

tests. But then my preparations, which in retrospect were so simple, seemed fairly strenuous. And in spite of all the differences between assembling that simple collection of materials and the complex field equipment necessary today, the aura of the last few days before a field trip remains very much the same.

Now, as then, it is necessary to survive all kinds of hazards—having the inoculations that make you feel clumsy and feverish (in 1971, I had five sets all in one day, and afterward crushed my finger in a door and bruised my knee getting out of a taxi); breaking your glasses; falling in love or having someone fall in love with you; trouble about passports and funds (in 1925, the Committee for the Biological Sciences of the National Research Council held a special meeting to decide whether they could advance \$450 of my stipend, because communications with Samoa were so difficult); clothes that do not get finished and things that have been ordered but have not come. In those days, however, everything went on the same ship with you. Today, field equipment is sent off three or four months in advance and you wait at home, hoping to receive the news that it has arrived safely, and then you fly to your destination.

At the end of the summer, when every problem had finally been solved, Luther and I had a last vacation together. Once more we borrowed his brother's car and my aunt lent us a cottage in Rhode Island for a week. On the way we spent a few days at Lake Winnepesaukee, where we visited Ruth and Stanley Benedict, the only occasion I ever spent any time with him.

Then I set off for Samoa and Luther sailed for Europe.

PART TWO

11

*Samoa:
The Adolescent Girl*

When I sailed for Samoa, I realized only very vaguely what a commitment to field work and writing about field work meant. My decision to become an anthropologist was based in part on my belief that a scientist, even one who had no great and special gift such as a great artist must have, could make a useful contribution to knowledge. I had responded also to the sense of urgency that had been conveyed to me by Professor Boas and Ruth Benedict. Even in remote parts of the world ways of life about which nothing was known were vanishing before the onslaught of modern civilization. The work of recording these unknown ways of life had to be done now—*now*—or they would be lost forever. Other things could wait, but not this most urgent task. All this came to a head at the Toronto meetings in 1924, where I was the youngest participant and everyone else had talked about “my people” and I had no people to talk about. From that time on I was determined to go to the field, not at some leisurely chosen later date, but immediately—as soon as I had completed the necessary preliminary steps.

But I really did not know much about field work. The course on methods that Professor Boas taught was not about field work. It was about theory—how material could be organized to support or to call in question some theoretical point. Ruth Benedict had spent a summer working with a group of quite acculturated Indians in California, where she had taken her mother along for a vacation, and she had worked in Zuñi. I had read Ruth's descriptions of the landscape, of how the Zuñi looked, of

the fierceness of the bedbugs and the difficulties of managing food, but I knew little about how she went about her work. Professor Boas always spoke of the Kwakiutl as "my dear friends," but this was not followed by anything that helped me to know what it was like to live among them.

When I agreed to study the adolescent girl and Professor Boas consented to my doing this field work in Samoa, I had a half hour's instruction in which Professor Boas told me that I must be willing to seem to waste time just sitting about and listening but that I must not waste time doing ethnography, that is, studying the culture as a whole. Fortunately, many people—missionaries, jurists, government officials, and old-fashioned ethnographers—had been to Samoa, and so the temptation to "waste time" on ethnography would be less. During the summer he also wrote me a letter in which he once more cautioned me to be careful of my health and discussed the problem he had set me:

I am sure you have thought over the question very carefully, but there are one or two points which I have in mind and to which I would like to call your attention, even if you have thought of them before.

One question that interests me very much is how the young girls react to the restraints of custom. We find very often among ourselves during the period of adolescence a strong rebellious spirit that may be expressed in sullenness or in sudden outbursts. In other individuals there is a weak submission which is accompanied, however, by a suppressed rebellion that may make itself felt in peculiar ways, perhaps in a desire for solitude which is really an expression of desire for freedom, or otherwise in forced participation in social affairs in order to drown the mental troubles. I am not at all clear in my mind in how far similar conditions may occur in primitive society and in how far the desire for independence may be simply due to our modern conditions and to a more strongly developed individualism.

