

480

The Ethnographer's Magic
and Other Essays in the
History of Anthropology

George W. Stocking, Jr.

The University of Wisconsin Press

© 1982

Contents

Retrospective Prescriptive Reflections	3
1 The Ethnographer's Magic: Fieldwork in British Anthropology from Tylor to Malinowski	12
2 The Boas Plan for the Study of American Indian Languages	60
3 Anthropology as <i>Kulturkampf</i> : Science and Politics in the Career of Franz Boas	92
4 Ideas and Institutions in American Anthropology: Thoughts Toward a History of the Interwar Years	114
5 Philanthropoids and Vanishing Cultures: Rockefeller Funding and the End of the Museum Era in Anglo-American Anthropology	178
6 Maclay, Kubary, Malinowski: Archetypes from the Dreamtime of Anthropology	212
7 The Ethnographic Sensibility of the 1920s and the Dualism of the Anthropological Tradition	276
8 Paradigmatic Traditions in the History of Anthropology	342
Postscriptive Prospective Reflections	362
References Cited	375
Manuscript Sources	419
Index	421

Paradigmatic Traditions in the History of Anthropology



Since the mid-nineteenth century, anthropology has claimed the status of a science—sometimes stridently, sometimes ambivalently; at times, by an assertive self-definition, at times, by a flexible redefinition of science itself. There is of course a long tradition of internal debate about epistemological issues and about the relationships of the component subdisciplines of a somewhat problematically integrated inquiry, some of which have, historically, a closer relation to the biological and earth sciences. Among themselves, anthropologists have been inclined to savor the fact that, unusually if not uniquely among scholarly disciplines, they participate in the umbrella associations of the natural sciences, the social sciences, and the humanities, and are able to assert a claim upon the financial resources of all three. But in facing the public, they have in general insisted on their status as members of a larger scientific community, and on the whole, the world of science has given credence to that claim—though not without an undercurrent of informal patronization, and moments of more serious questioning.

In the history of science, the status of anthropology has been somewhat marginal. When the history of what were then called the “behavioral” sciences achieved a journal in the middle 1960s, the “sciences of man” constituted one short and undifferentiated entry in the annual “Critical Bibliography” number of *Isis*, the leading journal in the history of science. But as the latter-day “revolt against positivism” gained momentum, various social scientific disciplines turned toward history; during the same period, increasing numbers of intellectual historians sought

new fields to plow. With the greater yield of work, the history of the behavioral/social/human sciences won a greater degree of recognition within the history of science. In 1981, psychology, sociology, economics, and cultural anthropology were each given separate recognition under “the sciences of man” (physical anthropology having already long been included under “biological sciences”); three years later, the encompassing rubric was gender-neutralized as “social sciences.”

Although one of my early articles was published in *Isis* (1964), and several in the early numbers of the *Journal of the History of Behavioral Sciences* (e.g., 1965), my ties to the historical profession began to stretch after I joined the University of Chicago Department of Anthropology in 1968. I served one term on the editorial board of *Isis*, and have been a member of the board of the *Journal of the History of Behavioral Sciences* since its founding. But I rarely attend annual professional meetings in history or the history of science. Nevertheless, I have always thought of myself, *au fond*, as an historian, and after 1981 this historical identity was to some extent reasserted, when, as the only “available” candidate, I became Director of the Morris Fishbein Center for the History of Science and Medicine, and continued in that position until 1992. In that capacity, I was of course particularly interested in furthering the history of the inquiries which, in the aftermath of the second revolt against positivism, were increasingly to be called the “human sciences”—for a number of years, in an informal interinstitutional faculty seminar known as the Chicago Group in the History of the Social Sciences, in 1986 at the Summer Institute in the History of the Social Sciences at the Center for Advanced Study in the Behavioral Sciences, and since 1983 in the History of the Human Sciences Workshop at the University of Chicago.

Even so, my role in the more general history of science community has continued to be a marginal one. As a senior representative of what to many historians of harder sciences is still a slightly dubious one, I am called upon occasionally to review books and manuscripts. But my only substantial contribution to the history of science literature *per se* is a recent attempt, for an encyclopedic compendium, to represent the whole history of anthropology, from the Greeks to the present, in six thousand words. The editors, reflecting no doubt the pervasive influence of Thomas Kuhn in the history of science over the last several decades, originally chose “Revolutions in Anthropology” as the rubric under which to represent the field in microcosm to a more general history of science community. Having moved toward the history of anthropology as a member of the same history department in the early 1960s, I had long found Kuhn’s work congenial. *The Struc-*

ture of Scientific Revolutions (1962) was for me one of those few radically innovative works that one reads with a sense of *déjà vu*—as if what one was inclined already to believe was being now finally made explicit. Construing paradigms as unstated bodies of assumption grounded more in practice than in theory, as incommensurable disciplinary “world views,” Kuhn’s argument seemed much in the tradition of modern American cultural anthropology. In some earlier writings, I made a fairly self-conscious effort to apply the paradigm notion, worrying whether the social sciences were “pre-paradigmatic” and to what extent they practiced “normal science” (1965, 1968a). But I always regarded Kuhn’s work as heuristic rather than definitive, and have been inclined to treat the idea of paradigms as a resonant metaphor, to be applied flexibly when it seemed to facilitate the understanding of particular historical episodes. Although well aware that philosophers and historians of science have engaged in debate about the meaning of paradigm and whether Darwinism was really a “scientific revolution” (Greene 1980), I have persisted in this loose construction, albeit not without a certain rhetorical and conceptual discomfort (1987a).

