In the liminal phase of Ndembu rites of passage, as in similar rites the world over, communitas is engendered by ritual leveling and humiliation. In hieratic social structures, communitas is affirmed by periodic rituals in which the lowly and the mighty reverse social roles. In such societies, too—and at this point Turner begins to draw his examples from European and Indian history—the religious ideology of the powerful idealizes humility, orders of religious specialists undertake ascetic lives, and cult groups among those of low status ritually play with symbols of power. The world over, millenarian movements originate in periods when societies are in liminal transition between different social structures. In the second half of the book, Turner glosses his illustrations from the traditional cultures of Africa, Europe, and Asia with comments on modern culture, referring briefly to Gandhi, Bob Dylan, and such current phenomena as the Vice Lords and the Hell's Angels.

But how do ritual symbols *work*?

According to Turner, they condense many references, uniting them in a single cognitive and affective field. In this sense, ritual symbols are multivocal, but their referents tend to polarize between physiological phenomena (blood, sexual organs, coitus, birth, death, and so on) and normative values (kindness to children, generosity to kinsmen, respect for elders, and the like). The drama of ritual action—the singing, dancing, feasting, and other acts—causes an exchange between these poles in which the biological referents are ennobled and the normative referents are charged with emotional significance. The exchange achieves genuinely cathartic effects, causing real transformations of character and of social relationships. It makes desirable what is socially necessary by establishing a right relationship between involuntary sentiments and the requirements of social structure. In this sense ritual action is a sublimation process in which symbolic behavior actually creates society.

Turner's formulation is a refreshing change from the pedantry of social anthropologists who have too often repeated the notion that religious symbols *reflect* social organization and promote social integration, and from the sophistry of psychoanalytic anthropology which reduces religion to a neurotic symptom. These approaches treat symbolic behavior as an epiphenomenon, while Turner gives it ontological status.

Turner has been developing his ideas in publications for more than a decade, and among anthropologists I believe that they now constitute what Thomas Kuhn would call normal science. They will probably remain the assumptions for most new research for another generation, but eventually anthropologists will have to face issues that Turner neglects. So far, no anthropologists have dealt in a sustained manner with the ways individuals and communities lose their religion, or with the failures of religious institutions to cope with historical changes initiated by scientific and technological knowledge.

Secularization processes continuously reappear in history, and, despite repeated failures of nerve, they seem to grow more pervasive through time. Considering Turner's convincing analysis of the source of religious rites in universal human circumstance, the wonder is not that people continue to create symbolic ritual systems, but that these systems go stale or become perverted, and that people lose belief, often with anxiety, but also with a sense of liberation.

CHARLES LESLIE
Department of Anthropology, University College of Arts and Science, New York University, Bronx

Boas in the Field

The Ethnography of Franz Boas. Letters and Diaries of Franz Boas Written on the Northwest Coast from 1886 to 1931. RONALD P. ROHNER, Ed. Translated from the German by Hedy Parker. University of Chicago Press, Chicago, 1969. xxx + 334 pp. + plates. $12.50.

This is an important book about this man, written principally by himself. Franz Boas was an outstanding figure in American anthropology for over half a century. His students, and students of his students, have trained the majority of today's senior anthropologists in the United States. In a brief but significant essay the editor-compiler and his wife describe the theoretical bases of North American ethnology when Boas began his Northwest Coast work and the standard field methods of ethnographers of that day. From there on, Boas is allowed to speak for himself through his letters to his family, except for a few short paragraphs that explain the backgrounds of the field trips: source of support, Boas's institutional affiliation at the time, and so on. An occasional explanatory parenthesis or footnote concludes the editing.

Rohner, by his minimal, always impartial comment, dons the armor of neutrality, thus shielding himself from charges of being an ill-willed detractor or an uncritical adulator. (The dust jacket says that "gradually more impartial assessments are being made" of Boas's contributions to anthropology, but then we know what kind of people believe what they read on dust jackets.) A less disciplined approach might have led to the replacement of the staid subtitle by something more vigorously descriptive, such as "The Ethnography of Franz Boas, or, The Captain Hated the Sea." For one of the first of the revelations to emerge from the letters is that Boas, the man who always stressed the need for more research in anthropology and less vacuous speculation under the guise of theory formulation, hated fieldwork. The actual collection of data, the long hours of recording data and texts, translating them, then transcribing the day's work until late at night, he took in stride, though he often mentioned his weariness, his fingers stiff and cramped from the hours of scribbling. What he detested was the ambient of fieldwork.