Another point in which I am interested is the excessive bashfulness of girls in primitive society. I do not know whether you will find it there. It is characteristic of Indian girls of most tribes, and often not only in their relations to outsiders, but frequently within the narrow circle of the family. They are often afraid to talk and are very retiring before older people.

Another interesting problem is that of crushes among girls. For the older ones you might give special attention to the occurrence of romantic love, which is not by any means absent as far as I have been able to observe, and which, of course, appears most strongly where the parents or society impose marriages which the girls may not want.

. . . Stick to individual and pattern, problems like Ruth Bunzel on art in Pueblos and Haerberlin on Northwest Coast. I believe you have read Malinowski's paper in *Psyche* on the behavior of individuals in the family in New Guinea. I think he is much too influenced by Freudians, but the problem he had in mind is one of those which I have in mind.

For the rest, there was G. Stanley Hall, who had written a huge book on adolescence in which, equating stages of growth with stages of culture, he had discussed his belief that each growing child recapitulated the history of the human race. There were also the assumptions set forth in textbooks, mainly derived from German theory, about puberty as a period of storm and stress. At that time puberty and adolescence were firmly equated in everyone's thinking. Only much later, students of child development began to say that there was perhaps a "first adolescence" around the age of six and a second crisis at puberty and that adolescence could be prolonged into the twenties and might in some sense reappear in adults in their forties.

My training in psychology had given me ideas about the use of samples, tests, and systematic inventories of behavior. I had also some very slight experience of social case work. My aunt Fanny was working at the Juvenile Protective Association at Hull House, in Chicago, and

one summer I had sat on the floor and read their records. This had given me an idea of what the social context of individual behavior was—how one had to look at the household and place the household in the setting of the community.

I knew that I would have to learn the language. But I did not know anyone who was colloquially proficient in the language of the people they studied except missionaries, or the children of missionaries, turned ethnologists. I had read only one essay by Malinowski and did not know how he had used the Trobriand language. I myself had never learned a foreign language; I had only "studied" Latin and French and German in high school. Our training in linguistics had consisted of short demonstrations of extremely exotic languages in the course of which we were confronted, without previous preparation, with phrases like these:

"Adē," nē'x-lata NEMŏ'guis lāxis ts'ā'yē Lŏ'La'watsa; "Joā'LEla sENS
 "Friend," ho said NEMŏ'guis to his younger Lŏ'La'watsa; do not us
 brother
 hēquā'lē yā'wix'idag-a x'ins qa yā'yats'ē sENS xunŏ'kuēx."
 go on in let us act us to go on the our son this."
 this way

In a way this was an excellent method of teaching. It prepared us—as our classes on forms of kinship and religious belief also did—to expect almost anything, however strange, unaccountable, and bizarre it might seem to us. This is, of course, the first lesson the field anthropologist must learn: that he may well meet up with new, unheard-of, unthought-of ways of organizing human behavior.

The expectation that we may at any time be confronted by some as yet unrecorded mode of behavior is the basis on which anthropologists often clash with psychologists, whose theories have developed out of their efforts to be "scientific" and out of their skepticism about philosophical constructs. It is also the basis of our clash with economists, political scientists, and sociologists, to the extent that they use the model of our own social arrangements in their studies of other societies.

The tough treatment given us by Professor Boas shook us up, prepared us for the unexpected, and be it said, the extremely difficult. But we did not learn how to organize work on a strange new language to the point at which a grammar could be worked out on the basis of which we could learn to speak the language. Sapir remarked parenthetically on the immorality of learning foreign languages; one was never really honest, he said, except in one's mother tongue.

There was, in fact, no *how* in our education. What we learned was *what* to look for. Years later, Camilla Wedgwood, on her first field trip to Manam Island, reflected on this point when she wrote in her first letter back: "How anyone knows who is anybody's mother's brother, only God and Malinowski know." Lowie, too, illustrated the startling differences between his field methods and mine when he inquired, "How does anyone know who is whose mother's brother unless somebody tells you?"