Responding to ideas Dell Hymes suggested about paradigms, traditions, and “cynosures” in his introduction to the history of linguistics symposium (Hymes 1974), I have recently found it convenient to think in terms of “paradigmatic traditions.” On the one hand, it seemed that certain episodes in the history of anthropology—notably, the emergence of social evolutionism around 1860 and its rejection after 1900—could fruitfully be thought of as scientific revolutions, in which the opposing points of view had something of the character of paradigms, insofar as they were held by distinct groups of inquirers with different assumptions about what the proper aims and methods of anthropological research should be. But since it was clear that the major alternatives before 1900 could be traced back to the Greeks, and that until well into the twentieth century the history of anthropology could be seen as an alternation of their cynosuric dominance, it seemed that the paradigm notion, which in Kuhn’s formulation emphasizes synchronic discontinuity, needed to be modified to allow for paradigm-like bodies of assumption that perdured through long periods of time. Hence, “paradigmatic traditions”—with apologies to Tom Kuhn and all those involved in the still on-going epistemological, conceptual, and methodological debate about his work.

Attentive readers of this essay will no doubt notice that after about 1920 the idea of paradigmatic traditions fades into the background. Before that time there is an implicit correlation of paradigmatic domi-

nance and periodization, with developmentalism/evolutionism dominant before 1800 and again after 1860, and the early-nineteenth-century ethnological tradition reemerged after 1900 (cf. 1978b, which attempted a schematic definition of six major periods in the history of anthropology). There is also a suggestion that, in the wake of the early-twentieth-century “revolution in anthropology,” disciplinary discourse in what I have called the “classical period” (c. 1920–c. 1965) was unified by a synchronic and broadly “functional” paradigm—which, in terms of the previous history of anthropology, might find its traditional roots in either Montesquieu or Herder, depending on whether ethnographic integration is conceived in British functional (or structural-functional) or in American cultural terms. More recently, coming from another national tradition, “structuralism” may claim an alternatively integrative paradigmatic status, for which a “traditional” ancestry may no doubt be found (Levi-Strauss 1962).

But by the end of the classical period, paradigms and periods—historiographical heuristics ever to be lightly held—become even more problematic. The fragmentation of anthropology: first among the “four fields” and then among the various “adjectival anthropologies” into which sociocultural anthropology has subsequently become ever more divided, makes it increasingly difficult to find a consistent reference point for the paradigm idea. A recent history of “political anthropology” identified six “paradigms” that had developed in this subfield by 1974 (the “action,” “processual,” “neo-evolutionary,” “structural,” “political economy,” and “culture history” paradigms), and referred as well to the “Oxbridge,” “transactional,” “symbolic interactional,” “game theory,” “subaltern,” “Marxist,” and “interpretive,” and an as-yet-undefined “new paradigm [of] the 1990s” (Vincent 1990:407, 418, 386, 402, 424)—thus reinstating the indeterminacy that historians and philosophers of science have found in the paradigm concept since its original formulation thirty years ago (cf. Kuhn 1974).

Periodization, too, seems not without its problems as one approaches the present. After devoting several seminars to anthropology before the “crisis” and since its “reinvention,” I am more inclined to see World War II as perhaps a significant break within the classical period. Assuming, however, that there was a substantial unity of anthropology from its “ethnographicization” to its “crisis,” the question remains how to characterize the years since then. It is an artifact of historical periodization that the last period always ends in the present moment; but whether that moment, or any other recent moment, marks a significant historical transition is another matter—about which my backward-looking historicist temperament makes me disinclined to specu-

late. For the present, then, here is my "big picture" of anthropology's past—painted on a very small canvas, in very tight strokes.

Defining the Domain of Anthropology

In 1904 Franz Boas defined the domain of anthropological knowledge as "the biological history of mankind in all its varieties; linguistics applied to people without written languages; the ethnology of people without historic records; and prehistoric archeology." More than any other "anthropologist," Boas may be said to exemplify the putative unity of this domain, since (virtually alone among his confrères) he made significant contributions to each of these four inquiries in the course of his long career. But despite the fact that he was perhaps the most important single figure in the institutionalization of an academic discipline called "anthropology" in university departments in the United States, Boas already felt in 1904 that there were "indications of its breaking up." The "biological, linguistic and ethnologic-archeological methods are so distinct," he believed, that the time was "rapidly drawing near" when the two former branches of anthropology would be taken over by specialists in those disciplines, and "anthropology pure and simple will deal with customs and beliefs of the less civilized peoples only . . ." (1904b:35).

Given the weight of institutional inertia and of residual commitment to the norm of disciplinary unity, it remains arguable today whether Boas' prediction is yet likely to be achieved. Nevertheless, the fact that its leading practical exemplar regarded the unity of anthropology as historically contingent rather than epistemologically determined suggests that no general historical account of that "science" may take its unity for granted. In spite of the all-embracing etymological singularity of the term *anthropology* (Greek *anthropos*: man; *logos*: discourse), the diverse discourses that may be historically subsumed by it have only in certain moments and places been fused into anything approximating a unified science of humankind. In continental Europe in Boas' time, "anthropology" referred (and often does today) to what in the Anglo-American tradition has been called "physical anthropology." As such, it was distinguishable from and historically opposed to "ethnology"—a discourse that, etymologically, was somewhat more diversitarian (Greek *ethnos*: nation).

