Kwakiutl (texts and translations)	357 pp.
Total	<u>2,425 pp.</u>

What do these general works deal with? In order to give a partial answer to this question, we reproduce their respective tables of contents below, with the number of pages devoted to each chapter or subject.

The Social Organization and Secret Societies of the Kwakiutl Indians

Chapter	Number of pages
I. The Indian Tribes of the North Pacific Coast	12
II. The Social Organization of the Kwakiutl	14
III. The Potlatch	18
IV. Marriage	9
V. Clan legends	28
VI. The spirits presiding over the religious ceremonials and their gifts	26

VII. The organization of the tribe during the seasons of the winter ceremonial	14
VIII. The dances and songs of the winter ceremonial	70
IX. The winter ceremony of the Kwakiutl	45
X. The winter ceremonial at Fort Rupert, 1895-96	63
XI. Ceremonials of other tribes of Kwakiutl lineage	16
XII. The Laolaxa	12
XIII. The religious ceremonials of other tribes of the North Pacific Coast	29
XIV. The growth of the secret societies	6
Appendix: Songs and texts	70

These chapter headings do not always indicate faithfully the contents of the chapters. For example, after a brief introductory paragraph in the chapter on clan legends, Boas says: "It seems desirable to introduce at this place a fuller description of the plan of the house than has heretofore been given. The houses of the Kwakiutl . ." and he goes on for more than four pages describing the houses before saying: "I proceed now to a discussion of the clan legends" (p. 371). The emphasis upon ceremonialism rather than upon social, economic, and political organization is, of course, obvious.

The Kwakiutl of Vancouver Island, 1909

"The title of this book is somewhat misleading," observes M. Mauss in his review in *L'Année Sociologique*. "It is not a new monograph on the Kwakiutl, which however would have been very desirable in order to place the former theories of M. Boas in harmony with the new documents which he has himself published. We have here merely a fragment of a study of their material civilization, and, more particularly, of their industrial and esthetic arts" (Mauss, 1909, p. 857, my translation. *The Social Organization of the Kwakiutl Indians* was reviewed by Emile Durkheim in *L'Année Sociologique*, 3:336-40, 1898-99; *Ethnology of the Kwakiutl* was reviewed by M. Mauss, *ibid.*, 1:417-19, 590-91, 1923-24).

Here is the Table of Contents of *The Kwakiutl of Vancouver Island* (abridged):

I. Introduction, p. 307
II. Industries, p. 310
Work in stone, p. 310
Flaking
Pecking
Perforating
Cutting and grinding
Making of stone hammers
Types of hammers, p. 314
Tools for woodwork, p. 319

- Woodwork, p. 327
- Examples of woodwork, p. 341
- Preparation of fibres, p. 369
- Basketry, p. 382
- Weaving, p. 395
- Netting, p. 399
- Preparation of skins, p. 400
- Painting, Decorating, Dyeing, p. 402
- Objects made of shell, p. 405
- Kelp bottles, p. 405
- Fire-making, p. 407
- III. Measurements
 - Measurement of Space, p. 410
 - Measurement of Time, p. 412
- IV. House and House-Furnishings, p. 414
 - Houses, p. 414
 - Household Utensils and Furnishings, p. 417
- V. Meals, p. 427
- VI. Travel and Transportation, p. 444
- VII. Fishing, and Hunting Sea-Mammals, p. 461
- IX. Hunting Land-Mammals and Birds, pp. 507-516
 - Plates XXXVII-LII and their explanations, pp. 517-522.

I have omitted all sub-subheadings except those under "Work in stone"; they are numerous and detailed.

This work consists almost exclusively of detailed descriptions of the manufacture and use of tools, and of various techniques employed in arts and crafts:

"The Kwakiutl tribes . . . used for their stone implements hard pebbles of dolerite and similar rocks (*tsEquls*), which are found . . . on the east end of Malcolm Island. The hardest and toughest pebbles that can be found, and that have a handy form—somewhat cylindrical, and tapering to a rounded point—are used for battering down the pebbles that are to be shaped. The battering-pebble is held in the hand loosely, and the battering-strokes are delivered slowly. Thus small fragments are removed, and this process is continued until the object has assumed the desired shape. Then the details are worked out with a smaller and lighter battering-stone, with which quite light strokes are given . . ." and so on (p. 310).

If one wished to make a Kwakiutl canoe he could do so by following the detailed description of materials and processes of manufacture set down on pp. 376-378. The text is illustrated with 173 figures and 52 plates; some of the latter are photographs of Indians, villages, etc.; others are of masks—some exceptionally beautiful ones in color—or other paraphernalia. About 80 pages are devoted to texts and translations, in vertical columns on a page.

The latter portion of *The Kwakiutl of Vancouver Island* deals with detailed processes of preparing and serving meals, and with rules of etiquette that are observed, especially when guests are present. Customs of eating are described:

"In eating with a spoon, the people squat down, the right elbow resting on the right knee [the position is illustrated by a photograph, Pl. XXXI]. The food is taken up on the point of the spoon, and is sipped. After eating, most people dip the point of the spoon in water that is passed about. Then the spoon cannot be used for purposes of witchcraft.

"While eating, the left hand is kept under the blanket. It is considered improper to eat with both hands.

"Noblemen and particularly girls of noble descent, must not eat much. When eating, they hardly open the mouth. They use pointed spoons, from which they sip. They must not show their teeth when chewing. Girls, while eating, must not look into the fire and avoid looking about in the house" (p. 427).

Bathing: "Young people generally bathe in fresh water, and only old people bathe in the sea. Young people should bathe every morning before breakfast, on account of their prospect of coming into contact with the supernatural world. Bathing removes the human smell; and when they are fresh and clean, the spirits will come to them and warn them of approaching danger. Ceremonial purification generally requires washing in ponds and rubbing the body with hemlock-branches until blood is drawn" (p. 456).

The Ethnology of the Kwakiutl, 1921

Chapter	Number of pages
I. Industries	116
II. Hunting, fishing, and food-gathering	50
III. Preservation of food	82
IV. Recipes	297
V. Beliefs and customs	147
VI. Social customs	45
VII. Social divisions	41
VIII. Family histories (texts and translations)	442
IX. Songs	38
X. Addenda (odds and ends)	72
XI. Vocabularies	78
XII. Critical remarks	8
Total	1,416

This is a fascinating volume. All the material is presented both in text, in the Kwakiutl language, and in English translation. Fifty items are treated in

Chapter I, "Industries." Here is a description of the way to make a "Dish for pounding Salal-Berries":

"The husband of the woman first goes to get a good piece of cedar-wood without knots, three spans long and four spans wide and one short span high. He takes his ax and chops out the inside, until it is hollow and like a box. When it gets thin, he takes his hand-adz, turns it bottom-side up, and adzes it over finely at the bottom and the ends, so that it does not slant; and after he has finished the outer side, he puts it bottom downward and he adzes it inside, so that there are no lumps. After he has finished this, he takes his straight

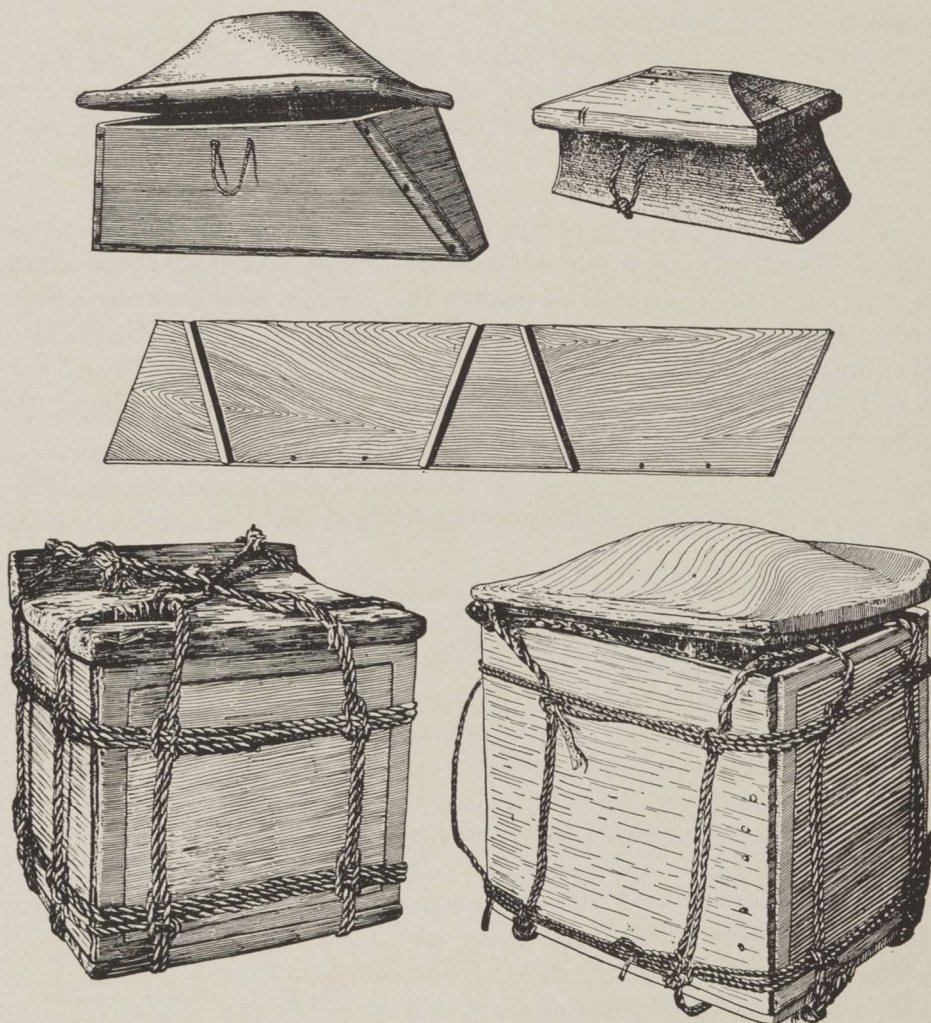


FIG. 7. Kwakiutl kerfed and bent wooden boxes. The "hunters' boxes" are tapered from top to bottom to permit storage in bow or stern of canoes. Ordinary box boards were cut straight. *Courtesy of the American Museum of Natural History.*

knife and his bent knife, and he cuts all around the corners with the straight knife, around the inside of what he is working at; and after he has done so, he takes his crooked knife and shaves out the inside until it is very smooth. This is the box for pounding salal-berries, and it is just like a box after it is finished. Now the box for pounding salal-berries is finished, for it is called that way" (pp. 59-60).

Almost 300 pages are devoted to 155 recipes for preparing and cooking



FIG. 8. Kwakiutl wooden dishes. *Courtesy of the American Museum of Natural History.*

various foods; there are 30 recipes for cooking salmon, alone. The chapters on "Beliefs and Customs" and "Social Customs" are a hodge-podge of all sorts of "social life and customs," ranging from ways of disposing of the navel-string of a baby boy, or prayers to young cedar trees, to huckleberry feasts and swear words. Much information is given about social ritual and etiquette. A recipe, for example, may tell us not only how the food is cooked, but how it is served to guests and the etiquette with which it is eaten. The section on "Social position and marriage laws" (12 pp.) deals with daughters of chiefs, chieftainesses, endogamy and exogamy, names and crests, etc. The following item, "Distribution of Seal," throws a spotlight on an important aspect of social life:

"The hair-seal also teaches the common people their place; for chiefs receive the chest, and the chiefs next in rank receive the limbs. They only give pieces of the body of the seal to common people of the tribes, and they give the tail of the seal to people lowest in rank. Therefore trouble often follows a seal-feast and a feast of short and long cinquefoil-roots; for when a man who gives a seal-feast with many seals hates another man, he gives him a piece of blubber from the body, although he may be of noble descent; and they do the same with the short cinquefoil-roots. That is all about this" (pp. 750-51).

Contributions to the Ethnology of the Kwakiutl (1925) is, in effect, a continuation of *Ethnology of the Kwakiutl* (1921). Its contents are: Dreams, 54 pp.; Rank, property, and inheritance, 56 pp.; The acquisition of names, 246 pp.