The Northwest Coast was a frontier when Boas began his work. Comfortably appointed hostelries were few and far between. The white pioneers built just enough shelter for themselves and their families; the casual traveler (read "anthropologist") they put up, not always with good grace, in a storeroom or a shed. At Kincolith in 1894 Boas wrote:

> My bed seems to get harder every day. It is just a soft cushion, not a mattress, so that I can feel the hard floor through it and my hips start to get numb. . . . When the east wind blows it comes in through the window, and how! [p. 163].

Food was something to be wolfed down to keep the body alive, not exquisite tidbits to delight the palate. At Bay Center, Washington, 1890:

> The fodder here is ghastly, especially the awful American bread, which lies in your stomach like a brick. And the beans! [p. 123].

And there was the rain rain rain and soggy cold that chilled one's very soul. And travel problems. Sailing schedules were irregular, or, more accurately, nonexistent. If one planned a fortnight

at some locality, the coasting steamer might call in again in a week, or four weeks later. If one missed her one might be marooned for another month. Boas had to live tense, ready to dash the moment a ship's whistle sounded down the channel to hustle his crates of collections dockward and make a pierhead jump aboard—to go on to the next Indian village, to repeat the performance. Once one was aboard, the southeaster blew, the seas ran high. Boas never got his sea legs. If he didn't get seasick crossing Queen Charlotte Sound it was something to boast about. His fellow passengers—often a gang of drunken lumberjacks—were rarely proper company for a distinguished scholar. Boas complained about these inconveniences bitterly, but tolerated them. What pained him most was a result of the vagaries of coastwise shipping: the breakdown in communications, the long intervals between letters from home. This was torture for a man with an obsessive nostalgia for his family.

> I am very impatient to get letters but it will take until some time in August, and depends on the boats. I am fed up with these trips into the wilderness [in 1897 during a fabulous pack trip up the Fraser and over into Bella Coola valley; p. 209].

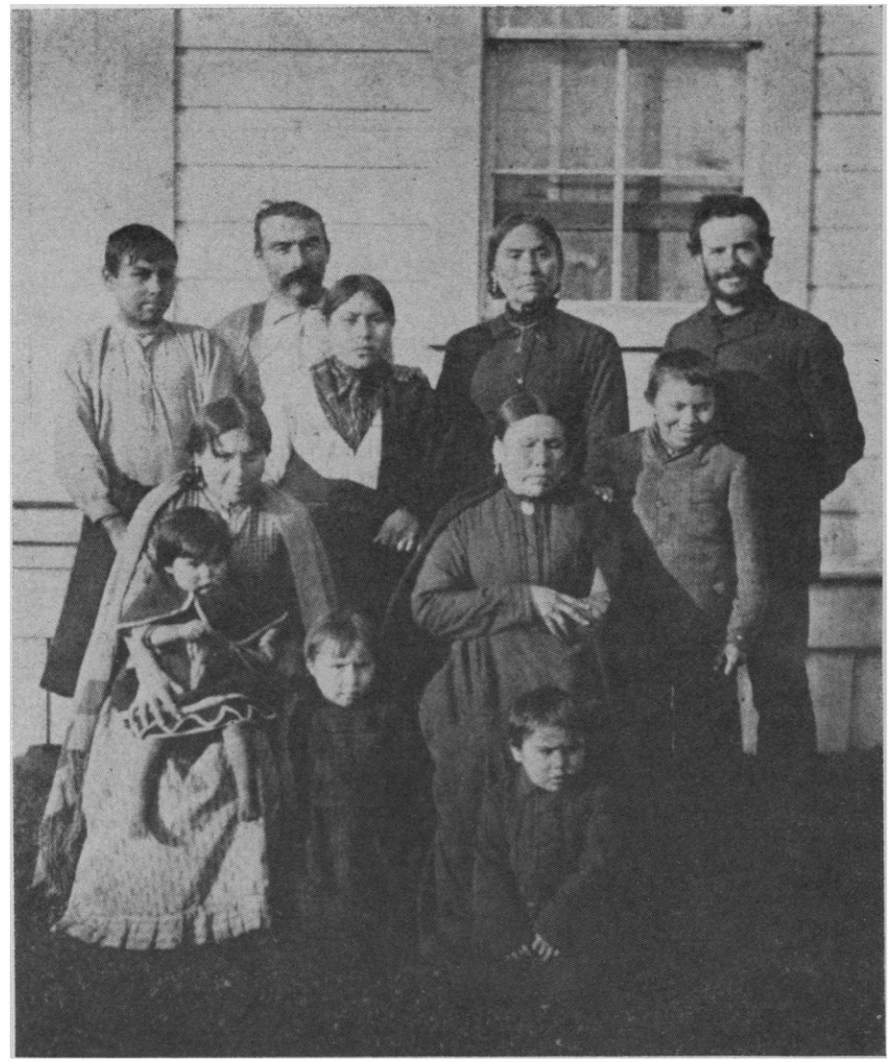
As is noted in the introductory essay, Boas's pattern of fieldwork was the survey, in which he "traveled from location to location for brief visits working on special problems, and then moved on." This meant, given the intensity of Boas's field effort, that there was no possibility of setting up semipermanent headquarters, where at least some of the discomforts could have been ameliorated. But, as is noted in the Rohners' introduction, "Boas' field procedures [meaning his adherence to the "survey" technique, despite his frequent comments on the desirability of a lengthy intensive study of a single group] remained essentially unchanged throughout the half century that he worked with Northwest Coast materials."