In this context, the historical development of anthropology may be contrasted to two ideal typical views of disciplinary development. The first is a Comtean hierarchical model in which the impulse of positive knowledge is successively extended into more complex domains of natural

phenomena. The second is a genealogical model in which, within each domain, disciplines may be visualized as growing from a single undifferentiated "ur"-discourse (with the biological sciences developing out of natural history, the humanities out of philology and the social sciences out of moral philosophy). As against these two fission models, "anthropology" in its inclusive Anglo-American sense is better viewed as an imperfect fusion of modes of inquiry that were quite distinct in origin and in character—deriving in fact from all three of these undifferentiated "ur"-discourses.

Insofar as a common denominator may be extracted from Boas' contingent descriptive definition of anthropology, it would seem to imply an opposition between Europeans, who have written languages and historical records, and "others," who have not. Indeed, it may be argued that the greatest retrospective unity of the discourses subsumed within the rubric "anthropology" is to be found in this substantive concern with the peoples who were long stigmatized as "savages," and who, in the nineteenth century, tended to be excluded from other human scientific disciplines by the very process of their substantive-cum-methodological definition (the economist's concern with the money economy; the historians' concern with written documents, etc.). From this point of view, to study the history of anthropology is to study the attempt to describe and to interpret or explain the "otherness" of populations encountered in the course of European overseas expansion. Although thus fundamentally (and oppositionally) diversitarian in impulse, such study has usually implied a reflexivity which reencompassed European self and alien "other" within a unitary humankind. This history of anthropology may thus be viewed as a continuing (and complex) dialectic between the universalism of "anthropos" and the diversitarianism of "ethnos" or, from the perspective of particular historical moments, between the Enlightenment and the Romantic impulse. Anthropology's "recurrent dilemma" has been how to square both generic human rationality and the biological unity of mankind with "the great natural variation of cultural forms" (Geertz 1973:22).

The Biblical, Developmental, and Polygenetic Traditions

A second unifying tendency within Boas' definition is historical, or more generally diachronic, since history in the narrow sense seemed precluded by the lack of documents. For Boas, the "otherness" which is the subject-matter of anthropology was to be explained as the product of change of time. Although Boas in fact wrote at the verge of a revolutionary shift toward a more synchronic anthropology, the history of anthropology up until his time may be schematized in terms of the interplay of two major diachronic traditions that were, in a broad sense, paradigmatic, both of

them counterpointed by a more synchronic tradition which, because of its heterodoxy, only very briefly achieved paradigmatic status. In the discussion that follows, these traditions will be designated as the "biblical" (or "ethnological"), the "developmental" (or "evolutionist"), and the "polygenetic" (or "physical anthropological").

The ultimate roots of anthropological thought are more often traced to the Greek than to the biblical tradition. However, it may be argued that during the period of European expansion the underlying paradigmatic framework for the explanation of "otherness" derived from the first ten chapters of Genesis. Many intellectual currents contributed to anthropological speculation, among them environmentalist and humanist assumptions from the Hippocratic and Galenic traditions, hierarchical notions from the "Great Chain of Being," medieval conceptions of the monstrous, etc. (Friedman 1981; Lovejoy 1936; Slotkin 1965). But the dominant paradigmatic tradition (paradigmatic in the sense of providing a more or less coherent *a priori* framework of assumption defining both relevant problems and the data and methods for their solution) was that iconically embodied in the second of John Speed's "Genealogies of Holy Scriptures" in the King James Bible. There, growing from the roof of the Ark resting on the top of Mount Ararat in Armenia, was a genealogical tree with three major branches: the descendants of Japhet in Europe, of Sem in Asia, and of Ham in Africa, traced on out to their various representatives in the ancient world ("Phrigians," "Bactrians," "Babylonians," etc.) (Speed 1611). In this context, the fundamental anthropological problem was to establish putative historical links between every present human group and one of the branches of a biblical ethnic tree that linked all of humankind to a particular descendant of Adam and Eve. Since what had diversified humankind in the first instance was the confusion of tongues at Babel, the privileged data for reestablishing connections were similarities of language, augmented by such similarities of culture as survived the degenerative processes that were a concomitant of migration toward the earth's imagined corners. Since all humans were offspring of a single family, and ultimately of a single pair, the physical differences among them were secondary phenomena, characteristically attributable to the influence of the environments through which they had migrated during the six millennia allowed by the biblical chronology—if not to the direct intervention of God (as in "the curse of Ham").

The biblical anthropological tradition, which saw the (characteristically degenerative) differentiation of humankind in terms of movement through space within a limited and event-specific historical time, may be contrasted with a Greco-Roman paradigmatic tradition deriving from the speculations of Ionian materialists. Perhaps most influentially embodied in Lu-

cretius's *De Rerum Natura*, this tradition saw time as an enabling rather than a limiting factor, and conceived diachronic change in progressive processual rather than degenerative historical terms. Rather than losing diversity given knowledge as they moved through space in time, human groups acquired knowledge gradually, responding to organic needs and environmental stimuli in an adaptive utilitarian manner, as they groped their way forward step by step from a state near that of the brutes to the most advanced civil society. Although human differentiation was construed in terms of status on a generalized developmental scale rather than as the product of a specific sequence of historical events, the Greco-Roman paradigm was still in a broad sense diachronic (Hodgen 1964).