One might suppose that *The Religion of the Kwakiutl Indians* (2 vols., 1930), which we have listed above among Texts, is a general treatise on the religion of the Kwakiutl, but such is not the case. Part I consists of texts recorded by Boas; Part II, of translations. But, here again, the material consists of odds and ends pertaining to supernaturalism: mythological concepts, shamanism, prayers—some 40 prayers are recorded—to the sunrise, of a woman after the death of her sister, to bears, fish, berries, etc.—medicines (for diarrhoea, for swelling of breasts, burns, etc.)

Everyone who reviews Boas' voluminous literary output is much impressed with it. "The massive achievement of Boas in the field of descriptive ethnography," says Herskovits (1953, p. 62), "would alone be sufficient to give him an outstanding place in his science." The magnitude of his literary output is, indeed, impressive. But one should not overlook the fact—as some have done—that a very considerable portion of it was the work of other men: the material for several of his major ethnographic works was collected and recorded by collaborators.

In the preface to *The Social Organization and Secret Societies of the Kwakiutl Indians* Boas says: "the great body of facts presented here were observed and recorded by Mr. George Hunt. . . . I am indebted to him also for

explanations of ceremonials witnessed by myself . . . and for finding the Indians who were able to give explanations on certain points" (p. 315). The *Kwakiutl Texts* (1905) "were recorded by Mr. George Hunt . . . between the years 1895 and 1900" (Boas, 1905b, p. 3). *Kwakiutl Texts*, second series (1906), also, "were recorded by Mr. George Hunt between the years 1900 and 1903" (Boas, 1906a, p. 2) In the preface to *Ethnology of the Kwakiutl* (p. 45), Boas states that Mr. Hunt "recorded data relating to the material culture, social life, customs, and beliefs of the Kwakiutl Indians. So far as accuracy and contents are concerned, he is responsible for the material contained in this book." The material for *Contributions to the Ethnology of the Kwakiutl*, texts and translations, "was obtained from Mr. George Hunt" (Boas, 1925b, p. v). Boas' Preface to *The Religion of the Kwakiutl Indians* is not wholly clear to me, but I conclude from his lengthy discussion of Hunt's work that he (Hunt) collected the material and probably made the translations as he had in previous works. Finally, Hunt contributed the Nootka myths published in *Tsimshian Mythology*.

George Hunt's father was a Scotchman, his mother a Tlingit Indian. He



FIG. 9. George Hunt and his wife, date unknown. Courtesy of the Provincial Museum of Natural History, Victoria, B.C.



FIG. 10. Kwakiutl woman with goat horn spoon at mouth and rocking cradle. *Courtesy of the American Museum of Natural History.*

grew up "at Fort Rupert among the Kwakiutl, and their language was his mother tongue." He took a "deep interest in everything pertaining to the ethnology of the Kwakiutl Indians." During the World Columbian Exposition in Chicago in 1893, Boas, who had charge of anthropological exhibits, began to teach Hunt, who was "in charge of a number of Kwakiutl Indians" at the fair, "to write Kwakiutl, his mother tongue." And in the same year (1893) Hunt "began to record material in the Kwakiutl language." "On repeated visits to British Columbia," writes Boas, "I had an opportunity to critically discuss with him the material he had written, which resulted in constant improvement in his method of writing and in my knowledge of the phonetics of the language. . . . The records made by Mr. Hunt were transmitted to me from time to time. After I had made a study of the texts I revised them critically, with the assistance of Mr. Hunt, in regard to both phonetics and grammar. This work was done during my visits to British Columbia in 1897 and 1900" (the preceding data are from Boas, 1897b, p. 315; 1905b, p. 3; 1909b, p. 308; 1930a, p. IX).

Hunt “spent several weeks in New York in 1901” in collaboration with Boas (Boas, 1921a, p. 45). Again in 1903, “Mr. Hunt visited New York to arrange the collections made by him and by myself for the American Museum of Natural History, and I [Boas] was able to improve his own writing and my own hearing” (Boas, 1930a, Pt. I, p. X).

In reviewing *Kwakiutl Texts*, second series, in *Science* (Boas, 1908, p. 178), Boas explained why “the bulk of this work was intrusted to Mr. Hunt.” It was “due to the fact that the Kwakiutl mythology is enormously extensive, and must be obtained from representatives of all the different families to whom the family traditions belong. The writer of these lines [Boas], who is responsible for the collection, could not undertake this work himself, and for this reason he taught Mr. Hunt to write Kwakiutl, and, by carefully controlling his work, trust-worthy material has been gathered.”

We summarize Hunt’s contribution to Boas’ publications on the Kwakiutl:

1897	The Social Organization and Secret Societies of the Kwakiutl	428 pp.
1905	Kwakiutl Texts	528 pp.
1906	Kwakiutl Texts, second series	267 pp.
1916	Nootka myths in Tsimshian Mythology	48 pp.
1921	Ethnology of the Kwakiutl	1,410 pp.
1925	Contributions to the Ethnology of the Kwakiutl	357 pp.
1930	The Religion of the Kwakiutl, Pts. I and II	572 pp.
	Total	3,610 pp.

Henry W. Tate, also, assisted Boas in his ethnographic work. Tate was a full-blood Indian who lived in Port Simpson, British Columbia; his native tongue was Tsimshian. Texts and interlinear translation of *Tsimshian Texts* “were written down by Mr. Henry W. Tate . . . in Tsimshian,” Boas wrote in the preface (p. 67), adding that “this material was revised by me.” The myths comprising *Tsimshian Mythology* were recorded during a twelve-year period by Tate in the Tsimshian language (Boas, 1916, p. 31). The translations were made by Boas, “based on a free interlinear rendering by Mr. Tate” (ibid.). Tate’s contribution to *Tsimshian Mythology* covered 335 pages, and that to *Tsimshian Texts*, 220 pages, making a total of 555 pages.

Thus, the contributions of Hunt and Tate to Boas’ monographic output amounted to 4,165 pages. We have no intention or purpose to minimize Boas’ output or contribution by any means. On the contrary, we readily recognize that the labor of supervising, revising, editing, proof-reading, and publishing the material recorded by them in itself involved enormous labor and was a signal achievement—perhaps the most significant one that can be credited to Boas; we shall return to this point later, however. Our purpose is merely to call attention to, and make explicit, the contribution that these Indians made

to Boas' field work and publications. Boas himself gave them full credit, but most commentators have tended to ignore their assistance.

PART II: ETHNOLOGY

Boas, "the faithful recorder was, above all, a thinker" (Lowie, 1947, p. 316).

We have reviewed the data collecting and recording labors of Franz Boas. We now turn to his interpretation of the material gathered. What was Boas' conception of his data? What was his "philosophy" of ethnology? How did he define his goals of interpretation? And, finally, what were his achievements?

Much has been made of the fact that when Boas was a university student he applied himself to physics. It has been claimed, again and again, that it was early training in an "exact" science that equipped Boas to introduce "rigorous scientific method" into anthropology. "All his life [he, Boas] thought like a physicist," says Kroeber (1943, p. 22; see also pp. 6, 7, 25; and Kroeber, 1935, pp. 539, 540, 541, 544, 545). Goldenweiser (1933, p. 153), Radin (1933, pp. 9, 60), and Spier (1943, pp. 109, 116) make similar statements. Among Boas' students, Lowie has stood apart notably on this point (Lowie, 1940, pp. 598-99; 1944a, pp. 59-60).

The plain fact is, however, that Boas expressly disavowed the goals and methods of interpretation of physics (as he defined them), and espoused a philosophy diametrically opposed to that of physics before he became an anthropologist. We have already seen that, as a student, Boas "followed Theobald Fischer from physics into geography" (Kroeber, 1956, p. 158). We have seen, also, that as early as 1882 he considered himself a geographer (Boas, 1882). In his significant and revealing article, "The Study of Geography," published in February, 1887—almost immediately after his first trip to the Northwest Coast—Boas contrasts the philosophy and methods of physics with those of cosmography and geography (a part of cosmography), and aligns himself with the latter. "... single facts become less important to him [the physicist], as he lays stress on the general law alone. On the other hand, the facts are the object which is of importance and interest to the historian . . . Cosmography . . . considers every phenomenon as worthy of being studied for its own sake" (p. 138). "Losing sight of the single facts, he [the naturalist] sees only the beautiful order of the world. The cosmographer, on the other hand, holds to the phenomenon which is the object of his study . . . and lovingly tries to penetrate into its secrets until every feature is plain and clear" (p. 140). Forty year later, Boas confirmed his earlier rejection of the method and goals of physics in favor of those of geography (Boas, 1936, p. 137). And "when from geography my interest was directed to ethnology," he wrote, "the same interest prevailed" (*ibid.*).

Boas' interest was, therefore, focused upon the particular, rather than the general. "In ethnology all is individuality," he said (Boas, 1887e, p. 589). "The general impression" that one gets of the culture of the Northwest Coast tribes, said Boas (1888a, p. 53) after his first field trip, "is that it is uniform; but the traveller finds many customs peculiar to one tribe, and not practiced by another." The more one looked the more different the cultures became; one no longer saw forests but only trees—or perhaps just branches and twigs. Boas' "first impression" of the Kwakiutl "was that of bewildering confusion" (Boas, 1909b, p. 307). Let us see, now, how he coped with these varied cultures as he found them in the field.

Boas had very definite ideas about the purpose and goals of ethnology by the time he had completed his first field trip to the Northwest Coast—if not before. His views are best set forth in an exchange of letters with O. T. Mason (principally) in *Science*, Vol. 9, 1887, and in a lecture, "The Aims of Ethnology," delivered in New York in 1888 (published in *Race, Language, and Culture*, 1940). "My view of the study of ethnology is this," he wrote in *Science* (vol. 9, p. 588):

"The object of our science is to understand the phenomena called ethnological and anthropological, in the widest sense of the words,—in their historical development and geographical distribution, and in their physiological and psychological foundation. These two branches are opposed to [i.e., distinguished from] each other in the same way as are biology and the so-called systematic 'organology,' or as I have called it in another place (*Science*, ix, No. 210), when treating of the study of geography, 'physical science and cosmography,' the former trying to deduce laws from phenomena, the latter having for its aim a description and explanation of phenomena. I tried to show that both branches are of equal scientific value" (Boas, 1887e).

In "The Aims of Ethnology" (1888h), he says:

"... the first aim of ethnological inquiry must be critical analysis of the characteristics of each people. This is the only way of attaining a satisfactory understanding of the cultures found in wider areas. The means at our disposal for making such an analysis are varied: bodily form, language and culture are results of historical processes and may, therefore, be utilized for the study of history" (p. 629). "... ethnology deals with the history of primitive peoples. ... The history of mankind is to be reconstructed by investigations of bodily form, languages, and customs" (p. 638).

The second task of ethnology, according to Boas (1888h) is the discovery of laws:

"A comparison of the social life of different peoples proves that the foundations of their cultural development are remarkably uniform. It follows from this that there are laws to which this development is subject. Their discovery is the second,

perhaps the more important aim of our science" (p. 634). "The frequent occurrence of similar phenomena in cultural areas that have no historical contacts suggests that important results may be derived from their study, for it shows that the human mind develops everywhere according to the same laws. The discovery of these is the greatest aim of our science" (p. 637).

Thus, as Boas saw it, the first task of ethnology was observation—the "critical analysis of the characteristics of each people"—then reconstruction of the culture history of peoples and areas, and finally the "discovery" [formulation] of laws of cultural development. Boas quickly and industriously set about to make observations in the Northwest Coast, which he reported yearly (with some exceptions) in his reports to the Committee of the British Association for the Advancement of Science, appointed to study the North-Western Tribes. He observed "bodily form, languages, and customs." And very quickly, also, he began to reconstruct culture history. But here there is some confusion among the commentators on Boas' ethnology.

