

HISTORY OF ANTHROPOLOGY • VOLUME 1

OBSERVERS OBSERVED

Essays on Ethnographic Fieldwork



Edited by George W. Stocking, Jr.

Observers Observed

HISTORY OF ANTHROPOLOGY

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ESSAYS ON
ETHNOGRAPHIC FIELDWORK

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HISTORY OF ANTHROPOLOGY
Volume 1

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Observers Observed

HISTORY OF ANTHROPOLOGY

Whence/Whither

At a time when bookshelves bulge with journals ever-more-costly and ever-less-often read, the launching of a new volume-series demands a brief self-explaining introduction. What is the audience whose unserved needs we address? What is the subject of our discourse? How do we intend to pursue it? And why do we begin with a particular aspect of it?

Although there has been occasional interest in the history of anthropology throughout the century since the emergence of the modern academic discipline, a more systematic concern may be traced to the Conference on the History of Anthropology stimulated by A. I. Hallowell and sponsored by the Social Science Research Council in 1962 (cf. Hymes 1962). Two decades later, what was once for the most part the episodic effort of reminiscent elder anthropologists or roving intellectual historians has become something approximating a recognized research specialization. *The History of Anthropology: A Research Bibliography* includes 2,439 titles culled from over 5,000 collected by its editors (Kemper & Phinney 1977), and for the past decade each biannual issue of the *History of Anthropology Newsletter* has recorded a substantial number of articles, doctoral dissertations, and books by scholars who think of themselves as historians of anthropology (Stocking, ed. 1973-).

The impetus for this development has come from both history and anthropology. Historians have no doubt been impelled in part by the inherent expansionism of a profession whose rapidly multiplying apprentices must find still unplowed fields for their research. But historical interest is also motivated by more general professional and social concerns centering on issues of knowledge and power. The long-run trend towards the professionalization of intellectual life within academic disciplines often lately pervaded by a sense of crisis has made these disciplines themselves seem historically problematic; issues of racial and ethnic relations in the decolonizing world have turned historians' attention to the ideology of race and culture (cf. Hinsley 1981).

Although doubtless variously motivated, the heightened retrospective interest of anthropologists reflects the special sense of disciplinary crisis that

has developed since about 1960. With the withdrawal of the umbrella of European power that long protected their entry into the colonial field, anthropologists found it increasingly difficult to gain access to (as well as ethically more problematic to study) the non-European "others" who had traditionally excited the anthropological imagination—and who seemed finally about to realize, through cultural change, the long-trumpeted anthropological prediction of the "vanishing primitive." Some envisioned "the end of anthropology" along with its traditional subject matter (cf. Worsley 1970). Some wondered whether anthropology was a reversible and universal form of knowledge or merely the way Europeans had explained to themselves the "others" encountered during the centuries-long period of European overseas expansion (Stocking 1982b:419). Still others proposed the "reinvention" of the discipline. Calling into question its institutionalization within the academy, turning for the first time in its history toward Marxist and feminist theory, they advocated a more "reflexive" study of social groups within Euro-American societies, and an active political involvement on behalf of its subjects (Hymes, ed. 1973). Whether it is being reinvented, or simply being carried along by institutional inertia, anthropology in the early 1980s continues to face profound issues of disciplinary identity (Hoebel, ed. 1982). The development of self-study by post-colonial "native anthropologists" raises new ethical and methodological problems; reflexive study in the metropolis contributes to the centrifugal proliferation of "adjectival anthropologies" without providing a unifying substantive focus; epistemological and ethical doubts have weakened methodological resolution without yet resolving the problematic character of fieldwork method; the questioning of old concepts and the legitimation of new theoretical alternatives has not established the basis for a new integrative orientation; and despite a growing concern with increasing non-academic employment options for its surplus doctorates, the discipline remains essentially an academic one.

In this context, some anthropologists have become increasingly conscious of the historical character of their discipline. Not only are the problems and the data of anthropology once again seen to be essentially historical after a half-century of predominantly synchronic emphases, but anthropology itself is increasingly viewed as an historical phenomenon. In order to understand their present predicament and to find and/or to legitimate approaches that might lead them out of it, a number of anthropologists have turned to the history of anthropology (e.g., Augé 1979; Crick 1976; Harris 1968).