Our training equipped us with a sense of respect for the people we would study. They were full human beings with a way of life that could be compared with our own and with the culture of any other people. No one spoke of the Kwakiutl or the Zuñi—or any other people—as savages or barbarians. They were, it was true, primitive; that is, their culture had developed without script and was maintained without the use of script. That was all the term "primitive" meant to us. We were carefully taught that there was no regular progression from simple "primitive" languages to complex "civilized" languages; that, in fact, many primitive languages were far more complex than some written ones. We were taught also that whereas some art styles had been elaborated from simple designs, other art styles were reduced to simpler forms from originally more elaborate ones.

We had, of course, had lectures on evolution. We knew that it had taken millions of years for the first human-like creatures to develop language, to make tools, to work out forms of social organization that could be taught to the next generation, for all these things, once acquired, had to be taught and learned. But we went to the field

not to look for earlier forms of human life, but for forms that were different from those known to us—different because particular groups of primitive people had lived in isolation from the mainstreams of the great civilizations. We did not make the mistake of thinking, as Freud, for example, was misled into thinking, that the primitive peoples living on remote atolls, in desert places, in the depths of jungles, or in the Arctic north were equivalent to our ancestors. True, we might learn from them how long it took to chop down a tree with a stone axe or even how much of the food supply women may have contributed in societies based on male hunting. But these isolated peoples were not in the line of our ancestors. Obviously our ancestors had been located at various crossroads where peoples met and exchanged ideas and traded goods. They had crossed mountains, they had sailed the seas and returned. They had borrowed and copied. They had stimulated and had been stimulated by the discoveries and inventions of other peoples to an extent that was not possible among peoples who lived in much greater isolation.

We knew that in our field work we could expect to find differences—differences far greater than those we would expect to find among the related cultures of the Western world or in the lives of people at different periods in our own history. The record of what we found out about the way of life of each primitive people we studied was to be our principal contribution to the accumulating store of exact knowledge about the world.

As far as anthropology was concerned, this was my intellectual equipment. I had, of course, acquired some knowledge of the techniques in use for categorizing, for example, the uses a people made of their natural resources or the forms of social organizations they had developed. And I had some practice in analyzing the observations that had been made by other fieldworkers.

But nobody really asked what were the young fieldworker's skills and aptitudes—whether he had, for instance, the ability to observe and record accurately or the intellectual discipline to keep at the job, day after day,

when there was no one to supervise, no one to compare notes with, to confess delinquencies to, or even to boast to on an especially successful day. Sapir's letters to Ruth Benedict and Malinowski's private diary are filled with self-flagellating confessions of idleness written at a time when, as we also know, they were doing prodigious work. No one considered whether we could stand loneliness. No one inquired how we would get along with the colonial or military or Indian Service officials through whom we would have to work; and no one offered us any advice.

The style, set early in the century, of giving a student a good theoretical orientation and then sending him off to live among a primitive people with the expectation that he would work everything out for himself survives to this day. In 1933, when I gave a girl student who was setting out for Africa some basic instructions on how to cope with the drinking habits of British officials, anthropologists in London sneered. And in 1952, when I arranged for Theodore Schwartz to spend a year learning the new technologies—running a generator, making tape recordings, and working with cameras—that we intended to use in the field, his professors at the University of Pennsylvania thought this was ridiculous. Men who are now professors teach their students as their professors taught them, and if young fieldworkers do not give up in despair, go mad, ruin their health, or die, they do, after a fashion, become anthropologists.

But it is a wasteful system, a system I have no time for. I try to work against it by giving students a chance to work over my own field preparations and notes, by encouraging them to work at photography, and by creating field situations for my class, in which they have to work out a real problem and face up to the difficulties of an actual situation in which there are unknown elements. For only in this way can they find out what kind of recording they do well—or very badly—or how they react when they discover they have missed a clue or have forgotten to take the lens cap off the camera for a critical picture.