According to Herskovits (1953, p. 59), "Boas thus approached the question of the historic contact between nonliterate societies, the task of reconstructing history which again and again he emphasized was the basic problem of anthropology . . ." Radin (1933, p. 17), however, asserts that "Boas' method is fundamentally unhistorical." Kroeber (1935, p. 541), too, says that "Boas has never really followed the historical method except in a rather narrow, special sense"; also, "Boas's work also is now recognized as mainly lacking specific historic content or result . . ." (Kroeber, 1946, p. 8). Goldenweiser objects strongly to the views of Radin and Kroeber: "As Boas' student for many years and his academic associate for as many more, I want to express my demurrer to this thoroughly erroneous interpretation" (1941, p. 156). Spier (1959, pp. 147–48) has tried to show what Boas meant by 'historical': "Boas meant only that each cultural trait and configuration must have had a specific antecedent form. This did not involve the need to provide a sweeping picture-in-time. . . . It sufficed for his purpose to envisage a 'before and after' picture at a particular time and place." We believe that Spier's interpretation is sound and just—but let us turn to the writings of Boas himself.

At the very outset of his investigations on the Northwest Coast Boas focused his attention upon this question: What was the origin of these cultures and how did they develop? And most of his researches were directed toward the solution of this problem. "For a long time," he wrote in 1888, "the remarkable culture of the Indians of Northwest America has attracted the attention of ethnologists; but, so far, no progress has been made in solving the difficult problem of the origin of this culture" (Boas, 1888a, p. 47). "The customs which we observe today are evidently the modern development of ancient forms" (Boas, 1899, p. 678).

"The culture of the Northwest American tribes, which to the superficial

observer seems so uniform, originates from many different sources, and that only a thorough knowledge of the languages, folklore, and customs of these tribes and their neighbors will enable us to trace at least a part of their obscure history" (Boas, 1887c, p. 428). "The general impression is that it ["the culture of the coast tribes"] is uniform but the traveller finds many customs peculiar to one tribe, and not practiced by another. These slight variations are one of the best clues for historical investigation. Among the Kwakiutl, for instance, we find a very elaborate system of secret societies, of which only faint traces exist among the Coast Salish and among the Tlingit. Therefore we must suppose that the general culture can be traced back to various sources" (Boas, 1888a, p. 53).

"... philological researches will prove a very powerful means of solving the questions regarding the history of the Northwest American tribes. . . . In the study of the evolution of the culture of these tribes, the question, what originally belongs to each tribe, and what has been borrowed from foreign sources, must constantly be borne in mind. But one of the fundamental questions to be answered before any definite results can be obtained is: what tribe and peoples have been influenced or have exerted an influence upon Northwest American culture? The answer to this question will define the area of our studies" (Boas, 1888a, p. 51).

And, finally, Boas states that the aim of the Jesup North Pacific Expedition "is the investigation of the history of man in a well-defined area. . . . The expedition has for its object the investigation of the tribes, present and past, of the coasts of the North Pacific Ocean" (Boas, 1898b, p. 4). "What relations these tribes bear to each other, and particularly what influence the inhabitants of one continent may have exerted on those of the other, are problems of great magnitude. Their solution must be attempted by a careful study of the natives of the coast, past and present, with a view of discovering as much of their history as may be possible" (ibid., p. 6).

Thus, the task Boas set himself was, in effect, the reconstruction of the culture history of the Northwest Coast. This was to be accomplished by determining how the various tribes had "influenced" each other; how one had acquired a custom or trait from another. And this, in turn, was to be achieved by "patient, careful and detailed studies of physical form, languages, folk-lore and customs of the tribes in the region." "The method of detailed comparison of contiguous tribes will reveal the effects of intermixture, linguistic borrowing, and exchange of cultural forms. By following out patiently and in detail the lines of interchange of culture, it is possible to trace the historical development of the tribes inhabiting a definite region" (Boas, 1905a, p. 91).

As early as 1888 Boas published a preliminary statement on "The Development of the Culture of North-West America" in *Science* (Vol. 12, pp. 194-96). This article exemplifies his method: he comments upon similarities and



FIG. 11. Kwakiutl masks, Vancouver Island, B.C. *Courtesy of the American Museum of Natural History.*

differences among the tribes, speculates upon the influence of one upon another, draws inferences about culture history, and makes some conclusions with regard to the course of events. And during the next decade or so, his writings are full of historical reconstructions: "This custom [a cannibal ritual] is principally practiced among all tribes of Kwakiutl lineage; but it is also found among the Bilqula and Komoks, who have evidently adopted it from the Kwakiutl" (Boas, 1887c, p. 426). Incidentally, Boas was inclined to believe that the Kwakiutl was exceptionally influential in North-West Coast culture history; this was the tribe to which he devoted particular attention and persevering labor for many years. The Tsimshian obtained certain dances from the Heiltsuk, "the Haida adopted them from the Tsimshian. In all these dances ornaments of cedar bark, dyed red, are used, and it appears that this custom also originated among the Kwakiutl" (Boas, 1889a, p. 240). "The legends of the Tsimshian favor the theory that they reached the coast much later than the other tribes. The Nutka, finally, are so much influenced by the Kwakiutl that a study of their customs does not reveal any facts as to their origin" (Boas, 1888e, p. 196). "It is evident that the culture of the Bilqula is very greatly influenced by that of the Kwakiutl . . ." (Boas, 1892a, p. 424).

Occasionally Boas worked out long and involved processes of cultural development—or history; Boas tended to use these terms interchangeably in certain contexts. "Summing up the preceding considerations, we may say that the Kwakiutl consisted in olden times of a series of village communities among which descent was counted in the paternal line, and the members of each community were considered descendants of one ancestor. These communities combined in groups, but the composing elements of the groups kept a certain degree of independence and continued to be considered as relatives. Each clan, as we may call the composing elements of the tribe, developed a clan tradition, which was founded upon the acquisition of a manitou by the mythical ancestor, the manitou becoming hereditary in the clan. Owing to the influence of the northern tribes, this manitou became attenuated to a crest, which, in consequence of the same influence, no longer descends in the male line, but may be given in marriage, so that it descends upon the daughter's children" (Boas, 1897b, pp. 337–38).

Also: "The traditions of the Bella Coola are, to a great extent, totemic. Only members of the clan have the right to relate their traditions and to use the carvings based on their traditions. This has led to a system of endogamic marriage which was intended to prevent the acquisition of clan rights by other clans. This system is breaking down under the influence of the Kwakiutl system of exogamic marriage" (Boas, 1897c, p. 537).

How does one determine that tribe A borrowed trait x from tribe B? One observes that both A and B possess trait x. But how does one tell that A bor-

rowed it from B rather than the reverse? Or, perhaps both A and B got the trait from tribe D. How does one determine the direction in which influence is exerted? Does tribe A influence B or does B influence A, or does each influence the other?

The difficulties of historical reconstruction did not appear to be great to Boas. He engaged in this kind of interpretation without hesitation and with the air of great confidence from the very start of his field work on the Northwest Coast. One can actually *see* culture history, that is, if one were well-trained and looked hard enough. It would appear that Boas simply assumed this from the following: "the study of the present surroundings [of a tribe] is insufficient [to explain its culture]: the history of the people, the influence of the regions through which it passed on its migrations, and the people with whom it came into contact, must be considered. *All of these are phenomena which may directly be observed by a well-trained observer*, or may be traced with greater or less accuracy by historical researches" (Boas, 1887e, p. 588; emphasis ours).

Boas was interested in psychological processes as well as historical processes in socio-cultural situations: "During these years, from 1886 to 1892, the information that was accumulating seemed to show that under the present conditions the Kwakiutl and Nootka offered the most promising fields of research, partly because they were less affected by the whites than other tribes, partly because they exhibited peculiar transitional stages, in which newly acquired customs appeared to have assumed novel significance—a condition favorable to the study of the psychological and historical processes which are characteristic of the cultural development of comparatively primitive tribes" (Boas, 1909b, p. 308).

Historical researches would provide one with the "history" of the development; psychological investigations would yield the "laws" of cultural development. It is interesting to note that Goldenweiser, one of Boas' early students, entitled his anthology *History, Psychology and Culture* (1933).

We find, therefore, Boas resorting to psychological, as well as historical, explanations of socio-cultural phenomena on the Northwest Coast. And he found psychological explanations quite as easy to make as historical reconstructions:

"One of the most remarkable features in the inner life of the tribes of the northern coast of British Columbia is the great importance of the clan legend, which is considered one of the most valuable properties of each clan or family. It is carefully guarded in the same way as material property, and an attempt on the part of a person not a member of the clan to tell the tradition as his own is considered one of the gravest offenses against property rights. The possession of a clan tradition is felt by the Indians to be one of his most important prerogatives. When, therefore, the Bella Coola settled on the Bella Coola river, and were thrown into contact with

the northern Coast tribes, the lack of a well-developed clan tradition must have been felt as a serious drawback. . . . The possession of clan traditions was felt as a great advantage, and consequently the desire developed to possess clan traditions. . . . The desire to guard the traditions which were once acquired led to the development of endogamic institutions, in order to prevent the spread of the traditions over the whole tribe" (Boas, 1898a, pp. 123, 125).

The validity of historical reconstructions. Almost everyone who has written about Boas has characterized him as "rigorous," "critical." Boas "insisted on discriminating between *absolutely established* fact and plausible conjecture" (Lowie, 1944a, p. 60; emphasis ours). Kroeber (1952, p. 146) tells us that Boas' "unsparing mind exacted proof even in the complex and difficult situations which prevail in culture, and he *refused to deal with problems in which strict proof* seemed impossible" (emphasis ours). Bunzel (1960, p. 403), Spier (1959, p. 154) and Kluckhohn and Prufer (1959, p. 21) say essentially the same thing. Let us see, then, how Boas arrived at "absolutely established fact," "strict proof."

Sometimes Boas was moderate and tentative. In his summary of his field work on the Northwest Coast, prepared for the Twelfth and Final Report of the Committee of the British Association for the Advancement of Science (1899), Boas wrote: "this decoration is so uniform that the explanation given here seems to be very probable" (p. 679); "I am not quite certain if the decoration of armour and weapons is totemistic or symbolic" (p. 680); and, "it is not possible to prove definitely that the secret societies developed in this manner from customs related to war expeditions, but the close relationship of the two cannot be doubted" (p. 678).

But as a rule he is quite positive: "only one explanation of this fact is possible . . ." (Boas, 1899, p. 672); ". . . linguistic evidence proves that the Bella Coola and the Coast Salish at one time inhabited contiguous areas on the coast . . ." (Boas, 1898a, p. 123); "I have found the Kwakiutl names used by the Nutka, Salish, Tsimshian, and Haida. This fact seems to indicate that these legends and customs have spread at a comparatively recent date over the coast, and it is a proof that they originated among the Kwakiutl" (Boas, 1888e, p. 195). Certain words (names) are used both among the Hopi and the Keres (Sia) in the Snake ceremony (White, 1962). I am inclined to believe that they are Keresan words (as was Fewkes)—perhaps because I have worked principally among the Keres as Boas worked chiefly with the Kwakiutl. But I cannot prove that they *are* Keresan words. And even if I could, would this prove that the Hopi borrowed the Snake ceremony from the Keres, or that Sia (Keres) borrowed it from the Hopi? We are reminded of a flat assertion by Elsie Clews Parsons, a disciple of Boas: "That Acoma got its clan system from the Hopi is proved [*sic*] by the fact that the paternal aunt gives

her godchild one of her clan's stock of personal names, a naming practice peculiar to the Hopi" (Parsons, 1936, p. 559).

In one instance, Boas arrives at finality and proof because he cannot imagine any other explanation for a situation:

"... it seems to my mind that this exceedingly intricate law ... can not be explained in any other way than as an adaptation of maternal laws by a tribe which was on a paternal stage. I can not imagine that it is a transition of a maternal society to a paternal society, because there are no relics [Tylor's "survivals"] of the former stage ..." (Boas, 1897b, pp. 334-35).

And before he has finished with this point he comes to "conclusive proof":

"The fact that they [i.e., certain traditions] invariably and always are explained by genealogies, such as the above, seems to my mind conclusive proof that a paternal organization of the tribe preceded the present one" (*ibid.*, p. 335).

