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**THE SOCIAL ORGANIZATION OF
ETHNOLOGICAL THEORY**

LESLIE A. WHITE

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by Leslie A. White

Science consists essentially of concepts and ways of dealing with concepts. These concepts interact with one another in the minds of individuals and in the social life of the scientific community. Science grows as new concepts are formed in the process of interaction between concepts and the external world, on the one hand, and interaction among concepts themselves, on the other. As concepts interact among themselves, new combinations and syntheses—i.e., new concepts—are formed.

Scientific concepts, as well as concepts in general, may be regarded as a distinct order of phenomena, resident in their human carriers. They diffuse from one people or region to another, and they flow through time from one generation and age to another. The realm of scientific concepts may be considered as a process *sui generis* in which concepts beget concepts. Thus, concepts held by Galileo and laws formulated by Kepler¹ were synthesized in the nervous system of Newton to form laws of motion and gravitation. In October, 1838, Darwin tells us in his autobiographic sketch, concepts derived from Malthus' essay on population were integrated by him with other concepts that had come from many sources, past and contemporary. Up to that time Darwin had wrestled with the problem of new species without success. But when the Malthusian concept was integrated with others in his mind, "here then," he wrote, "I had at last got a theory by which to work" (Darwin, 1896, I, 68). Later, Mendelian concepts were added to Darwinism to form still further biological theory. The experiments of Faraday, the mathematical formulations of Clerk Maxwell, the mechanical inventions of Hertz, the invention of the radio by Marconi constitute a related sequence of concepts. And so it is in every area of scientific activity: concepts interact with the external world and among themselves; new concepts are formed.

But suppose that elements of a different order are introduced into this

Editor's Note: Mr. White is Professor of Anthropology, The University of Michigan, Ann Arbor.

conceptual process? Suppose that Newton's religion or Descartes' personality were considered to be an integral part of their respective contributions to mathematics so that these contributions would be evaluated in terms of religious beliefs or of personality. Suppose that allegiance to a capitalistic or a communist system were injected into biology, as was done by Lysenko. The scientist, as a scientist, would protest. One can no more talk sensibly about Christian mathematics or communist biology than of sweet triangles or melancholy cubes.

Aspirin, the trade name of an acetate of salicylic acid, is composed of carbon, hydrogen, and oxygen ($C_9H_8O_4$). It has distinctive pharmacological properties because its component parts are interrelated in certain proportions and within a certain configuration. Aspirin is aspirin whether manufactured and sold by a Caucasian capitalist or a Mongolian communist. But the "makers" (i.e., merchants) of aspirin try to persuade buyers that Alpha's aspirin is better than that made by Beta, or Gamma; attempt is made to introduce a nonpharmacological factor into a pharmacological process and product. Much the same sort of thing takes place occasionally in scientific communities: scientific work—selection and rejection of premises, procedures, and goals—and the evaluation of results are sometimes profoundly affected by nonscientific factors such as personality, race, and nationality. This influence is all the more intense and effective when these factors are organized in the form of "schools."

In cultural anthropology, as in other sciences, we find conceptual processes in operation in which premises and goals are determined, and results evaluated, by the concepts and logic of science. But we also find social organizations—schools—of ethnological theory which closely integrate such nonscientific factors as personality, nationality, and race with scientific concepts such as evolution, diffusion, independent development, integration, determinism, choice, and law. These schools are by their very nature deceptive. They make it appear and would have everyone believe that their choice of premises and goals has been determined by scientific considerations and that the value of their results is measured by the yardsticks of science alone.

This is definitely not the case, as we shall later demonstrate. We are not accusing members of schools of an intent to deceive; they are obviously sincere. Their sincerity and group loyalty tend, however, to persuade and consequently to deceive. It is impossible to understand much of the work in ethnological theory without taking social organization—schools—into account. Viewed in the light of social organization—of personalities organized into groups—things that are otherwise incomprehensible or even incredible become clear. We shall attempt to demonstrate our thesis by a consideration of two schools of ethnological theory, those of Franz Boas and A. R. Radcliffe-Brown.

In our examination of these schools, we undertake to appraise the schol-

arly accomplishments of their leaders and followers; to compare our appraisals with those offered by adherents and by scholars who are not members of the schools; to draw inferences concerning the nature of social relationships between leaders and followers; and to describe various aspects of the personalities of both leaders and followers. One who follows procedures such as these incurs the risk of being accused of indulging in nonscholarly, personal attacks upon those whom he discusses. Such a charge is, in fact, expectable and completely in keeping with the thesis of this essay. We wish to state that no personal attacks are intended, and to repeat that the objective of this essay is to present an interpretation of the role of schools in the development of ethnological theory.

The Boas School

(Franz Boas was born in Minden, Westphalia in 1858, the son of a prosperous business man. He attended universities at Heidelberg, Bonn and Kiel, receiving his doctorate at Kiel in 1881. He majored in physics and mathematics, but changed, while still a student, to physical and cultural geography. He spent a year, 1883-84, in Baffinland in geographical exploration and ethnological researches. In 1886 he made his first field trip to the North Pacific Coast. From 1888 to 1892 he taught anthropology at Clark University. He served as curator at the American Museum of Natural History from 1896 to 1905. In 1896 he was appointed lecturer in physical anthropology at Columbia University. Three years later he became Professor of Anthropology at Columbia, where he remained until his retirement in 1937. He engaged extensively in researches in physical anthropology, ethnology, and linguistics for many years. He was elected to the National Academy of Sciences and to the presidency of the American Association for the Advancement of Science. He died in New York in 1942 [Kroeber, 1943; Herskovits, 1953].)

Many voices have been raised in protest against use of the word "school" in connection with Boas. "There is no 'Boas school,' and never has been in the sense of a definable group following a definable, selective program" (Kroeber, 1935, p. 540). (Redfield [1937, p. x] cites this statement with approval.) The "term 'Boas school' . . . is a misnomer" (Herskovits, 1953, p. 23). "Herskovits' insistence that there was no 'Boas school' is especially commendable" (Lowie, 1954, p. 47). ". . . there is no Boas school . . ." (Mead, 1959a, p. 31). ". . . I do not believe that there ever was such a 'school' . . ." (Strong, 1953, p. 394).

But it is interesting to note that some of the very persons who have denied the existence of a Boas school have, along with others, spoken freely of such a group. Kroeber (1931b, p. 248) refers to ". . . the heart of the Boas school" (see also Kroeber, 1931a, p. 260). Lowie discusses "The Boas

school" in *The History of Ethnological Theory* (1937, p. 136; see also Lowie, 1920a, p. 454). Others who have objected to this term but have nevertheless used it are Edward Sapir, "Boas and his school . . ." (1912, p. 461; see also Sapir, 1920, p. 377); Paul Radin (1933, pp. xii, 5, 7, 24, 261); and Leslie Spier, "the school . . . founded by Boas" (1931, p. 457). Goldenweiser (1941, p. 158) not only speaks of a "Boas school," but tells us who its members, "in the narrow sense," were: "Robert H. Lowie, Paul Radin, Edward Sapir, G. Speck, Truman Michelson, and A. A. Goldenweiser." In the third edition of their *Introduction to Anthropology* (1965, p. 715), Beals and Hoijer also refer to "Boas and his school."

Although Herskovits "insists" that there was no Boas school, he gives us a very good description of it:

The four decades of the tenure of his professorship at Columbia gave a continuity to his teaching that permitted him to develop students who eventually made up the greater part of the significant professional core of American anthropologists, and who came to man and direct most of the major departments of anthropology in the United States. In their turn, they have trained the students who . . . have continued in the tradition in which their teachers were trained (1953, p. 65).

Kroeber did not merely assert that there never has been a Boas school; he tells us *why* such a group could not have existed: "It is evident that for a school to be such it must have something of the quality of a cult. There must be a positive creed or dogma or slogan. The excess of negative component in Boas' intellectual make-up would have tended to prevent such a formation" (Kroeber, 1956, p. 157). We may grant the "excess of negative component in Boas' intellectual make-up," but we believe elements of a cult are easily discernible.

In defining 'cult,' *Webster's New International Dictionary* uses such phrases as "a system of worship," "great or excessive devotion to some person, idea, or thing," "a body of followers, practitioners, or worshippers."

Kroeber, who denies the existence of a Boas school nevertheless tells us that Boas "was literally worshipped by some of those who came under his influence" (Kroeber, 1943, p. 23). To Lowie, Boas was "my revered teacher" (Lowie, 1944a, p. 324). I have personally heard Lowie speak of Boas in these terms several times. Lowie (1922, p. 235) also refers to ". . . all who sat at Prof. Boas' feet . . ." Gene Weltfish (1959, p. 9) "sat at the feet of the great Franz Boas" when she was a student at Barnard College. Boas was Herskovits' "revered master" says Lowie in his review of Herskovits' biography of Boas (Lowie, 1954, p. 47). Section III of Goldenweiser's "Recent Trends in American Anthropology" (1941) is devoted to the "Disciples" of Boas. Lowie (1947, p. 315) speaks of Boas' "disciples," and Radin (1939, p. 303) refers to ". . . all Boas' disciples." Melville Jacobs (1959, pp. 123, 126, 129, 131, 136) uses the word disciple repeat-

edly. The Reverend Joseph J. Williams, S. J. (1936, p. 203), who might be expected to be discriminating in his use of terms pertaining to religion, speaks of Boas' followers as "his disciples," whose "permanent loyalty" was "effectively established" by Boas. "Once the contact [between student and Boas] is formed," said Goddard (1926, p. 316), "a relation of master and disciple grows up and in nearly every case continues . . . men and women who were studying at Columbia, came within the field of Professor Boas' attractions, and forsook all to follow him." One recalls another revered Teacher for whom fishermen dropped their nets that they might follow him.

It should not be difficult to predict the conception of Boas' role in anthropological science held by his disciples. "In the beginning was Boas . . .," thus May Edel begins *The Story of People, Anthropology for Young People* (1953) in a chapter on "Papa Franz," one section of which is entitled "Anthropology Begins." "Although Herskovits occasionally refers to precursors and contemporaries [in *Franz Boas: The Science of Man in the Making*]," says Lowie (1954, p. 47), "there are times when Boas is assigned the role of a mythical culture-hero, bringing light out of Cimmerian darkness." Goldenweiser (1941, p. 153) is more explicit: "To anthropology in this country Franz Boas, the 'Man,' came as such a culture-hero." Benedict (1943b, p. 61) sees Boas bringing order out of chaos: "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 . . ." Kroeber says that Boas "found anthropology a play-field and jousting ground of opinion: he left it a science . . ." (1952a, p. 146). Spier says, ". . . so far as anthropology is a science he [Boas] made it one" (1943, p. 108; 1959, p. 146). La Barre (1949, p. 156) calls Boas the "father of American anthropology."

Members of the Boas school have advanced and reiterated the thesis that it was field work that undermined the theoretical work of their predecessors, specifically, theories of cultural evolution. Thus, Steward observes that the field work of twentieth century anthropologists "tested and cast doubt on the validity" of "the specific evolutionary formulations of such writers as Morgan and Tylor . . ." (1949, p. 1; he repeats this observation in Steward, 1955, p. 15). Herskovits (1933, pp. 67, 72; 1937, p. 259), Sapir (1920, p. 377), Hoebel (1958, pp. 611-12), and others subscribe to this thesis. Members of the Boas school have often pictured their predecessors as armchair philosophers—or "closet philosophers," as Sapir (1920, p. 377) called them. The field of cultural evolutionism was a "happy hunting ground for the exercise of the creative imagination" (Goldenweiser, 1921, p. 55).

"With Boas, anthropology planted its feet firmly upon empiricism" (Hoebel, 1958, p. 612). "Boas must be understood, first of all, as a field

worker" (Lowie, 1937, p. 131). So generally has this view been accepted that we find a British social anthropologist, Meyer Fortes, attributing "a change which determined the whole course of anthropological studies in the twentieth century" to Boas, who "by the end of the [nineteenth] century . . . had established the method of systematic field work by professional anthropologists as the basis of American anthropology" (Fortes, 1951, p. 338). These statements disregard the fact that competent scientific field work had been established as a tradition in American anthropology long before Boas' influence was felt. Lewis H. Morgan did intensive field work among the Iroquois in the 1840's, and extensive work among Plains tribes years later (see White, ed., 1959).² At its inception in 1879, the Archaeological Institute of America asked Morgan to outline a program of field research. And the Bureau of American Ethnology emphasized field work from the start (1879). One need only mention such names as Frank H. Cushing, Matilda Coxe Stevenson, Albert S. Gatschet, Washington Matthews, J. Owen Dorsey, James Mooney, Adolph F. Bandelier, W. J. McGee, Alice Fletcher, and J. N. B. Hewitt to indicate how extensively field work was carried on in the United States before, or independently of, Boas' influence.

Boas spent a year, 1883-84, in Baffinland. "Besides the mere geographical work, which took most of my time," he wrote (1884, p. 271), "I made ethnographical collections and observations." The result of this expedition was a number of articles on geography and ethnology, of which *The Central Eskimo* (1888) is the principal item.

Boas did some ethnographic and linguistic work in the Southwest and other areas also, but the bulk of his research was on the Northwest Coast. He spent "more than 50 years . . . [in] Kwakiutl and Northwest Coast work"; his "publications total more than 10,000 printed pages," of which 5,255 were devoted to "major" works on the Kwakiutl (Codere, 1959, pp. 73, 61, 62). Over 4,000 of these pages, however, were produced by two Indians, George Hunt and Henry W. Tate, who were trained by Boas and who worked under his direction for many years. What does this great mass of material consist of?

In the thousands of pages of Boas' monographs on the Kwakiutl we find an immense amount of raw material, but very little else. "The essence of his method," says Radin (1939, p. 301), "was . . . to gather facts and ever more facts . . . and permit them to speak for themselves." Thus we find hundreds and hundreds of pages of texts and translations of myths, legends, descriptions of arts and crafts, ceremonies, folk beliefs and practices, and just plain odds and ends (see White, 1963a, pp. 21-30). In the *Ethnology of the Kwakiutl* (749 pp.), we find 155 recipes for cooking fish, along with data on hunting and fishing, "beliefs and customs," and miscellany. But no interpretation is included. "It is easy to go through a thousand pages of his

[Boas'] monographs without encountering a line of interpretation" (Lowie, 1947, p. 316). Marcel Mauss (1909) found the title of Boas' *The Kwakiutl of Vancouver Island* "somewhat misleading." "It is not a new monograph on the Kwakiutl. . . . We have here merely a fragment of a study of their material civilization, and, more particularly, of their industrial and esthetic arts," he observes in his review in *L'Année Sociologique* (my translation). *The Religion of the Kwakiutl Indians* is not a systematic treatise on the religion of the Kwakiutl, but consists of texts and translations of miscellaneous data on shamanism, mythological concepts, prayers, medical treatment for burns, etc. Regarding this work, Radin (1933, p. 65) observes that Boas fails to "add one single word by way of explanation of specific details that are quite meaningless without annotation." It is no wonder that Konrad Theodor Preuss, while "acknowledging the unique amplitude of [Boas'] Kwakiutl material, wondered what it all meant" (Lowie, 1944b, p. 62).

It is fairly clear that Boas never really understood Kwakiutl culture, or, at least, Kwakiutl social organization. Did the Kwakiutl have clans (*gentes*)? In 1889, after two field trips to the Northwest Coast, Boas reported that "the tribes of the northwest coast of America are all divided into *gentes*" (Boas, 1889a, p. 202). For some years thereafter Boas speaks of the clans, or *gentes*, of the Kwakiutl; the word clan appears in translations of texts in *The Kwakiutl of Vancouver Island* (1909, pp. 433, 435). By 1920, however, he had abandoned the terms clan and gens because they were "misleading," and used the Kwakiutl term *numaym* instead, but did not define *numaym*. When "The Growth of Indian Mythologies" (1896) was reprinted in *Race, Language and Culture* (1940), "division" was substituted for "clan." Did the Kwakiutl then have clans or not? Two of Boas' most distinguished students interpret Boas in exactly opposite ways. Goldenweiser: "Being important in all tribes [on the Northwest 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, 1948b, p. 259).

If the Kwakiutl did have clans, were they exogamous or not? In 1890, Boas (1890, p. 828) believed that the clans were exogamous. A year later he reported: "The *gentes* are not exogamous" (Boas, 1891, p. 610). Within a few years, however, he decided that the clans were exogamous (Boas, 1897, p. 334, and 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. It is, perhaps, not surprising to find ethnologists interpreting Boas differently. Goldenweiser (1933, p. 221) asserts that Kwakiutl clans "are not exogamous," and, in a footnote, he cites a "personal communication from Boas" to support this view. Irving Goldman (1937, p. 195), in his essay on Kwakiutl culture, says that "the Kwakiutl *numayms* are exogamous."