But I am also constantly defeated. A year's training in how to protect every object so that it will withstand humidity or being dropped in the river or the sea does not keep a young fieldworker from turning up with a unique copy of a manuscript wrapped in brown paper, or with her passport and money in a flimsy handbag, or without an airtight container for a valuable and essential camera. Yet students in other disciplines do learn; chemists learn laboratory procedure and psychologists learn to use a stopwatch and to write protocols.

The fact that anthropologists insist on learning everything over again for themselves, often including all the theory they have been taught, is, I think, an occupational disease that may well be inseparable from field work itself. For field work is a very difficult thing to do: To do it well, one has to sweep one's mind clear of every presupposition, even those about other cultures in the same part of the world in which one is working. Ideally, even the appearance of a house should come to one as a new, fresh impression. In a sense it should come to one as a surprise that there are houses and that they are square or round or oval, that they do or do not have walls, that they let in the sun or keep out the wind or rain, that people do or do not cook or eat in a dwelling house. In the field one can take nothing for granted. For as soon as one does, one cannot see what is before one's eyes as fresh and distinctive, and when one treats what is new merely as a variant of something already known, this may lead one far astray. Seeing a house as bigger or smaller, grander or meaner, more or less watertight than some other kind of house one already knows about cuts one off from discovering what *this* house is in the minds of those who live in it. Later, when one has come to know the new culture, everything has to be reassimilated into what is already known about other peoples living in that area, into our knowledge about primitive peoples, and into our knowledge about all human beings, *so far*. But the point of going to the field at all is to extend further what is already known, and so there is little value merely in identifying new versions of the familiar when we

might, instead, find something wholly new. But to clear one's mind of presuppositions is a very hard thing to do and, without years of practice, all but impossible when one is working in one's own culture or in another that is very close to it.

On one's first field trip one doesn't know all this. All one knows is that there is a tremendous job ahead—that it will be a struggle to learn the language well enough to hear and speak it, to find out who everyone is, to understand a myriad of acts, words, glances, and silences as they are integrated into a pattern one has no way of working out as yet, and finally, to "get" the structure of the whole culture.

Before I started out for Samoa I was warned that the terms in which others had written about the culture were anything but fresh and uncontaminated. The recorded grammar was contaminated by the ideas of Indo-European grammar and the descriptions of local chiefs by European notions about rank and status. I knew I would have to thread my way through this maze of partial understandings and partial distortions. In addition, I had been given the task of studying a new problem, one on which no work had been done and for which I had no guidelines.

But in essence this is true of every worthwhile field trip. Today students sometimes are sent to the field to work on a small problem that does not involve much more than making the observations necessary to fill out a prearranged questionnaire or giving a few specific tests. Where the questions are unsuitable or the tests are wholly uncogent to the unwilling subjects, these may not be the easiest things to do. But if the culture has already been properly studied, this kind of work may do as little harm as it does good. But it is not the same thing as being charged with getting the whole configuration of the culture down correctly.

But at the same time one has always to remember that the pattern one discerns is only one of many that might be worked out through different approaches to the same human situation. The grammar you work out is not *the*

grammar but a grammar of the language. But as it may be the only grammar anyone will ever make, it is crucial that you listen and record as minutely and as carefully as you can and, as far as possible, without reference to the grammar you are tentatively putting into shape.

All this is important, but it gives no sense of what the day-to-day tasks will be. For there is no way of knowing in advance what the people will be like or even what they will look like. There may be photographs of them, but by the time one arrives they may look different. The summer I worked among the Omaha Indians, the girls were getting their first permanent waves—something I could not have foreseen. One doesn't know what the particular officials, the planters, the police, the missionaries, or the traders will be like. One doesn't know where one will live or what there will be to eat or whether it will turn out to be a good thing to have rubber boots, mosquito boots, sandals that keep one's feet cool, or woolen socks to absorb the sweat. So there is a great tendency—and when fieldworkers were poor there was a greater tendency—to take along as little as possible and to make very few plans.