Perhaps it was Boas' habit of presenting his inferences and intuitions as "proofs," and his aggressiveness of expression that have led Kroeber, Lowie, Spier, Bunzel, and others to the conclusion that he insisted on "absolutely established fact" and "strict proof." In "The Growth of Indian Mythologies," for example, he writes as follows: "the phenomena ... can be explained only by the theory ..." (p. 2); "only one explanation of this fact is possible" (p. 3); "the very complexity ... cannot possibly be explained by any other method" (p. 6); "these are facts that cannot be disputed" (p. 9); "I take clearly and expressly issue with the view of those modern anthropologists who ..." (pp. 9-10); "But I insist ..." (p. 10); "This is the method which I insist is necessary ..." (p. 11).

How did Boas arrive at his conclusions—his "absolutely established facts," his "strict proofs"? He observed the incidence and distribution of customs, myths, masks, etc., and then he simply decided that tribe A had influenced tribe B, or had borrowed trait x from tribe C. It "seemed to his mind" that these were reasonable assumptions. Perhaps they were, but this does not constitute proof; other assumptions equally reasonable might have been made. Where is the technique of verification? He "concludes," on the basis of mythologic and meager archaeological evidence, that the Kwakiutl clan "was originally a village community" (Boas, 1897, p. 334). He relies upon "recent tradition, the historical truth of which cannot well be doubted. ..." (Boas, 1920b, pp. 111-112). "It remains to substantiate what I have said [about a transition in Kwakiutl social evolution from a paternal stage to a maternal stage] by telling the legends of a few clans" (Boas, 1897b, p. 335). "According to their own [the Tsimshian] statements they [certain dances] were obtained by intermarriage with the Heiltsuk" (Boas, 1889a, p. 240). "The Kwakiutl state that this custom was introduced among them no longer than

sixty years ago, and that it originated [*sic*] among the Heiltsuk" (1899, p. 678). Well, if the Kwakiutl said so . . .

But the most remarkable bit of historical reconstruction indulged in by Boas is the following: "The physical appearance of the Bella Coola proves that at one time they must have intermarried to a great extent with the Bella Bella. Through these marriages the peculiar customs of the Coast tribes were first introduced among them" (Boas, 1898a, p. 123). If this means what it says, it borders on the incredible: one goes about among the Bella Coola and the Bella Bella tribes, taking note of their physical appearance. This *proves* that "at one time" they "must have intermarried to a great extent," and as a consequence of this certain customs were borrowed *by* one tribe *from* another (why not the other way around?). Lowie's remark about "Boas's insistence on definite proof of cultural diffusion" (Lowie, 1947, p. 304) is a loyal tribute to his "revered teacher," as Lowie (1944b, p. 324) has called him. But it falls somewhat short of biographical accuracy.

Mythology and Folk-Lore

Boas was tremendously interested in mythology and folk-lore as is evidenced by his extensive collections of materials and his numerous articles on the subject. "It is one of the most interesting problems of ethnology to study the development of a system of mythology," he wrote almost at the outset of his field researches on the Northwest Coast (Boas, 1889a, p. 238). He was editor of the *Journal of American Folk-lore* from 1908 to 1925.

How are myths and folktales of preliterate peoples to be explained? This question had, of course, concerned many scholars before Boas became an anthropologist, and continued to be a live subject of inquiry during his lifetime.

Some of Boas' articles on mythology are polemic in nature. In "The Growth of Indian Mythologies" (1896), he bristled: "I take clearly and expressly issue with the view of those modern anthropologists who go so far as to say that he who looks for acculturation [diffusion] as a cause of similarity of culture has not grasped the true spirit of anthropology" (pp. 9-10). It may be safely presumed, I believe, that he was referring to Brinton's address as retiring president of the American Association for the Advancement of Science in Springfield, in August-September of 1895, in which he extolled "the psychical unity of man" as the "corner stone of true anthropology," and went on to say that "the comparative mythologist or the folk-lorist of the old school" who failed to appreciate independent origins and development "had not caught up with the progress of ethnologic science" (pp. 4, 10).

Unfortunately, however, Boas does not clearly distinguish the issue involved in the interpretation of mythologies. On the one hand there is the

problem of explaining mythology *in general*. Are myths explanations of natural phenomena, subsequently transformed, as some have held? Are they the result of the transformation of human heroes into gods? Are they the expression of unconscious wishes or frustrated desires? Or are they the stuff of which dreams—or sheer fantasies—are made? On the other hand is the *particular* problem of why *this* tribe has the *specific* myths and tales that it has. There are two kinds of problems: the *general* and the *particular*, and Boas does not distinguish clearly between the two. Instead, he criticizes, or opposes, the attempts to solve the general problem by pointing out that they cannot solve the *particular* problems—which is precisely like the Boasian anti-evolutionist argument that a *general* theory of cultural evolution cannot explain the *histories* of particular peoples or regions (see White, 1957; and 1959b, p. 116).

Of course a general theory of myths cannot tell us why this or that tribe has the particular myths that it has; this is a matter of culture history, not psychology. And Boas is primarily interested in particulars (recall his dictum: “in ethnology everything is individuality”) and therefore in *history*. Psychological theories of the origin and nature of myths in general are not at all in conflict with the techniques of determining why each tribe has the particular myths that it has. Had Boas clearly realized this he would have spared himself some effort and emotion and he would not have confused his students and readers; i.e., led them to believe that the history of particulars is opposed to a psychological explanation of the general, and that history is to be preferred to psychology.

“The teachings of a childlike primeval philosophy ascribing personal life to nature at large, and the early tyranny of speech over the human mind, have thus been two great and, perhaps greatest agents in mythological development,” according to Tylor (1929, I:304), adding, however, that “other causes, too, have been at work.” Tylor went on to say that “so uniform indeed is such development that it becomes possible to treat myth as an organic product of mankind at large, in which individual, national, and even racial distinctions stand subordinate to universal qualities of the human mind” (*ibid.*, pp. 415–16). Here the emphasis is definitely upon the general, the universal.

But emphasis upon myth in general, as the product of the human mind, might easily slip over into something else: each tribe has the particular myths that it does because of the universal character of the human mind—or, to put the matter in the conventional idiom of anthropology, because of the psychic unity of mankind, each tribe’s myths are to be accounted for in terms of universal qualities of the human mind. This would mean that similarities in non-contiguous areas are due to independent development rather than to diffusion (or dissemination, to use Boas’ early term). And, carried to the extreme,

it would mean that similarities of myths and tales in *contiguous* areas would still be due to independent origin rather than to diffusion. "Not more than twenty-five years ago," wrote Boas in 1914 (Boas, 1914, p. 380), "Daniel G. Brinton asserted that the similarity of Iroquois and Algonquian mythologies was due to the sameness of the action of the human mind, not to transmission." Michelson (1929) makes the same assertion. Brinton was, of course, an extreme exponent of the theory of independent development as opposed to diffusion to account for cultural similarities in different areas, contiguous or non-contiguous. But neither Boas nor Michelson cites the reference to such an assertion in Brinton's writings. In *American Hero-Myths* Brinton considers "an absolute identity of mythological conception . . . between the two nations [Iroquois and Algonkin]" which he attributes to "parallel development" rather than to "historical identity." "The impressions which natural occurrences make on minds of equal stages of culture are very much alike," he says. This is especially true of primitive peoples. "This is a simple and reasonable explanation for the remarkable sameness which prevails in the mental products of the lower stages of civilization, and does away with the necessity of supposing a historic derivation one from the other or both from a common stock" (pp. 61-62). *American Hero-Myths* was published in 1882, however, 33 years before Boas' statement.

Boas does not deny that the impact of natural phenomena upon the human mind finds its expression in mythology. "Certainly," he says (1896e, p. 5), "the phenomena of nature are at the bottom of numerous myths, else we should not find sun, moon, clouds, thunder-storm, the sea and land playing so important a part in *all* mythologies" (emphasis mine). And, "there can be no doubt that the impression made by the grandeur of nature upon the mind of primitive man is the *ultimate cause from which these* myths spring" (Boas, 1896e, p. 9; emphasis ours). "What I maintain," he goes on to say, "is only that the *specific myth* cannot be simply interpreted as the result of observation of natural phenomena. Its growth is much too complex" [i.e., derived from many sources] (ibid., p. 5, emphasis mine).

Of course, no one, as far as I know, has ever asserted that a *general* theory of myths could explain why *each tribe* had certain *specific* myths at a particular time—no one, that is, unless perhaps Brinton did. Nevertheless, Boas is moved repeatedly to reject the theory of nature myths: "Therefore they cannot be explained as symbolizing or anthropomorphizing natural phenomena. . . ." (Boas, 1891b, p. 20). And, "we are therefore led to the conclusion that from mythologies in *their present form* it is impossible to derive the conclusion that they are mythological explanations of phenomena of nature *observed by the people to whom the myths belong*, but that many of them, *at the place where we find them now*, never had such a meaning" (Boas, 1896e, p. 5; emphasis mine). Boas would have been willing to grant,

as a previous quotation has made clear, that *originally*—or “ultimately,” to use his own word—a myth may have been inspired by a phenomenon of nature. But, his point is that a tribe might have borrowed a “nature myth” from another tribe, who in turn might have borrowed it from still another, and so on, so that the reason why a particular tribe has a certain myth is not because it originated it to explain a phenomenon of nature, but because it borrowed it.

“. . . I draw the conclusion,” says Boas (1896e, p. 5), that the mythologies *as we find them now* are not organic growths [is he, perhaps, referring to Tylor here? See our previous quotations from Tylor], but have gradually developed and obtained their present form by accretion of foreign material. Much of this material must have been adopted ready-made, and has been adapted and changed according to the genius of the people who borrowed it” (emphasis mine).

The tenor of Boas’ treatment of mythologies is not to distinguish for the reader the two kinds of problems involved—the general and psychological, on the one hand and the particular and historical on the other—and to make it clear to him that one can address himself to the one or the other, as he pleases; that there is no conflict between the two. Instead, Boas leaves the reader with the distinct impression that historical analysis and interpretation are to be preferred over general psychological interpretations:

“For an explanation of myths,” he wrote in 1891, “we need, first of all, a careful study of their component parts, and of their mode of dissemination, which must be followed by a study of the psychology of dissemination and amalgamation. *Only after these have been done* shall we be able to attack the problem of an explanation of myths with the hope of success” (Boas, 1891b, p. 20; emphasis mine).

In “The Growth of Indian Mythologies” (1896e), Boas generalizes from mythologies in particular to culture in general: “In order to understand the growth of the peculiar psychical life of the people, the historical growth of its customs must be investigated closely, and the only method by which the history can be investigated is by means of a detailed comparison of the tribe with its neighbors. This is the method which I insist is necessary in order to make progress towards the better understanding of the development of mankind. . . . In order to investigate the psychical laws of the human mind . . . we must treat the culture of primitive people by strict historical methods. . . .” (p. 11).

Two years later, in his introduction to Teit’s “Traditions of the Thompson Indians of British Columbia,” Boas wrote: “the present character of Indian mythologies can be understood *only* by historical studies regarding their origin” (p. 16; emphasis mine).

Boas was impressed with evidence of diffusion of myths and tales from the very start of his field work in the Northwest Coast: “On the north-west coast

of America this study [‘the development of a system of mythology’] is the more interesting, as we can show how legends migrated from tribe to tribe” (Boas, 1889a, p. 238). A decade later he wrote: “I have tried to show that the material of which they [myths] are built up is of heterogeneous origin, and that much of it is adopted ready-made” (Boas, 1898c, p. 18). And he formulated some principles of diffusion:

1891: “Whenever we find a tale spread over a continuous area, we must assume that it spread over this territory from a single center” (Boas, 1891b, pp. 14–15). Also, “I believe we may safely assume that, whenever a story which consists of the same combination of several elements is found in two regions, we must conclude that its occurrence is due to diffusion. The more complex the story is . . . the more this conclusion will be justified” (ibid., pp. 13–14).