The founding of *History of Anthropology* (hereafter, HOA) is an outcome of this double disciplinary impulse. Until now, there has been no arena in which both anthropologists and historians might pursue historical problems of common concern before an informed and interested audience. Articles on various aspects of the history of anthropology have been scattered hither and

yon, appearing now and then in anthropological journals little read by historians, but as often in historical journals scarcely seen by anthropologists. *HOA* will attempt to provide a single central forum for their mutual discourse.

The duality of our audience is not the only problematic issue suggested by our title. Despite anthropology's century as an academic discipline, its definition is in some respects more problematic today than at the time of its early institutionalization. Depending on national tradition, sub-disciplinary identification, and theoretical orientation, its external and internal boundary relations vary considerably (cf. Hannerz, ed. 1983; Diamond, ed. 1980). The embracive ("four-field") conception of anthropology has been most characteristic of the American and certain phases of the British tradition. On the European continent the term long referred primarily to the study of "man" as a physical being; and there are those in the United States today who would separate "socio-cultural" anthropology as sharply from biological anthropology as, for example, from psychology or economics.

One might resolve the issue by defining a fundamental problem-orientation underlying the historical diversity of disciplinary definition: the history we encompass is that of "the systematic study of human unity-in-diversity." Such a formula allows a place not only for biological anthropology (e.g., Haraway 1978), archeology (e.g., Trigger 1980), anthropological linguistics (e.g., Hymes, ed. 1974), and socio-cultural anthropology (e.g., Boon 1983), but for such historical or national variants as "ethnology" and *volkskunde*, as well as the "anthropological" aspects of psychology, aesthetics, economics, etc. It allows us also to consider as historically problematic the processes by which certain approaches to or aspects of human diversity are (or are not) incorporated into such systematic study (cf. Kuklick 1980)—for example, the changing fate of Marxist or feminist perspectives on social organization (e.g., Rosaldo & Lamphere, eds. 1974), or the exclusion of missionary ethnography in favor of "scientific" fieldwork (cf. Clifford 1982).

Nevertheless our formula—which is itself full of problematic concepts—tends still to suggest an orientation, however flexible, toward the history of a "discipline." No doubt much of our historiography will be thus construed—or constrained. But in principle we recognize no sharp borders surrounding the "discipline" of anthropology. It is not merely a matter of including western "folk anthropology" as part of the historical background from which "scientific anthropology" emerged (cf. Hallowell 1965). It is also one of recognizing that in every period the "systematic study of human unity-in-diversity" is itself constrained—some might say systematically structured—by the ongoing and cumulative historical experience of encounters and comprehensions between Europeans and "others." These comprehensions articulate closely

with ideologies of European self-knowledge—as the evolutionary equation of savage/madman/peasant/child/woman suggests—and the often bloodily expropriative nature of these encounters gives them a special weighting of moral concern. The history of anthropology is thus the history of a “discipline” whose enmeshment in world-historical structures and processes especially compels attention.

From the broadest point of view, then, the history of anthropology we propose to encompass is that of the systematic study of human unity-in-diversity, against the background of historical experience and cultural assumption that has provoked and constrained it, and which it in turn has conditioned.

The launching of HOA takes place in the context of a more general rapprochement between the two inquiries. For just as anthropologists lately have turned to history, many historians (quite aside from the interest in the history of disciplines) have turned recently to anthropology for conceptual and methodological orientations (Gaunt 1982). Despite this rapprochement, and an underlying substantive and epistemological kinship, there nevertheless tend to be differences in the approaches that anthropologists and historians take to the history of anthropology. To borrow categories used elsewhere some years ago to describe motivation and style in historical inquiry, anthropologists are more likely to be “presentist” and historians more “historicist” in treating the history of issues currently debated within anthropology (Stocking 1965; cf. Stocking 1982a). To put the matter another way, anthropologists are more likely to be committed to one side or another, and historians to be (relatively) disinterested observers, and the histories they write are likely to reflect this fact. Each approach has advantages and disadvantages. If historians are less likely to be blinkered by theoretical bias, they are also more likely to suffer from a lack of technical sophistication and relevance; and if an anthropologist’s commitment may inhibit understanding of the “losing” side, it can also illuminate issues that remain below the threshold of a more disinterested concern.