When I set out for Samoa I had half a dozen cotton dresses (including two very pretty ones) for I had been told that silk rotted in the tropics. But when I arrived, I found that the Navy wives dressed in silk. I had a small strongbox in which to keep my money and papers, a small Kodak, and a portable typewriter. Although I had been married for two years, I had never stayed alone in a hotel and I had made only short journeys by train as far as the Middle West. Living in cities and small towns and in the Pennsylvania countryside, I had known a good many different kinds of Americans, but I had no idea of the kind of men who enlisted in the United States Navy in peacetime, nor did I know anything about the etiquette of naval life on an outstation. I had never been to sea.

At a party in Berkeley, where I stopped briefly on my way out, Professor Kroeber came and sat next to me on the floor and asked in a firmly sympathetic voice, "Have

you got a good lamp?" I did not have any lamp at all. I had six large fat notebooks, some typing and carbon paper, and a flashlight. But no lamp.

When I arrived in Honolulu I was met by May Dillingham Freer, a Wellesley friend of my mother's. She and her husband and daughter were living in their house up in the mountains, where it was cooler, but she said I could live in Arcadia, their beautiful big house in the town. The fact that my mother had known May Dillingham and her sister-in-law Constance Freer at Wellesley made all the difference in my comings and goings in Honolulu for many years. May Dillingham was the daughter of one of the original missionary families, and her husband Walter Freer had been governor of the Hawaiian Islands. She herself was strangely out of place in her great, extensive, wealthy family; she was full of delicate sentimentalities and was childlike in her approach to life. But she was able to command any resources she needed and her influence, which extended even to Samoa, smoothed my path in a hundred ways. Under her guidance everything was arranged. The Bishop Museum made me an honorary member of the staff; Montague Cook, a member of another old Honolulu family, drove me to the Museum every day; and E. Craighill Handy gave up a week of his vacation to give me daily lessons in Marquesan, a language related to Samoan. A friend of "Mother May"—as I immediately named her—gave me a hundred little squares of torn old muslin "to wipe the children's noses," and she herself gave me a silk boudoir pillow in response to one of the few bits of practical advice I was given, in this case by a biologist who said, "Always take a little pillow and you can lie on anything." Someone else took me to visit two part-Samoan children in school; this meant that their family would be on hand to help me in Samoa.

It was all extremely pleasant. Basking in the Freer and Dillingham prestige, I could not have had a more felicitous beginning to my field trip. But this I only vaguely realized, for I did not know how to sort out the effects of influence and the courtesies that were entirely routine.

But many a young fieldworker has known heartbreak in those first weeks. He has been made to feel so miserable, so unwelcomed, and so maligned—perhaps in terms of another anthropologist who got everyone's back up—that his whole field trip has been ruined before he has really got under way. These are the incalculable risks from which one can only try to protect one's students. But the factor of accident is great. Mrs. Freer might have been away from Honolulu when I arrived. Just that.

After two weeks I left, weighed down with flower leis, which in those days we threw from the ship's deck into the sea. Nowadays the Samoans give shell leis, because the admission of flowers and fruits into other ports is forbidden, and they bring a plastic bag in which to carry the leis home. But in those other days the ship's wake was bright with floating flowers.

And so I arrived in Samoa. Remembering Stevenson's rhapsodies, I was up at dawn to see with my own eyes how this, my first South Sea island, swam up over the horizon and came into view.