1896: “The identity of a great many tales in geographically contiguous areas has led me to the point of view of assuming that wherever considerable similarity between two tales is found in North America, it is more likely to be due to dissemination than to independent origin” (Boas, 1896e, p. 4).

1914: “Two rules have been laid down as necessary for cautious progress:

“First, the tale or formula the distribution of which is investigated, and is to be explained as due to historical contact, must be so complex that an independent origin of the sequence of non-related elements seems to be improbable. . . .

“The second rule is, that for a satisfactory proof of dissemination, continuous distribution is required. The simpler the tale, the greater must be our insistence on this condition.” (Boas, 1914, p. 381).

Boas’ position seems to be both cautious and reasonable. He was not an extreme diffusionist. “I am, of course, well aware,” he wrote, “that there are many phenomena of social life . . . which we have good cause to believe have developed independently over and over again” (Boas, 1896e, p. 10). “How much [of similarity in different areas] is due to independent thought or to gradual adaptation, under the influence of environment and of new social conditions, remains to be determined by detailed comparative studies” (Boas, 1898c, p. 16); “in many cases, the final decision will be in favor of independent origin; in others in favor of dissemination” (Boas, 1896e, p. 10). And in some instances he is quite unwilling to consider the possibility of diffusion: “It seems to me that the idea of a chain of arrows reaching from the earth to the sky [found in myths in Melanesia and on the Northwest Coast] is not so complicated as to allow us to assume necessarily a single origin. Furthermore, the distance between the two countries in which the element occurs is so great, and there is apparently such a complete absence of intermediate links, that I am not convinced of the sameness of the elements. Even the apparently complicated story of the Invisible Fish-Hook . . . which is common to Melanesia and Northwest America, does not convince me” (Boas, 1914, p. 384).

The Kwakiutl

The principal object of Boas' ethnographic researches was the Kwakiutl Indians of Vancouver Island. His investigations of this tribe embraced a half-century, and his major publications devoted to them exceed 5,000 pages (Codere, 1959, pp. 61-62).

We do not know very much about how Boas conducted himself in the field, what his relationships with the Indians were like, and so on. Was he ever accompanied by his wife and children? This invariably affects profoundly the relationship of the ethnographer to the people he is studying. Did he reside in a household as a "member" of a family? Did he take part in the daily life of the people? For one who "must be understood first of all as a field worker" (Lowie, 1937, p. 131), we know precious little about his life and work in the field.

At the outset of his field work on the North Pacific Coast the Indians feared that he might interfere with their customs (potlatches and winter dancing were then prohibited by law), but he assured them that he had come only to acquaint himself with them and their ways of life. They gave him a feast, and he reciprocated. Laudatory speeches were exchanged. But these were mere formalities. "On one of my later visits," he wrote, "I had received an Indian name, Heiltsakuls, 'the one who says the right thing,'" (Boas, 1896c, p. 232). And this is all we have known, from Boas directly, about his field experiences until his daughter, Mrs. Helene Boas Yampolsky, published, in 1958, "Excerpts from the Letter Diary of Franz Boas on his first field trip to the Northwest Coast."

Some of Boas' principal students, those who were close to him in the early years, have given us their opinions concerning Boas as a field worker, but they are not in agreement on this point. Kroeber (1956, p. 158): "I conclude that Boas liked direct encounter with cultural and linguistic phenomena. . . ." But this might well mean *data* (phenomena) rather than Indians, as he adds: "He got intensive satisfaction out of contact with such phenomena, out of dealing with them—absorbing, analyzing, describing them" (ibid.). Goldenweiser (1941, p. 155) tells us that "In field work Boas found a sort of chastening influence. It took him away from books, from theory, from speculation, from students and lectures." Spier (1943, p. 114) believed that "it is probable that he [Boas] did not wholly enjoy field work, that he disciplined himself to it."

The diary letters published by Mrs. Yampolsky give us a vivid picture of Boas on his first field trip. They complain a great deal about the difficulty of finding good informants, and about their unreliability; he comments frequently about his "much running about" and his consequent fatigue; he is impatient to fill his notebooks, and complains about "time lost"; he is oppressed with the dirt, drunkenness, and various discomforts.

From these letters and other clues, and from the nature of his publications, we would judge that Boas' principal aim, when in the field, was to sit down with a good informant and fill his notebooks—and then go home. There is no indication whatever, as far as my knowledge goes, that he ever thought of himself as a "participant observer," that he ever tried to take part in their daily life and become personally acquainted with the people.

Melville Jacobs (M.A. Columbia University, 1923; Ph.D. 1931) speaks about Boas' "resistance to identification with people. The austere visitor probably mingled politely with the natives, but with some discomfort and always with a feeling of pressure to get the scientific task accomplished. Participant observation was much less possible for him than for most of his students. His temperament would not have permitted him to devise a method of analysis which dwelt upon people's feelings about their myths and tales" (Jacobs, 1959, p. 127).

There is more than a little evidence to indicate that Boas was prudish and puritanical. "Obscenity was too distasteful to permit him to make inquiries about sexual components of stories," says Jacobs (1959, p. 126); "One cannot suppose that he enjoyed slang or primitive music; he felt that both were rather crude." Kroeber (1956, p. 158) speaks of a "puritanism" that was "characteristic" of Boas. "In almost none of Boas' writings or in those of George Hunt is the ribald in Kwakiutl life visible" (Codere, 1959, p. 69); and she cites an Indian informant who quotes Boas' disapproval of "a lot of nearly naked women" that they saw in a theater in New York (*ibid.*). There is no way of estimating the extent to which this trait of Boas operated as a censor of the ethnographic data he collected. He, himself, was aware of this, as he states in the prefaces of some of his volumes of texts and myths and tales.

Perhaps the unpublished material in the archives of the American Philosophical Society will throw much more light upon Boas, the field worker.

"The social organization of the Kwakiutl is very difficult to understand," Boas wrote after some three or four field trips (Boas, 1891a, p. 608). And, indeed, his difficulties were great. We shall review a few simple, but fundamental, points upon which early investigations centered: Did the Kwakiutl have clans (or gentes)? If so, were they exogamous or not? If so, were they matrilineal or patrilineal? These are simple questions, and one would think that definite answers could be found after some field investigation. But this proved not to be the case. Boas had great difficulty with all of them.

After two field trips to the Northwest Coast Boas wrote: "the tribes of the northwest coast of America are all divided into gentes" (Boas, 1889c, p. 202), and for some years Boas discussed the clans, or gentes, of the Kwakiutl. The word clan appears in translations of texts in *The Kwakiutl of Vancouver Island* (1909b, pp. 433, 435). But by 1920, he had abandoned "clan" and "gens" because they were "misleading," and used the Kwakiutl term, *numaym*, instead (Boas, 1902b, p. 115). When "The Growth of Indian Mythologies"



FIG. 12a.



b.



c.



d.

(a) Kwakiutl woman wearing cedar bark cape. (b) Kwakiutl chief holding broken copper, with son. (c) Kwakiutl woman with artificially deformed head, left profile. (d) Same woman as c, full front. *All Courtesy of the American Museum of Natural History.*

(1896) was reprinted in *Race, Language and Culture* (1940), the word clan, which had been used a number of times, was replaced by "division." And, it is interesting to note, two of Boas' early (and most distinguished) students, interpreted the record in opposite ways. Goldenweiser: "Being important in all tribes [on the North-West Coast], the clan reaches its maximum development among the Kwakiutl" (Goldenweiser, 1933, p. 223; written in 1910). Lowie: "The Kwakiutl of Vancouver . . . are without clans" (Lowie, 1948, p. 259). Did the Kwakiutl, then, have clans (or gentes)? Whatever the answer may be, it appears definitely not to be either yes or no.

In the Fifth Report of the Committee of the British Association (1890), Boas reports: "Neither was I able to arrive at a fully satisfactory conclusion regarding the question whether marriages inside a gens of the Kwakiutl are absolutely prohibited, but I believe that such is the case" (p. 828). In the Sixth Report, the following year, he says that "the gentes are not exogamous, but marriages between cousins are forbidden" (p. 610). Before the decade of the 1890s had ended, Boas had decided that the clans are exogamic (Boas, 1897b, p. 334; 1898a, p. 122). I interpret a passage in "The Social Organization of the Kwakiutl," written in 1920, to mean that the groupings in question were not exogamous: "The observations . . . make it clear that among the southern Kwakiutl ['The Southern Kwakiutl are the famous 'Kwakiutl' of the North Pacific Coast,' Codere, 1961, p. 431] . . . the village community is conceived as a closed group and forms the basis of modern social organization. The exogamous lines, which are superimposed upon the village communities and embrace all of them, and which are an essential feature of the social system of the northern tribes, do not occur" (p. 123). And again, two other anthropologists differ diametrically on this point. Goldenweiser says that the Kwakiutl clans "are not exogamous; here, in fact, a woman is advised to marry into her own clan for among her own people she is likely to receive better treatment"—and there is a footnote at this point which reads "personal communication from Boas" (Goldenweiser, 1933, p. 221). Goldman, on the other hand, says "the Kwakiutl numayms are exogamous" (1937, p. 195). Question: were the Kwakiutl clans, or numayms, exogamous or weren't they?

We turn now to the last of our three questions, namely, were the clans (gentes, or numayms) matrilineal or patrilineal?

In a report for 1888, Boas stated that a child belongs to the gens of the father "among the Kwakiutl and Salish" (1889c, p. 202). But in another report for the same year (Boas, 1889a, p. 237), he says that "among the Salish and the Kwakiutl the child follows, *as a rule*, the father's gens, but he may also acquire his mother's gens" (emphasis ours).

In the following year: "The marriage customs [of the Kwakiutl] are of peculiar interest on account of the transition from maternal to paternal institutions that may be observed here. . . . the marriage ceremonies of the

Kwakiutl seem to show that originally matriarchate prevailed also among them" (Boas, 1890, pp. 838, 829). In his report on his summer's field work in 1890 Boas wrote: "The child does not belong by birth to the gens of his father or mother, but may be a member of any gens to which his father, mother, grandparents, or great-grandparents belonged. Generally each child is made a member of another gens . . ." (Boas, 1891a, p. 609). In *The Social Organization and Secret Societies of the Kwakiutl Indians* Boas postulates "an adaptation of maternal laws by a tribe which was on a paternal stage. I can not imagine that it is transition of a maternal society to a paternal society" [in 1889 the supposed transition was from maternal to paternal; see above]; he also finds what "seems to his mind," "conclusive proof that a paternal organization of the tribe preceded the present one" (Boas, 1897b, pp. 334-335). In 1898: "The Kwakiutl have a peculiar organization, which may be considered a transitional stage between maternal and paternal institutions. Descent is in the paternal line; but [a man inherits from his maternal grandfather]" (Boas, 1898a, pp. 121-22). In 1920, in "The Social Organization of the Kwakiutl," we learn that a child could be assigned to his father's numaym, to his mother's, or to still some other (p. 116). Also, Boas observes that he does "not see any reason for a change of . . . [his] opinion in regard to the relative antiquity of the transfer of names and privileges through the male or female line" (p. 122). This apparently refers to Boas' previously stated opinion that "maternal descent was later than paternal" (p. 124).

Again, we note how some of Boas students interpreted Boas' account of Kwakiutl social organization. Goldenweiser: "Paternal descent prevails among . . . [the southern Kwakiutl], although certain curious traces of maternal descent have also been observed" (1933, p. 221). Reichard: "The Kwakiutl Indians . . . have taken over certain aspects of matriliney which show themselves to be adjustments to a patrilineal system" (1938, p. 425). Benedict, also, says that the Kwakiutl "compromised" between patriliney and matriliney (1934, pp. 185-86, 227-28).