Drawing its editorial board from both disciplines, HOA will be receptive to a variety of historical and anthropological points of view. We favor studies grounded in concrete historical research, but we hope to rise well above anecdotal antiquarianism to contribute to the critical understanding of general issues of serious current anthropological concern. We hope to encourage the development of a disciplinary historiography that is both historically sophisticated and anthropologically knowledgeable.

It should be emphasized that we are not proposing a division in which anthropology provides subject matter and history methodological orientation. In this respect the history of anthropology differs significantly from that of certain other inquiries. For the historian of physics, the methods and con-

cepts of that discipline do perhaps have relevance only as subject matter. For the historian of anthropology, they are not only the object of inquiry, but may provide also a means by which it is pursued. As Hallowell argued several decades ago, the history of anthropology should be approached as "an anthropological problem" (Hallowell 1965).

The history of anthropology, however, is not one but many such problems, each with many facets, which may be approached in a variety of particular ways. And each problem may engage not only particular groups of anthropologists and historians, but also sociologists of science and literary historians, as well as others with specific or general interests in the human sciences. With these constraints in mind, *HOA* has adopted a format of periodic book-length volumes organized around particular themes announced and developed in advance. In addition to substantive articles of varying length, documentary materials, personal reminiscences, critical essays, and essay reviews relating to the volume theme, each volume of *HOA* will include one or more "miscellaneous studies," in order to allow a place for high-quality research outside our chosen topics. Should the muses of our authors prove more generally resistant to coordination, we will not hesitate to publish an occasional miscellaneous volume. In this manner, we hope to bring together the best work being produced, and to stimulate research which, if it does not find a place in *HOA*, may enrich the pages of other journals that will retain an interest in the history of anthropology.

As theme for the inaugural volume of *HOA*, we have focussed on the development of ethnographic fieldwork in socio-cultural anthropology. Both for practitioners and outsiders, a distinguishing feature of modern anthropology is the commitment to fieldwork by "participant-observation." Entering as a stranger into a small and culturally alien community, the investigator becomes for a time and in a way part of its system of face-to-face relationships, so that the data collected in some sense reflect the native's own point of view. This style of inquiry is much more than a mode of data-gathering widely (although by no means universally) adopted in a particular discipline of the human sciences. At once setting anthropology apart from other such inquiries and linking it to a broader European tradition of participatory cultural exoticism, it is the basis for a most unlikely image of the academic intellectual: "the anthropologist as hero" (Sontag 1966). It is a kind of shared archetypical experience that informs, if it does not generate, a system of generalized methodological values or disciplinary ideology: the value placed on fieldwork itself as the basic constituting experience not only of anthropological knowledge but of anthropologists; the value placed on a holistic approach to the cultures (or societies) that are the subject of this form of knowledge; the value placed on the equal valuation of all such entities; and the

value placed on their uniquely privileged role in the constitution of anthropological theory (Stocking 1982b; cf. Mandelbaum 1982). It has, in short, been the legitimizing basis for anthropology's claim to special cognitive authority (cf. Clifford 1983).

During the decades of socio-cultural anthropology's "classical period"—roughly 1925 to 1960 (cf. Stocking 1978)—fieldwork evoked relatively little systematic questioning or analysis (Nash & Wintrob 1972). Certain aspects of it were subject to a degree of formal elaboration that enabled them to be taught as technical skills (Epstein, ed. 1967). For the most part, however, fieldwork training was a matter of learning by doing, and this less in the tradition of apprenticeship than of "sink-or-swim." There was a certain amount of formal scholarly discussion of certain methodological issues, but there is little in the published record to suggest a serious consideration of the fundamental epistemological, psychological, or ethical issues involved in research where the investigator was expected—if archetype were to be realized—to become rather intimately involved in the processes he or she was studying. As befits the central methodological rite in a discipline whose national communities continued into the 1950s to resemble the face-to-face *gemeinschaften* they archetypically studied, fieldwork was enacted more than it was analyzed; part of the community's oral tradition, it was the subject of considerable mythic elaboration.

