

# Margaret Mead

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# Letters

from the

# Field

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1925-1975



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LETTERS FROM  
THE FIELD  
1925–1975

MARGARET MEAD

*The American Museum of Natural History*  
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LETTERS FROM THE FIELD, 1925-75.

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**LETTERS FROM THE FIELD**  
**1925 - 1975**

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## Introduction

These letters from the field are one record, a very personal record, of what it has meant to be a practicing anthropologist over the last fifty years.

Field work is only one aspect of any anthropologist's experience and the circumstances of field work—the particular circumstances of any one occasion—are never twice the same nor can they ever be alike for two fieldworkers. Yet field work—the unique, but also cumulative, experience of immersing oneself in the ongoing life of another people, suspending for the time both one's beliefs and disbeliefs, and of simultaneously attempting to understand mentally and physically this other version of reality—is crucial in the formation of every anthropologist and in the development of a body of anthropological theory. Field work has provided the living stuff out of which anthropology has developed as a science and which distinguishes this from all other sciences.

Field work is, of course, very ancient, in the sense that curious travelers, explorers and naturalists have gone far afield to find and bring home accounts of strange places, unfamiliar forms of plant and animal life and the ways of exotic peoples. Ancient records refer to the unusual behavior of strangers, and for thousands of years artists have attempted to capture some living aspects of the peoples and creatures evoked in travelers' tales or the sacred mythology of some distant, little-known people. A generation ago students still were given Greek and Latin texts through which they not only learned about high civilizations ancestral to our own but also gained a view of exotic peoples as they were described by Greeks and Romans in their own era. In fact, generation after

generation, philosophers and educators, historians and naturalists, polemicists and revolutionaries, as well as poets and artists and storytellers, have drawn on the accounts of peoples who seemed more idyllic or more savage or more complexly civilized than themselves.

But only in this century have we attempted systematically to explore and comprehend the nature of the relationship between the observer and that which is observed, whether it is a star, a microscopic particle, an ant hill, a learning animal, a physical experiment or some human group isolated for hundreds, perhaps thousands, of years from the mainstream of the world's history as we know it. Throughout my lifetime the implications of the inclusion of the observer within the circle of relevance have enormously widened and deepened. Einstein lectured at Columbia University while I was an undergraduate at Barnard. I read Erwin Schrödinger's *Science and the Human Temperament* when it appeared in English in 1935. And of course I belong to the generation of those who learned from Freud that observers of human behavior must become aware of how they themselves have become persons and respond to those whom they are observing or treating. This kind of consciousness was systematized in psychoanalytic theory and practice as transference and counter-transference; analysts, attending intensively to the slightest change in the rhythm of their analysands' speech or movement, learned to attend at the same time to their own flow of imagery and to grasp the relationship between the two.

As these insights became widely known and were incorporated in scientific thought and practice, a counter-tendency also developed among certain scientists concerned with the study of human behavior. Having discovered how deeply the observer is involved in what is observed, they made new efforts to ensure objectivity and to systematize methods of observation that would minimize the effect of observer bias. Sophisticated statistical methods were developed that effectively eliminate the individual observation as well as the individual observer. Experiments were devised using double-blind methods and observers were given formal check lists on which to note, for example, the behavior of infants in such ways that no hint of intuitive response would be preserved in the records that eventually saw the light of day.

In the natural sciences students were carefully trained to cast

every experiment within a rigid framework that controlled the development of hypotheses, the use of methods of recording and analysis and the limits of the conclusions—a style of research recording that for a long time almost completely disguised the actual complexities of scientific advance under a mask of uniform orderliness. Following this precedent, social scientists elaborated the paraphernalia of objective social science. Their methods, identified as “science,” were pitted against what were called “impressionistic” methods, in which the records of the human observer were presented without the sanitizing operations which appeared to remove the observer from the scene.

In this conflict between those who attempted to mechanize the intelligence and skills of the observer and those who tried to make the most of the idiosyncratic skills and intuitions of the observer, by enlarging and deepening the observer’s self-awareness, anthropologists occupied a middle ground.