While the biblical and the developmental traditions represent the dominant paradigmatic alternatives in Western anthropological thought before 1900, it is useful to distinguish a third major paradigmatic tradition: the polygenetic. Foreshadowed in tribal and classical notions of autochthonous origin, it became a matter of more serious speculation in the aftermath of the discovery of the New World, the peopling of which posed a major problem for the orthodox monogenetic tradition. A few writers, most notoriously Isaac de la Peyrère in 1655, went so far as to suggest that the peoples of the New World did not descend from Adam (Popkin 1987). However, it was nearly a century before Linnaeus included mankind (American/choleric; European/sanguine; Asiatic/melancholic; African/plegmatic) in the *System of Nature* (1735), and still a generation later before systematic comparative human anatomical data began to be collected. Even then, most of the early physical anthropologists remained, like Johann Blumenbach, staunchly monogenist. But given the growth of comparative data within the framework of a static pre-evolutionary view of biological species, a "polygenetic" approach to human differentiation became in the nineteenth century an alternative to be considered seriously. From this point of view, human "races" (often distinguished by the forms of their crania) were, like animal species, aboriginally distinct. Unaffected by the forces of environment, they had remained constant throughout the relatively short span of human historical time—as the images on the 4,000-year-old monuments discovered by Napoleon's expedition to Egypt confirmed (Slotkin 1965).

The Darwinian Revolution and the Differentiation of National Anthropological Traditions

Although Rousseau had envisioned in 1755 a unified science of man carried on by philosopher-voyagers who, shaking off "the yoke of national prejudices," would "learn to know men by their likenesses and their dif-

ferences" (1755:211), it was more than a century before his dream began to be realized. For most of that time, the vast bulk of anthropological data was collected incidentally by travellers, missionaries, colonizers, and naturalists. Insofar as the activity was tied to a knowledge-tradition, it was much more likely to be that of natural history than social theory. Furthermore, the forms of "anthropology" institutionalized in the major European nations differed strikingly in their relation to the three paradigmatic traditions just described.

During the pre-Darwinian nineteenth century, the focal anthropological issue was posed by the explosion of the data of human diversity that was produced by European expansion, in the context of advances in the regnant sciences in the human and biological domains – comparative linguistics and comparative anatomy. From a classificatory and/or genetic point of view, the central question was "Is mankind one or many?" Until midcentury, comparative Indo-European (i.e., Japhetic) linguistics provided a model of inquiry which promised to provide a classification of humankind in terms of its most distinctive feature, but which would also link all human groups to a single source. Exemplified in the works of the staunchly monogenist James Cowles Prichard, this goal was institutionalized in several of the "ethnological" societies founded around 1840 (GS 1971, 1973a).

By the 1850s, however, a distinctly physical anthropological current, modelling itself on comparative anatomy and often polygenist in tendency, had begun to separate itself from the ethnological (formerly biblical) paradigm. Foreshadowed in the works of certain French investigators, and in the "American School" of Samuel G. Morton (Stanton 1960), this trend was realized institutionally in the "anthropological" societies founded by Paul Broca in Paris in 1859 and by James Hunt in London in 1863 (GS 1971). Although the term *anthropological* had in fact been previously employed as a theological/philosophical category, it was now used to affirm the need for a naturalistic study of humankind as one or more physical species in the animal world.

This newly asserted physical anthropological tendency in fact proved resistant to Darwinism, which seemed to the polygenetically-inclined simply a new and speculative form of monogenism (GS 1968a:44–68). However, the Darwinian revolution was to have a major impact on speculation in the older ethnological tradition. On the one hand, the greatly extended "antiquity of man," confirmed by the discoveries at Brixham Cave in 1858, made the gradual formation of contemporary races by modification of a single apelike progenitor seem more plausible. On the other, the revolution in time made extremely unlikely the ethnological task of establishing plausible historical connections over the whole span of human existence. Furthermore, Darwinism posed a problem for which the new "prehistoric"

archeology offered extremely inadequate evidence: providing a convincing evolutionary account of the cultural development that might link modern man with an apelike ancestor. In this context, the development paradigm came again to the forefront of anthropological attention in the last third of the nineteenth century, especially in the Anglo-American tradition (GS 1987a; cf. Van Riper 1990).

During this period, sociocultural evolutionists attempted to synthesize the data of contemporary "savagery" collected by travellers and naturalists (including that now obtained by correspondence or in response to more formal questionnaires such as the *Notes and Queries in Anthropology* prepared by a committee of the British Association for the Advancement of Science in 1874). By arranging such present synchronic data on a diachronic scale, it was possible for "armchair" anthropologists to construct generalized stage-sequences of development in each area of human culture. In Britain, E. B. Tylor (1871) traced the evolution of religion from primitive "animism" through polytheism to monotheism, while John McLennan (1865) followed the evolution of marriage from primitive promiscuity through polyandry to monogamy. In the United States, Lewis Henry Morgan (1877) traced a more general development from "lower savagery" through three phases of "barbarism" up to "civilization."

These sequences depended on a generalized assumption of human "psychic unity," which enabled anthropologists to reason backward from an irrational "survival" in a higher stage to the rational utilitarian practice underlying it. However, the sequences thus reconstructed by the "comparative method" in fact assumed a polar opposition between "primitive" and "civilized" mentality. And in the mixed Darwinian/Lamarckian context of late-nineteenth-century biological thought these cultural evolutionary sequences took on a racialist character. The human brain was seen as having been gradually enlarged by the accumulative experience of the civilizing process, and the races of the world were ranked on a double scale of color and culture (as when Tylor suggested that the Australian, Tahitian, Aztec, Chinese, and Italian "races" formed a single ascending cultural sequence). While much of day-to-day anthropological inquiry reflected a continuing interest in the ethnological affinities of different groups, what is sometimes called "classical evolutionism" was both the theoretical gyrosure and the dominant ideological influence in anthropology in the later nineteenth century (GS 1987a).