Were the Kwakiutl patrilineal or matrilineal? Boas had trouble here, also. In a report on his investigations in 1888 (1889a, p. 202) he stated that a Kwakiutl child belonged to the gens of the father. But in another report for the same year (1889b, p. 237), he says that a child follows his father's gens "as a rule . . . but he may also acquire his mother's gens." A year or so later Boas decided that the Kwakiutl were in a transitional stage "from maternal to paternal institutions" (1890, pp. 838, 829). In his report on field work for 1890 (1891, p. 609) he states that "the child does not belong by birth to the gens of his father or mother. . . . Generally each child is made a member of another gens." Boas then reversed himself on an earlier point and argued that the Kwakiutl were in transition from patriliney to matriliney (1897, pp. 334-335). Finally, in 1920 he states that "a child could be assigned to his father's numaym, to his mother's or to still some other" (1920, p. 116). Boas' thesis that the Kwakiutl were changing from patrilineal to matrilineal descent was used by some of his students to contradict evolutionist theory, which held to the priority of matriliney.

It seems highly probable that Boas was confronted with ambilineal, rather than unilineal, lineages among the Kwakiutl, i.e., lineages in which a child might belong either to his mother's line or to his father's line. Since the concept of ambilineal lineages was not current in ethnology at that time, he apparently decided that the Kwakiutl were in a stage of transition from one kind of descent to another. He was followed in this interpretation by various of his students or co-workers: Goldenweiser (1914, p. 420), Swanton (1904, p. 479; 1905, p. 671), Benedict (1934, pp. 185-186, 227-28), Reichard (1938, p. 425), and others.

"The social organization of the Kwakiutl is very difficult to understand," Boas wrote after four or five field trips (1891, p. 608). Great "obstacles to a clear understanding" of their social organization still remained in 1920 (Boas, 1920, p. 111), after more than thirty years of investigation.

One of the best known of Boas' ethnographic findings in the Northwest Coast is the potlatch, a socio-ceremonial ritual in which wealth is distributed—and at times destroyed—to validate or enhance status or rank. Boas' description of this ritual (Boas, 1897) was later reprinted in *Source Book in Anthropology*, edited by A. L. Kroeber and T. T. Waterman (Berkeley, 1920; New York, 1931). It has been described in numerous textbooks and, in Benedict's *Patterns of Culture*, it has been made known to untold thousands of readers.

Boas presented the potlatch as an economic institution: "The economic system . . . finds its expression in the so-called 'potlatch'"; the "underlying principle is that of the interest-bearing investment of property" (Boas, 1899, p. 681; 1897, p. 341). It has been described in terms of usury (Benedict, 1934, p. 184), credit, and capitalism (Radin, 1927, p. 326). I believe

it is unfortunate that a ritual should have been described, in the terminology of Western capitalistic society, as an economic institution.

Boas' description of the potlatch seems also to have been inaccurate. It has been pointed out that the system could not work if every borrower returned the loan with 100 percent interest. Kroeber treats this shortcoming lightly: "Boas' fault," he says (1956, p. 152), "is not that he never knew better—he undoubtedly did—but that he never took the time to re-explain the system." Boas' treatment of potlatching among the Kwakiutl was deficient in still 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."

In his descriptions of the social organization of the Northwest Coast Boas bequeathed to ethnology another misrepresentation, namely, that the tribes were divided into classes—nobles, commoners, and slaves (Boas, 1890, pp. 823-830; 1891, p. 569; 1906, p. 242). Boas has been followed in this interpretation by Radin (1927, p. 322), Goldenweiser (1922, pp. 54-55), and others. Lowie (1920b, pp. 351, 353, 389) uses Boas' description of class structure on the Northwest Coast to refute the "palpable nonsense" of Lewis H. Morgan, who maintained that American Indian society was democratic.

Helen Codere, a relatively recent student of the Kwakiutl, tells us that Boas regarded Kwakiutl society as "classless," and that after 1920 "he described the Kwakiutl as classless" (Codere, 1957, pp. 474, 485). In "The Social Organization of the Tribes of the North Pacific Coast," (1924), however, Boas distinguishes five classes among the Bella Bella (p. 331), and speaks of "the nobility" (p. 329) and "aristocracy of the tribes" (p. 331). In 1937 Lowie asserts—again contravening Morgan's thesis of democracy in aboriginal America—that "slavery and class stratification were thoroughly established for British Columbia" (Lowie, 1937, p. 57; see a similar argument in Lowie, 1936b, p. 170). In an essay published in 1938 under the editorship of Boas, Julius Lips (1938, pp. 511-12) speaks of "chiefs, nobility, middle class, bondsmen, and slaves" among the tribes of the Northwest Coast, and cites Boas' 1924 paper on "The Social Organization. . ." as authority. In 1952, Herskovits, one of the most faithful followers of Boas, speaks of "a relatively stable class system composed of nobles, commoners, and slaves" among the Kwakiutl (1952, p. 476).

Boas' conception of the class structure of tribes of the Northwest Coast became fairly well established in American ethnology, but his description of their social organization in the terms of modern Western culture as nobles, commoners, slaves seems quite unwarranted. In Western culture this hierarchy was characterized by political subjection and economic exploitation by force of one class by another. In the Northwest Coast the so-called slaves were prisoners of war, and the "nobility" had no monopoly, or near monop-

oly, of political and economic power. Boas' caricature of Indian tribal society is exposed by Goldenweiser: ". . . the economic position and daily life of the . . . slave does not greatly differ from that of his owner. The slaves live in the houses with the other people, they eat with them, work, hunt and make war on a par with the others. It is only on occasions where social prestige and ceremonial prerogatives are involved that the disabilities of the slave become conspicuous" (Goldenweiser, 1922, p. 55, emphasis mine; see White, 1959a, pp. 199-202, for further discussion of this point).

"Every detail—linguistic, physical, archaeological and cultural—was, it seemed to him [Boas], grist for his mill" (Benedict, 1943b, p. 60). There are, however, many significant omissions in Boas' ethnographic work. We have already noted the case of play potlatching. Boas "presented relatively little material to work with on the more amiable side of Kwakiutl life. . ." (Codere, 1956, p. 336). "In almost none of Boas' writings or in those of George Hunt is the ribald in Kwakiutl life visible" (Codere, 1959, p. 69). He "neither recorded nor caused to be recorded much about informal behavior, as distinct from formal public affairs, myths, family histories, and such surely cultural matters" (Codere, 1959, p. 69). A few pages on terms of relationship in *Tsimshian Mythology* (1916, pp. 489-495) constitute his principal reference to this important subject. In the preparation of his monograph *Alcohol and the Northwest Coast Indians* (1954), Lemert states that he "searched his [Boas'] 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" (Lemert, 1956, p. 561).

Opinions understandably differ regarding the content and quality of Boas' ethnographic work on the Northwest Coast (see Ray, 1955 and 1956; Kroeber, 1956; Lowie, 1956). Ray, who has been much concerned with this area, judges Boas' work as follows:

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*] despite the fact that this was Boas' principal area of ethnographic research (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 [*sic*] and his presentation of this picture as representative of Kwakiutl life." Murdock (1949, p.

xiv) characterizes Boas' ethnography on the Kwakiutl as follows: "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 [i.e., *Social Structure*, 1949]."

Finally, we may observe that after decades of research and more than 5,000 pages devoted to the Kwakiutl (Codere, 1959, pp. 61, 62, 73), Boas never worked out and presented a synoptic picture of Kwakiutl culture. Lowie observes (1947, p. 313) that he never did "complete a single large-scale portrait of a tribal culture, not even of his beloved Kwakiutl." Furthermore, "it is not possible to present a synthesized account of Kwakiutl culture based upon Boas' works," in the opinion of Helen Codere, herself a student of this culture, and she doubts that Boas' unpublished *Kwakiutl Ethnography* "will fully realize his goal of synthesizing Kwakiutl culture" (1959, pp. 66, 73).

Boas' inability to grasp Kwakiutl culture as a whole and to present it intelligibly to others is a notable fact and requires some explanation. We have already shown that Boas did not really understand the social organization of the Kwakiutl, and without this understanding he could not comprehend Kwakiutl culture as a whole. One reason why he could not do this was his obsession with particulars. Very early in his life, he tells us: "I aligned myself clearly with those who are motivated by the affective appeal of a phenomenon that impresses us as a unit" (Boas, 1936, p. 137). "In ethnology all is individuality" (Boas, 1887b, p. 589); that phenomena "seen by the observer in overwhelming number" create "a confused impression" (Boas, 1887a, p. 139). "The general impression" that one gets of the Northwest Coast said Boas (1888b, p. 53) after his first field trip, "is that it is uniform," but he soon became impressed with its diversity. His "first impression" of the Kwakiutl "was that of bewildering confusion" (Boas, 1909, p. 307). He speaks of "the chaos of beliefs and customs. . ."; the "student stands aghast before the multitude and complexity of facts. . ." (Boas, 1898b, pp. 3-4). "The complexity of each phenomenon dawns on our minds. . . . Heretofore we have seen the features common to all 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. . ." (Boas, 1898b, pp. 3-4).

"To see what is general in what is particular and what is permanent in what is transitory is the aim of scientific thought," (A. N. Whitehead, n.d., p. 11). To Boas, however, "a detailed study of particulars seemed. . . more rewarding than the building of systems; . ." (Benedict, 1943a, p. 33). He preferred to look at the trees rather than the forest—or even to inspect branches and twigs rather than whole trees. In 1920 Boas returned to the complexity of Kwakiutl culture and to "obstacles to a clear understanding

of the social organization" (Boas, 1920, p. 111). He thereupon "tried to clear up the situation by recording the histories of a number of families *in all possible detail*" (ibid.; emphasis mine)—which only increased the confusion. In 1932, after almost fifty years of ethnological labors, he characterizes anthropology as "one of the sciences the interest of which centers in the attempt to understand the individual phenomena . . ." (Boas, 1932b, p. 612).

To understand "the particular" or part, one must understand the general or whole, if one is dealing with systems. But Boas did not see systems; he saw a "chaos of beliefs and customs," a "multitude and complexity of facts" before which "the student stands aghast." "The object of our science," Boas wrote almost at the outset of his career, "is to understand the phenomena called ethnological and anthropological . . . in their historical development and geographical distribution, and in their physiological and psychological foundation" (1887b, p. 588). These were his objectives in his labors on the Northwest Coast. He reconstructed the culture history of the area, and he provided psychological explanations of various institutions.

Almost everyone who has written about Boas has characterized him as "rigorous" in his thinking. He insisted on proof. "His unsparing mind," wrote Kroeber (1952a, p. 146), "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." "Boas did insist on discriminating between absolutely established fact and plausible conjecture" (Lowie, 1944b, p. 60). "The rigid requirements of 'scientific proof' were sacred to Boas. . ." (Kardiner and Preble, 1961, p. 159). Spier (1959, p. 154), Bunzel (1960, p. 403), and others have dwelt upon the same theme.

Boas did insist upon proof from others. He "challenged" Sir Arthur Keith "to prove that race antipathy is 'implanted by nature' and not the effect of social causes. . ." (Boas, 1931, p. 7). He questioned the value of theories of cultural evolution unless "proof is given that the same phenomena could not develop by any other method" (Boas, 1896b, p. 905). (This argument might also be used against the theory of biological evolution.) But the burden of proof rested lightly upon Boas' own shoulders. He concludes, on the basis of mythologic and meager archeologic evidence, that the Kwakiutl clan "was originally a village community" (Boas, 1897, p. 334). Many other statements of Boas are based on similarly tenuous evidence. 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 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, 1888c, p. 195). "It remains to substantiate what I have said [about transition from a paternal to a maternal stage of

development among the Kwakiutl] by telling the legends of a few clans" (Boas, 1897, p. 335).

Boas placed heavy reliance upon Indian traditions and upon assertions of informants: "Recent tradition, the historical truth of which cannot well be doubted, shows clearly. . ." (Boas, 1920, pp. 111-112).³ "According to their [the Tsimshian] own statements they [certain dances] were obtained by intermarriage with the Heiltsuk" (Boas, 1889b, p. 240). "The Kwakiutl state that this custom [ceremonial cannibalism] was introduced among them not longer than sixty years ago, and that it originated [sic] among the Heiltsuk" (Boas, 1899, p. 678).

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, because there are no relics [Tylor's "survivals"] of the former state. . . (1897, pp. 334-35).

Before finishing with this point he comes to "conclusive proof": "The fact that they [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). (We have already examined Boas' assumption that the Kwakiutl had reversed the "normal" course of social evolution and had proceeded from paternal to maternal organization.)

The most remarkable example of historical reconstruction offered 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" (1898a, p. 123). This seems to say that one goes about among the Bella Coola and the Bella Bella tribes, taking note of the physical appearance of their members. This observation then *proves* that "at one time" they "must have intermarried to a great extent," and as a consequence certain customs were borrowed by one tribe from another. But why not the other way around? Lowie's remark about "Boas' insistence on definite proof of cultural diffusion" (Lowie, 1947, p. 304) is a loyal tribute to his "revered teacher." But it falls somewhat short of biographical accuracy.

Boas' historical reconstructions are inferences, guesses, and unsupported assertions—his own or those of his informants. They range from the probable through the possible to the preposterous. Almost none is verifiable except in the very general assumption that any myth found distributed throughout a large multitribal area must, in all probability, have diffused from tribe to tribe. Boas' reconstructions, in particular and in detail, are

virtually beyond the possibility of verification. And his deduction of cultural diffusion, by way of intermarriage, from the physical appearance of two tribes, is little short of fantastic.

If all of his historical reconstructions on the Northwest Coast were proven valid beyond doubt, they would say only that cultural elements may, and often do, diffuse from one tribe or locality to another, a conclusion that is hardly new or profound.

Boas apparently believed that his historical reconstructions were 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*" (1896b, p. 907; emphasis mine). Indeed, Boas believed that one could actually *see* culture history—that is, if one were well-trained and looked hard enough. The "study of the present surroundings [of a tribe] is insufficient [to explain its culture]," he said (Boas, 1887b, p. 588); "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" (emphasis mine). The notion that his historical reconstruction might be as hypothetical, as conjectural as—and if anything, more unverifiable than—the evolutionists' hypotheses seems never to have entered Boas' mind. Lowie echoes Boas on this point, stating that Boas "stressed real history as a corrective to evolutionary schemes . . ." (Lowie, 1940a, p. 599).

Boas' penchant for positive and dogmatic utterance may have helped his disciples to believe that he insisted upon "absolutely established fact" and "strict proof." Phrases such as "can not be explained in any other way" (Boas, 1897, p. 344), "only one explanation of this fact is possible" (Boas, 1899, p. 672), "these are facts that cannot be disputed" (Boas, 1896a, p. 9), "But I insist. . ." (ibid., p. 10), are not uncommon in his writings.

Boas offered psychological as well as historical explanations of socio-cultural phenomena on the Northwest Coast, and these he made quite as easily as his historical reconstructions:

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 (1898a, pp. 123, 125).

Boas' theory does indeed "explain" the facts—but so does an Acoma myth "explain" why the swallow has a forked tail.

Boas' admirers extol his intellectual powers in general and his scientific mind in particular. Boas, "the faithful recorder was, above all, a thinker" (Lowie, 1947, p. 316), an "independent and erudite thinker" (Lowie, 1937, p. 151). "What marked him out from others and made him a leader was the union of critical acumen with a still higher quality, to wit, originality" (Lowie, 1944b, p. 60). Indeed, he played a "revolutionary role . . . in the history of anthropology," according to Goldenweiser (1933, p. 153). Ruth Benedict (1943a, p. 28), Marian Smith (1954, p. 111), C. K. Shipton (1943, p. 15), and Ernest R. Trattner (1938, p. 365) likewise speak of the "revolution" wrought by Boas.

Boas "seemed to personify the very spirit of science"; he was "the champion of scientific method in all anthropological research" (Lowie, 1947, p. 315; 1920b, p. vi). Goldenweiser (1941, p. 155) speaks of him as "the natural scientist that he was." And Naroll (1961, p. 391) tells us that "Franz Boas was ferociously [sic] determined to make anthropology a science"—and then repeats the much-used phrase about "rigorous testing."

In fact, Boas spurned primary and fundamental procedures of science; namely, classification and generalization. He was "fundamentally impatient of all classifications," according to Kroeber (1935, p. 544); he had a "deep-seated reluctance to generalize, summarize, or classify" (Kroeber, 1956, p. 152). "Under his [Boas'] stimulation," says Radin (1939, p. 301), "ethnologists set to work to describe facts 'as they really were,' to make no judgments and scrupulously to avoid generalizations." According to Lowie (1943, p. 184), Boas "abhorred facile generalizations and all-embracing systems." In another connection, however, Lowie could write: "We cannot blame Tylor for striving to bring the greatest possible number of phenomena under a single formula, for *that is the aim of all science*" (1948a, p. 122; emphasis mine).

As for original and creative thought, Lowie (1943, p. 184) has testified that "one might well read a thousand pages of his output without finding more than a faithful, intelligent collector of raw detail." Radin (1933, p. 253) tells us that under Boas' leadership "an exaggerated distrust of theories of whatsoever description" was engendered. Berthold Laufer (1930, p. 162), in a review praising one of Lowie's books, declared that he "would no longer give a dime to anyone for a new theory." Indeed, so great was the aversion to theory among the Boasians that Kluckhohn (1939, p. 333) was moved to observe that "to suggest that something is 'theoretical' is to suggest that it is slightly indecent."