We were slowly devising ways in which our reports on the culture of a primitive people could be made objective in the sense that another fieldworker, comparably trained, might be expected to elicit the same order of data from members of the same culture. This was particularly the case in linguistics, since methods of standardized phonetic recording can be used to reproduce the regularities of an unwritten language in such a way that the data can be analyzed and used for comparative purposes by other linguists. In this work the sensitivities of the individual human ear are fully enlisted, both the ear of the native speaker of the language to whom the field linguist must present alternative sound sequences and the ear of the fieldworker who writes down the language. Today this can be supplemented by tape recordings of the process, which allow another listener to hear and compare.

With less initial precision—for language has the special advantage of being coded by speaker and listener in the same way—cultural anthropologists learned to record the kinship usages of a people by fitting the terms to the biological phenomena of reproduction, so that the terms for mother’s brother, for example, or daughter’s son can be as unequivocally specified as the method by which the outrigger of a canoe is lashed to the canoe can be described and diagramed.

Through the use of such techniques—and the training of students to use these techniques reliably and confidently—the ethno-

graphic monograph came to contain a large body of ordered information which was reasonably independent of observer bias, whether that bias was owing to ethnocentricity, temperamental preferences, research interests or applied aims. Our methods of describing a ceremony or an economic exchange or the complex details of an agricultural process and of recording the texts of folktales and myths have become sufficiently formalized so that, if a large body of such diversified data is split in half, others trained in the same paradigms may be expected, by careful analysis, to arrive at comparable results.

But we were also developing a special approach to field work as a whole. That is, while we were learning how to apply the various formal techniques in the field—how to take down linguistic texts in phonetic script and how to learn a language and record it, how to trace socially contrived relationships through the ramifications of biologically derived relationships, how to relate a people's own color classifications to a color chart based on our contemporary understanding of the psycho-physiology of color perception and, especially, how to teach our informants how to teach us—we were also learning how to live in the field. This became known as "participant observation." It began as the observer moved from the mission compound or from the rocking chair on the porch of some inn or the office of a colonial administrator to the place where the people actually live.

However, this is only the beginning. Living in the village by night as well as by day and for long uninterrupted months, the field anthropologist witnesses thousands of small events which never would have become visible, let alone intelligible, at a greater distance. It is, in fact, a very peculiar situation, for while the anthropologist "participates" in everyday life he—or she—also observes that participation and both enters into genuinely meaningful and lasting relationships with individuals and learns from those relationships the nature of "relating" in that society.

It is sometimes assumed that participant observation means taking on a kind of protective coloring or even assuming a disguised or a fictitious role—an "as if" relationship to the people among whom one is living—as a way of observing them. Actually there is a kind of absurdity in this, as the fieldworker is always present notebook in hand, asking questions, trying to learn and to understand, and the field work becomes rich and rewarding to the

extent that the people one is studying accept the legitimacy of one's work and at least some of them, in turn, begin to develop the second-level consciousness of self-awareness.

This new kind of field work, in which anthropologists live for an extended period in the midst of the people whose way of life they wish to understand, was just beginning when I entered anthropology. During the next decade it was developed, almost independently, in England by Bronislaw Malinowski and his students and in the United States by the students Franz Boas sent into the field to work on new kinds of problems in which an intimate understanding of many individual members of a primitive society was necessary. Our methods, which developed out of the conditions in which we worked, were grounded—as they still are—in certain fundamental theoretical assumptions about the psychic unity of mankind and the scientist's responsibility to respect all cultures, no matter how simple or how exotic, and to appreciate the worth of the people who are studied in order to increase our systematic understanding of the capacities and potentialities of *Homo sapiens*.