By 1960, this situation had begun to change. To some extent this may have been the result of the growth of anthropology itself. Especially in the United States, where there were substantial numbers of undergraduates taking anthropology courses, the community became large enough to provide a publishing market; and given its central role in the anthropological mystique, the field experience was bound eventually to become a marketable commodity. Field training, however, continued for the most part to be extremely informal, and the interest of publishers tended to lag behind a changing disciplinary consciousness, which by 1960 was beginning to respond to the changing circumstances of ethnographic inquiry in the era of decolonization. In that context, the publication of certain books did play a role in the emergence of the new consciousness—most notably Malinowski's *Diary in the Strict Sense of the Term* (1967). Suddenly there seemed to have been uncovered a long-repressed Conradian horror—what the culture-hero of the fieldwork myth had "actually" been feeling during his long and presumably empathetic immersion in the Trobriand *gemeinschaft*. Longing for white civilization and for white womanhood, he had relieved his frustration with outpourings of aggression against the "niggers" who surrounded him. In a political context in which anthropologists were being attacked for indirect or active complicity in the defense of colonial power (cf. Asad, ed. 1973), and

even, despite the discipline's half-century critique of racial ideology, as themselves racist, Malinowski's "niggers" were profoundly disturbing indeed.

Anthropologists have yet to come to terms with all the implications of Malinowski's diaries. But the years since 1967 have seen a considerable body of literature on the fieldwork process. The heightened consciousness of its problematic character has produced numerous discussions of the epistemological, methodological, psychological, ethical, and political implications of fieldwork, as well as a number of autobiographical accounts of varying length (cf. Agar 1980). Increasingly, ethnographies are accompanied by or even presented in the form of accounts of the fieldwork that produced them (cf. Cesara 1982). But while historians of the discipline have approached aspects of its development, there is as yet no general historical account of the modern anthropological fieldwork tradition. It is in this context that we have chosen our theme and subtitle for the first volume of *HOA*: "Observers Observed: Essays on Ethnographic Fieldwork."

Few if any themes in the history of anthropology can be systematically explored within the confines of a single 200-page volume of essays written by authors whose motivating interests are in fact quite varied. Since one of our purposes is to provoke further research, it may be useful to reflect briefly on some of the limitations of the history we have sketched. Granting that our choice of episodes was heavily conditioned by the circumstances of current work-in-progress, one can of course imagine a multitude of particular alternatives. We might have begun with Lewis Henry Morgan, whose kinship-terminology questionnaires and trans-Mississippi expeditions of the late 1850s were perhaps the earliest attempts systematically to collect data bearing on a specific ethnological problem; our failure even to mention Margaret Mead must surely strike many readers as anomalous.

The issue is perhaps better approached, however, in terms of certain limitations of the overall picture we have conveyed. Neglecting the development of fieldwork traditions in other areas of anthropology, we have shut off a wide range of reflective insights that might have been offered by contrasting, for instance, the modalities of archeological fieldwork with those of ethnography. Although we have included material from three major national ethnographic traditions, we have only touched upon their interaction, and comparisons between them have been for the most part implicit and juxtapositional (cf. Urry n.d.). More seriously, perhaps, our episodic approach has significantly distorted the presentation of the American ethnographic tradition. Skipping from Cushing in the Southwest and Boas in Baffinland forward to Barnett's disillusionment with summertime trait surveys, we have in fact omitted what many would consider the most characteristic manifestation

of Boasian ethnography: the style that produced Boas' "five-foot shelf" of Kwakwilt texts. Building on the traditions of European humanistic scholarship, particularly on linguistic and folklore study, this approach saw ethnography as the construction of a body of textual material directly expressive of the native mind, produced with the active and acknowledged assistance of native ethnographic intermediaries like George Hunt. The contrast with the British tradition is by no means absolute—Hocart, too, aspired to construct his ethnographies in these terms. But if we characterize different ethnographic modes in terms of the forms of data that they privilege (such as artifactual, textual, and behavioral), the contrast between the classic Boasian and Malinowskian modes is clear enough.