We turn now from Boas' ethnographic publications to two books which he published with commercial publishers: *The Mind of Primitive Man*

(1911; rev. ed., 1938c) and *Anthropology and Modern Life* (1928; rev. ed., 1932a). *Race and Democratic Society*, a collection of addresses and articles, was published posthumously in 1945.

The Mind of Primitive Man is largely concerned with the subject of race and its relation to mental ability and to cultural development. It also contains an attack upon, and a rejection of, theories of cultural evolution. *Anthropology and Modern Life* consists of essays on various aspects of modern Western society and culture: race, nationalism, eugenics, criminology, education, etc. It ignores completely one of the most fundamental and important factors in modern culture, namely, the industrial and fuel revolution and its impact upon social, political, and economic institutions. The great cleavage in Western society between capital and labor is not touched upon. Phenomena such as the Paris Commune, the I.W.W. and the House of Morgan receive no mention. And the emergence and establishment of the Soviet system, as an instance of social evolution as well as revolution, find no place. In reviewing this book Sapir (1929) calls Boas "rigorous," but observes that he cannot "give himself."

Boas' distinctions between race, nationality, language, and culture were designed to oppose the racist doctrines of Gobineau and Houston Stewart Chamberlain, and of later writers such as Madison Grant (*The Passing of the Great Race*, 1916) and Lothrop Stoddard (*The Rising Tide of Color*, 1920), and no doubt they had a salutary effect in certain quarters. But we may well question the value of his contribution to the problems of race conflict.

Boas, who was "of Jewish extraction" (Lowie, 1947, p. 310), had been intensely concerned with anti-Semitism since his "formative years" (Kluckhohn and Prufer, 1959, p. 10). He wrote voluminously on racial problems, as did some of his prominent students. As I have argued elsewhere (White, 1947a), however, he never got to the heart of the matter. Much of his argument was based upon anthropometry and anatomy, which were largely irrelevant because race prejudice and conflict do not arise from lack of knowledge of facts of this sort. In addition to citing anatomical evidence, Boas postulated a psychological basis of race prejudice: "The prejudice is founded essentially on the *tendency of the human mind* to merge the individual in the class *to which he belongs*, and to ascribe to him all the characteristics of his class" (Boas, 1945, pp. 77-78; emphasis mine). Boas did "not wish to deny that the economic conflict may be a contributing cause. . . . It would, however, be an error to seek in these sources the fundamental cause of the antagonism; for the economic conflict . . . presupposes the social recognition of the classes" (ibid., p. 79). What then is the remedy? The "only fundamental remedy . . . is the recognition that the Negroes have the right to be treated as individuals, not as members of a class" (ibid.). This is undoubtedly true, but it is also a tautology. "Strong

minds" might "free themselves from race prejudice . . ." but "the weak-minded will not follow their example" (ibid., p. 80). Education, Boas reasons, cannot "overcome the general human tendency of forming groups that in the mind of the outsider are held together by his emotional attitude toward them" (ibid., p. 79). What, then, can eradicate the conflict between races? Boas' answer was miscegenation:

Intermixture will decrease the contrast between the extreme racial forms, and in the course of time, this will lead to a lessening of the consciousness of race distinction. If conditions were ever such that it could be doubtful whether a person were of Negro descent or not, the consciousness of race would necessarily be much weakened. In a race of octoroons, living among Whites, the color question would probably disappear (ibid., p. 80).

It would seem therefore, to be in the interest of society to permit rather than to restrain marriages between white men and Negro women. . . (ibid., p. 80).

Thus it would seem that man being what he is, the Negro problem will not disappear in America until Negro blood has been so much diluted that it will no longer be recognized just as anti-Semitism will not disappear until the last vestige of the Jew as a Jew has disappeared (ibid., p. 81).

Boas' reasoning is, of course, flawless: if one cannot distinguish a Negro from a white, or a Jew from a Gentile, then prejudice and conflict between them will cease to exist. However, advocacy of intermarriage between Negro and white might well accentuate, rather than diminish, race prejudice and conflict; nothing arouses the hostility of white supremacists more than the prospect of intermarriage. This is recognized by Negro leaders, one of whom recently sloganized: "We want to be the white man's *brother*, not his brother-in-law." An explanation of race conflict along social, political, and economic lines might have had more effect, but this Boas never offered.

Boas had virtually a closed mind, if we may trust Kroeber's judgment on this point:

Those [ideas] that he [Boas] had grown up with and accepted, that he had originated, dealt with, or appropriated—that he held some ownership in, in short—he adhered to unflinchingly and persistently and would have died for. Other ideas, those that came into his orbit from outside or late, that impinged on his 'tree' of personality, instead of having been created or annexed by it, he tended to react to with reserve or even suspicion (Kroeber, 1956, p. 156).

Smith remarks: "Born in the same year which saw publication of the *Origin of Species* [he was in fact born one year earlier], Boas could profit from the first great intellectual response which greeted man's entrance into the world of nature" (1959, p. 46). But Boas "was not much interested in biological evolution" (Kroeber, 1956, p. 156); Kluckhohn and Prufer (1959, p. 22) speak of his "relative lack of interest in Darwinian evolution." And he was "sceptical about Mendelian heredity" (Kluckhohn and Prufer, 1959, p. 22). According to Kroeber (1943, p. 7), Boas "demonstrated that some human traits are not transmitted according to simple

Mendelian rules." He "flatly rejected" historical linguistics (Kroeber, 1956, p. 156). Boas was "consistently anti-Freudian" (Kroeber, 1956, p. 154); Bunzel (1960, p. 403) speaks of his "unalterable opposition to Freud and psychoanalysis." Boas himself gives us the best picture of his attitude toward "ideas that he had not grown up with." He speaks of the "doubtful—and in my mind fanciful—interpretations of psychoanalysis" (1938a, p. 666). He also observed that "the elaborate theories of psychoanalysis seem also unnecessary for the purpose of explaining the wonderful elements of folk tales or of mythological figures. The free play of imagination operating with everyday experience is sufficient to account for their origin" (1938b, p. 611). Radin observes:

Of the great intellectual events in Boas' lifetime, assuredly the two most significant were the theory of evolution and the economic interpretation of history. To the first Boas always took a prevailingly antagonistic position. The second he never so much as mentioned until the 1938 edition of 'The Mind of Primitive Man.' There he dismissed it in a paragraph as of only secondary importance. . . . Marx and Engels are never referred to (Radin, 1939, p. 303).

Although Boas was "not much interested in biological evolution," he was intensely concerned with cultural evolutionism. The evolutionist theory established in ethnology by Tylor "was challenged, notably by Boas . . . and a good part of his energies and those of his school had to be devoted to disproving it" (Radin, 1933, pp. 3-4). Radin also reports that "to all Boas' disciples Morgan has since remained anathema and unread" (1939, p. 303). "But if Boas and his school rejected the developmental schemes of Tylor and Morgan this must, in no sense, be ascribed to the inadequacies and crudities of those schemes, but rather to the fact *they rejected all developmental sequences*" (Radin, 1939, p. 303; emphasis mine). In a laudatory essay, Father Williams (1936) has much to say of the "initiative and indefatigable effort" of Boas (p. 196) in "the attack on cultural evolution" which he led "for more than a quarter of a century" (pp. 199-200). The harm done to cultural anthropology by this campaign, waged by Boas and his disciples for years against one of the most fundamental and fruitful theories in all science, now seems incalculable (White, 1959b).

Boas never accepted the culturological point of view as set forth by Tylor in Chapter I of *Primitive Culture*, or by Durkheim in *The Rules of Sociological Method*, and I find nothing in his writings which indicates that he ever comprehended it. When Kroeber expressed this point of view in "The Superorganic" (1917), Boas observed that "it seems hardly necessary to consider culture a mystic entity that exists outside the society of its individual carriers and that moves by its own force. . . . The forces that bring about the changes are active in the individuals composing the social groups, not in the abstract culture" (Boas, 1932a, pp. 245-46). It seems fairly clear from other statements made by Boas that he never grasped the concept

of culture as a distinct order of phenomena which require for their interpretation a distinct science:

Thus an examination of our problems suggests that the whole group of anthropological phenomena may be evanescent, that they may be at bottom biological and psychological problems, and that the whole field of anthropology belongs either to the one or to the other of these sciences . . . anthropology will become more and more a method that may be applied by a great number of sciences, rather than a science by itself (Boas, 1908, pp. 7, 10).

"Language, invention, art, religion, social organization and law" were "mental phenomena" to Boas (1904, p. 513), and the "discovery of a system of the evolution of culture" was a psychological problem (Boas, 1904, pp. 521, 517). "It has never been sufficiently realized," says Benedict (1943a, p. 31), "how consistently throughout his life Boas defined the task of ethnology as the study of 'man's mental life' . . ."; he conceived "of the study of culture as a 'mental' science" (Herskovits, 1953, p. 50). Boas ventured "his first definition of it [culture] at the age of seventy-two . . ." (Kroeber and Kluckhohn, 1952, p. 151); and this definition—or rather conception—was so phrased as to include subhuman species (Boas, 1930a). Culturology lay well beyond the grasp and comprehension of Franz Boas.

We find little evidence of intellectual growth in Boas' published writings over a period of fifty years. When he went to British Columbia in 1886 he had little qualification for scientific work in ethnology. He had a background in physics, mathematics, and geography. He explored anthropological literature, apparently mostly German. Authors cited in his essay, "The History of Anthropology" (1904) are principally German—Herder, Klemm, Bastian, Ratzel, Wundt, Steinthal, George Gerland (mentioned four times). Also cited are Spencer and Tylor; Quetelet and Tarde—but not Durkheim; and only one American, Daniel G. Brinton. There is little indication that Boas was influenced appreciably by what he read. His conception of ethnology—its philosophy, goals, and techniques of investigation—was already formed in 1887-88, as is made clear by "The Study of Geography" (1887), "Museums of Ethnology and their Classification" (1887), and "The Aims of Ethnology" (1888). He never deviated from this conception in later years. Indeed, as Ruth Bunzel, one of his disciples, has said: "So consistent was his theoretical position that it is frequently hard to tell whether a paper was written in 1888 or 1932" (Brunzel, 1960, p. 404).

"With the death of Franz Boas," said Kroeber (1943, p. 5), "the world lost its greatest anthropologist." There have been many who have thought that Boas was "the greatest." In what did his greatness consist? A satisfactory answer is difficult to find, and this difficulty is experienced by his followers as well as others. "It has long been notoriously difficult to convey the essence of Boas' contribution in anthropology to non-anthropologists"

(Kroeber, 1943, p. 24); ". . . no label fits him." Radin (1933, p. 19) has observed that "others, besides myself, have found it difficult to define Boas's position." Kroeber undertook to interpret Boas in "History and Science in Anthropology" (1935), to which Boas (1936, p. 137) replied: "I wish to express my complete disagreement with his interpretation." This difficulty, this uncertainty, is perhaps not surprising inasmuch as Boas himself is described as being "at a loss to explain his position in terms intelligible to the members of schools" (Kroeber, 1935, p. 541).

The disciples differ occasionally among themselves in their interpretation of Boas' work. For example, was it historical or not? According to Goldenweiser (1933, p. 154), "Boas's outstanding contribution was the historical point of view. . . ." Herskovits (1953, p. 59) says that Boas emphasized "again and again" that "the task of reconstructing history" was "the basic problem of anthropology." But Radin (1933, p. 17) declares flatly that "Boas's method is fundamentally unhistorical"; his approach "encouraged a militant antagonism to historical reconstructions" (Radin, 1939, p. 302). In Kroeber's opinion, "Boas has never really followed the historical method except in a rather narrow, special sense" (1935, p. 541); his "work also is now recognized as mainly lacking specific historic content or result. . ." (1946, p. 8).

Boas' students have not been very specific concerning his achievements. "He made no one great summing discovery . . ." (Kroeber, 1943, p. 24). His "greatness rests not on some one startling discovery . . ." (Lowie, 1943, p. 183). It depends instead upon "the totality of his output, the total impact of his personality [sic] upon the young science to which he turned. . ." (ibid.).

Many of Boas' followers speak, however, of the great quantity of his writing. Herskovits (1953, p. 62) tells us that "the massive achievement of Boas in the field of descriptive ethnography would alone be sufficient to give him an outstanding place in his science." His "contributions overwhelm the spectator by their sheer massiveness" (Lowie, 1944b, p. 61). "The output of Boas . . . is staggering in its immensity and diversity" (Trattner, 1938, p. 365). And, says Kroeber (1943, p. 22), "in all his enormous output it is difficult to find even minute instances of error of either fact or procedure."

Boas' output was indeed great, even when one considers that 4,165 pages were contributed by his Indian co-workers, Hunt and Tate. One may ask if mere magnitude of output is a valid criterion of achievement in the field of science? If it were, how would Boas compare with such prolific writers as Sir James Frazer, who wrote many books in addition to his 12-volume *The Golden Bough*, or Father Wilhelm Schmidt, with his 12-volume *Der Ursprung der Gottesidee* plus a host of other books, articles, and reviews?

Boas' enormous energy excited the wonder and admiration of his followers. "As was apparently his custom throughout his life, almost every moment

of an eighteen-hour day, seven days a week, was filled with activity" (Mason, 1943, p. 63). "No student of culture has ever been more tireless" (Benedict, 1943b, p. 60). "Dr. Boas was endowed with a tremendous physical energy which permitted driving himself ruthlessly [sic] throughout his long life" (Spier, 1943, p. 125). A psychiatrist, Abram Kardiner, seems to have thought this ruthlessness significant. He wrote: "He [Boas] worked like a man fighting for his life. He reminds one of the athlete who compensates for a lack of natural talent by hard work, determination, and courage, and often succeeds where the natural athlete fails" (Kardiner and Preble, 1961, p. 134).

We have noted that Boas has been erroneously described as the initiator of scientific field work in ethnology. Many accounts also refer to his lengthy work in the field, but an examination of the facts presents another picture (see White, 1963a, p. 10). If we omit his year in Baffinland, where "geographical work . . . took most of my [Boas'] time" (Boas, 1884, p. 271), and his year in Mexico, where his ethnographic researches appear to have been very limited, the amount of time Boas spent in the field is about 33½ months, 29½ of which were on the North Pacific Coast, an average of less than one month per year between his first trip in 1886 and his last in 1930-31.

Kroeber (1959, p. vi) attempts to describe Boas' fundamental contribution: "It is perhaps Boas' fundamental contribution to science and civilization: to treat human data with rigorous scientific method on a scale unattempted before." "It was Boas who first made us all able to see and deal better with process as such. This is his great contribution; this and the unswerving rigor of his critical standards" (Kroeber, 1935, pp. 568-69). Kroeber gives no explanation of the meaning of "process."

Other students of Boas have also credited him with specific contributions, although their statements are not always comprehensible or supportable. "The great revolution which Boas introduced in ethnological methodology," writes Benedict (1943a, p. 28), "was the inevitable consequence of the way in which he phrased the basic problem." What the "basic problem" was or how Boas phrased it are not made clear. "Thus, it is one of Boas' outstanding achievements to have directed attention to the primitive artist's psychological position toward his art" (Lowie, 1936a, p. 304). Leslie Spier (1959, p. 154) states: "In many ways the most fundamental of Boas' contributions was the rigor of scientific method: careful analysis, caution, and convincing demonstration."

The list of fundamental contributions with which Boas is credited does not seem impressive: a great, but unspecified, revolution which he introduced; aid in understanding "process as such"; directing attention to the primitive artists' psychological position toward his art, unsupported claims of the unswerving rigor of his critical standards and his convincing demon-

strations; and the unfounded statement that he initiated scientific field work in ethnology.

One of the most revealing statements was made by the follower of Boas who perhaps loved him most, Robert H. Lowie (1937, p. 151): "It is one of the most difficult tasks to expound Boas' greatness to those who have failed to come into personal contact with him." "To readers who had never met him [Boas]," Lowie later wrote (1943, p. 184), "his influence remained an enigma." Kroeber (1943, p. 21) observes: "Whether the greatness of Boas lay more in his intellect or his character, it would be difficult to say." Has personal acquaintance been necessary to appreciate the greatness of Newton, Galileo, Einstein, Darwin, Descartes, Freud, Tylor, Spencer, Morgan, or Durkheim?

One of Boas' followers, Marian Smith, states: "It is not surprising to find Boas in a series with such figures as Marx, Freud and Einstein" (Smith, 1954, p. 111). Rabbi Trattner places Boas alongside Darwin, Marx, Freud, Copernicus and Einstein in his essay on Boas in *Architect of Ideas*. Lowie (1944b, p. 64) once likened Boas to "a Michael Angelo and a Beethoven," and, in another writing, rhapsodizes:

In the anthropological science of his time Boas has been the great exemplar, fearless of authority, relentlessly self-critical, driven by a sacred thirst to ever new Pierian springs, gaining ever deeper insights into the nature of man (Lowie, 1940a, p. 599).