In Pago Pago there was no one to meet me. I had a letter of introduction from the Surgeon General of the Navy, who had been a fellow student of Luther's father at medical school. But that day everyone was too busy to pay attention to me. I found a room at the ramshackle hotel and hurried back to the square to watch the dances that had been arranged for the visitors from the ship. Everywhere there were black umbrellas. Most of the Samoans wore cotton clothes—the men were dressed in standard style and the women in heavy, unbecoming overblouses. Only the dancers wore Samoan dress. The chaplain, thinking I was a tourist with whom he could take liberties, turned over my Phi Beta Kappa key to look for my name. I said, "It isn't mine." And this confused things for many months afterward.

Then came the period that young fieldworkers find so trying, even today, no matter how hard we try to prepare them for it. I was in Samoa. I had a room in the hotel—the hotel that had been the scene of Somerset Maugham's story and play, *Rain*, which I had seen performed in New

York. I had letters of introduction. But I still had to establish some kind of base for my work. I called on the governor, an elderly and disgruntled man who had failed to attain the rank of admiral. When he told me that he had not learned the language and that I would not learn it either, I incautiously said that it was harder to learn languages after one was twenty-seven. That did not help.

Without the letter from the Surgeon General I do not know whether I would have been able to work as I wished. But that letter opened the doors of the Medical Department. The chief nurse, Miss Hodgeson, assigned a young Samoan nurse, G. F. Pepe, who had been in the United States and spoke excellent English, to work with me an hour a day.

I then had to design a way of working the rest of the time. I was very conscious of being on my own and yet of being responsible to the Fellowship Committee whose members had objected to giving me three months' stipend in advance. As there was no other way of measuring how hard I was working, I decided that I must at least work for eight hours a day. For one hour I was tutored by Pepe. The other seven I spent memorizing vocabulary and so, by accident, hit the best way to learn a language, which is to learn as much of it as fast as possible so that each piece of learning reinforces each other piece.

I sat in the old hotel and ate the dreadful meals prepared by Fa'alavelave—whose name meant Misfortune—which were supposed to accustom me to Samoan food. Occasionally I was asked up to the hospital or to the home of one of the medical staff. The National Research Council had insisted on mailing my checks to me, and the next boat overcarried the mail. This meant that for six weeks I had no money and could not plan to leave until I had paid my hotel bill. Every day I walked about the port town and tried out Samoan phrases on the children, but it was not an atmosphere in which I could do field work.

Finally the boat arrived again. And now, with the help of the mother of the half-Samoan children I had met in Honolulu, I was able to move to a village. She arranged



In Vaitogi: in Samoan dress, with Fa'amotu

for me to spend ten days in Vaitogi and to live in the household of a chief who enjoyed entertaining visitors. It was there I had all my essential training in how to manage Samoan etiquette. His daughter Fa'amotu was my constant companion. We slept together on a pile of mats at the end of the sleeping house. We were given privacy from the rest of the family by a tapa curtain, but of course the house was open to the eyes of the whole village. When I bathed I had to learn to wear a sarong-like garment which I could slip off under the village shower as I slipped a dry one on in full view of the staring crowds of children and passing adults.

I learned to eat and to enjoy Samoan food and to feel unabashed when, as a guest, I was served first and the whole family sat about sedately waiting for me to finish so that they, in turn, could eat. I learned the complex courtesy phrases and how to pass kava—something I never did as it is only appropriate for an unmarried woman to do so. However, in Vaitogi I did not tell them that I was married; I knew too little about what the consequences might be in the roles that would be assigned to me. Day after day I grew easier in the language, sat more correctly, and suffered less pain in my legs. In the evenings there was dancing and I had my first dancing lessons.

It was a beautiful village with its swept plaza and tall, round, thatched guesthouses against the pillars of which the chiefs sat on formal occasions. I learned to recognize the leaves and plants used for weaving mats and making tapa. I learned how to relate to other people in terms of their rank and how to reply in terms of the rank they accorded me.

There was only one difficult moment when a visiting talking-chief from British Samoa started a conversation based on his experience with the freer sex world in the port of Apia. Still struggling with the new language, I explained that marriage would not be fitting owing to our respective ranks. He accepted the phrasing, but added regretfully, "White women have such nice fat legs."