It is primarily the latter that undergirds the presently dominant disciplinary ideology; and it could be argued that, after all, our history has been structured by a corresponding disciplinary myth-history of fieldwork. By focussing on developments since 1880, we have encouraged a de facto separation of modern ethnography from the long preceding experience of contact between Europeans and "others." Furthermore, we have only briefly treated what was lost (as well as gained) with the emergence of a scientific academic anthropology. At the same time, we have perhaps sustained a somewhat backward-looking romantic image of the academic ethnographer: all our anthropologists are European, and if they did not all work alone, the "others" that they studied were for the most part inhabitants of geographically distant precincts of cultural exoticism. There is little reflection of the historical roots of a more reflexive ethnography—nothing on European folklore or *volkskunde*, nothing on anthropological research in more complex societies. In short, by limiting ourselves to the last century, by emphasizing its earlier phases, by orienting ourselves toward academic anthropology, and by focusing on the critical role of individuals who figure prominently in the disciplinary myth, we have to some extent perpetuated a picture which, although presented in more concretely historical terms, is in basic outline rather conventional.

But though this is in some ways a limitation, we make no apology for it. If we focus on the familiar, it is our intention to defamiliarize it. To do this need not always require recomposition from scratch. It may be a matter of directing a brighter, fuller light on figures whose proportions have been distorted and whose surroundings have been cast into shadow—or of trying to set their stereotyped postures once more in motion. No doubt other stand-points might have been adopted, other lamps held, other perspectives revealed. For anthropologists (prospective, certified, retired, or *manqué*) fieldwork is an endlessly engaging topic, which will surely appear again in *HOA*. In the meantime, we will try to remain open to approaches that go beyond

explicit or implicit disciplinary definitions, in the hope that by defamiliarizing the past, we may perhaps help to open up the future.

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“THE VALUE OF A PERSON LIES IN HIS *HERZENSBILDUNG*”

*Franz Boas' Baffin Island
Letter-Diary, 1883–1884*

DOUGLAS COLE

When Franz Boas was twenty-five years old, he travelled to Baffin Island to undertake anthropological and geographical research among the Eskimo. In view of his later eminence as reigning patriarch of American anthropology during the first third of the twentieth century, the letter-diaries that he kept during his *erstlingsreise* (Boas 1894:97) have a special interest for the history of the discipline.

Boas had secured his doctorate from Kiel University in the summer of 1881. Although his dissertation had been in physics, he had already chosen one of his minor fields, geography, as his future speciality. After pursuing for a time certain problems of the psychophysics of sense perception suggested by his doctoral studies, he began to focus his interests on the relationship between people and their natural environment. By April 1882, during the year of his required military service, he had begun planning “an investigation of the dependence of contemporary Eskimo migrations upon the physical relationships and forms of their land” (BPP: FB/A. Jacobi 4/10/82; cf. Kluckhohn & Prufer 1959, Stocking 1968).

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The reason for selecting the Eskimo (or Inuit) is not apparent at first glance. Boas seems to have felt that their environmental dependence was the most simple case with which to begin, though the paucity of information available upon the region and its natives weighed against the advantages of apparent simplicity. Perhaps the choice was a quite personal one, its roots lying far back in Boas' youth. As early as 1870, when he was but a boy of twelve, he wrote to his sister of undertaking an expedition to the north or south pole after completing university (BFP: FB/T. Boas 12/3/70). The probability that polar exploration was a long-standing idea and not a passing boyhood fantasy receives support from the course he took in 1878–79 at Bonn on the geography and research of polar areas.

Having decided to study the Inuit and their environment, Boas set about his preparations for an expedition. He moved to Berlin, where, among other things, he studied meteorological, astronomical, and magnetic observation with W. J. Förster of the Berlin Planetarium and anthropological measurement with Rudolf Virchow, as well as cartographic and topographical drawing. He also worked at both the Inuit and Danish languages, consulted Heymann Steinthal on linguistic points, examined the Arctic collections at the Berlin museum under the eye of Adolf Bastian, and learned photography. Through his developing Berlin acquaintances Boas was able also to organize the practical matters of launching the expedition. Bastian put him in touch with Georg von Neumayer, chairman of the German Polar Commission, which at that time was supporting scientific parties at Baffin and South Georgia islands. Neumayer promised transportation to Baffin Island with the Commission's ship and generously allowed Boas to have his pick of the returning station's instruments and supplies. Boas persuaded the editors of the *Berliner Tageblatt* to advance 3,000 marks against fifteen promised articles.