In general, anthropological thought in the late nineteenth century attempted to subsume the study of human phenomena within positivistic natural science. However, "anthropology" itself was by no means a transnational scientific category. In England, the post-Darwinian intellectual synthesis of ethnological and polygenist tendencies in classical evolutionism

was reflected institutionally in 1871 by the unification of the Ethnological and Anthropological Societies in the Anthropological Institute. In the United States, a similarly inclusive viewpoint was evident in J. W. Powell's governmental Bureau of Ethnology (1879), which, despite its title, had as its avowed mission the organization of "anthropologic" research among American Indians (Hinsley 1981). In principle if not always in practice, "anthropology" in the Anglo-American tradition attempted to unify the four fields later specified by Franz Boas. By contrast, on the Continent, where Darwinism did not exert such a strongly unifying influence, "anthropology" continued to refer primarily to physical anthropology. Although Broca's *École d'Anthropologie* included chairs in sociology and ethnology, those studies had for the most part a quite separate development, largely under the aegis of Emile Durkheim and his students (Gringeri 1990; GS 1984b). And although by 1900 the fossil gap between existing primate forms and the anomalously large-brained Neanderthals had been narrowed by the discovery of "Java Man," physical anthropology continued to be heavily influenced by a static, typological approach to the classification of human "races," primarily on the basis of measurements of the human cranium, using the "cephalic index" developed by Anders Retzius in the 1840s (Erickson 1974).

The Critique of Evolutionism in American Cultural Anthropology

In this context, the critique of evolutionary assumption elaborated by Franz Boas between 1890 and 1910 contributed to a revolutionary reorientation in the history of anthropology. Born of a liberal and assimilated German-Jewish family, and trained in both physics and geography, Boas began his career from a position of cultural marginality and scientific internecinity, somewhere between the dominant positivistic naturalism on the one hand, and the romantic and *Geisteswissenschaft* traditions on the other (an opposition classically delineated in his 1887 essay on "The Study of Geography"). After a year of ethnogeographic fieldwork among the Baffin Island Eskimo, Boas settled in the United States, carrying on general anthropological fieldwork among the Indians of the Pacific Northwest, where he worked under the auspices of both Powell's Bureau of Ethnology and a committee of the British Association for the Advancement of Science chaired by Tylor. By 1896, Boas had developed a neo-ethnological critique of "the comparative method" of classical evolutionism. Arguing on the basis of a study of the borrowing and diffusion of cultural elements among Northwest Coast Indians, he insisted that detailed historical investigations of specific culture histories must precede the attempt to derive laws of cul-

tural development. Parallel to this, Boas criticized the evolutionary idea of "primitive mentality," arguing that human thought generally was conditioned by culturally varying bodies of traditional assumption—a viewpoint sustained also by his analyses of American-Indian grammatical categories. Similarly, his physical anthropological researches—including a study of the modification of headform in the children of European immigrants—called into question racialist arguments based on cranial typology.

Boas' anthropology was characteristically critical rather than constructive. Nevertheless, his work laid the basis for the modern anthropological conception of culture as pluralistic, relativistic, and largely freed from biological determinism. His student A. L. Kroeber, a major articulator of the cultural viewpoint, initially invoked the autonomy of the cultural in 1917, simply as a heuristic device, and since then, there has been a recurrent anthropological interest in the culture/biology interface. But the general thrust of Boasian anthropology was to mark off a domain from which biological determinism was excluded. Initially, that delimitation depended on an insistence on the essentially historical character of cultural phenomena, as exemplified in Edward Sapir's "Time Perspective in Aboriginal American Culture: A Study of Method" (1916). But if the first-generation Boasians occasionally spoke of themselves as the American Historical School, the major thrust of Boasian anthropology after 1920 was in fact away from historical reconstruction. On the one hand, the emergence of a more time-specific archeology (with the development of stratigraphic approaches after 1910, augmented after World War II by carbon 14 dating) tended to devalue historical reconstructions based on the distribution of "culture elements" over "cultural areas." On the other, the Boasian interest in the cultural basis of human psychological differences led toward a synchronic study of the integration of cultures and of the relation of "culture and personality"—tendencies archetypified in Ruth Benedict's widely influential *Patterns of Culture* (1934).

Although the "culture and personality" movement and the study of "acculturation" were being superseded by the 1950s by more sociologically oriented approaches, "culture" remained the predominant focus of anthropological inquiry in the United States. As graduate training began its explosive spread beyond the four centers founded before World War I (Harvard, Columbia, Berkeley, and Pennsylvania) and the half-dozen additions of the interwar period, it usually continued to include at least introductory training in each of the "four fields." Most practitioners, however, had long since specialized in no more than one of them; and physical anthropologists, linguists, and archeologists had, during the interwar period, founded their own professional organizations. While the American Anthropological Association (founded in 1902) continued to include special-

ists in all four fields, it was dominated by those who specialized in what Boas and the first generation of his students still called ethnology — which by the 1930s was in the process of being rechristened cultural anthropology.