It seems fairly clear that Boas never really understood Kwakiutl social organization. His descriptions of it are both confused and contradictory, and as a consequence, some of his outstanding students were led to differing, and even contradictory, conclusions about clan organization, exogamy, etc., as we have seen. He did not possess, and was unable to formulate the concept of ambilateral lineage, in which descent may be reckoned in either the male or the female line, (see White, 1959a, pp. 176-82, for a discussion of ambilateral lineages with particular reference to the Kwakiutl); he persisted indefinitely in thinking in terms of unilateral clan organization. Being unable to see, and to accept, the Kwakiutl as they were, i.e., ambilaterally, or bilaterally, organized, he had to interpret them as being transitional from one stage of evolutionary development to another. When Boas came finally to drop

such terms as clan, gens, and sib as "misleading," and to substitute the Kwakiutl term *numaym*, he did not define the latter term. And his essay, "The Social Organization of the Kwakiutl," 1920, falls far short of a lucid explanation of their social structure.

There were two types of social structure on the Northwest Coast according to Drucker (1955, p. 108): "autonomous local group consisting of a [matrilineal] *lineage*," or "*an extended family* . . . in which descent may be reckoned through either line, or both." The Kwakiutl lived in an area where "there were no moieties, clans, or lineages. Descent was reckoned bilaterally, with only a slight preference for the male line" (*ibid.*, p. 116). Codere (1961, pp. 441-42) states that the Kwakiutl were "both lineage-based and bilateral," a condition which she says seems "somewhat paradoxical on first glance" (ambilateral lineages are no more paradoxical than unilateral lineages; Codere remarks that "the paradox disappears on close examination").

We venture to suggest, at this point, the reason why Boas did not, and could not, understand Kwakiutl social organization—or their whole culture, for that matter. The reason derives from Boas' fundamental philosophy of ethnology, which we have already noted. He was so obsessed with particulars ("in ethnology everything is individuality") that he could not see general outlines or forms. In the Introduction to Volume I of the publications of the Jesup North Pacific Expedition (1898), Boas made these revealing and significant statements:

"The history of anthropology is but a repetition of that of other sciences. When the facts begin to array themselves in seeming order, the ultimate goal of inquiry appears to be near at hand. The fundamental laws which governed the growth of culture and civilization seem to manifest themselves conspicuously, and the *chaos of beliefs and customs* appears to fall into beautiful order. But investigation goes on incessantly. New facts are discovered, and shake the foundation of theories that seemed firmly established. The beautiful simple order is broken, *and the student stands aghast before the multitude and complexity of facts that belie the symmetry of the edifice* he had laboriously erected. . . .

"Anthropology has reached that point of development where the careful investigation of facts shakes our firm belief in the far-reaching theories that have been built up. *The complexity of each phenomenon dawns on our minds.* . . . Heretofore we have seen the features common to all human thought [i.e., cultures]. *Now we begin to see their differences.* We recognize that these are no less important than their similarities, and *the value of detailed studies becomes apparent.* . . ." (pp. 3-4; emphases mine).

No better, more realistic portrait of the mind of Franz Boas was ever sketched than the one we have just exhibited. Boas "was saturated with a

sense of the complexity of the phenomena he investigated," wrote Lowie (1944a, p. 60), "and any simple explanation evoked his distrust." What with his laboriously detailed investigations, his penchant for conjectural history, his reliance upon "recent tradition, the historical truth of which cannot well be doubted" (Boas, 1920b, pp. 111-112), his acceptance of the testimony of informants on ethnological problems, his ambivalent attitude toward theories of cultural evolution which allowed him to regard the Kwakiutl as transitional between two stages of development on the one hand, and to reject "unilinear evolution" on the other, Boas came fairly close to leaving the "chaos of beliefs and customs" just about where he found it. "A clear understanding of the constitution of the *numaym* is made very difficult. . . ." he wrote in 1920b after three decades of "analyzing their culture by careful and exact methods" (p. 111). Great "obstacles to a clear understanding of the social organization of the Kwakiutl" still remained in 1920 (Boas, 1920b, p. 111). And Boas "tried to clear up the situation by recording the histories of a number of families *in all possible detail*" (ibid., p. 111; emphasis mine). The genealogy described and illustrated by Boas on pages 112-13, makes the confusion almost complete.

But if Boas found an understanding of Kwakiutl social organization difficult, the reconstruction of their culture history was incredibly easy. Since we have already examined Boas' techniques of historical reconstruction and his canons of demonstration, we shall merely quote here a comprehensive account of the history of Kwakiutl culture as Boas reconstructed it. Characteristically enough, it is found, not in a work on Kwakiutl culture, but in his essay, "The Growth of Indian Mythologies" (1896e, p. 8):

"It seems that the Kwakiutl at one time consisted of a number of village communities. Numbers of these village communities combined and formed tribes; then each village community formed a clan of the new tribe. Owing probably to the influence of the clan system of the northern tribes, totems were adopted, and with these totems came the necessity of acquiring a clan legend. The social customs of the tribe are based entirely upon the division into clans, and the ranking of each individual is the higher—at least to a certain extent—the more important the legend of his clan. This led to a tendency of building up clan legends. Investigation shows that there are two classes of clan legends: the first telling how the ancestor of the clan came down from heaven, out of the earth, or out of the ocean; the second telling how he encountered certain spirits and by their help became powerful. The latter class particularly bear the clearest evidence of being of a recent origin; they are based entirely on the custom of the Indians of acquiring a guardian spirit after long-continued fasting and bathing. The guardian spirit thus acquired by the ancestor became hereditary, and is to a certain extent the totem of the clan,—and there is no doubt that these traditions, which rank now with the fundamental myths of the tribe, are based on the actual fastings and acquisitions of guardian spirits of ancestors of the present clans. If that is so, we

must conclude that the origin of the myth is identical with the origin of the hallucination of the fasting Indian. . . .”

As we noted at the outset, Boas began his study of the Kwakiutl in 1886. Fifty years later he was working with a Kwakiutl informant in New York. His writings on this tribe greatly exceed 5,000 printed pages. Yet, with all this, he did not “complete a single, large-scale portrait of a tribal culture, not even of his beloved Kwakiutl” (Lowie, 1947, p. 313). And Codere, who has gone over Boas’ Kwakiutl studies very thoroughly believes that “it is not possible to present a synthesized account of Kwakiutl culture based upon Boas’ works” (Codere, 1959, p. 66). What are the reasons for this?

We have already elucidated the principal reason, I believe: 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. “Can’t see the forest for the trees” is a well-worn, trite saying, and we dislike to repeat it *ad nauseam*. But how can one say it better?

Boas edited the thousands and thousands of pages of ethnography produced by George Hunt and Henry W. Tate. He may have re-ordered the material to some extent (although we still find a discussion of Kwakiutl house types in a chapter on clan legends; 1897b). And in some places, perhaps, he amplified or clarified it. But for the most part, we are justified in believing that he left it very much as he found it. The justification derives from the fact that Boas presented great masses of his own ethnographic data without commentary, without the bare information that would be needed to render it intelligible to the reader. And Kroeber (1956, p. 151) tells us that “much of this [‘corpus of data’] was collected and written out by Hunt in Kwakiutl; and, in line with his method and principles, Boas left it unaltered.”

“One criticism which can justly be made of Boas’ *Kwakiutl Tales*,” wrote Sapir (1912, pp. 197–98), is that they are inadequately annotated. Outside of references to earlier published versions of Kwakiutl myths . . . practically no assistance is given to the student of Kwakiutl mythology and culture toward the understanding of the tales. This is the more regrettable in that the stories are full of ethnological references requiring elucidation. One not infrequently finds himself in doubt as to the exact significance of a passage, for which it would not be altogether easy to find an explanation in Boas’ other writings.”

In Boas’ Kwakiutl and Tsimshian monographs, wrote Radin (1933, pp. 8–9), “we find the bare facts presented without comment, unless it be to indicate the distribution of certain specific traits. Neither the individuals from whom the data were obtained nor the data themselves are evaluated, and this manifestly is not an accident. Surely it is not unwarranted to assume that he wishes the facts to speak for themselves.”

"It is easy to go through a thousand pages of his [Boas'] monographs without encountering a line of interpretation" (Lowie, 1947, p. 316).

Boas' failure to make his monographic material intelligible to the reader was especially marked in the case of *The Religion of the Kwakiutl Indians* (1930), according to Radin (1933, pp. 64-65): "What does he tell us about the informants from whom he obtained his data? Occasionally their names are given and the group to which they belong indicated. Not always that. Nor does Boas add *one single word by way of explanation of specific details that are quite meaningless without annotation*" (emphasis mine). Why did not Boas help his readers toward a comprehension of the data he made available to them? Did he, or did he not, understand them himself?

It is no wonder that "Konrad Theodor Preuss, acknowledging the unique amplitude of the Kwakiutl material, wondered what it all meant" (Lowie, 1944a, p. 62).

One of the best—if not *the* best—known of Boas' ethnographic findings on the Northwest Coast is that of the potlatch of the Kwakiutl. It has been described in numerous textbooks, and through Benedict's *Patterns of Culture*, has been made known perhaps to millions. Close readers of his account have pointed out that it would be impossible for the potlatch to function as he described it: with each borrower returning a loan with 100 percent interest; his description was inaccurate. But, says Kroeber (1956, p. 152), "Boas' fault is not that he never knew better—he undoubtedly did—but that he never took the time to re-explain the system." Kroeber was the senior editor of *Source Book in Anthropology* which, in two successive editions, reprinted the Boas-Hunt description of the potlatch of the Kwakiutl.

Boas' account of Kwakiutl potlatching was deficient in another respect: "Boas certainly knew about play potlatching," says Codere (1956, p. 344), although he did not publish anything about it. It was one of the many things he had still to communicate about the Kwakiutl" [after fifty years of research and more than 5,000 pages of publications, L. A. W.].

Boas' treatment of the potlatch has led to distortion and confusion in ethnology: he presents this social ritual as an economic institution. The "economic system . . . finds its expression in the so-called 'potlatch,'" (Boas, 1899, p. 681; see White, 1959a, pp. 238-42, for our distinction between an economic process and a social ritual). The "underlying principle" of the potlatch, says Boas (1897b, p. 341), "is that of the interest-bearing investment of property." Others use such terms as credit, capital, and usury in describing the potlatch or Kwakiutl culture generally. Radin (1927, p. 326) observes that "such a ritual [*sic*] could have of course developed only in an atmosphere pervaded by the spirit of capitalism . . ." And a recent textbook speaks of the "overdone capitalism of the Kwakiutl" (Hoebel, 1958, p. 396). It is unfortunate, first of all, that this social ritual ("the ultimate motivation of the potlatch is rivalry,

not profit," Bunzel, 1938, p. 358) should be presented as an economic institution, and doubly unfortunate that economic terms proper to Western capitalist society should be applied to this preliterate, non-monetary culture. As for the social organization of the production, distribution, use, and consumption of goods as an economic process, Boas has virtually nothing to say—except in so far as it may appear in texts.

Boas' researches on the Northwest Coast have bequeathed to ethnology another source of misrepresentation and confusion, namely, his account of the social structure of the tribes of that region. "The people of this country," he observed in his Fifth Report (1890, pp. 830, 823), "are divided into three classes: common people, middle class, and chiefs." Among the Lkungen "there are three classes of people, the nobility . . . the middle class . . . and the common people" (1891a, p. 569). In 1906, he distinguished four classes "on the north Pacific coast": "chiefs, nobility, common people and slaves" (Boas, 1906b, p. 242). Boas has been followed in this delineation of class structure by virtually all of his principal students and by others as well. "All the tribes of the Pacific Coast of America are divided into three classes, a nobility, common people and slaves" (Radin, 1927, p. 322). Lowie uses Boas' description of class structure among "the natives of the coast of British Columbia" to refute the "palpable nonsense" of Lewis H. Morgan who maintained that American Indian society was democratic (Lowie, 1920, pp. 351, 353, 389).

Codere, however, tells us that Boas regarded Kwakiutl society as "classless," and that after 1920 "he described Kwakiutl society as classless" (Codere, 1957, pp. 474, 485). I am not sure that I fully understand Codere here, nor have I found Boas' description of Kwakiutl society as classless. In "The Social Organization of the Kwakiutl" (1920b, p. 116), he says that "the conditions among the Kwakiutl and the Nootka must have been quite similar in so far as a sharp line between the nobility and the common people did not exist"—how the phrase "in so far as" qualifies the presence or absence of a "sharp line" is not clear to me, either.