A cynical interpretation might be made. Instead of reading the constant flow of complaint in the letters to mean that Boas, goaded by a near-masochistic urge to salvage data on Northwest Coast cultures, forced himself to suffer through the long, uncomfortable trips, one might suspect the complaints were a blind, the trips escapes. Could the trips have been a scholar's version of other men's "having to work late at the office," with the escapes made palatable at home by the constant reiteration that Papa didn't like it, that he wasn't *really* having fun? Amusing though it may be to toy with this notion, internal evidence from the letters destroys it. The plethora of other intimate details of family concern—worries about job security in the early years, worries about money, worries about the children; the sad memories of a dead child; the constant stress on affection—are all too charged with emotion for it to seem possible that the nostalgia was deliberate falsehood.

Boas's methodologic inconsistencies were not limited to his recognition of the value of long-term intensive study of a single "tribe" and his reluctance to change his pattern of fieldwork. As the Rohners note: "A field worker's most valuable skill, according to Boas, is a knowledge of the language." Yet the "language" Boas learned and used when he had no interpreters was Chinook Jargon, a crude, limited, and imprecise artificial communication system. Boas's first contact with the Southern Kwakiutl in their own land was at Neewittee in October 1886. In 1930, after 44 years had passed, during which he had spent a vast number of hours in linguistic analysis of hundreds of pages of texts, Boas wrote of the Kwakiutl tongue:

> I talk with difficulty and understand only after I write it; I follow conversation only partly [p. 290].

Of course linguistic analysis is one thing and speaking command another, but one would think that a man who expounds the view that knowledge of language is the "most valued skill" would try to become fluent in at least one native tongue, if only as a point of pride.



Franz Boas with George Hunt and his family at Fort Rupert, British Columbia (1894?). [Courtesy American Philosophical Society]

Then one wants to know about Boas and the Indians. How did he interact with them?

> Today the Tsimshian woman . . . was unwilling to tell me anything. I had to give up after several attempts and left in anger [1886; p. 27].
>
> I had trouble in Somenos this morning because I wanted to make a drawing. The Indians always try to bluff strangers with their impudence [1886; p. 51].
>
> My informant was very unsatisfactory the first two days until I gave him a piece of my mind. Today he was all right. He was not punctual enough for my taste. This is a typical fault of the Indians. I am very strict with them when I pay them [1894; p. 158].

Another aspect of Boas's attitude is to be found in the description of an Indian wedding at the mission village of Kincolith:

> I went with them to the church . . . and saw the happy couple getting married, entirely European fashion with orange blossoms, white veil, and white dress; the groom had no top hat, however. *It was truly comical* to see how out of place the bride and groom, the bridesmaids, and the best man felt [1894; p. 156 (italics supplied)].