Much remained to be done, but the means for the expedition and its planned outline were clear. He would travel to Baffin Island's Cumberland Sound with the *Germania*, a ship built in 1869 for Arctic use. She would take him deep into Cumberland Sound to Kingawa, where Dr. Wilhelm Giese's scientific party had spent the International Polar Year of 1882–83. Should the Scottish station at Kikkerton Island seem more favorable as a base than the Kingawa hut, Boas had a letter from Crawford Noble, its Aberdeen owner, asking the resident master for his cooperation. Boas planned to take with him an assistant and a servant. Although Lieutenant von den Goltz, Neumayer's recommendation as assistant, backed out at the last moment, servant Wilhelm Weike, who had been in the Boas family service, remained. With the advice of old Arctic hands, Boas secured in Hamburg a large stock of provisions, guns, ammunition and trade goods, a thirteen-foot dinghy intended for the interior lakes, and a small steel sled.

His research strategy was developed from his rapid mastery of Arctic lit-

erature and honed by several contributions that he made to it during his preparatory year. The most important article, ostensibly about the homeland of the Netsilik Eskimo, partly described and partly postulated extensive routes of trade and travel between Inuit groups of the central and eastern Arctic (Boas 1883: 223–33). According to the article, these well-established routes extended from Ugluk, a settlement at the western tip of King William Island, eastward through Iglulik on Fury and Hecla Strait, and from there in two directions on Baffin Island—to Pond Inlet at the northern end, and down along the western coast, connecting eastwards to Cumberland Sound.

Boas fitted these interests and postulates into a plan for a one-year investigation based at Cumberland Sound. Aside from cartographic and meteorological research, his intention was to study Eskimo migration, hunting areas, trade routes, and the relationships of one group to another. He would travel in the summer and fall of 1883 to Lake Kennedy (Lake Nettilling), an inland sheet of fresh water, and from there attempt to reach the west coast of Baffin Island and follow it north to Iglulik. Returning to winter in Cumberland Sound, he would “collect ethnographic material and make a thorough study of the language, customs and habits of the Eskimo” (BFP: “Als Ausgangspunkt” n.d.). In the spring he would return to Iglulik and then, by the route postulated in his article, travel north to Pond Inlet, perhaps yet farther north to Devon Island. He would return to Cumberland Sound in July along the Davis Strait, and sail home in the fall aboard a whaler.

This ambitious itinerary and the tenacity to which Boas held to a trip to the west coast, despite overwhelming setbacks, indicates that he was exceedingly intent upon demonstrating that portion of the routes he had set out in his Netsilik article. There was probably more to it, too. An overland trip westward from Cumberland Sound would bring him to one of the largest unexplored regions of the Arctic and onto an apparently easy route north to Iglulik, Pond Inlet, and beyond. It would be a significant piece of geographic discovery.

Privately, Boas anticipated a different ending to his expedition. Knowing that vessels traded along Davis Strait, he hoped to be picked up by an American whaler. The reasons behind this desire to visit America were partly professional: for a number of reasons, including the recent upsurge of anti-Semitism, he was not convinced that his future lay in Germany. Another motive was personal and concerned Miss Marie Krackowizer.

Marie Krackowizer was the daughter of Dr. Ernst Krackowizer, an Austrian Forty-Eighter who became a prominent New York doctor before his death in 1875. The Krackowizers were close friends of another New York physician and German Forty-Eighter, Abraham Jacobi, who was Boas' uncle by marriage. When, in the summer of 1881, the Krackowizers and Jacobi holidayed together in the Harz mountains of Germany, they were joined by Boas, who

had just finished sitting his doctoral exams at Kiel. He had only turned twenty-three, and Marie was not quite twenty. For three days they were almost constantly together, walking in the park at Wernigerode, looking down from the cliff of the Regenstein. They had an unforgettable early morning in the wild and picturesque Bodethal before all left for the Boas home in Minden, where Franz and Marie had two more days together. Although the Krackowizers settled temporarily in Stuttgart, the relationship lay dormant until Boas attended the Geographical Congress at Frankfurt in the spring of 1883 and feigned an appointment in Stuttgart as an excuse to call upon Marie. That April first afternoon, a beautiful spring Sunday, they stood under the old Schiller Oak "and told one another everything except what we really thought" (BFP: FB/MK 6/24/83). The omission was removed by a flurry of letters at the end of May. Less than three weeks before his departure for Baffin Island, they were quietly engaged. Her farewell letter, read as the *Germania* sailed down the Elbe, ended with "*Vorwärts! Ich warte dir!*"—"Onward, I wait for you!" (BFP: MK/FB 6/19/83). *Vorwärts* became a word repeated time and again by Boas to himself as he pursued his labors in the lonely barrens of Baffin Island.