Boas' followers tended to ascribe greatness to almost anything he did. Kroeber calls *The Central Eskimo*, Boas' first ethnography, "monumental" (1943, p. 9). Bunzel (1960, p. 246) describes it as ". . . still one of the ethnographic classics and a model of lucidity." This monograph is a simple, straightforward, and 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 folk tales and traditions, and 10 to poetry and music. It treats many aspects of Eskimo culture superficially or not at all. In one paragraph, for example, Boas states that: (1) women do housework in front of lamps; (2) women take care of puppies; (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 entirely omitted from *The Central Eskimo* is relationship terms. Eggan observes (1960, p. 180): ". . . not only did [Boas] not collect kinship terms, but for some reason did not utilize those published by Morgan a decade earlier from the same region [although Boas drew extensively upon published works on the Eskimo and the Arctic in writing his monograph]."

The Central Eskimo (1888) is a much smaller work than Morgan's *The League of the Iroquois*, published seven years before Boas was born, and is

much less comprehensive in its scope. If *The Central Eskimo* is monumental, what are we to call Matilda Coxe Stevenson's *The Zuni Indians* (1904), or *The Omaha Tribe* (1911) by Alice C. Fletcher and Francis LaFlesche?

The most obvious defects and shortcomings of Boas have been defended by his admirers and sometimes labeled as virtues. His literary style affords an interesting example. The writings of Boas contain many stylistic counterparts of the sample passage that follows:

Although we must lay stress upon the subjective character of the groups that we isolate and make the subject of our studies, it is important to bear in mind that the processes by which extended groups of mental activities are systematized by retrospective thought (that is by reason), occur also as an ethnic phenomenon in each social unit, so that the unification of heterogeneous material that we attempt as an ill-founded scientific method, is only one aspect of a wide range of ethnic phenomena, the essential feature of which is the remodeling of activities, thoughts, and emotions under the stress of a dominant idea (Boas. "The Origin of Totemism," as reprinted in *Race, Language and Culture*, pp. 318-19).

"It would be unfair to comment on his [Boas'] English style," says Lowie (1944b, p. 63), "since he wrote mainly in an acquired language. . . . But even in his native tongue we find a characteristic baldness that contrasts strangely with the rich German diction . . . of Berthold Laufer . . ." The difficulty which some have had in understanding Boas' prose is not due to "lack of lucidity," says Kroeber (1935, p. 543); "I doubt whether there is an argument or a sentence by Boas in print whose meaning is not perfectly clear and exact, providing it is approached with reasonable intelligence."

Kroeber elsewhere admits, in effect, "an absence of style" in Boas' writings, but he also provides a justification for it and transforms this shortcoming into a virtue. He remarks that Boas "would undoubtedly have answered that style belonged to literature and he was doing science. His appreciations in literature as well as music show that he was far from devoid of aesthetic sense; but his basic principle of control would have led him to check absolutely any impulses toward literary quality in his work" (Kroeber, 1943, p. 25). Kroeber also states that one could find functionalist interpretation in Boas' work "were one inclined to look for it there, under the overlay of piled up fact, demonstration, self-criticism, and stern repression of impulses toward aesthetic form" (Kroeber, 1931b, p. 248). Indeed, Kroeber (1943, p. 16) assures us that Boas "was too intent on intellectual substance; and even a partial concession would have seemed to him weakness or a sort of prostitution."

Herskovits (1953, p. 9) speaks of "the leanness and austerity of [Boas'] prose," and of Boas' "disdain of any of those devices that ease the effort of the reader. . . . If important conclusions were to fall in the middle of a paragraph deep in a chapter of a book, or at the center of an article, it was the business of the reader to find them for himself." As Ray (1955, p. 139)

has observed, Herskovits "seems to make a definite virtue of it in the sense of its serving as an ordeal for the users of the material."

Other commentators on Boas' literary style have been less charitable. Boas "wrote English in a way which proved that German was his native tongue, and his cumbersome prose was loaded down with 'insights,' 'outlooks,' 'standpoints,' and so on" (Chase, 1959, p. 20). Morris Cohen (1912, p. 98), reviewing *The Mind of Primitive Man*, speaks of "a marked carelessness as to the form that cannot but detract from its usefulness as a popular exposition. Occasional slips of syntax (e.g., p. 143), pronouns without antecedents (e.g., last line p. 29), the use of *but* for *and* . . . are indications of a certain looseness in the use of language that certainly does not help to give point to the argument used."

Boas' writings may accurately be described as disorderly. He describes house types in a chapter on "The Clan Legends" in *The Social Organization and Secret Societies of the Kwakiutl Indians* (pp. 366-67). Probably his most succinct reconstruction of Kwakiutl culture history is presented in "The Growth of Indian Mythologies." Reviewing *Dakota Grammar* by Boas and Deloria, Voegelin states (1943-44, p. 268): "Everything is described everywhere. Phonetic description is interrupted by morphological detail; the morphological sequel of stem types is interrupted by seven pages devoted to the behavior of terminal vowels. . . . All of this is typical of Boas . . ."

The image of Boas presented by his followers bears little resemblance to reality, as we have seen. But it remains to touch upon a myth about him which is characteristic of imaginative image-building. It is said that Boas was able to transform ethnology into a rigorous science because he was trained as a physicist.

The rigor of scientific method, moreover, was forcibly [sic] ushered in through the labors and personality primarily of one man, Franz Boas, whose training in exact science, combined with a mercilessly critical mind, fitted him admirably for that muckraking work in the domain of anthropological vagaries . . . (Goldenweiser, 1921, p. 60); . . . by his early studies in the physical sciences and mathematics . . . (Goldenweiser, 1933, p. 153).

"It was his original training in rigorous method as a physicist . . . to which we owe our debt for most of the scientific foundations of anthropology" (Spier, 1943, p. 109). "To sum up," says Radin (1933, p. 60), "Boas's viewpoint is essentially that of the physicist or mathematician . . ." (see also p. 9). Kroeber observes: "From physics Boas brought into anthropology a sense of definiteness of problem, of exact rigor of method . . ." (1935, p. 540; see also pp. 539, 541, 544, 545). According to Kroeber (1943), Boas was a "spiritual physicist" (pp. 6, 25); "his beginnings as a physicist heavily determined his whole intellectual career" (p. 7); "all his life [he] thought like a physicist" (p. 22).

Boas left physics for physical and cultural geography while still a student. In "The Study of Geography" (1887) he tells us why he did so. Many years later he had to remind Kroeber of his change of attitude and objective:

It is quite true that as a young man I devoted my time to the study of physics and geography. In 1887 I tried to define my position in regard to these subjects, *giving expression to my consciousness of the diversity of their fundamental viewpoints*. I aligned myself clearly with those who are motivated by the affective appeal of a phenomenon that impresses us as a unit. . . . In other words the problem that attracted me primarily was the intelligent understanding of a complex phenomenon. When from geography my interest was directed to ethnology, *the same interest prevailed* (Boas, 1936, p. 137; emphases mine).

Thus, for virtually all of his adult life as a scholar, Boas had followed not the road of physics but of something far different from the "exact sciences." Among Boas' followers only Lowie seems to have seen "the supposed physicist masquerading in ethnographer's clothing . . ." (Lowie, 1940a, p. 598; see also p. 599). "True enough," says Lowie (1944b, pp. 59-60), "Boas once devoted himself to physics and mathematics; but the astonishing thing is precisely his escape [sic] from their specific implications when he turned to his later interest. Thus, whereas physics aims at impressive generalizations, Boas very early recognized that there could be legitimate scientific aims apart from the discovery of laws; *and this conviction deepened with the years*" (emphasis mine).

It is Sigmund Freud who helps us to understand Boas—and his disciples as well. Freud has observed:

"And now it begins to dawn upon us that all the features with which we furnish the great man are traits of the father, that in this similarity lies the essence, which so far has eluded us, of the great man. The decisiveness of thought, the strength of will, the forcefulness of his deeds, belong to the picture of the father; above all other things, however, the self-reliance and independence of the great man; his divine conviction of doing the right thing, which may pass into ruthlessness. He must be admired, he may be trusted, but one cannot help being also afraid of him" (1939, pp. 173-74).

It is not without significance that Boas was generally called "Papa Franz," at least by his female admirers. Kroeber, in his later years, came to recognize Boas as "a powerful father figure," "a true patriarch" (Kroeber, 1956, p. 156). The Boas of the popular image has also some of the characteristics of the divine father. Kroeber has asserted that he was "literally worshipped" by some of his followers. "One thing Boas did not permit: that anyone speak for him" (Herskovits, 1953, p. 24); ". . . Boas, who was a law unto himself" (Strong, 1953, p. 394).

The picture now becomes clear; the enigma of Boas' reputation is solved. Boas has all the attributes of the head of a cult, a revered charismatic teacher and master, "literally worshipped" by disciples whose "permanent

loyalty" has been "effectively established." We can now understand such things as calling *The Central Eskimo* "monumental," and the inclusion of Boas in a series with Marx, Freud, and Einstein. We see in a new light the assertion, so contrary to fact, that Boas' unsparing mind insisted on definite proof. We face what Freud (1930, p. 36), in characterizing religion, has called a "delusional transformation of reality." Boas' personality and character are so confused with his anthropological work that his disciples cannot distinguish one from the other. Only a few of Boas' followers, of whom Kroeber and Lowie stand out most prominently in this respect, appear to have sensed this, and only vaguely.

It is sometimes said of Boas that he might not have been so great a scientist as represented but he produced exceptional students. Boas had a great many students during his forty years at Columbia University. Most of these, however, have achieved little distinction in anthropology. Some did not continue in the field. The students who achieved the greatest distinction, with but few exceptions (notably, Benedict, who received the Ph.D. in 1923; Herskovits, in 1923; Mead, in 1929) completed their study for the doctorate in the early years of the present century. These are Kroeber, 1901; Lowie, 1908; Sapir, 1909; Goldenweiser, 1910; Radin, 1910; and, somewhat later, Spier, 1920. These early scholars became professors of anthropology or established themselves otherwise professionally in the field in a day when anthropologists were few. Elsie Clews Parsons met Boas in 1915 after a noteworthy career in sociology and became "his devoted disciple" (Jacobs, 1959, p. 123). Berthold Laufer (1874-1934), a German-born anthropologist who was associated with Boas both at the American Museum of Natural History and at Columbia University, might be called an associate member of the Boas school (see his reviews of Lowie's *Culture and Ethnology* and *Are We Civilized?*).

Let us have another look at the Boas school, the small, compact group of scholars that were gathered about the leader. The earliest were principally foreign-born or the children of immigrants. Goldenweiser was born in Kiev; Radin in Lodz; Lowie in Vienna, and Sapir in Pomerania. Kroeber's father was born in Cologne, and his mother was American-born, of German antecedents. All were fluent in the German language. Like Boas, most were of Jewish ancestry. John Sholtz, writing in *Reflex: a Jewish Magazine* (Vol. 6, p. 9, 1935) has observed that "in the one field of anthropology alone, it is interesting to note the disproportionate position held by Jewish scientists in this country. Men like Boaz [sic], Golden weiser [sic], Lowie, Radin are easily the leaders in the field."⁴

A school by definition tends to be a closed society or group. Kroeber tells of how George A. Dorsey, an American-born gentile and a Ph.D. from Harvard, tried to gain admittance to the select group but failed:

George Dorsey once confided to me . . . that Boas was like a tree all over which there grew as fruits those whom he admitted to his friendship—to participation and identification, in modern terms. But those not so admitted would never be close to Boas, and Dorsey himself, for all his trying, remained wholly shut out. . . . It shows Boas functioning as a powerful father figure, cherishing and supporting those with whom he identified in the degree that he felt they genuinely were identifying with him, but, as regards to others, aloof and probably fundamentally indifferent, coldly hostile if the occasion demanded. A true patriarch, in short . . . (Kroeber, 1956, p. 156).

Clark Wissler, also an American-born gentile, was a student of Boas in the formative years of the "school," but "broke personally with him about 1906" (Kroeber and Kluckhohn, 1952, p. 151). Although much of Wissler's work (e.g., his studies of the Blackfoot) was like that of Lowie, Kroeber, and Spier on Great Plains tribes, he never became a member of the school. Oral tradition in American anthropology has it that a clash of personalities and temperaments between Boas and Linton caused the latter to leave Columbia and go to Harvard.

Another characteristic of a school is that it tends to ignore, attack, or criticize adversely those not belonging to the group:

Boas 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: see the reviews in *Race, Language and Culture* of Ehrenreich, Ripley, Dixon, Graebner, Locher, MacCurdy. He used the material or results of others sparingly, except for his students and followers—and these he referred to very generously . . . (Kroeber, 1956, p. 156).

Boas and the members of his school virtually ignored the work of Émile Durkheim and his co-workers, although they were contemporaries. Many works of the Boas group were reviewed, however, in *L'Année Sociologique*.

In particular did Boas ignore, or treat lightly, American anthropology prior to 1900 despite the fact that—in my opinion—anthropology was the only science of the nineteenth century in which the United States equalled—and I believe, excelled—Europe. In his address, "The History of Anthropology" (1904), Boas mentions only one American anthropologist: Brinton. He makes no mention of the founding of the American Ethnological Society in 1842, the American Antiquarian Society, the Archaeological Institute of America, the Bureau of American Ethnology, the Smithsonian Institution, or the anthropology section in the American Association for the Advancement of Science.

This attitude toward American science and scholarship is understandable. Lowie has written about the attitude of "educated Germans, Forty-Eighters and those of similar convictions" towards the United States (1948b, pp. 284-85). He quotes a letter written by Friedrich Kapp, who had "fled from Germany [during the revolution of 1848-49] to New York," to his intimate

friend, the philosopher Ludwig Feuerbach, which, Lowie says, "expressed sentiments that may be regarded as typical of a whole class." He strongly advises his friend against coming to America:

A German peasant who turns farmer cannot make a more fortunate choice; an academically educated man degenerates spiritually, rarely prospers materially, and at best creates a carefree future for his children. . . . Your fame as a writer would rather hurt than help you. . . . If you are through with Europe in every, in every respect, if you are convinced that it has nothing whatsoever to offer you, then come here; otherwise under no condition.

In a letter to one of Boas' relatives, Theobald Fischer, whom Boas "followed from physics into geography" (Kroeber, 1956, p. 158), "urges at length that Boas make his professional career in Germany (in spite of being a Jew) rather than in 'the rotten Yankee United States'" (Kluckhohn and Prufer, 1959, p. 9).

Scholars outside the field of anthropology have understandably echoed the evaluations of Boas made by his followers. An anthropologist tells us, however, that this is not always the case. "Tell an acute psychologist, economist, sociologist to read Boas' article *Anthropology* in the *Encyclopaedia of the Social Sciences*," said Clyde Kluckhohn, "and—it is an induction from my experience—he comes away disappointed and feels empty-handed" (Kluckhohn, 1943, p. 210).

The School of Radcliffe-Brown

(Alfred Reginald Radcliffe-Brown was born in England in 1881. He was educated at Trinity College, Cambridge. In 1906 he went to the Andaman Islands where he engaged in ethnological field work for two years. In 1910 he did field work in Australia. He occupied chairs of social anthropology at University of Capetown, 1921-26; at Sydney University, 1926-31; and at the University of Chicago, 1931-37. In 1937 he went to Oxford where he taught until his retirement in 1946. Among his honors were the Rivers and Huxley Memorial medals of the Royal Anthropological Institute, of which he was president in 1939-41. He died in London in 1955.)

Meyer Fortes (1949, p. v) asserts that Radcliffe-Brown "never countenanced the growth of an 'ism' or school based on his theories." Eggan and Warner (1956, p. 546) tell us that he was "not interested in disciples." Radcliffe-Brown himself once observed that there is no place in social anthropology for schools; "It is not . . . [a teacher's] business to make disciples" (Radcliffe-Brown, 1940a, p. 1). He also asserted that "this Functionalist School does not really exist; it is myth invented by Professor Malinowski" (*ibid.*). But there is abundant evidence that Radcliffe-Brown did gather about him a group of disciples and that they did constitute a school.

The question of "membership" in the Radcliffe-Brown school is complicated by the fact that many of Radcliffe-Brown's students were, at one time or another, students of Bronislaw Malinowski (1884-1942) also. Each was a charismatic teacher and each had his own following. But, says Elkin (1956, p. 246), "some [students], personally influenced by him [Radcliffe-Brown] in their early years of anthropological endeavour, became Malinowski functionalists as they passed through the latter's seminars, but after Malinowski's death they transferred their allegiance [sic] back to Radcliffe-Brown."

Shortly after Radcliffe-Brown came to the University of Chicago, a shrewd American observer, Ruth Benedict, remarked upon his "disciplinism": "As it stands, I don't think Brown is fighting for good work over against bad, but for work done by disciples over against work done by non-disciples" (Mead, 1959b, p. 327). In Australia, Elkin, commenting upon the assertion that Radcliffe-Brown "never countenanced the growth of an 'ism' or school based on his theories," observes: "But he did give rise to 'cults' of himself, so that he became the oracle, almost infallible, to its members, that is to those who became personally attached to him. Then no praise could be too great to be bestowed on him; whatever he did was for them in the superlative degree. The cult depended upon his presence, not on his writing or direct contribution" (Elkin, 1956, p. 246). More than one observer has commented upon the importance of personal contact between Radcliffe-Brown and his students. His "major interest has been in conveying ideas directly to students and colleagues by personal contacts" (Evans-Pritchard and Eggan, 1952, p. v). He "was primarily interested in conveying ideas directly through personal contacts" (Eggan and Warner, 1956, p. 546).