Comments of this sort, and these are not the only ones, are more frequent in letters from the early trips than from the later ones but are not replaced by anything more positive. Although Rohner elsewhere (*Pioneers of American Anthropology*, 1966, June Helm, Ed., p. 210) has said that friendship colored the relationship between Boas and George Hunt that lasted from 1888 to Hunt's death in 1933, it does not show through in these letters. Although occasionally in the later years there are statements that Hunt was "very useful," disparaging remarks prevail (pp. 183, 237, 243–44, 289, *et passim*). George Hunt, a remarkable man in his own right and Boas's good right hand in Northwest Coast ethnography for decades, seems never to have been anything but cheap hired help to the scholar.

Reading through these letters inevitably produces a reaction. These were private utterances not intended for public consumption. Besides the information on Boas's field procedures, the letters are filled with familial intimacies—abundant phrases of love and affection, a clumsy apology after a quarrel, anxieties about money, disclosures of petty meannesses, and other things that are really no one else's business. As one reads, a feeling of embarrassed discomfort grows, a guilt feeling, as though one were peeking through a keyhole at intimate scenes. This discomfort is compounded by the thought that the editor-compiler foresaw just this reaction on the part of readers and assumed his Olympian aloofness to avoid his share of guilt. I would rather Rohner had done the keyhole peeking and then summarized what he saw.

PHILIP DRUCKER
Department of Anthropology,
University of Kentucky,
Lexington

On the Origins of Agriculture

The Domestication and Exploitation of Plants and Animals. Proceedings of a meeting of the Research Seminar in Archaeology and Related Subjects, London, May 1968. PETER J. UCKO and G. W. DIMBLEBY, Eds. Aldine, Chicago, 1969. xxvi + 582 pp., illus. $17.50.

Food in Antiquity. A Survey of the Diet of Early Peoples. DON BROTHWELL and PATRICIA BROTHWELL. Praeger, New York, 1969. 248 pp., illus. $8.50. Ancient Peoples and Places, vol. 66.

Although the domestication of plants and animals was perhaps the most important human achievement until modern times, the origins of domestication remain enigmatic. *The Domestication and Exploitation of Plants and Animals* includes papers given at an international symposium as well as some papers specially written for the volume. Despite minor flaws, it is an excellent book. To a large extent the essays grapple with the problems of the domestication process. The exploitation of domestic animals is discussed by the authors only cursorily, insofar as they consider it relevant to the origins of domestication. The exploitation of nondomestic plants and animals is given little attention by the majority of contributors, although it is probably of considerable relevance to the domestication process. Thus in spite of the range of topics (fruit size of Swiss prehistoric apples, the exploitation of mollusks, fungi and Southeast Asian food technology, the introduction of baobab into India, changes in the fleece of sheep after domestication, to mention a few) and the varieties of disciplines from which the authors come (archeology, botany, ethnography, geography, history, zoology, and others), the book is more unified than most collections of essays based on comparable interdisciplinary conferences that I have seen.

But although the book is focused in its concern, no consensus emerges. There does seem to be general agreement that previously advanced environmental interpretations of domestication, mainly based on the assumption of postglacial desiccation, are wrong, yet that environment is not irrelevant. But how the "environment" might have induced hunters and gatherers to domesticate is left as problematic as ever. The essay of C. Vita-Finzi, "Geological opportunism," puts the formerly exaggerated emphasis on climatic changes in a corrected perspective, succinctly stating what ought to have long been obvious, namely that "human populations at the hunting and gathering stage . . . are not vulnerable to climatic blackmail."

An emphasis on ecology is replacing the older one on climatic change. There are a number of reasons for this, advances in ecological method being only one. Others, I suspect, have more to do with the ecology of university environments than anything else. Indeed, this is as much as stated by W. F. Grimes: "the ecological approach is achieving for archaeology full status as a science, with the incidental benefit that sources of research grants previously closed are now beginning to open." The difficulty with putting too much weight on ecology is that ecology can only specify the conditions under which domestication could occur; it is helpless to explain why domestication did in fact occur when it did, so very long after suitable conditions, ecologically and genetically