Fieldwork, Functionalism, and British Social Anthropology

In Great Britain, the early-twentieth-century "revolution in anthropology" took a somewhat different course. As in the United States, where the Boasians carried on and elaborated the fieldwork tradition pioneered by the Bureau of Ethnology, a key factor was the development of a corps of academically trained ethnographic fieldworkers. However, what was to become the archetypical field situation for British anthropologists differed considerably from that of their early Boasian counterparts. In the United States, where transcontinental railways facilitated relatively short visits to Indian reservations, ethnographers studied the "memory culture" of elder informants, often by collecting "texts" (which Boas thought might provide for a nonliterate culture the equivalent of the documentary heritage that was the basis of humanistic study in the Western tradition). By contrast, British ethnographers, travelling weeks by sea to the darker reaches of the world's largest empire, became the archetypical practitioners of extended participant observation of the current behavior of still-functioning social groups. Foreshadowed in the work of Baldwin Spencer and Frank Gillen among the Australian Arunta in 1896, implemented among the graduates of A. C. Haddon's Torres Straits Expedition and by younger members of the "Cambridge School" in the first decade of the century, the "tone-ethnographer" model of inquiry was in fact formalized by W. H. R. Rivers in his description of the "concrete method" for the 1912 revision of *Notes and Queries*. The person most closely associated with this development, however, was Bronislaw Malinowski, who came from Poland in 1910 to study under Edward Westermarck and Charles Seligman at the London School of Economics. During World War I, Malinowski spent almost two years among the Trobriand Islanders off the northeastern coast of New Guinea, and in 1922 he gave the new methodology its mythic charter in the opening chapter of *Argonauts of the Western Pacific*.

During the 1920s, Malinowski moved briefly toward Freudian psychoanalysis by offering the matrifocal Trobriand family to suggest a modification of the universal Oedipus complex (GS 1986b). However, there was no British analogue to the American culture-and-personality movement. The latter may be regarded as offering an explanatory alternative to nineteenth-century evolutionary assertions of racial mental differences. In Britain, however, the critique of evolutionism focussed not on its bio-

logical implications, but rather on its tendency, archetypified in the *Golden Bough* of James G. Frazer, to explain human behavior in intellectualist utilitarian terms (Ackerman 1987). By 1900, attacks had already begun on Tylor's doctrine of animism, which had explained human religious belief as a premature and failed science (with the experience of dreams and death suggesting the hypothesis of a soul distinct from the human body). Echoing William James, R. R. Marett suggested a "pre-animistic" basis of religious belief in the much more affect-laden Melanesian concept of *mana* (an awe-inspiring supernatural power manifesting itself in the natural world). During the following decade, theoretical discussion centered on the mixed socioreligious phenomenon of totemism, which McLennan had defined in 1869 in terms of the linkage of animistic belief and exogamous matrilineal social organization. To this, William Robertson Smith had added the idea of the occasional communal consumption of the totem animal — an armchair conception which to Frazer seemed confirmed ethnographically by Spencer and Gillen's research among the Arunta (R. Jones 1984). In the decade before World War I, social anthropological debate swirled about the problem of totemism, with special reference to the Arunta and other Australian data, which were assumed by evolutionists to provide evidence of the most primitive human state.

It was in this context that British anthropology, which in its Tylorian and Frazerian phase gave priority to the problem of religious belief, shifted toward the study of religious ritual, and more generally, toward the study of kinship and social organization, which had been a special concern of the American evolutionist Lewis Henry Morgan during his pre-evolutionary "ethnological" phase (Trautman 1987). Building on his own pioneering ethnographic study of the Iroquois in the 1840s, Morgan had attempted to solve the problem of the peopling of America by using an ethnographic questionnaire to collect worldwide data on *Systems of Consanguinity and Affinity* (1871). Recast in developmental terms, his distinction between the "classificatory" and "descriptive" systems of kinship provided a conceptual framework for the ethnographic work of his Australian correspondents Lorimer Fison and A. W. Howitt. Augmented by the "genealogical method" developed by Rivers in the Torres Straits in 1898, Morgan's approach was eventually to provide the conceptual groundwork for modern British social anthropology, although not, however, until it had been detached from its diachronic evolutionary framework.

That process took place in two phases in the work of Rivers and his student A. R. Radcliffe-Brown. Rivers himself underwent a "conversion" from evolutionism to a diffusionary "ethnological analysis of culture" in 1911. However, his attempt to reconstruct *The History of Melanesian Society* (1914a) was still heavily dependent on the evolutionary concept of

"survival," which assumed that certain existing social customs or kinship terms need not be explained in terms of their present function, but rather in terms of their correspondence with prior social organizational forms. In contrast, Radcliffe-Brown moved away from evolutionism via the more functionalist sociology of Emile Durkheim. His break with Rivers focussed specifically on the utility of "survivals" in sociological analysis, and involved a general rejection of any "conjectural" approach to diachronic problems in "social anthropology," which in 1923 he took some pains to differentiate from "ethnology" (GS 1984b).

At that time British anthropology was excited by the confrontation between the "heliothetic" diffusionism of Rivers' disciples Grafton Elliot Smith and William Perry at University College London and the psychobiological functionalism of Malinowski at the London School of Economics. Sustained by grants from the Rockefeller Foundation, Malinowskian functionalism had, by 1930, become the dominant British current. But during the next few years some of Malinowski's more important students shifted their theoretical allegiance to Radcliffe-Brown, who after two decades of academic wanderings (from Cape Town to Sydney to Chicago), finally succeeded in 1937 to the chair at Oxford. Although the Association of Social Anthropologists formed at Oxford in 1946 included representatives of several different viewpoints, it was Radcliffe-Brown's synchronic natural scientific study of "social systems" — overlaid upon the Malinowskian fieldwork tradition — that gave British social anthropology its distinctive character.