However, in 1924 Boas distinguished five *classes* among the Bella Bella, "according to rank," one of them being a "nobility" (Boas, 1924, pp. 376-77). And Lips, in an essay published under Boas' editorship in 1938 said, speaking of the tribes of the Northwest Coast: "We find chiefs, nobility, middle class, bondsmen, and slaves, and there is even another class of preferred people . . ." (Lips, 1938, pp. 511-12; a footnote reference at this point cites Boas' 1924 paper on the Social Organization of the Tribes of the North Pacific Coast as authority). And in 1952 Herskovits, a devoted follower of Boas, speaks of "a relatively stable class system composed of nobles, commoners, and slaves" among the Kwakiutl (Herskovits, 1952, p. 476). Class structure survives in the literature at least.

It is unfortunate that Boas should have presented social distinctions as to

rank as a *class structure*. And it is doubly unfortunate that he should have designated them with such misleading terms as noble, commoner and slave, borrowed from relatively modern Western European culture.

Whole areas of Kwakiutl culture are either ignored or treated very superficially in Boas' ethnography. He has a few pages on Kwakiutl "Terms of Relationship" in *Tsimshian Mythology* (pp. 489-95), but this is about all he has on the subject. Social life in general, the composition of households, the family, the rearing of children, and many other subjects are ignored or mentioned in passing in texts.

According to Codere (1959, p. 69), Boas "neither recorded nor caused to be recorded [Boas gave George Hunt instructions about what he was to investigate and record, as Boas tells us in a number of instances, L. A. W.] much about informal behavior, as distinct from formal public affairs, myths, family histories, and such surely cultural matters."

Also: "It is clear, however, that Boas presented relatively little material to work with on the more amiable side of Kwakiutl life, that Benedict ignored such materials as were present . . . and that both Boas with his 'atrocious but amiable' Kwakiutl and Benedict with her 'paranoid' Kwakiutl took the structural material at face value for purposes of determining the meanings and qualities of the culture" (Codere, 1956, p. 336).

We have already noted the almost complete absence of "the ribald" in the writings of both George Hunt and Boas (Codere, 1959, p. 69).

In reply to Codere's observation (1955, p. 1304) that "there is no single reference to any of Boas' publications on the Kwakiutl, although material on Kwakiutl drinking figures prominently in the volume [Lemert's *Alcohol and the Northwest Coast Indians*]," Lemert replies:

"With regard to my neglect of Boas, I may say that I searched his writings at length for some reference to Kwakiutl drinking but with no success. This is extremely puzzling to me in the light of the obvious existence of the whiskey feasts among the Kwakiutl—at the very least among those of Kingcome Inlet" (Lemert, 1956, p. 561).

Murdock, also, has found Boas' ethnography of the Kwakiutl to be deficient: "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*]" (Murdock, 1949, p. xiv).

The following summary judgment of Boas' studies of the Kwakiutl is made by Verne F. Ray:

"Boas' picture of the Kwakiutl is not only deficient because he failed to heed the cautions which he enumerates for others but also because he allowed this one-sided portrait to stand, not only for all Kwakiutl culture but for the Northwest

Coast generally. His overgeneralization for the Kwakiutl and his failure to speak out in correction of the errors of his students, such as Benedict, has had the result that the ethnographic picture for the Northwest Coast as visualized, taught, and accepted by many anthropologists is that which in fact applies only to the nobility of the southern Kwakiutl. This situation, so painful to research scholars of the Northwest Coast, is not given attention by Herskovits [in *Franz Boas, The Science of Man in the Making*, 1953] despite the fact that this was Boas' principal area of ethnographic research" (Ray, 1955, p. 140).

"A major deficiency in Boas' work with the Kwakiutl," according to Ray (*ibid.*, p. 139), "was his neglect of the patterns and behavior of the lower classes: his nearly exclusive concern with the nobility and his presentation of this picture as representative of Kwakiutl life."

Conclusion

I do not propose to offer here a comprehensive and definitive evaluation of Boas' labors and achievements in ethnography and ethnology on the basis of this review of much—but far from all—of his work. But, having read, re-read, or scanned scores of articles and monographs—thousands of pages—in the course of writing this article, a few comments of my own may be in order.

I am impressed, as many before me have been, by the sheer magnitude of Boas' output. Even when one takes account of the thousands of pages recorded by Hunt and Tate, the result is still impressive and, of course, the labor of editing and proof-reading the work of Hunt and Tate was very great—and tedious. And we may well believe that he did this himself in view of the fact that he "copied out in his own hand" the 344 pages of *Keresan Texts* (1925a) "for zincograph reproduction, in order to save the Ethnological Society the added cost of typographical composition" (Kroeber, 1943, p. 21); Jacobs (1959, p. 121, and Bunzel, 1960, p. 461, also remarks upon this labor of love and economy. Boas' "contributions [to the study of folklore] overwhelm the spectator by their sheer massiveness" (Lowie, 1944a, p. 61). But, I suppose that almost everyone would agree that mere quantity is not enough. I bring up this point, though, because some of Boas' admirers appear to believe that there is merit in size alone ("the massive achievement of Boas in the field of descriptive ethnography would *alone* be *sufficient* to give him an outstanding place in his science," Herskovits, 1953, p. 62; emphasis mine).

We have already noted that the value of much of Boas' text material is seriously impaired by his failure to provide that information which is essential for its comprehension and use by the reader ("... a thousand pages of his monographs without encountering a line of interpretation"). But we shall return to this point later.

We turn now to some of his ideas, his philosophy of ethnology.

"The first general theoretical problem on which he [Boas] worked," says

Benedict (1943, p. 61), "was that of the importance of the diffusion of traits in human culture." We believe this to be a fair and accurate statement. But the idea of diffusion was not novel by any means. As early as 1865 E. B. Tylor "had discussed diffusion with insight and acumen," as Goldenweiser (1931, p. 661) has remarked. Lowie (1917), too, has asserted that, although Tylor was "certainly a strong believer in the independent evolution of cultural phenomena in distant areas of the globe, he was very much alive to the influence of diffusion" (p. 264); as a matter of fact, says Lowie, "in many concrete instances," Tylor "goes much farther [as a diffusionist] than at all events modern American ethnologists are inclined to follow" (p. 265). Tylor's assigning the Mexican game of patolli an Asiatic origin, for example, was much more daring than anything Boas proposed. Actually, Boas was a conservative diffusionist in that he insisted, as we have already noted, upon continuous distribution. Lewis H. Morgan, another "evolutionist," also invoked diffusion freely in his ethnological work (I have cited many instances of this in White, 1945, pp. 341-43). Indeed, Brinton (1896, p. 9) criticized both Tylor and Morgan for their unwillingness to accept independent development in certain instances. And then there was Boas' own countryman, Friedrich Ratzel, who was an early exponent of diffusionism. Thus we see that Boas was not an innovator in espousing diffusionism, and as a diffusionist he was modest and conservative in his claims.

It should be remembered, however, that although Boas had distinguished and influential predecessors in the field of diffusionism, he had a contemporary in the United States during the 1890s who was a stout champion of independent origin and development in its extreme form: Daniel G. Brinton. It is reasonable to suppose that some, at least, of Boas' animus toward independent development, and his preference for diffusion, was due to Brinton's position and influence.

But Boas' enthusiasm for diffusion led him into a grievous error, namely, that the occurrence of diffusion negates theories of cultural evolution. "It was the assembled documentation of this truth [that each culture is a composite result of diffusion]," says Benedict (1943, p. 61), "that led him [Boas] to oppose the rational reconstructions of cultural evolution."

Boas' article, "The Limitations of the Comparative Method of Anthropology" (1896), is a significant landmark in his ethnological writings, it seems to me. In it he makes a definite break with "the view of by far the greater number of living anthropologists" (p. 901), namely, that of independent development—"the working of the uniform laws governing the human mind" (p. 901). At the outset of the article Boas pays his respects to the "momentous discovery" that "human society has grown and developed everywhere in such a manner that its forms, opinions and its actions have many fundamental traits in common" (p. 901). He recognizes that certain cultural forms

have "arisen independently over and over again" (p. 903). "We agreed," he says, "that certain laws exist which govern the growth of human culture, and it is our endeavor to discover these laws" (p. 905). *But*—and here comes the break: his manifesto of a new procedure.

The assumption of psychic unity and of independent development leads, Boas reasons, "to the conclusion that there is one grand system according to which mankind has developed everywhere" (p. 904). But "this theory has for its logical basis the assumption that the same phenomena are always due to the same causes" (ibid.). He then "demonstrates" (argues) that like effects may result from unlike causes, which leads him to conclude that "we must also consider all the ingenious attempts at construction of a grand system of the evolution of society as of very doubtful value. . . ." (p. 905).

Boas then contrasts the "comparative method" (evolutionism) with the "historical method." The former, he says, "has been remarkably barren of definite results, and I believe," he says, "it will not become fruitful until we renounce the vain endeavor to construct a uniform systematic history of the evolution of culture. . . ." (p. 908; emphasis mine).

Boas sets forth the specifications of the "historical method"; they turn out to be just what he had been doing on the Northwest Coast ever since he set foot in British Columbia. It consists of:

"A detailed study of customs in their relation to the total culture of the tribe practicing them, in connection with an investigation of their *geographical distribution* among neighboring tribes, [which] affords us almost always a means of determining with considerable accuracy the *historical causes* that led to the formation of the customs in question and to the *psychological processes* that were at work in their development" (p. 905; emphases mine).

This is the core of Boas' ethnology.

The belief that the occurrence of diffusion negates theories of cultural evolution was impressed upon Boas' students. "The extensive occurrence of diffusion," says Lowie (1920, p. 434) "by itself lays the axe to the root of *any* theory of historical laws" (emphasis mine). Seventeen years later (Lowie, 1937, p. 60), he wrote that "diffusion plays havoc with any universal law of sequences."

But Boas never really understood the philosophy of cultural evolution. "We must try to understand more clearly what the theory of unilinear cultural development implies. It means that *different groups* of mankind started at a very early time from a general condition of lack of culture; and, owing to the unity of the human mind, and the consequent similar response to outer and inner stimuli, developed everywhere approximately along the same lines" (Boas, 1938, p. 178; emphasis mine). But he has confused the culture history of *peoples* with the evolution of *culture*. Evolutionist theory did not say that

Seneca school children would have to work their way through hieroglyphic writing before they could learn the alphabet, but that *in the evolution of writing* a series of stages—picture writing, rebus, or hieroglyphic writing, and alphabetic writing—would be traversed.

The effect upon ethnology of Boas' antievolutionary crusade has been exceedingly unfortunate, but as I have discussed this matter elsewhere (see White, 1959b, for a summary of previous papers), I shall not deal further with it here.

History. We need add little to what we have already said about Boas' historical reconstructions. They range from the probable through the possible to the preposterous. And almost none is verifiable except in a very general way, namely, that a myth, found distributed throughout a large multiracial, contiguous area, must, in all probability, have diffused from tribe to tribe. But his reconstructions in particular and in detail are virtually beyond the possibility of verification. And his deduction from the physical appearance of two tribes that they must have intermarried at one time, and in that way one tribe borrowed traits from the other is little short of fantastic.

Boas apparently really believed that his historical reconstructions were perfectly valid, for in contrasting the "comparative [i.e., evolutionist] method" with the "historical method," he maintained that the latter was "much safer," "because *instead of a hypothesis* on the mode of development *actual history* forms the basis of our deductions" (Boas, 1896f, p. 907; emphases mine). The idea that his reconstructions might be as hypothetical, as conjectural as—and actually, if anything, more unverifiable—than the evolutionists' hypotheses apparently never entered his mind. And Lowie (1940, p. 599) faithfully echoes Boas on this point: Boas "stressed real history as a corrective of evolutionary schemes . . ."