The expedition meant a difficult separation for two such recently declared lovers. For twelve or more months any communication between them would be impossible. In the circumstances, they both kept diaries of unpostable letters. What matter if they could not be answered or even read for months?

These circumstances make Boas' letter-diary a very peculiar document. In a sense it is a single, 500-page letter composed over a fifteen-month period. Much of it is an outpouring of affection, an extended love letter, in which amorous effusions often overwhelm description of his field activities. The letter-diary served purposes which his simpler field journal could not. Like a letter, it provided an escape from present circumstances into indirect communication with someone dear and far away. Like a diary, it was a personal document where he could relieve himself of otherwise contained emotions—love, frustration, joy and despair. Under especially trying conditions, it sometimes ceased even to be a personal document and merely duplicated the sparse entries of his daily field journal. At other times, there are gaps of days, even longer, invariably followed by apologies and catch-up reports. While not a perfect way to reconstruct Boas' first field experience, it does allow considerable insight into his soul and travail.

The letter-diary is a very hard document to read. Iglus and tepiks possessed no writing desks and the letter diary went with him over the estimated 3,000 or so miles he travelled; by his own admission to Marie, his handwriting was often little more than "chicken-scratches" (*Krackelfüsse*—letter-diary 11/5/83). The extant document is not even "original" for the most part, but a

carbon copy made on perforated 17.5 by 8.5 cm notepads. The original was mostly in pencil (there was a problem of keeping ink liquid) and the carbon is often smudged. Its legibility was a test even for the late Helene Boas Yampolsky, daughter of Boas, whose work in creating a translation cannot be praised too highly. While relying very much upon this translation, I have succeeded in filling some of her gaps and have made changes where I thought a better reading possible.

The poor legibility of the text and the necessity of turning German scribbled in the field into acceptable English make textual integrity impossible. Inuit personal names are given as accurate a rendering as possible, using *Baffin-Land* (Boas 1885) when they are mentioned there, but more often relying upon the most common or most clear form of the Boas manuscripts. Geographical names have been standardized, except Kikkerton and Kingawa, to Boas' list in *Baffin-Land* (90–94). Several other terms (e.g., Doctorā'dluk) have also been regularized—although it is important to note that Boas later insisted on the methodological significance of such “alternating” renditions (1889). As reproduced here, the letter-diary is rather severely abridged. The early shipboard sections, which occupy almost a third of the original manuscript, are almost entirely omitted; in all, the text is cut to about one quarter of the original. What remains, however, will perhaps convey the essence of Boas' ethnographic initiation.

[The letter-diary opens three days after the *Germania* sailed from Hamburg on June 20, as it was passing from the Elbe into the North Sea. “My best beloved! Today I am beginning to write my diary to you and must tell you first of all how much I love you.” Boas describes life on board, his cramped and smelly cabin, how he tried to give Wilhelm lessons in English (“He has a terribly thick head. Things don't penetrate very readily”), and how, by July 5, life had become “very monotonous.” On his birthday, July 9, when the ship passed Greenland's Cape Farewell into the Arctic Ocean, he was so seasick that only in the afternoon could he even look at the letters and presents Weike had for him. Two days later, he ruminated on the purpose of the trip: “It is funny how everybody thinks I am making this trip for fame and glory. Certainly they do not know me and I would have a poor opinion of myself if that was a *goal* for which I put in work and effort. You know that I strive for a higher thing and that this trip is only a means to that goal. I suppose it is true that I want external recognition for my achievements, but only in so far as I wish to be known as a man who will carry out his ideas and act upon them. That is the only kind of recognition I can think of. Empty glory means nothing to me.”

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