"As a teacher he [Radcliffe-Brown] is unrivalled," wrote Fortes in 1949 (p. v); he had a "gift for imparting to students the thrill of new discovery and the desire to join [sic] in the task of further research." Elsewhere Fortes has observed:

As a teacher, Radcliffe-Brown excelled. He warmed to an audience, especially if it was young. . . . He was unfailingly helpful to younger students. He read their manuscripts, gave them advice, and made fertile theoretical suggestions for their work, without stinting time or patience. . . . His exceptional ability to expound difficult problems with ease and clarity and his delight in systematic analysis were most happily expressed in his lectures, or better still in the informal sessions with friends and pupils over drinks which he loved. . . . He took the greatest pleasure in good food, good wine and, most of all, good conversation. . . . He was gentle with his friends and happy in their affection. For a man so detached in his personal relationships and his philosophy of life, his bonds with them were very close (Fortes, 1956, *passim*, pp. 152-153).

Elkin (1956, p. 246) speaks of the "Chicago and English 'cult groups'" which presented Radcliffe-Brown with volumes of essays, "thus making readily available the selected words of the 'master.'" These three books are

not only highly valuable in themselves, but also are a remarkable tribute to the attachment to, and almost veneration of, Radcliffe-Brown felt by those who knew him well." Radcliffe-Brown "had not found in Sydney that fellowship, or rather that cult-position, which his soul seemed to crave," says Elkin (1956, p. 243); "unsettled, 'frustrated' and irked, Radcliffe-Brown once again shook the dust (this time the Sydney dust) off his feet, and went to a sphere where he would be, and was, appreciated . . ." (ibid., p. 244).

Radcliffe-Brown was well fitted to become the leader of a school—or the head of a cult, as Elkin puts it. "He [Radcliffe-Brown] was strikingly handsome and a virile personality," says Firth (1956, p. 289); "his conversation was forceful and brilliant." Fortes (1956, p. 151) remarks that "his [Radcliffe-Brown's] brilliance as a lecturer, his wide learning, his connoisseurship in the arts . . . his distinguished bearing, attracted notice both inside and outside the University. . . . He quickly became one of the best-known academic personalities of South Africa."

The first personal pronoun singular was much used by Radcliffe-Brown: "I was led to formulate the following law: . . ." (1929, p. 306); "Thirty-seven years ago . . . I formulated briefly a general theory . . ." (1945, p. 2; but, he goes on to say, this theory had been held by a Chinese philosopher in the third century, B.C., and by several European scholars who preceded him). "My view of natural science . . ." (Radcliffe-Brown, 1940a, p. 2). In many ways, some explicit, some subtle, he gave the impression that much of his learning and science had originated with him. He had difficulty in "distinguishing between first-hand and second-hand knowledge. This intellectual deafness was a reflex of his egocentrism—all that he learnt became an integral part of himself and was fitted into his own personality. One result was sometimes a propensity to lecture people upon their own subject" (Firth, 1956, p. 296; "but," adds Firth, "his conversation had great point and often great charm . . .").

Radcliffe-Brown frequently lectured—talked down—to his audience. In a presidential address he explains the meaning of the word system to the Royal Anthropological Institute:

You will perceive that by using the word 'system' I have made an assumption, an important and far-reaching assumption; for that word implies that whatever it is applied to is a complex unity, an organized whole (Radcliffe-Brown, 1941, p. 3).

According to E. L. Grant Watson, who was in the field with Radcliffe-Brown and knew him well, he gave the impression of omniscience: ". . . he was brilliantly informed on *all* subjects" (quoted by Firth, 1956, p. 289). Firth (1956, p. 299) also speaks of the "amusement" occasioned at Oxford by Radcliffe-Brown's "foibles of omniscience."

Some anthropologists outside the "cult group" found Radcliffe-Brown arrogant, supercilious, patronizing, or condescending. Firth (1956, p. 289)

reports that "some of his contemporaries . . . found him intellectually arrogant and irritatingly assertive." "He seemed to me impenetrably wrapped in his own conceit," Ruth Benedict wrote after talking with Radcliffe-Brown at the meeting of the American Anthropological Association at Atlantic City in 1932 (Mead, 1959b, p. 327). With what may be British understatement, Elkin (1956, p. 243) observed that Radcliffe-Brown's "air of academic superiority and a somewhat exotic and social pose raised a slight barrier between himself and some of his University colleagues [in Sydney]." A member of the "cult group," however, found Radcliffe-Brown's vanity "in him, a sympathetic quality" (Fortes, 1956, p. 149).

However friendly and sympathetic Radcliffe-Brown was with his students and friends, "he could not suffer gladly those whom he regarded as fools—that is, those who differed from him or did not belong to his 'cult group'" (Elkin, 1956, p. 248). Margaret Mead worked under Radcliffe-Brown's direction in the Admiralty Islands in 1928. In a letter to Ruth Benedict of that year she remarked that Radcliffe-Brown "identifies himself with every idea he has ever voiced and any disagreement, tacit or uttered, with his ideas he takes as a slap in the face . . ." (Mead, 1959b, p. 310). After discussing an ethnological problem with Radcliffe-Brown, Mead wrote: "He said, pontifically, that the similarities between Tonga, Fiji and Samoa were much more interesting than the differences. He really is rather insufferable because he is so sulky and rude whenever he is crossed . . ." (ibid., p. 309).

If on the one hand Radcliffe-Brown tended to give the impression that he was the originator of the views he held and the author of the doctrines he espoused, he was inclined to ignore or to belittle the work of others.

The first part [of *The Andaman Islanders*] is marred by too much stress being laid on the value of the author's own observations and too little on that of his predecessors, especially of so meticulously accurate a recorder as Mr. E. H. Man, who had exceptional chances of observing, spread over a long series of years. Indeed, Mr. Brown seems on occasion to go out of his way to disagree with the results of his predecessors, sometimes on quite minor points, even when they have been, like himself, students of experience, but with better opportunities for observation (Temple, 1922, p. 121).

Elkin (1956, pp. 249-50) remarks that "unhappily, too, Radcliffe-Brown was inclined to ignore the contributions of previous workers and what he derived from them. This is particularly so in the case of R. H. Mathews. . . . [Radcliffe-Brown] was quite familiar with Mathew's writings but, regarding him as an amateur, apparently underestimated his ability for careful recording and sound generalization. This, however, did not prevent him adopting the results of much which Mathews had accomplished." According to Lawrence (1937, p. 336), Radcliffe-Brown adopted some of Thomas' data "with minor modifications and without acknowledgement to Thomas."

The bibliography of Radcliffe-Brown's monograph, *The Social Organization of Australian Tribes*, contains 188 references to 101 titles by 52 authors, but Lewis H. Morgan is not cited, despite his eminence and the fact that he had written three items on Australian kinship before Radcliffe-Brown was born. Morgan is mentioned in the text of the book, only to be "controverted."

After a conversation with Radcliffe-Brown in 1932, Ruth Benedict observed that he had "scorn of work so far done in America" (Mead, 1959b, p. 326). "He told me," she wrote, "that he was getting from two students 'the first' two studies of American Indian social organization: Sol Tax for the Fox and Eggan for the Hopi . . ." (ibid., p. 326). I was present at the meeting of the Central States Anthropological Association at the University of Chicago in 1932 and heard Tax and Eggan, then graduate students, present their papers which, they emphasized, were the first scientific studies to be made of the social organization of North American Indian tribes.

These two traits of Radcliffe-Brown—the tendency to assume originality for himself, and to ignore or depreciate the work of others—undoubtedly did much to establish and sustain the school of which he became the head.

Unlike Boas, Radcliffe-Brown had to share honors to some extent with another anthropologist, Bronislaw Malinowski. Like Boas, however, Radcliffe-Brown was called a "creator," and, like Boas, he was "revolutionary." "Together with Bronislaw Malinowski," writes Fortes (1962, p. 874C), "Radcliffe-Brown revolutionized the study of social anthropology . . ." (see also Srinivas, 1958, p. xii). He was "one of the creators of modern social anthropology" (Fortes, 1956, p. 153); "no living scholar has had so decisive influence on . . . [its] development" (Fortes, 1949, p. v). To Eggan and Warner (1956, p. 544), Radcliffe-Brown was "the primary creator of modern social anthropology."

Colonel Sir Richard C. Temple, at one time Chief Commissioner of the Andaman and Nicobar Islands, entitled his review of *The Andaman Islanders* "The Revolutionary Theory of Social Anthropology," and repeatedly emphasized its revolutionary character (Temple, 1922, pp. 125, 126). Although Radcliffe-Brown "owed much to his predecessors," says Fortes (1956, p. 153), "he made additions of such originality to the body of anthropological science that we are still assimilating them." Let us examine Radcliffe-Brown's work with reference to these claims.

By "revolutionary" Temple meant, apparently, that *The Andaman Islanders* differed markedly in purpose and outlook from the earlier works of such men as E. B. Tylor, James Frazer, Max Müller, R. R. Marett, et al. Professor Srinivas helps us most to understand what was meant by revolutionary. Sir James Frazer, in his inaugural lecture in 1908 at the University of Liverpool, "defined clearly the nature and scope of social anthropology," writes Srinivas (1958, p. xii); "but that did not put a stop to pseudohistori-

cal and psychological explanations for social facts and events. Radcliffe-Brown was the first English-speaking anthropologist to argue that sociological facts demanded an explanation in terms of sociological laws and not in terms of individual psychology or reconstructed history." Moreover, as Srinivas adds (p. xi), in order to appreciate the significance of Radcliffe-Brown's revolutionary approach, "it is necessary to recall that in 1923 [when Radcliffe-Brown published "The Methods of Ethnology and Social Anthropology"—which "was a charter of revolt when it first made its appearance," p. xii] W. H. R. Rivers' reputation was still at its peak. . . . Rivers' ethnological approach [i.e., his diffusionism] and his marked bias for psychology were both threats to the growth, if not the existence, of the nascent discipline of social anthropology."

This view is shared by other observers. "The supreme merit of . . . [*The Andaman Islanders*]," says Firth (1956, p. 294), "is its interpretation of Andamanese custom and belief in terms not of their origins, as was the current fashion, but of their contemporary meaning to the people themselves, by reference to their social effects." Fortes (1962, p. 874C) remarks that Radcliffe-Brown "swept away the fog of pseudohistorical misunderstanding of the social basis of clans, totemism, alleged matrilineal survivals and other classical problems of anthropology." Robert Redfield (1937, p. ix) finds Radcliffe-Brown's "signal contribution" to lie in "his emphasis on a strictly nonhistorical, sharply scientific method in anthropology."

Radcliffe-Brown did not originate the nontemporal ("functionalist" or "structuralist") conception and interpretation of cultural phenomena. His predecessors are many. Herbert Spencer's "morphology and physiology" of societies—which he regarded as "organisms"—concerns nontemporal aspects of sociocultural systems. Beliefs and worship, wrote Morgan in *The League of the Iroquois* (1851, pp. 149-50), "become so interwoven with the civil and social institutions of men, and by nurture and habit acquire such a firm hold upon their affections, that they form a part of the living, thinking, acting mind. Without a knowledge, therefore, of the religious life of a people, their institutions, and their political and domestic transactions would be wholly inexplicable." Interest in nontemporal relations among cultural phenomena may also be found in Tylor's writings, along with evolutionist and historical interpretations.

"Under the influence of Haddon," writes B. Z. Seligman (1950, p. 305), "a school arose which aimed at studying 'savage' man *in situ*. More interest arose in what manner of man he was, how he behaved and what he believed" [than in evolutionary development of culture]. Radcliffe-Brown early "came into close contact . . . with Haddon" (Fortes, 1956, p. 149). Rivers, an evolutionist turned historian, remarked in his presidential address to the Royal Anthropological Institute in 1922, that "it is a characteristic of the simpler societies . . . that though it is possible to distinguish in their cul-

tures the different aspects we label social, political, economic, religious, aesthetic, etc., these aspects are so interdependent, the social functions of different kinds so closely related, that it is hopeless to expect to understand any one department of culture without an extensive study of other departments . . ." (Rivers, 1922, p. 12). Radcliffe-Brown "was River's first pupil in anthropology" (Fortes, 1956, p. 149). In the United States in 1920, Lowie writes of "the mutual dependence of apparently disparate branches of culture" on the first page of his *Primitive Society*. We have already noted that Frazer "defined clearly the nature and scope of social anthropology" as early as 1908.

Radcliffe-Brown can hardly be credited with originality in "his emphasis on a strictly nonhistorical . . . method in anthropology" (Redfield)—but what scientist has not had predecessors? We believe, however, that Radcliffe-Brown's role in modern anthropology can justifiably be called revolutionary. There seems to have been nothing wholly new or truly unique in *The Origin of Species* (except, possibly, its detailed and explicit presentation of the theory of natural selection), yet the work and impact of Darwin were revolutionary. In much the same way, the movement inaugurated in non-biological anthropology by Radcliffe-Brown may be called revolutionary. *But the achievement lay in the organization and mobilization of anthropological effort toward a goal—in the social organization of ethnological theory—rather than in the birth of an idea.* The revolution was sociological rather than conceptual.

"Professor Radcliffe-Brown . . . is the foremost exponent of the anti-historical method in anthropology"—thus did one of his students, C. W. M. Hart (1938, p. 114), characterize the point of view of his teacher. We have already noted Redfield's assertion (1937, p. ix) that Radcliffe-Brown's "signal contribution" came from "his emphasis on a strictly nonhistorical, sharply scientific method in anthropology." Radcliffe-Brown himself frequently spoke out against historical reconstructions, but, he said, "my position has often been misunderstood. My objection to conjectural history is not that it is historical, but that it is conjectural" (Radcliffe-Brown, 1941, p. 1). Srinivas (1958, p. xiii) states that "Radcliffe-Brown welcomed proper history where sufficient documentary material was available. . . ." And Eggan and Warner (1956, p. 546) testify that their teacher encouraged their historical researches "where documentary or other data were available to check historical inferences." But in *A Natural Science of Society* Radcliffe-Brown asserts flatly that "there exists one general obstacle to the development of such a science ["a theoretical science of society"]—the historical fallacy. [The view "that historical explanation" is "the only valid explanation"] is so entrenched that it constitutes a real obstacle to the development of social science" (p. 146). It is hardly surprising that some anthropologists

got the impression that Radcliffe-Brown was, "throughout his career," "contemptuous of the work of historical anthropologists" (Murdock, 1959, p. 40).

Radcliffe-Brown's criticism of the facile and unverifiable reconstructions of culture history of the sort indulged in by Boas and members of his school is certainly justified. But he was guilty of a serious error in this connection: he confused history (the name of a temporal-particularizing process) with evolution (the name of a temporal-generalizing process). He speaks of "the historical method, which explains a given institution . . . by tracing the *stages of its development*" (Radcliffe-Brown, 1923, p. 125; emphasis mine). "The system of beliefs and customs that exists today in the Andamans," he says, "is the result of a long process of evolution. To seek the origin of these customs . . . is to seek to know the *details of the historical process* by which they have come into existence" (Radcliffe-Brown, 1948, p. 229; emphases mine). He adds in a footnote to the foregoing statement: ". . . by their very nature *all such hypotheses are incapable of verification*" (emphasis mine). "What Radcliffe-Brown did," says Fortes (1955, p. 17), "was to distinguish between the search for (unverifiable) *historical* and *evolutionary* origins—the preoccupation of ethnology—and the study of the (verifiable) *laws of custom and social organization*—the province of social anthropology" (emphases mine).

Radcliffe-Brown was not alone in confusing history and evolution. Virtually the entire Boas school also did the same. To both schools there were only two types of interpretation: *history* (temporal) and *science* (nontemporal). In "History, Evolutionism, and Functionalism: Three Types of Interpretation of Culture" (1945a), I argued for a threefold, rather than a twofold, interpretation of cultural phenomena, and I shall not review the argument in detail here. It may be pointed out, however, that working out the stages of the development of writing is not the same kind of thing as tracing the diffusion of the alphabet. The temporal-generalizing process manifested in the evolution of money is not the same as the temporal-particularizing process involved in the diffusion of coinage. To be sure, both are *temporal* processes, but to lump them together—to fail to recognize the fundamental difference between them—is like putting birds and reptiles in the same zoological class because both lay eggs. Professor Kroeber replied to my argument in "History and Evolution" (1946), in which he defended the "history-science" (i.e., the temporal vs. nontemporal) dichotomy. I returned to the discussion in 1959 in "The Concept of Evolution in Cultural Anthropology" (pp. 111-113).

It is interesting to note, in passing, that Boas and Radcliffe-Brown were at opposite poles on the "history-science" issue. Boas preferred the historical to the comparative method because he felt that he had "actual history" at his disposal which was better than reliance upon hypotheses (White, 1963a,

p. 62). Radcliffe-Brown, on the contrary, rejects all but documentary history as "conjectural," and looks to nontemporal generalizations or laws for valid interpretations.