Boas' historical reconstructions were repeated by his students, and through repetition came to be regarded as "absolutely established fact" (Lowie, 1944a, p. 60). "The Kwakiutl . . . originally lived in local bands. . . ." (Benedict, 1934, p. 185; what can *originally* possibly mean here?). Boas' thesis that the Kwakiutl were in a transition stage between patriliney and matriliney was accepted without hesitation or question by Swanton (1904, p. 479; 1905, p. 671), Goldenweiser (1914, p. 420), Benedict (1934, pp. 185–86, 227–28), Reichard (1938, p. 425), and others.

Two of Boas' prominent students could be severely critical of historical reconstructions when they were made by others, however. Both Goldenweiser and Lowie reviewed W. H. R. Rivers' *The History of Melanesian Society* in which the author reconstructed the culture history of an area. Goldenweiser (1916, p. 827) wrote: "Deliberately evading any attempt to furnish proof of diffusion in specific instances, the author erects a *purely hypothetical structure*, based on a bewildering maze of assumptions invari-

ably favoring interpretations through diffusion while *disregarding alternative interpretations*" (emphases mine). Lowie (1915, p. 589): "[Lowie] cannot avoid feeling that some other student who shared Dr. Rivers's knowledge of this area might construct an argument no less consistent, elaborate, and plausible, yet widely diverging from that here presented. At many a particular point alternative interpretations suggest themselves. . . ." These are precisely the points that might be made with regard to Boas' historical reconstructions. But we do not find them in the literature.

Psychology. His psychological explanations are little, if any, better than his historical reconstructions: "The possession of clan traditions was felt as a great advantage, and consequently the desire developed to possess clan traditions. . . . *The desire to guard the traditions* which were once acquired *led to the development of endogamic institutions*, in order to prevent the spread of the traditions over the whole tribe" (Boas, 1898a, p. 125; emphasis ours). One is tempted to remark that this is the way Boas would have felt and behaved if he had been a Bella Coola Indian. One wonders how a scholar like Kroeber could have written the following: "His [Boas'] unsparing mind exacted proof even in the complex and difficult situations which prevail in culture, and he refused to deal with problems in which strict proof seemed impossible" (Kroeber, 1952, p. 146).

C. M. Barbeau is the only anthropologist, as far as I know—I have not made an exhaustive survey of the literature—who has seriously criticized Boas' field work and his interpretation of his data—and this concerned only Boas' *Tsimshian Mythology* (1916); Barbeau had done field work himself with this group. Boas' sections on "Social Organization of the Tsimshian" "teem with controversial matters," according to Barbeau (1917, p. 557) who disagrees with Boas on many points. Alternative hypotheses to those proposed by Boas suggested themselves to Barbeau on every hand; i.e., many interpretations were possible but virtually none was verifiable.

"We cannot help feeling," writes Barbeau, "that had Dr. Boas had a prolonged opportunity of studying in the field [instead, presumably, of relying upon Tate] Tsimshian village, kinship, and clan organization, he would have revised many of his views on the subject, resulting as they do partly from the scantiness of his data and from the evident lack of insight evinced by Tate" (ibid., p. 557).

Texts. The latter comment brings up the question again of having natives record texts. Boas favored this, as we have seen, because he believed it tended to eliminate "the almost unavoidable distortion contained in the descriptions given by the casual visitor *and student*" (Boas, 1909b, p. 309; emphasis ours). But what about the bias, and consequent distortion, wrought by the native? I have worked intermittently with Indian informants among the Pueblos of the Southwest for thirty years and I have repeatedly noted a tend-

ency on their part to present an account of their culture that would depict it in a favorable light—from their point of view; this meant omitting some things, and exaggerating others. I do not believe for a minute that a good native informant is less given to bias and distortion than a good scientist. And, in Barbeau's estimation, Henry Tate had many serious shortcomings (Barbeau had considerable specific and direct information about Tate). The text material on social organization "furnished him [Boas] by Tate was one-sided and very incomplete," falling "far short of the requirements when they are expected to yield a satisfactory perspective of a confused domestic history and an intricate social structure" (Barbeau, 1917, p. 553). Moreover, Tate was "lower class," which Barbeau believed handicapped him as an informant.

Laws. In 1888 Boas asserted that "the discovery of these laws [according to which "the human mind develops everywhere"] is the greatest aim of our science" (Boas, 1888i, p. 637). Incidentally, Boas always spoke of "discovering," or "finding," laws, not of formulating them: "the concept that scientific laws are found in nature rather than formulated by the scientist is typical of Boas" (Buettnner-Janusch, 1957, p. 321). In 1896 "we agreed that certain laws exist which govern the growth of human culture, and it is our endeavor to discover these laws" (Boas, 1896f, p. 905) even though at that very moment he was abandoning the generalizing "comparative method" for the particularizing "historical method." Decades later, toward the close of his scientific career, he set forth this, his final view on the subject:

"In short, the material of anthropology is such that it needs must be a *historical science*, one of the sciences the interest of which centers in the attempt to understand the *individual phenomena* rather than in the establishment of *general laws* which, on account of the *complexity of the material*, will be necessarily vague and, we might almost say, so self-evident that they are of little help to a real understanding" (Boas, 1932b, p. 612; emphasis mine).

The similarity of this point of view to that expressed in "The Study of Geography" forty-five years earlier is impressive; it is the same Boas down to the last detail. "So consistent was his theoretical position," says Bunzel (1960, p. 404), "that it is frequently hard to tell whether a paper was written in 1888 or 1932." In his early years in anthropology Boas paid his respects to laws and to the importance of "discovering" them. But as he made perfectly clear in "The Study of Geography" (1887), he was not interested in generalizing but in particularizing—in ethnology "everything is individuality." It is not surprising, therefore, that Boas never did discover any significant laws, and, at the close of his scientific life he believed that none of much value was attainable.

What, then, might reasonably be said in concluding our tour of much of Boas' ethnography and ethnology?

1. He collected, and caused to be collected and recorded, a vast mass of

factual material. But, 2. he not only did not produce a coherent and intelligible picture of Kwakiutl culture as a whole; it is not possible for others to do this on the basis of Boas' data (Codere, 1959, p. 66). And, according to Ray (1955, p. 140), Boas and some of his students have presented a distorted picture, not only of the Kwakiutl, but of the entire Northwest Coast. 3. His historical reconstructions are worthless, for the most part, and fantastic in some respects. He made a convincing case for the diffusion of myths, but in this respect he was not original, or outstanding among diffusionists. And if all of his detailed historical reconstructions *could* have been proved correct they would have added up only to the conclusions that contiguous socio-cultural systems interact and affect one another, which is hardly a great scientific discovery. 4. His unwillingness to generalize tended to oppose the development of a science of culture. 5. His antievolutionism did cultural anthropology great harm from which it is only recently recovering.

In my opinion, Boas' greatest achievement, his most enduring contribution, will turn out to be the textual material that he, George Hunt, and Henry Tate so laboriously collected and so painstakingly recorded. But even here it is exceedingly difficult to come to a satisfying conclusion with regard to their value. As we have seen, they are very inadequately annotated, and we are left without the information that is necessary to their adequate comprehension. And, so far, no one, as far as I know, with the possible exception of Werner Müller (*Weltbild und Kult der Kwakiutl-Indianer*), has undertaken to make extensive use of this material. Benedict's portrait is a distortion; Goldman's sketch is commendable as far as it goes, but it certainly does not encompass or utilize a vast amount of the material available. Therefore, it remains to be seen what use can be made of this storehouse of fact in the future; any judgment at this time seems premature to me.

But, regardless of the use to which this textual material may be put in the future—which is to say the value and significance of these data—I am inclined to believe that these texts constitute Boas' principal contribution to cultural anthropology. After all, what are they to be compared with? Not his "critical approach," his "rigorous scientific method," his "convincing demonstrations," or his constructive theories. I am much inclined to agree with Swanton who, in a review of Boas' *Kwakiutl Texts*, wrote:

"Therefore, though unattractive, not to say repellent, to the average reader, accustomed to garbled and Europeanized fragments of Indian legends from which the Indian spirit has utterly departed, these texts will be turned to again and again, not only by the philologist, folklorist, mythologist, and student of religious phenomena as to an inexhaustible source of reliable information regarding the thought life of the people from whom they were obtained" (Swanton, 1907, p. 744).

Reichard, also, believed that the texts are "the strongest rocks in Boas' self-built monument" (1943, p. 55).

The Purpose of this Study

Since Boas has been so sacrosanct in the annals of American anthropology (he "was literally worshipped by some . . ." Kroeber, 1943, p. 23), and since he is still held in highest esteem in many quarters, a review of his work that falls short of eulogy may well evoke the resentment of some, and perhaps incline them to impute unfriendly motives to the author. In complete honesty and sincerity I can say that I harbor no hostile attitude towards Boas the man or Boas the scientist. I do believe, however, that Boas' stature as a cultural anthropologist has been greatly—even grotesquely—exaggerated by his disciples and others. And, as a consequence of this, many anthropologists, not only in the United States but in other countries as well, have distorted and unjustifiable conceptions of many important ethnological problems. All this, I think, is unfortunate.

The purpose of this study has been to provide a more realistic picture of Boas as ethnographer and ethnologist than we have ever had before. We have taken considerable pains to dig out and assemble some facts about Boas' field work that no one has done before. We have let Boas speak for himself in his own words. The result of all this is a portrait of Boas that is different—shockingly different—from the highly idealized image of him created by his disciples ("worshippers"). I hope that my study will help to correct many misconceptions, both with regard to the work of Boas and to various ethnological problems with which he was concerned.

Were it not for the long-established adulation of Boas his errors and other shortcomings as ethnographer and ethnologist would occasion little surprise, and would merit both understanding and sympathy. When he went to British Columbia in 1886 he had little qualification for scientific work in ethnology other than intense interest, industry, and perseverance. It is true, of course, that he ventured into the field of anthropological literature, but there is little evidence that it made any impression upon him: his mind was made up in 1887–88 as "The Study of Geography" (1887d), "Museums of Ethnology and their Classification" (1887e) and "The Aims of Ethnology" (1888h) make clear.

The science of ethnology was but little advanced in the 1880's and '90s, and some concepts that are commonplace today were unknown at that time. Take the concept of ambilateral lineages, for example; it did not enter ethnological theory until the 1920s. No one would criticize a field worker of the closing years of the nineteenth century for not using this concept in describing the social structure of the Kwakiutl. Boas' use of (1) the concept of unilateral lineages, and (2) the theory of the sequence of stages from matriliney to patriliney was in close accord with the ethnology of the times. But one might reasonably raise the question why an "independent and erudite thinker" (Lowie,

1937, p. 151), a man who was on the way to becoming "the world's greatest anthropologist" (Kroeber, 1943, p. 5), could not have formulated this conception—the facts lay right before his eyes. Here again we have the contrast between a man, a real man, in a real situation in time and place, with both its resources and its limitations, and the glorified image of that man. It is not that Boas is belittled in this study; it is the grotesque image of him that is exposed.

Our study helps us—or at least me—to understand how a number of Boas' most prominent students could come to believe that Boas founded the science of anthropology. He "came to anthropology as a culture-hero," said Goldenweiser (1941, p. 153). "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. . . ." (Benedict, 1943, p. 61). Boas "found anthropology a playfield and jousting ground of opinion; he left it a science . . ." (Kroeber, 1952, p. 146). Boas was "the man who made anthropology into a science. . . ." (Mead, 1959b, p. 35).

There is no need to list and enumerate here the anthropologists of considerable scientific stature who preceded Boas; they are too well known for that. How, then, could his students have come to believe that it was Boas who "made anthropology into a science"? On the one hand "he cited the interpretations of others rather infrequently, and he disparaged more often than he approved or concurred in their views. When he formally analyzed work, it was usually to point out errors in method. . . . He used the material and results of others sparingly, except for his students and followers . . ." Kroeber, 1956, p. 156). On the other hand, he presented to "his students and followers" an image of anthropology, *his* anthropology. If Boas created anthropology, he created it in his own image. The anthropology that he presented to his students was composed of the sort of studies that he had engaged in all of his professional life. The belief that anthropology as a science was the creation of Boas is, of course, unwarranted (grotesque). But it is at least understandable.

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