The Synchronic Revolution, the "Classical Period," and the Emergence of International Anthropology

Despite these differences of phase and focus, there were many common features in the development of British social and American cultural anthropology in the first half of the twentieth century. In both countries, anthropology in the pre-academic museum period had been oriented largely toward the collection of material objects (whether artifacts or bones) carried into the present from the past; in both cases there was a dramatic turn toward the observational study of behavior in the present. Although an interest in evolutionary or historical questions never disappeared entirely from either national tradition, anthropological inquiry was no longer primarily conceived in diachronic terms. And while Radcliffe-Brown insisted, during his Chicago period, on the differences between his viewpoint and the more dilute "functionalism" of some American cultural anthropologists, there is a looser sense in which one may speak of synchronic functionalism as a paradigm in the Anglo-American tradition. This was even

more the case after World War II, when American anthropologists went overseas in large numbers for fieldwork, and began at home to feel the influence of functionalist theory in American sociology.

In both countries, one may speak of anthropology as having become "ethnographicized." Although the goal of cross-cultural comparison and scientific generalization continued to be acknowledged, the most distinctive common feature of Anglo-American anthropology in what may be called its "classical" period (c. 1925-c. 1965) was the central role of ethnographic fieldwork. Rather than providing items of information for armchair anthropological theorists, fieldwork became the certifying criterion of membership in the anthropological community and the underpinning of its central methodological values: i.e., participant observation in small-scale communities, conceived holistically and relativistically, and given a privileged role in the constitution of theory. In both countries, this ethnographically oriented study of social and cultural behavior tended to separate from and to dominate the other anthropological subdisciplines, although in the more pluralistic structure of American academic life, the ideal of a general anthropology uniting the traditional four fields continued to have a certain potency.

Elsewhere, however, the course of interdisciplinary development was rather different. On the European continent, where the inclusive four-field tradition had never taken root, physical anthropology continued to have a largely separate development on into the twentieth century, and to be relatively unaffected by the Boasian critique — especially in Germany, where during the Nazi period, the discipline was redefined as *Rassenkunde* (Proctor 1988). In Germany and in central Europe, the ethnological tradition continued to be strongly diffusionist and historical up until the mid-twentieth century, although some ethnographic fieldwork was carried on. In France a modern ethnographic tradition did not develop until after the founding in the 1920s of the Institut d'Ethnologie, in which Durkheim's nephew Marcel Mauss played a leading role (Clifford 1982; Gिंगeri 1990). It was not until 1982 that the French equivalents of cultural anthropologists were to take the lead in founding the Société d'Anthropologie française after the American model. This development reflected not only the intellectual interchange that had occurred between the French and the Anglo-American traditions after 1960 under the influence of the structuralism of Claude Lévi-Strauss but also the influence of a tendency that can be called international anthropology, or the internationalization of the Anglo-American tradition.

Although international congresses of "anthropologists" or "prehistorians" or "Americanists" had been held periodically since the 1860s, it is only since World War II that International Congresses of Anthropological and Eth-

nological Sciences have been held on a regular basis over a long period (in Philadelphia, Moscow, Tokyo, Chicago, Vancouver, Delhi, and Zagreb). Reinforced after 1960 by the international journal *Current Anthropology*, edited by Sol Tax, these congresses have been at the same time forums for diversity and media for the diffusion of a certain homogenizing tendency, in which sociocultural anthropology in the emergent Anglo-Franco-American mode has predominated, but the other major subdisciplines have continued to be represented. However, the embrace four-field conception associated with the American tradition has still had a certain inertial influence, reinforced by the overwhelming numerical predominance of American anthropologists within the world anthropological community.

The "Crisis" and "Reinvention" of Anthropology

In the very period in which an international anthropology began to be realized, however, there were dramatic changes in the world historical relationship of the peoples who had traditionally provided the scholars and the subject matter of anthropological inquiry. For more than a century, the anticipated disappearance of "savage" (or "primitive" or "tribal" or "pre-literate") peoples under the impact of European expansion had been a major impetus to ethnographic research, which was carried on under an umbrella of colonial power. By the 1930s, these categories had already become problematic, and field research was beginning to be undertaken in "complex" societies. But despite the postwar interest in peasant communities and the processes of "modernization," anthropology retained its archetypically asymmetrical character, as a study of dark-skinned "others" by light-skinned Euro-Americans. With the end of colonial empires, however, the peoples that anthropologists had traditionally studied were now part of "new nations" oriented toward rapid sociocultural change, and their leaders were often unresponsive to an inquiry which, even after the critique of evolutionary racial assumption, continued to be premised on sociocultural asymmetry. Indeed, many Third World intellectuals now began to regard as ideologically retrograde (and even as racist) the characteristic modern anthropological attitude of relativistic tolerance of cultural differences. What had served in the 1930s to defend "others" against racialism seemed now to justify the perpetuation of a backwardness founded on exploitation. In the politically charged context of major episodes of postcolonial warfare, there had developed by the late 1960s what some were inclined to call the "crisis of anthropology" (GS 1982b).