Actually, of course, it is not that histories or evolutionist accounts are "unverifiable," and that "laws of custom and social organization" are "verifiable," as Meyer Fortes puts it. Stratigraphic studies in archeology, and geographic distributional studies of living cultural traits, provide bases for sound historical studies. As a matter of fact, it would not be extravagant to claim that the broad continental and intercontinental culture histories that have been worked out for both the Old and the New Worlds may well be counted as the most substantial and illuminating achievements of cultural anthropology. Studies of the evolution of writing, of the plow, of money, clan organization, parliamentary government, mathematics, etc., can of course be abundantly substantiated with facts and reason.⁶ Radcliffe-Brown's belief (conviction) that only nontemporal generalizations are verifiable is naive and narrow.⁷

"Radcliffe-Brown . . . was not at heart a field worker nor a director of field work," according to Elkin (1956, p. 245). Although he "had undertaken to give courses of training to officers from Papua and New Guinea . . . he had not himself in five years visited those territories to obtain at first hand even impressions of the actual situation and of the anthropological problems presented there" (ibid., p. 244). Lowie (1937, p. 221), also, observes: "Radcliffe-Brown is most emphatically not a field man by temperament." Radcliffe-Brown is reminiscent of Boas when he discourses on "determined efforts . . . to introduce stricter methods, both in observation and in interpretation," and notes that "in recent years there have been an increasing number of students trained in strict methods of observation" (1923, pp. 145, 146). In the judgment of Phyllis Kaberry (1957, p. 75), "Radcliffe-Brown did not make any particular contribution to the development of field work techniques . . ." "In Australia, as in the Andamans, he never used the vernacular with any fluency, but relied on a *lingua franca* or an interpreter" (Firth, 1956, p. 291). His ethnographic reports "lack the personal touch" (Lowie, 1937, p. 221). "The descriptive chapters [of *The Andaman Islanders*] fail to give a picture or impression of a living society, of social groups engaged in the business of life" (Elkin, 1956, p. 246). In Firth's view (1956, p. 291), "a great part of his [Radcliffe-Brown's] documentation consisted of native statements about social behaviour, not his own observations of that behaviour. Hence his formulations tended to deal rather aridly with rules only, and omit consideration of actual variations [see Lowie's comment on this same point; 1937, p. 222]."

As in the case of many another anthropologist, instances of discrepancy between theory and practice may be found in the life and work of Radcliffe-

Brown. "Though again and again he [Radcliffe-Brown] asserts the basic importance of the family and the local group in Australian social structure," writes Kaberry (1957, p. 75), "we are not given a description of them as functioning units in the tribes which he studied. Many of the questions posed by Malinowski in his *The Family among the Australian Aborigines* (1913) about the actual working of the family as a social institution cannot be answered from Radcliffe-Brown's published field-work." In view of his emphasis upon the necessity of viewing a social system as a whole, and of understanding parts of such a system in terms of their relationship to the whole, it is interesting to note that "this herald of the 'systematic unity' of cultures has not essayed a single integrated cultural picture since his avowedly immature treatise on the Andamans" (Lowie, 1937, pp. 223-24; see also p. 221).

Radcliffe-Brown's field work was not extensive. After his trip to the Andaman Islands he "did significant work in Australia, but none of his relevant writings touch more than selected phases of native life. Of original investigation among the natives of Polynesia and South Africa as a result of prolonged residence in these areas there is no record" (Lowie, 1937, p. 222).

When Radcliffe-Brown was actually in the field he appears to have been a careful worker:

On the testimony of Grant Watson, who had every opportunity of seeing Radcliffe-Brown collecting his data, he was a most careful and patient field worker. He treated his native informants with courtesy and gentleness and seemed to have the faculty of arousing their interest and inspiring their confidence. Genealogies and kinship material he took with great precautions, cross-checking them wherever he could (Firth, 1956, p. 291).

In contrast with Franz Boas, Radcliffe-Brown did not write much, but everything he wrote is described by his followers as superlative, even "perfect."

[Radcliffe-Brown] has not, considering that he has been engaged in teaching and research . . . for almost fifty years, written as much as most persons of his academic eminence. What he has written, however, has been faultless . . . the point of view which he expresses could not have been better expressed. Each of the essays is perfect in conception and in expression . . . (Evans-Pritchard and Eggan, 1952, p. v).

Fortes observes that Radcliffe-Brown "wrote little in comparison with other leading anthropologists of his generation. [But] it is important to add that everything he wrote is still significant . . ." (1956, p. 153); ". . . his writings are ranked among the classics of anthropology" (1949, p. v). One of the reasons "for his relatively meager output," according to Srinivas (1958, p. ix), was "his fastidiousness": he "wrote with great care, handling words like precious stones." Referring to a letter written by Radcliffe-

Brown, published posthumously in the *American Anthropologist* (58:363, 1956), Fortes writes: "It is a fragment, but authentically the work of the master, as clear, pungent and unerring as his earliest controversial articles" (1956, p. 149). He "wrote with elegance and style" (Eggan and Warner, 1956, p. 546).

Radcliffe-Brown did write some noteworthy essays, a small book (*A Natural Science of Society*), and a significant monograph (*The Social Organization of Australian Tribes*), in addition to his first work, *The Andaman Islanders*. He had considerable skill in literary composition. Faultlessness and literary perfection are, however, exaggerations—but understandable when they are viewed as expressions of the bonds between a charismatic leader and his followers.

There is much repetition over the years in his writings, which is perhaps necessary for a person who is trying to communicate and to establish his point of view. Some of his theoretical essays—for example, "The Present Position of Anthropological Studies" (32 pp.) and "The Methods of Ethnology and Social Anthropology" (24 pp.)—are extremely verbose as well as repetitious. His writings also contain many platitudes, such as the following examples from his *The Andaman Islanders*:

When a person dies the social bonds that unite him to the survivors are profoundly modified (p. 244).

The position in the social life occupied by a child is different from that of an adult . . . (p. 276).

The meaning of the marriage ceremony is readily seen. By marriage the man and woman are brought into a special and intimate relation to one another; they are, as we say, united (p. 236).

In the peace-making ceremony the purpose of the whole rite is to abolish a condition of enmity and replace it by one of friendship (p. 242).

Radcliffe-Brown is often described by his followers as a scientist and philosopher. He was very conscious of science. "All that a teacher can do is to assist the student in learning to understand and use the scientific method" (Radcliffe-Brown, 1940, p. 1). He also liked to play the role of philosopher.⁸ Both of these interests and emphases are the *raison d'être* of *A Natural Science of Society*, which Eggan (1957, p. xii) says presents "the essential Radcliffe-Brown." Let us examine some of his scientific and philosophic teachings.

Radcliffe-Brown said repeatedly that the task of the social anthropologist is (1) to classify societies (or social systems, or structural systems; he uses all of these terms), (2) to make comparisons of societies, or types of societies, so that (3) broad or "universal" generalizations may be made about them.

"I propose," he said (1957, p. 33), "that no scientific study of societies can get very far until we have made some progress towards a classification

of social systems into whatever types, groups, or classes suggest themselves as expedient . . ." (see also Radcliffe-Brown, 1940a, p. 6). But, he states further, "there is an enormous number of difficulties in the way of any satisfactory classification . . ." (Radcliffe-Brown, 1957, p. 34). One of the difficulties is that of defining the term society: "Is the British Empire a society, or a collection of societies? Is a Chinese village a society, or is it merely a fragment of the Republic of China?" (Radcliffe-Brown, 1940a, p. 4). In 1923, Radcliffe-Brown (1923, p. 136) distinguished between "differentiated societies," e.g., "tribes divided . . . into groups of kindred" and "undifferentiated societies (such as the Andaman Islanders). . . ." In 1937 (Radcliffe-Brown, 1957, p. 33), he notes that a number of sociologists have "suggested" classifications, but adds "we can see that we have hardly taken the first steps toward a scientific classification." In 1940 (Radcliffe-Brown, 1940a, p. 6), he observes that the "classification of types of structural systems" is "a complex and difficult task, to which I have myself devoted attention for thirty years. . . ." Although he achieved little or nothing in this respect during three decades of effort, he "believed some progress is being made" (*ibid.*, p. 6).

In 1931 Radcliffe-Brown wrote, "By comparing a sufficient number of diverse types we discover uniformities that are still more general, and thus may reach the discovery of principles or *laws that are universal in human society*" (1931, p. 162; emphasis mine). "My own view," he says (*ibid.*, p. 166), "is that any attempt to discover [*sic*] the general laws of human society must be based on the thorough detailed study and comparison of widely different types of culture . . ." This "view" is repeated in subsequent publications: Radcliffe-Brown, 1935, p. 535; 1940b, p. xi. In another place (1940b, p. xi) he speaks of discovering "the universal, essential, characters which belong to *all human societies, past, present and future*" (emphasis mine); he says the same thing, and in virtually the same words, in Radcliffe-Brown, 1941, p. 16.

It is not clear what value, or significance, these "universal laws" might have. If we compared several "types or classes" of animals—bats, badgers, whales, and baboons—we "discover" that they are warm-blooded, their young are born, not hatched, and they suckle their young. But this does not appear to be a very significant scientific achievement. What sort of universal laws may we discover about human societies?

"Every human society has a religion." This statement "does not mean all the societies I have *observed*," says Radcliffe-Brown (1957, pp. 71-72); "it means all human societies of the past, present, and the future are characterized by religion. The statement will, if true, be true of societies a thousand years from now as well as those of half a million years ago." But, he adds, "I am not suggesting it is true"—which leaves us in a somewhat uncertain

position. He then offers a statement that he does believe to be true: "Every human society has a system of morals" (p. 72).

"How are we going to prove a proposition of that kind?" he asks (Radcliffe-Brown, 1957, p. 72). "Is it possible to prove that all human societies have a system of morals? Must they all have religion (whatever that may be)? It is obvious that if such propositions can be proved, we have statements of the most general kind, statements of natural laws which relate to all human societies." He suggests that propositions such as these are the ones "which we want to discover and prove" (*ibid.*, p. 72).

The discussion of universal laws of human society leads to Radcliffe-Brown's conception, or conceptions, of law in general. The "purpose of scientific studies," he says (1948, p. 229), "is to discover [not formulate, L.A.W.] general laws." There are a number of "*kinds* of natural laws" (1957, p. 14) but he appears to subscribe to the pre-Aristotelian, Ephesian concept that natural law is immanent in the universe. "A natural law may be defined as a statement of characteristics of a certain class of natural systems" (*ibid.*, p. 63). "A natural law is merely a statement of invariant relations in a class of systems" (*ibid.*, pp. 54-55). "The usual word for a generalization about a class of phenomena is 'law'"—providing it is supported, or verified, by empirical evidence (Radcliffe-Brown, 1952, p. 14; 1935, p. 402). He also speaks of "a general law or tendency" (1931, p. 152).

"The standard example of a law of nature is the statement that all men are mortal" (Radcliffe-Brown, 1952, p. 14). A "very typical natural law" is the statement that "every male lion has a mane" (Radcliffe-Brown, 1957, p. 32). Thus it is possible to derive a great many laws from male lions: they have ribs, livers, lungs, hearts, etc. Speaking of hearts, Radcliffe-Brown (1949, p. 505) says that "it is a law of nature that human beings have the heart on the left side. . . ." He adds: "There are exceptional people who have the heart on the right [side]." Thus, presumably there is one law for people who have their hearts on the left side and another law for those who have their hearts on the right side.

Beals and Hoijer (1965, p. 725) observe that "it cannot be reported that Radcliffe-Brown or his followers have produced much, as yet, in the way of social laws or generalizations," which is, perhaps, something of an understatement. One of Radcliffe-Brown's "discoveries" in the realm of "laws" is the following:

An important principle, which in this instance is a universal sociological law though it is not yet possible to formulate precisely its scope, namely that in certain specific conditions a society has need to provide itself with a segmentary organization (Radcliffe-Brown, 1931b, p. 441).

Lowie comments upon the above as follows:

The grandiloquent use of the term 'law' is most regrettable and in some circumstances leads to absurdity. . . . Whoever heard of a universal law with an

as yet undefinable scope, of a law that works in certain specific *but unspecified* conditions? Is it a law that some societies have clans, and others have not? (Lowie, 1937, p. 225).

Let us now turn to the subject of Radcliffe-Brown and the concept of culture. We would agree fully with Beals and Hoijer (1965, p. 724) who say that "Radcliffe-Brown directs his attention to the study of society rather than to culture, and regards himself more as a sociologist or social anthropologist than a student of culture." "Until 1931," says Srinivas (1958, p. xvi), "Radcliffe-Brown described the subject matter of social anthropology as culture or social life. Subsequently, however, he used increasingly 'social structure' and 'social system,' and he began to drop the use of 'culture.'" Kroeber (1952, p. 164), on the other hand, remarks that Radcliffe-Brown has been "most notable among anthropologists for evincing resistance to the concept of culture as such . . . though he has made some concessions of late. . . ."

In 1923 Radcliffe-Brown observed that "social anthropology will then become the purely inductive study of the phenomena of culture," whereas ethnology is the "attempt to reconstruct the history of culture" (Radcliffe-Brown, 1923, p. 138; he uses the term culture freely in this address). "Culture" appears rather frequently in "The Sociological Theory of Totemism" (1929); it is used in "The Present Position of Anthropological Studies" (1931) and in the Preface to *The Andaman Islanders* (dated 1932). His most extensive discussion of this concept is to be found in "The Nature of a Theoretical Natural Science of Society" (1937, mimeographed; published by The Free Press in 1957 as *A Natural Science of Society*). In "On Social Structure" (1940), he continues his negative treatment of the concept of culture.

Radcliffe-Brown has various definitions, or conceptions, of culture. Culture is (1) "an adaptive and integrative mechanism" (1932, p. ix). Culture is (2) a "certain standardization of modes of behavior, inner and outer" (1957, p. 95; see also pp. 96, 97, 107). But culture (3) "will also be a *description* of standardized modes of behavior" (*ibid.*, p. 96; emphasis mine). Culture (4) "consists of a set of rules of behavior" (*ibid.*, p. 99). But (5) "it does not matter much whether one regards the *social usages* as constituting culture *or the rules behind them* as doing so" (*ibid.*, p. 103; emphasis mine). Culture is (6) "the process of cultural tradition . . . [which] is a social process of interaction of persons within a social structure" (1949, pp. 510-11). He states further (7) that the "word culture denotes, not any concrete reality, but an abstraction, and as it is commonly used a vague abstraction" (1940a, p. 2). Therefore, we cannot "observe a culture" (*ibid.*, p. 2). Why does Radcliffe-Brown entertain so many and diverse conceptions of culture? Perhaps it is because he really has no use for the term and therefore is indifferent to its definition. Despite the variety

of conceptions of culture held by Radcliffe-Brown, he is nevertheless able to tell us what culture *is*: "I hope I have shown you that culture is the *name* for that standardization . . ." (1957, p. 97); he does not say that this is the way *I* wish to use the term whereas others wish to use it in other ways; he says this is what culture *is*.

In "The Sociological Theory of Totemism" (1929, p. 300), Radcliffe-Brown uses the phrase "science of culture" more than once. It appears in "The Present Position of Anthropological Studies" (1931, p. 165), and again in the 1932 Preface to *The Andaman Islanders* (p. ix). But in his 1937 seminar at the University of Chicago he says that a science of culture is impossible:

This brings us to a fundamental axiom [sic] of the science of society, as I see it. Is a science of culture possible? Boas says it is not. I agree. You cannot have a science of culture. You can study culture *only as a characteristic of a social system. Therefore, if you are going to have a science, it must be a science of social systems* (1957, p. 106; emphasis mine).

Fred Eggan, probably the foremost student of Radcliffe-Brown at the University of Chicago, observes in his Foreword to *A Natural Science of Society* (p. xi) that this "challenging conclusion that no science of culture is possible" is "particularly important"—but he does not tell us what its import is.

Radcliffe-Brown's position that a science of culture is an impossibility is not surprising. It follows logically from his distinction between society and culture. He has defined culture in such a way that it cannot constitute the subject matter of science. Science must have real things and events as its subject matter. If culture "denotes, not any concrete reality, but an abstraction" and a "vague abstraction" at that, an abstraction that "cannot be observed," it can hardly become the subject matter of scientific observation and interpretation. Radcliffe-Brown illustrates what he means with an example:

A few years ago, as a result perhaps of re-defining social anthropology as the study, not of society, but of culture, we were asked to abandon this kind of investigation in favour of what is now called 'culture contact.' In place of the study of the formation of *new composite societies*, we are supposed to regard what is happening in Africa as a process in which an entity called African culture comes into contact with an entity called European or Western culture, and a third new entity is produced . . . which is to be described as Westernized African culture. To me this seems a fantastic reification of abstractions. European culture is an abstraction and so is the culture of an African tribe. I find it fantastic to imagine these two abstractions coming into contact and by an act of generation producing a third abstraction. There is contact, but *it is between human beings*, European and African, and it takes place within a definite structural arrangement (1940a, pp. 10-11; emphases mine).