We knew that we had been bred in our culture and could never lose our own cultural identity; we could only learn about others through the recognition that their membership in their culture and our membership in ours, however different in substance, were alike in kind. But we did not yet recognize that every detail of reaching the field and of interchange with those who tried to bar or who facilitated our way to our field site were also part of our total field experience and so of our field work. This we have learned very slowly as we have learned to use our disciplined subjectivity in the course of a long field trip among isolated peoples distant in time and space from our own society. We have learned that every part of the field experience becomes part of our evolving consciousness—the impressions gained on the journey, our interchanges with government personnel at many levels, with missionaries and teachers and businessmen, the inaccurate as well as the accurate information accumulated from other travelers, the bright or the subdued light in which we first glimpse the villages where we intend to work, the letters that reach us, the books we read, the chills and fevers that accompany work in hot jungles or high, cold mountains.

When I started to write these letters, I had no sense that I was

discussing the making of a method, that in making what I was doing intelligible to myself and to my family and friends I was recording steps in the development of a new kind of holistic approach. But I returned from my first field trip to Samoa to discuss the relationship between Samoans and the United States Navy not in terms of an ideologically defined separation of exploiting imperialists and an exploited people, but in the light of my own experience of the way both groups, through their perceptions of each other, were becoming part of a larger whole. However, it was only twenty-five years later—and only after the Manhattan Project had produced the atom bomb—that I realized the basic difference between such a project, which could be pursued in isolation from the rest of the society, and the applications of anthropological knowledge, which depend on the diffusion throughout the wider society of the particular findings about the capacities of our human nature and the constraints imposed by our shared common humanity.

From my own first field trip to Samoa participation has involved entering into many facets of the life of the people I have worked among—eating the food, learning to weave a mat or make a gesture of respect or prepare an offering or recite a charm as they had been taught to do, using the disciplined awareness of how I myself felt in the circumstances as one further way of coming to understand the people who were my teachers as well as the subjects of my study.

For the anthropologist living in the midst of a village, waking at cock crow or drum beat, staying up all night while the village revels or mourns, learning to listen for some slight change in the level of chatter or the cry of a child, field work becomes a twenty-four-hour activity. And everything that happens, from the surly refusal of a boatman to take one across the river to one's own dreams, becomes data once the event has been noted, written up, photographed or tape recorded.

As the inclusion of the observer within the observed scene becomes more intense, the observation becomes unique. So the experience of each fieldworker on each particular field trip differs from all other comparable experience. This, too, must be part of one's awareness. And the more delicate and precise the methods of recording—and I have lived through all the improvements from pencil and notebook and still photography to video tape—the



more fully these unique experiences become usable parts of our scientific data. Equipped with instruments of precision and replication that were developed to meet the requirements of natural scientists for objectivity and replicable observations, human scientists are able to bring back from the field records of unique, subjectively informed experience which can be analyzed and later re-analyzed in the light of changing theory.

But the process of obtaining the information is very curious and exacting. Psychoanalysts, who must pay such intense and continuous attention to every slightest nuance in the communication process, at the end of the day can close the door of their consulting room, turn off their insightful attention and go out into the world to become, apparently, as unselfconsciously unaware as the least analyzed of their acquaintances. The field anthropologist cannot give the same kind of undivided attention to the full kaleidoscope of events, all of which together become the background experience which must be turned into data—the behavior of a woman with a fish to sell, the behavior of two children watching an old man who is preparing to tell a tale, the expression of a boy with a bleeding cut to be bound up. But field anthropologists never can turn off their attention. Visitors from outside this closed circle of attention are both a temptation and an interruption. Letters from home wrench one's thoughts and feelings inappropriately away.

Nevertheless, letters written and received in the field have a very special significance. Immersing oneself in life in the field is good, but one must be careful not to drown. One must somehow maintain the delicate balance between empathic participation and self-awareness, on which the whole research process depends. Letters can be a way of occasionally righting the balance as, for an hour or two, one relates oneself to people who are part of one's other world and tries to make a little more real for them this world which absorbs one, waking and sleeping.

Over the years I have come to realize that each generation of young anthropologists can only build on the present. They can't go back and they can't do it over again. They have to go on in a world that has changed, making observations and developing theory in ways that were not yet possible before their own teachers went to the field and that will no longer fully satisfy their own students when they, in turn, begin their field research. Books and monographs record the outcome of field experience. But we have