The sense of malaise — which was widespread in the human sciences — manifested itself in a number of ways: substantively, ideologically, methodologically, epistemologically, theoretically, demographically, and insti-

tutionally. In the face of rapid social change and restrictions on access to field sites, it was no longer realistic, even normatively, to regard the recovery of pure, uncontaminated non-European "otherness" as the privileged substantive focus of anthropological inquiry. Nor was it possible to regard such inquiry as ethically neutral or innocent of political consequences. A new consciousness of the inherently problematic reflexivity of participant observation called into question both the methodological and epistemological assumptions of traditional ethnographic fieldwork. In the context of a general questioning of positivist assumption in the human sciences, there were signs of a shift from homeostatic theoretical orientations to more dynamic ones. And even the very growth of the field was now a problem, as the government funding of the 1950s and 1960s began to be restricted, and Ph.D.s began to overflow their accustomed academic niches, beyond which anthropology had yet to establish a viable claim to significant domestic social utility. In the face of predictions of the "end of anthropology," there were, by the early 1970s, radical calls for its "reinvention" (Hymes 1972).

The majority of anthropologists, however — reflecting either a residue of prelapsarian confidence or a sense of the weight of institutional inertia — seem to have taken for granted that the discipline would carry on indefinitely. And indeed, it seemed clear that by the mid-1980s, the crisis had been domesticated. A decade after the call for the discipline's reinvention, the major academic anthropology departments continued to carry on a kind of business as usual, despite the difficulties of funding research and the still-constricted job market for the students they were training. Nevertheless, it seems clear that the classical period of modern anthropology had come to an end sometime after 1960, and the usual business of postclassical anthropology differed in significant respects from what had gone before.

Reflexivity, Fission, and the Dualism of the Anthropological Tradition

At the demographic center of the discipline in the United States, the centrifugal forces observed by Boas in 1904 had multiplied. It was no longer a question simply of the coherence of the four major subdisciplines, but of a multiplication of "adjectival anthropologies" (applied, cognitive, dental, economic, educational, feminist, historical, humanistic, medical, nutritional, philosophical, political, psychological, symbolic, urban, etc.) — many of them organized into their own national societies. And while it was possible to interpret this proliferation as a sign of the continued adaptive vigor (or the successful reinvention) of the disciplinary impulse, there

was inevitably concern about how, in the last decade of the twentieth century, that impulse might be defined.

Once the reflexivity implied in the original anthropological impulse had been raised permanently to disciplinary consciousness, and the forces of sociocultural change had removed many of the more obvious distinctions on which an asymmetrical anthropology had been premised, it was clear that "anthropology pure and simple" would not "deal with the customs and beliefs of the less civilized peoples only." But it was less clear how a more anthropologically embrative study would be carried on. In many situations, both in the developing countries and the traditional centers of the discipline, the line between anthropology and applied sociology was no longer clear. At the same time, the traditional concern with exotic otherness persisted, although now once again historically and textually oriented, in the context of rapid cultural change and the reaction against positivistic natural scientific models. Not only were particular cultural groups beginning to be studied in more historical terms, but the distinctive features of otherness itself—including now the notion of the "tribe"—were beginning to be seen as contingent products of the historical interaction of European and non-European peoples in the context of world historical processes. As the manifestly observable differences between peoples diminished, culture was pursued into the crevices of encroaching homogeneity. In this context, there was an increasing sense of the problematic character of the central concept in terms of which otherness had long been interpreted by anthropologists.

For more than a century, the idea of culture had been the single most powerful cohesive force in anthropological inquiry. Although that concept was relativized and given an autonomous determinism by the Boasian critique of evolutionary racial assumption, biological and evolutionary concerns were not eliminated from anthropology. And while a systematic evolutionary viewpoint was slow to inform physical anthropology and archaeology, the period after World War II saw important developments in the field of "paleoanthropology," as well as the resurgence of a submerged neo-evolutionary tendency within American cultural anthropology. During the same years, in the context of a closer association with Parsonian sociology, cultural anthropologists began to think more seriously about just what "culture" was. By the end of the 1960s, a conceptual polarization was beginning to be evident. On the one hand, there was a tendency—most strikingly evident in what came to be called symbolic anthropology—to treat cultures in humanistic idealist terms as systems of symbols and meanings, with relatively little concern for the adaptive, utilitarian aspect of cultural behavior. On the other hand, there was a materialistic countercurrent which insisted that culture must be understood scientifically in

adaptive evolutionary terms, whether in the form of "techno-environmental determinism," or in the even more controversial form of "sociobiology," which seemed to many to threaten a resurgence of racist thought in the human sciences.

Although the vast majority of American anthropologists came to the defense of Margaret Mead when a critique of her Samoan fieldwork was generalized as an attack on the notion of cultural determinism (Freeman 1983a), it is by no means clear that the ambiguities of the culture concept have been resolved. Indeed, it might be argued that beneath the recent polarization lies the paradigmatic opposition that characterized thinking about human differences before the early-twentieth-century "revolution in anthropology." In the case of Greco-Roman developmentalism, the continuity with neo-evolutionism is manifest; in the case of the biblical/ethnological paradigm, it is less clearcut. But the fact that the emergence of symbolic and interpretive anthropology is spoken of as the hermeneutic turn, and also the fact of preoccupation with linguistic phenomena, suggest a level at which it may exist. Be that as it may, the historically constituted epistemic dualism underlying modern anthropology is real enough, and seems likely to endure. From this point of view, Boas—who in other writings insisted on the independent legitimacy of both the *natura* and the *geisteswissenschaftliche* approaches to the study of human phenomena—may perhaps serve as a guide to the future as well as to the past of the discipline.