This is a rather silly statement.⁹ Of course there is no such thing as

culture—customs, institutions, languages, works and forms of art, beliefs, tools, utensils, etc.—without the actions of human beings. It is true, also, that the culture of Western Europe was, in this example, brought into Africa by human beings who then came into contact with peoples—natives of Africa. But the significant thing, from a nonbiological point of view, is not that human beings, human organisms, came into contact with one another but that their respective customs, beliefs, tools, techniques, etc., came into contact with each other, interacted with each other, and produced definite observable effects or results. When an adult Frenchman comes to the United States as an immigrant and learns English, it is proper and meaningful to say that the French language comes into contact with the English language and produces an English dialect with a French accent. Unfortunately, Radcliffe-Brown allows the fact that man is essential to the *existence* of culture (as we have here used this term), to obscure the fact that he is *not* necessary, or relevant, to a *scientific explanation of changes and variations of cultures, their interactions and syntheses*.

If a science of culture is impossible, why was the principal course given by Radcliffe-Brown at the University of Chicago entitled "Anthropology 211: The Comparative Science of Culture"? Radcliffe-Brown described this course as "planned to provide an introduction to the social sciences." In the words of one of his prominent students of the time, he considered this course as "his basic course; most of his other courses were specializations of it in one way or another."

It follows logically from Radcliffe-Brown's definition (conception) of culture that he is "unable to attach any definite meaning to such phrases as the evolution of culture," but "thinks that social evolution is a reality which the social anthropologist should recognize and study" (1940a, p. 11; see also his essay, "Evolution, Social or Cultural?"). But it is not the evolution of *society* that makes intelligible the evolution of mathematics, or currency, or the steam engine; it is the evolution of systems of things, concepts, and techniques. Albert Einstein and Leopold Infeld's *The Evolution of Physics* (1938)—a proper and justifiable title—is not a study of the evolution of social structure but of conceptual structure—of "knowledge and belief," which Tylor lists as parts of culture (1929, [1871], p. 1).

In another place, Radcliffe-Brown (1957) hedges considerably on the question of a science of culture:

While no complete science of culture is possible by itself, you can have an independent scientific treatment of certain aspects, of certain portions of culture [Which of his numerous conceptions of culture he is using here is uncertain.]. They will not give you the final [What does he mean by this?] scientific conclusions, but they will give you a certain number of quite important provisional ones. The science of linguistics is a good example (p. 108).

Therefore, it is possible to study language largely irrespective of a society's characteristics. Not completely so . . . changes in the meanings of words may

often be traced directly to changes in the structure of the society. Nevertheless, I believe that linguistics stands apart as having a purlieu of its own (p. 143).

Three years after his 1937 seminar at the University of Chicago, Radcliffe-Brown (1940a, p. 7) repeats his observations regarding the science of linguistics:

Thus the general comparative study of languages can be profitably carried out as a relatively independent branch of science, in which the language is considered in abstraction from the social structure of the community in which it is spoken.

If language can be "considered in abstraction from the social structure of the community," why cannot mathematics, technology, currency, architecture, metallurgy, etc., be so considered? Radcliffe-Brown himself seems to admit this possibility. After discussing the possibilities and achievements of the scientific study of language as an "independent" enterprise, he says: "I think you can do the same with other aspects of culture" (1957, p. 108).

Radcliffe-Brown focuses upon society rather than upon culture. "While I have defined social anthropology as the study of human society," says Radcliffe-Brown (1940, p. 2), "there are some who define it as the study of culture. It might be thought that this difference of definition is of minor importance. Actually it leads to two different kinds of study, between which it is hardly possible to obtain agreement in the formulation [and, we would add, the solution] of problems."

We agree completely with this statement. There is a fundamental difference between the study of society and social interaction (sociology) and the study of the interaction of culture traits (as Tylor defined culture and as Kroeber, Lowie, Wissler, and numerous other American anthropologists have used this term), which is culturology. As we have pointed out already in this essay, the significant thing is not the interaction of members of the species *Homo sapiens*; it is the interaction of customs, beliefs, tools, techniques, etc., which can be "considered in abstraction from the social structure" just as language can be so considered.

The failure of Radcliffe-Brown to comprehend the culturological point of view is unfortunate because the emergence of the science of culture marked an advance in the scientific comprehension of "man and his works" (see White, "The Expansion of the Scope of Science," and other papers). It is all the more unfortunate that Radcliffe-Brown fell short of a philosophic grasp of the premises and point of view of the science of culture (outlined almost a century ago by E. B. Tylor), because much of his interpretation of ethnographic data is culturological in spirit and procedure (White, 1949, p. 98).

As a philosopher of science, Radcliffe-Brown has something to say about causation and explanation. In 1923 he said:

Now while ethnology with its strictly historical method can only tell us that certain things have happened, or have probably or possibly happened, social anthropology with its inductive generalizations can tell us *how and why* things happen, i.e., *according to what laws* (1923, p. 141; emphases mine).

"The modern theoretical scientist," says Radcliffe-Brown (1957, p. 41), "excludes the concept of cause from his scientific investigation."¹⁰ How, then, are social phenomena to be explained?

"The essence of induction,"¹¹ he observes (1923, pp. 126-27), "is generalization: a particular fact is explained by being shown to be an example of a general rule." Are Seneca clans "explained" by pointing out that they are particular examples of clan organization in general—or of a "principle of segmentation"?

In "The Social Organization of Australian Tribes," Radcliffe-Brown wrote the following:

Professor Sapir has suggested that there is a correlation between the custom of the levirate and the general principle of classificatory systems of *terminology*. In that I think he is right, but I think he is in error in suggesting a direct causal relation between the two whereby the custom of classifying the father's brother with the father is regarded as the effect of the levirate. In general I believe that it is false procedure to look for the cause of one social institution in another particular institution. In the present instance my own view is that both the levirate and the classificatory principle in terminology *are the results of the action of a single sociological principle*, namely that which I have called the principle of the social equivalence of brothers. This principle is *at work*, I believe, wherever we find the levirate and wherever we find a classificatory system of terminology. Its action is more effective in some societies than in others [why?], and it is combined with *the action of other principles*. Thus in some societies we may find a classificatory system without the levirate [this, presumably, is due to the "action" of the classificatory principle], and in others we may find the latter [the levirate] without the former [due, no doubt, to the action of the principle of the levirate—or the principle of the equivalence of brothers] (Radcliffe-Brown, 1931b, p. 429; emphases mine).

In another place in the same publication he states that he "regards correlated systems as part of a system behind which are certain *active principles*, which not merely help to *produce the institutions* at their origin but *serve to maintain* them in existence" (1931b, p. 441; emphases mine).

A decade later he wrote: "The custom of marriage with this relative is simply the *result* of the *application of the principle* of the unity of the lineage in a system of patrilineal lineages" (1941, p. 14; see also p. 13; emphases mine).

Why Radcliffe-Brown rejects the concept cause as an explanatory device but uses "principles" for this purpose is far from clear. If culture is an abstraction what is a principle? If culture is "a reification of an abstraction" as Radcliffe-Brown (1940a, pp. 10-11; 1957, p. 97) says it is, what is a

principle that "acts" to "produce institutions" and "serves to maintain them"?

Radcliffe-Brown hardly acquits himself with credit as a philosopher of science.

Many scholars have remarked that Radcliffe-Brown was greatly influenced by Émile Durkheim, and Radcliffe-Brown himself frequently testifies to this influence. Durkheim has also influenced the majority of contemporary anthropologists except members of the Boas school.¹² The relationship between Radcliffe-Brown and Durkheim, however, has taken on the quality of a legend. Some have attempted to identify, or at least to associate Radcliffe-Brown with Durkheim in a personal way. Radcliffe-Brown "had visited France about this time [ca. 1909-1910]," says Fortes (1956, p. 150), "and had been in touch [sic] with Durkheim and Mauss . . ." If Radcliffe-Brown had actually met Durkheim personally, to say nothing of discussing sociological matters with him, unambiguous evidence of the contact would seem expectable. Firth (1956, p. 301) states flatly that "he [Radcliffe-Brown] and Durkheim never met." Nevertheless, France was Radcliffe-Brown's "intellectual home" as England was "his spiritual home" (Eggan and Warner, 1956, p. 545).

Shortly before Radcliffe-Brown's death he gave to J. G. Peristiany, a British social anthropologist (Peristiany, 1960), a letter dated November 9, 1913. The letter has been published in facsimile and in English translation by Peristiany (ibid). Although it does not bear Radcliffe-Brown's name, the letter was unquestionably addressed to him, and thanks him for the receipt of a paper on Australian social organization in which Radcliffe-Brown sets forth an interpretation different from that offered by Durkheim. The letter is signed "É. Durkheim." The article sent to Durkheim was undoubtedly Radcliffe-Brown's "Three Tribes of Western Australia" (*Journal of the Royal Anthropological Institute*, XLIII, 1913). No other letter from Durkheim has been found among the papers of Radcliffe-Brown, who "carefully preserved all letters received from Mauss and other eminent sociologists" (Peristiany, 1960, p. 319).

While similarities between Radcliffe-Brown and Durkheim are fairly numerous, Radcliffe-Brown did not hesitate to say that Durkheim had "failed" in a particular instance or that his interpretation was inadequate in another. But these differences of conception or interpretation were relatively insignificant when compared with the areas of agreement. There is one respect, however, in which the difference between Radcliffe-Brown and Durkheim is radical and fundamental: Durkheim strove for, and gave expression to, the conceptions of culturology; these not only lay beyond the grasp of Radcliffe-Brown but were explicitly opposed by him.

Durkheim was talking about culture—culture in the Tylolean sense, cul-

ture as A. L. Kroeber, R. H. Lowie, Clark Wissler, and a whole generation and more of American anthropologists have conceived of it—when he wrote:

But we must remember that the greater part of our social institutions was bequeathed to us by former generations ([1901] 1938, p. xlv).

Collective representations are the result of an immense co-operation, which stretches out not only into space but into time as well; to make them, a multitude of minds have associated, united and combined their ideas and sentiments . . . ([1912] 1947, p. 16).

Durkheim had culture in mind when he wrote: "We need to investigate, by comparison of mythical themes, popular legends, traditions, languages, the manner in which social representations adhere to and repel one another, how they fuse or separate from one another, etc., . . ." ([1901] 1938, p. li). Durkheim does not think of culture as "a fantastic reification of abstractions." He does not say that it is people, or societies, that do the interacting; for him it is the elements of the tradition called culture that interact. He says:

Now this synthesis has the effect of disengaging a whole world of sentiments, ideas and images which, once born, obey laws all their own. They attract each other, repel each other, unite, divide themselves, and multiply. . . . The life thus brought into being even enjoys so great an independence that it sometimes indulges in manifestations with no purpose or utility of any sort, for the mere pleasure of affirming itself (Durkheim, [1912] 1947, p. 424).

One could hardly find a better statement of the conception of culture as a process *sui generis* than the following: ". . . collective ways of acting or thinking have a reality outside the individuals who, at every moment of time conform to it. These ways of thinking and acting *exist in their own right*" ([1901] 1938, p. lvi; emphasis mine).

Many other statements of Durkheim make it clear that his central concern was culture and not people. "The proposition which states that social facts are to be treated as things . . . [lies] at the very basis of our method" (Durkheim, [1901] 1938, p. xliii). "These ways of thinking and acting therefore constitute the proper domain of sociology" ([1895] 1938, p. 4). "Sociology can then be defined as the science of institutions, of their genesis and of their functioning" ([1901] 1938, p. lvi). He does not say societies of people; he refers to "institutions," and states that "social phenomena are external to individuals" (*ibid.*, p. xlvii).

Let those who think of sociology as the science of society note Durkheim's conception of society:

First, it is not true that society is made up only of individuals; it also includes material things, which play an essential role in the common life. The social fact is sometimes so far materialized as to become an element of the external world. For instance, a definite type of architecture is a social phenomenon; but it is par-

tially embodied in houses and buildings of all sorts which, once constructed, become autonomous realities, independent of individuals. It is the same with the avenues of communication and transportation, with instruments and machines used in industry or private life which express the state of technology at any moment in history, of written language, etc. Social life, which is thus crystallized, as it were, and fixed on material supports, is by just so much externalized, and acts upon us from without ([1897] 1951, pp. 313-314).

As we have seen earlier, Radcliffe-Brown regarded "European culture," "West African culture," etc., as "fantastic reifications." He accused Durkheim of "reification of the collective consciousness"—and regarded reification as a "fallacy" (Radcliffe-Brown, 1957, p. 97). To reify is, of course, to regard something which is *not* a thing as a thing (see White, 1959c, p. 239). This is precisely what Durkheim does *not* do. "Social phenomena are things and ought to be treated as things" (Durkheim, 1938, p. 27). To Durkheim, a custom, a way of thinking, and "machines used in industry," are not reifications; they are real things and events. It is a "fundamental principle" in Durkheim's science, that "social facts" have an "objective reality . . . ultimately, everything rests on this principle and grows out of it" ([1901] 1938, p. lvii). In line with his conception of culture as a reification, Radcliffe-Brown (1957, p. 30) observes: "You will not, for instance, say of culture patterns that they act upon an individual, which is as absurd as to hold a quadratic equation capable of committing a murder."

A quadratic equation may not be capable of committing a murder, but a "social fact"—a cultural tradition—can cause a person to commit suicide: hara-kiri. Radcliffe-Brown fails completely to comprehend, to accept and use, the culturological point of view and its techniques of interpretation as set forth by Durkheim.

How far removed Radcliffe-Brown is from the thinking of the great French savant may be seen in the following:

Here, then, is a category of facts with very distinctive characteristics: it consists of ways of acting, thinking, and feeling external to the individual, and endowed with a power of coercion, by reason of which they control him (Durkheim, [1895] 1938, p. 3).

Collective tendencies [culture in motion] have an existence of their own; they are forces as real as cosmic forces, though of another sort; they, likewise, affect the individual from without, though through other channels (Durkheim, [1897] 1951, p. 309).

As we have previously noted, Radcliffe-Brown argues that a culture trait cannot "cause" anything, but that a "principle" can. He rejects Sapir's suggestion that there is a causal relationship between the institution of the levirate and "the general principle of classificatory systems of terminology" (Radcliffe-Brown, 1931b, p. 429). Instead, he says, "both . . . are the results of the action of a single sociological principle. . . ." How different from Durkheim: "The determining cause [sic] of a social fact should be

sought among social facts preceding it . . ." (Durkheim, 1938, p. 110).

We are not here arguing that the levirate "caused" the "classificatory" system of terminology; there may be a better explanation. In a terminological system in which a person calls his cross cousins by a term which means also "spouse," we may justifiably look for the presence of cross-cousin marriage, or reasonably infer its former existence. The principle of causality is pertinent here—and we know which is the cart and which the horse. A people do not resort to cross-cousin marriage in order to give meaning to a terminological usage; the terminology is a consequence of a particular form of marriage; we do not "explain" both as the results of the action of a single sociological principle.

To Radcliffe-Brown societies of individual human beings are the independent variable; the external tradition that we call culture is the dependent variable. "You can study culture *only as a characteristic of a social system*. Therefore, if you are going to have a science, it *must be a science of social systems*," i.e., not of culture (Radcliffe-Brown, 1957, p. 106; emphases mine). He remarks further:

. . . you cannot have the continuity of culture without continuity of social structure. The social structure consists of the social behavior of actual individual human beings, who are *a priori* to the existence of culture. Therefore if you study culture, you are always studying the *acts of the behavior of a specific set of persons* who are linked together in a social structure" (Radcliffe-Brown, 1957, pp. 107-8; emphasis mine).

(We assume that by *a priori* Radcliffe-Brown meant "prior to," although the dictionary does not warrant this usage.)

Radcliffe-Brown states (1957, p. 107), "Culture cannot exist of itself even for a moment." To be sure, culture could not exist without man. But, as we have pointed out, Radcliffe-Brown allows the self-evident fact that man is necessary for the existence of culture to obscure the fundamental fact that man is not necessary for a scientific explanation of variations of culture. We do not explain the institutions of polygyny or monogamy in terms of the "social behavior of actual individual human beings who are *a priori* [i.e., prior to] to the existence" of these institutions. On the contrary, we explain the behavior of "the actual human beings" as the result of the action of an external, coercive tradition of "ways of thinking and acting" (i.e., culture) upon them. And we explain the institutions of polygyny and monogamy in terms of the interactive culture-process in which social facts, as things, as culture traits, "attract each other, repel each other, unite, divide themselves and multiply" (Durkheim, [1912] 1947, p. 424).

When *The Rules of Sociological Method* "appeared for the first time," wrote Durkheim in the preface to the second edition, "it aroused lively controversy. Current thought, shaken out of itself, resisted at first so loudly that

for a time it was almost impossible for us to make ourselves heard" ([1901] 1938, p. xli). He goes on to say, also, that he was thoroughly misunderstood on important issues: "On the very points on which we had expressed ourselves most explicitly, views were freely attributed to us which had nothing in common with our own; Our critics . . . imputed to us certain opinions that we had never upheld . . ." (ibid., p. xli).

It is not surprising that Durkheim was misunderstood and resisted. He presented bold and radical doctrines, and he unfortunately phrased cultural philosophy in psychological terms: "group mind (*l'âme collective*)" (Durkheim, 1938, p. 8); "collective conscience"; "collective sentiments" (p. 96), etc. The culturological point of view is difficult enough for many social scientists to comprehend when it is set forth in adequate terminology and simple prose (see White, 1947b; Ch. 14, "The Science of Culture," in White, 1949; White, 1959c; White, 1963b). But when it is disguised in an inadequate and therefore misleading terminology, the difficulties of comprehension are increased enormously. Only those who have a clear conception of a science of culture, as Tylor first defined and outlined it long ago, are able to "decode" *The Rules of Sociological Method*.

Why did not Durkheim use the term culture? He was thoroughly familiar with E. B. Tylor's *Primitive Culture*, as references to this work in *The Elementary Forms of the Religious Life* make abundantly clear. He was well acquainted, of course, with the tradition of *kulturgeschichte* in Germany. He may even have read the works of Gustav Klemm (*Allgemeine Culturwissenschaft*, 1854-55). But French scholars have avoided, apparently deliberately, the word culture. "There are still great civilized nations—the French, for instance—who refuse to admit the word 'culture' into their intellectual vocabulary" (Kroeber, 1949, p. 182). E. B. Tylor's *Primitive Culture* has been translated *La Civilisation Primitive* (Paris, 1876, 1878). Perhaps Durkheim was influenced in this matter by the example set by August Comte, who coined the term sociology. Whatever the reason, Durkheim did not avail himself of this all important term¹³ and concept: *culture* as distinguished from *society*; *cultural* as distinguished from *social*.¹⁴ Radcliffe-Brown has followed Durkheim in this serious shortcoming.

Thus we see that, although there are similarities between Radcliffe-Brown and Durkheim, there is a fundamental difference. Radcliffe-Brown not only missed the point of what seems to me Durkheim's most important theoretical contribution, but doggedly opposed it. This fact is noted by A. P. Elkin:

Professor White's theory and method would seem to be a logical development of Durkheim's approach. It is therefore most interesting that Radcliffe-Brown would have nothing of it. For him there can be no science of culture, and yet he was a recognized and self-proclaimed exponent and developer of Durkheim's theories (1958, p. 167).

Despite the numerous criticisms of Radcliffe-Brown that we have made,

in our opinion there is no question of his importance in the history of modern anthropology. He was the leader of a considerable group of productive scholars for a matter of decades. He undoubtedly did much to recruit these scholars ("imparting to students the thrill of new discovery and the desire to join in the task of further research," as Fortes, 1949, p. v, has remarked). They looked to him for inspiration, guidance and support: "Essentially, what Radcliffe-Brown . . . [did] over this long period ["over forty years"] was to guide and lead the development of social anthropology as a discipline" (Firth, 1956, p. 301).

We believe that Radcliffe-Brown's importance lay in the fact that he was prominent and influential in a *social organization of scholars* rather than in his contributions to *scientific fact or theory*. He added little to science's stock of ethnographic fact. He made no significant discovery such as, for example, the discovery of the classificatory system of kinship terminology. He originated virtually nothing in the matter of ethnological theory, despite claims to the contrary. He was not the first to employ nontemporal interpretations of sociocultural systems; neither he nor Malinowski "invented" Functionalism. On the other hand, he exerted himself to disparage evolutionist and historical interpretations—which he confused with each other—and to discourage other scholars from engaging in these potentially fruitful endeavors. Whatever validity may be given to the claims that Radcliffe-Brown was "the creator of modern social anthropology" derives, it seems to us, from his role in a social movement—a school. He "established social anthropology as a reputable academic study in some of the most famous universities in the English-speaking world, and elsewhere," as Fortes (1956, pp. 152-153) has stated. We believe that Elkin has characterized Radcliffe-Brown's role most succinctly. He "was a starter and a stirrer . . . [he] excelled in this role, a very valuable and important one" (Elkin, 1956, p. 239).

Schools and Cults

It is not our aim here to undertake a general scientific study of the social phenomena of schools and cults. They have long been numerous in Western culture and they have common characteristics: a charismatic leader, followers, and a body of doctrine. As schools or cults, they must have allegiances, loyalty, esprit de corps, and solidarity. They may flourish in a variety of fields, the revolutionary labor movement, religion, and in philosophy and science. The numerous utopian societies, such as Brook Farm and the Oneida Community, that flourished in the United States in the nineteenth century provide other and very interesting examples of cult-formation. Our concern here, however, is with schools in science in general and in the field of nonbiological anthropology in particular.

The German-Austrian Kulturkreis school is instructive. It came into being in the closing years of the nineteenth century and the early years of the twentieth century. Philosophically considered, it was a reaction against the doctrines of the psychic unity of mankind and the independent development of culture among various peoples and in diverse regions. In short, it was a reaction against cultural evolutionism. It substituted the process of diffusion for that of evolution: cultural similarities in noncontiguous regions were to be accounted for in terms of diffusion rather than of independent development.

Friedrich Ratzel (1844-1904) is cited by most historians as the one who initiated the movement which became the Kulturkreis school, first under the leadership of Fritz Graebner (1877-1934), and later, of Father Wilhelm Schmidt (1868-1954). But the doctrine of diffusion did not originate with Ratzel. E. B. Tylor employed it, as an explanatory device, frequently and in a bold manner. Lewis H. Morgan elevated diffusion to the level of a major process of culture change (see White, 1945b, pp. 341-343). What, then, did Ratzel do, what did he contribute? An examination of the role of the individual in the culture process will help us to find a plausible answer.

Our culture inclines us to hold the anthropocentric view that it is "the leader" who does things; he initiates, creates, establishes, builds. But one cannot be a king without subjects. The immaturity of the science of culture at the present time does not make it possible for us adequately to explain the formation of schools, but we would certainly want to begin with the conception of a sociocultural process within which the school—both leader and followers, master and disciples—is formed. To "explain" Mormonism in terms of the individuality of Joseph Smith or Brigham Young is as superficial as it is easy. The sociocultural, or economic-political, process may *create* a leader, or at least select an individual and elevate him to a position of leadership, as in the case of the German nation and Adolph Hitler, or the United States and Abraham Lincoln. There is point to saying that followers often precede (antedate) their leader.

The foregoing considerations help us to understand Ratzel's role in the history of ethnological theory. Cultural evolutionism had had a long and successful run in England, Germany, and the United States where it was a distinct, coherent conceptual tradition. An awareness of the diffusion of culture and of its significance was not wanting. It was merely dormant, so to speak; it did not constitute an organized conceptual tradition. For reasons which we cannot explain, diffusionism became a coherent, integrated conceptual tradition *in the 1890's*. Perhaps it was an instance in ethnological theory of "the pendulum effect": when one theory has advanced to a certain point a reaction sets in and a rival theory takes the field. This, apparently, is what happened when the Diffusionist school was born. Ratzel happened to be the forerunner, Graebner—and later, Father Schmidt—the leaders.

The "Functionalist" school, too, may well have originated as an intellectual-cultural reaction against evolutionism, historical reconstruction, and psychological interpretation, as Srinivas (1958, pp. xi-xii), Temple (1922), and Firth (1956, p. 294) have suggested.

We should distinguish between a mere conceptual school and a conceptual-social group school. The Evolutionist school was merely a body of doctrine, or theory; it was not a school in the sociological sense: there was no cult of a leader and disciples. But the *Kulturkreis* school was a social organization as well as a doctrine.

The organization and conduct of academic life is conducive to the formation of schools. A professor, especially if he is aggressive and has a charismatic personality, tends to attract some students and to repel others; the process is selective from the start. In addition to according the professor their admiration and respect, the students come to look to him for assistance in obtaining funds for field work, for recommendations for teaching positions, and for guidance and help in the publication of their work. The students enhance their prestige by identifying themselves with the master and by fostering solidarity among themselves. By contrast, it is interesting to note that some eminent men, such as Darwin, Spencer, and Morgan, were not university professors and did not become leaders of schools.

We are now better prepared to understand the schools of both Boas and Radcliffe-Brown. Both were social organizations. The Boas school was a "closed society," as we have seen. One belonged or he didn't; and some persons could not get in. In the school of Radcliffe-Brown, the unity and integrity of the social group was affected by the co-existence of Malinowski; there was a sharing of disciples until the latter's death when, as we have seen, the followers of Malinowski returned to Radcliffe-Brown. In both cases, the schools were social movements as well as doctrines in action. How these social movements, within the field of scholarship, are to be explained, specifically and in detail, is a question that I am not prepared to answer—any more than I can explain the origin of Mormonism or the Oneida Community. I am confident, however, that the explanation must be *culturological*, not psychological or anthropocentric.

Are schools "good" or "bad"? Do they further the interests of science or are their influences injurious to its cause? The answer is probably that something may be said for both sides. The "establishing of social anthropology as a reputable academic study in the most famous universities in the English-speaking world, and elsewhere"—no mean achievement—may be attributed to the school of Radcliffe-Brown. As one who observed the phenomenon at close range for a number of years, I would say that in the United States the Functionalist school, under the leadership of both Radcliffe-Brown and Malinowski, injected new life and spirit along with a definite, positive goal into the arena of an arid and worn-out Boasism.¹⁵

Franz Boas, also, helped to establish anthropology in American universities and did much to stimulate field research. But so far as constructive and progressive achievements in the realm of science—its philosophy, outlook, and goals—are concerned, there is precious little with which the Boas school can be credited. On the contrary, we believe that Boas' influence was reactionary and antiscientific at some points, such as for example, his attitude toward cultural evolutionism. This is not to say, of course, that no good anthropological work was done by individual members of the school of Boas. Good work was certainly done. But, I believe, the cult of Boas tended to constrict rather than to extend these achievements.

From the standpoint of the welfare and progress of science I believe that the effect of schools may be injurious, and we would so label the schools of Boas and Radcliffe-Brown. The injection of nonscientific elements—personality, character, father figures, loyalty, devotion, worship, charisma, closed-group solidarity—into a scientific enterprise can be exceedingly harmful. For one thing, it establishes a false, or at the very least an unrealistic, conception of the science—that it was created by its leader. The school has its folklore (Boas, the Physicist; Radcliffe-Brown, the Durkheimian). For another, the doctrines of the school constitute "the only true faith" for its members; all other doctrines are false, trivial, or irrelevant, and therefore must be opposed or ignored.¹⁶ The school sets the goals of endeavor, and these, like the doctrines, are the only valid ones to pursue. Each school has a monopoly of the "roads to truth." Finally, and most unfortunate of all, are the judgments and evaluations made by the school of its own achievements. Distortion, misrepresentation and even perversion of fact and truth have been the sorry fruit of the social organization of ethnological theory.

Schools are as mortal as the individuals who compose them. Eventually the leader must pass from the scene, and one by one the disciples follow in death as they did in life. Then science is released and a free play of concepts, goals, and values again becomes possible. The personification of human endeavor has been with us for a long time; it is an expression of the age-old philosophy of anthropomorphism and anthropocentrism. Whether or not this has been an advantage—has had biological survival value—to societies in the past—in religion, politics, and war—is a question that we shall not go into here. But personification in science has had, we believe, a stultifying and distorting effect. Schools in cultural anthropology have been means of mobilizing human effort and of providing it with inspiration and incentive. But their effect upon this science—its premises, goals, and the evaluation of its achievements—has been on the whole unfortunate. Perhaps with increased maturation our science may provide its own incentives and loyalties, determine its own goals, and establish its own criteria of values.

NOTES

1. This is an anthropomorphic way of saying it. It would be just as proper to say that concepts "held" Galileo, and that the interactive conceptual process, which is social as well as individual in dimensions, formulated laws in the nervous system of Kepler.
2. "He [Morgan] was the first anthropologist to organize systematic field research to solve definite scientific problems" (Fred Eggan, "Lewis H. Morgan and the Future of the American Indian," *Proceedings of the American Philosophical Society*, 109 [1965], 272).
3. In his later years, Boas expressed a different view of the reliability of tribal tradition: "The tradition of primitive people is an unsafe guide for the reconstruction of history . . ." (p. 668). "More often remote tradition becomes fantastic and intermingled with mythical tales. Tales of migrations are particularly liable to be purely mythological" ("Methods of Research," in *General Anthropology*, Franz Boas, ed. [Boston, 1938], p. 669).
4. I have discovered upon more than one occasion that merely to mention that a scholar is a Jew is to expose one's self to the suspicion—or accusation—of anti-Semitism. To cite a recent example: I quoted the foregoing passage from John Sholtz in "Individuality and Individualism: a Culturological Interpretation" (*Texas Quarterly*, Vol. 6 [Summer, 1963]; reprinted in *Innocence and Power*, Gordon Mills, ed. [Austin, 1965]). A reviewer (American) injected this passage into a review of another work of mine (*Man*, 74 [March-April, 1964], 63) with the comment that it was "open to a less charitable interpretation"—less charitable, that is, than his judgment upon another point in my work which he found "at best, unsubstantiated."

John Sholtz may himself be Jewish. He was writing in "a Jewish Magazine." He said nothing derogatory about the role of Jews in American anthropology. Quite the contrary: he found them "easily the leaders in the field." It is not clear to me why anyone, regardless of his religious faith or ethnic background, should take offense to Sholtz' statement or to my use of it.
5. By "scientific" here I believe that Radcliffe-Brown and his students meant "non-temporal" as distinguished from historical.
6. In *The Social Organization of Australian Tribes* Radcliffe-Brown places the systems of kinship terminology of the Karia and Aranda tribes in an evolutionary sequence (p. 452). I believe he justifies this with both evidence and reason. Lowie has called attention to this instance of evolutionary—or conjectural as Radcliffe-Brown would call it—interpretation (1937, p. 226).
7. Lowie's judgment (1937, p. 224) is that some of Radcliffe-Brown's nontemporal generalizations (about clanless societies and totemic ceremonies, etc.) "remain quite as unverified as the historical hypotheses against which . . . [he] inveighs. Secondly, the primary generalization offered is a trite statement of certain descriptive facts. It does not even pretend to explain those very important rituals which do not purport any bearing on the food supply."
8. In 1950-51 Radcliffe-Brown gave a series of lectures on "Philosophical Prolegomena of the Social Sciences" (Firth, 1956, p. 300).
9. This passage is omitted in the reprinting of "On Social Structure" in *Structure and Function in Primitive Society*.
10. It is true that some physicists, such as Sir James Jeans, Irving Langmuir, et al., have rejected the concept of cause, but this is far from true for all "modern theoretical scientists," or even physicists in general. In the year preceding Radcliffe-

Brown's 1937 seminar at the University of Chicago, in which his statement about causality was made, the Nobel prize-winning physicist, Max Planck, wrote:

It is true that the law of causality cannot be demonstrated any more than it can be logically refuted: it is neither correct nor incorrect; it is a heuristic principle; it points the way, and in my opinion it is the most valuable pointer that we possess in order to find a path through the confusion of events, and in order to know in what direction scientific investigation must proceed so that it shall reach useful results (*The Philosophy of Physics*, [New York, 1936], pp. 82-83).

11. "We now realize, with special clarity," wrote Albert Einstein, "how much in error are those theorists who believe that theory comes inductively from experience" ("Physics and Reality," *The Journal of the Franklin Institute*, Vol. 221, No. 3 [1936], 360). "There is no inductive method which could lead to the fundamental concepts of physics. Failure to understand this fact constituted the basic philosophical error of so many investigators of the nineteenth century. . . . Logical thinking is necessarily deductive; it is based upon hypothetical concepts and axioms" (*ibid.*, pp. 365-66).
12. See, e.g., Kazuta Kurauchi's essay, "Durkheim's Influence on Japanese Sociology," in *Émile Durkheim, 1858-1917*, Kurt H. Wolff, ed. (Columbus, Ohio, 1960).
13. Without having undertaken systematic search, I have noted the word culture in the English translations of *Les Règles de la Méthode Sociologique* (p. 9 of the translation) and *Les Formes Élémentaires de la Vie Religieuse* (p. 93 of the English translation). In the former instance, "culture" is the English rendering of "phénomènes sociaux"; in the second case "culture" appears in ". . . sociétés les plus différentes par la nature et le degré de culture. . . ."
14. For years A. L. Kroeber frequently used *social* instead of *cultural*, or used the words interchangeably: "And I was still so crude," he wrote in 1948, "as to call them [i.e., "cultural phenomena"] 'social' half the time when I obviously meant 'cultural'!" (Kroeber, "White's View of Culture," *American Anthropologist*, L [1948], 413, fn. 24; reprinted in Kroeber, *The Nature of Culture*).
15. In 1923 Edward Sapir wrote the following to Robert H. Lowie: "How melancholy it is to realize that his [Boas'] practical influence in American anthropology is now all but nil!" (*Letters from Edward Sapir to Robert H. Lowie*, with an Introduction [1956] and Notes by Robert H. Lowie [published in multigraph form by Luella Cole Lowie, 1965], p. 46).
16. In 1945 I published an essay, "History, Evolutionism and Functionalism: Three Types of Interpretation of Culture" (*Southwestern Journal of Anthropology*, I [1945], 221-248) in which I argued that all three of these kinds of interpretation are equally valid. As far as I know, this was the first time that such a point of view had been made explicit, at least among English-speaking anthropologists.

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