At the end of the ten days, which were as delightful and satisfying as the previous six weeks had been labori-

ous and frustrating, I returned to Pago Pago to prepare to go to the island of Tau in the Manu'a group. Everyone agreed that the Manu'an islands were much more old-fashioned and were, therefore, much better for my purposes. There was a medical post on Tau, and Ruth Holt, the wife of Chief Pharmacist's Mate Edward R. Holt, who was in charge of the post, was in Pago Pago having a baby. The chief medical officer arranged that I would live on the post, and I crossed over with Mrs. Holt and the new baby on a minesweeper that had temporarily replaced the station ship. In the dangerous landing through the reef a whaleboat carrying schoolchildren upset, and Mr. Holt was relieved to get his new baby, named Moana, safely ashore.

My living quarters were set up on the back veranda of the dispensary. A lattice separated my bed from the dispensary porch and I looked out across a small yard into the village. There was a Samoan-type house in front of the dispensary where I was to work with my adolescents. A Samoan pastor in the next village presented me with a girl who was to be my constant companion, as it would have been unsuitable for me ever to be alone. I set about working out living arrangements with the Holts, who also had a little boy, Arthur, who was not quite two years old and spoke both Samoan and English.

I soon found that having my base in the dispensary household was very useful, for had I lived in a Samoan household I could have had nothing to do with children. I was too important for that. People knew that when the fleet came in to Pago Pago I had dined on the admiral's flagship; that had established my rank. Reciprocally, I insisted that the Samoans call Mrs. Holt *faletua*, so that there would be no questions about where and with whom I ate.

Living in the dispensary, I could do things that otherwise would have been wholly inappropriate. The adolescent girls, and later the smaller girls whom I found I had also to study, came and filled my screen-room day after day and night after night. Later I borrowed a

schoolhouse to give "examinations," and under that heading I was able to give a few simple tests and interview each girl alone. Away from the dispensary I could wander freely about the village or go on fishing trips or stop at a house where a woman was weaving. Gradually I built up a census of the whole village and worked out the background of each of the girls I was studying. Incidentally, of course, I learned a great deal of ethnology, but I never had any political participation in village life.

My field work was terribly complicated by a severe hurricane that knocked the front veranda off the dispensary, knocked down the house I was to have had as a workroom, and destroyed every house in the village and ruined the crops. Village ceremonies were almost completely halted while the village was being rebuilt and, after I had painfully learned to eat Samoan food, everyone had to live on rice and salmon sent over by the Red Cross. The Navy chaplain who was sent over to supervise the food distribution added to the crowding of our small house. In addition, his presence was deeply resented by Mr. Holt, who was only a chief pharmacist's mate because he had refused to go to college; now he smarted under every display of rank.

In all those months I had almost nothing to read, but it did not matter very much because I worked as many hours a day as I could stay awake. My only relief was writing letters. My accounts of life in family bulletins were fairly evenly balanced between pain and pleasure, but in my letters to friends I laid such heavy stress on points of difficulty that Ruth concluded I was having a hard and disappointing time. The truth was that I had no idea whether I was using the right methods. What were the right methods? There were no precedents to fall back on. I wrote to Professor Boas just before I left Pago Pago, outlining my plans. His reassuring answer arrived after I had finished my work on Tau and was ready to leave!

Still, it is letters that bring back to life that distant scene, and in one I wrote:



In Vaitogi: with Paulo

The pleasantest time of day here is at sunset. Then accompanied by some fifteen girls and little children I walk through the village to the end of Siufaga, where we stand on an iron bound point and watch the waves splash us in the face, while the sun goes down, over the sea and at the same time behind the cocoanut covered hills. Most of the adult population is going into the sea to bathe, clad in lavalavas with buckets for water borne along on shoulder poles. All the heads of families are seated in the *faletele* (village guesthouse) making kava. At one point a group of women are filling a small canoe with a solution of native starch (arrowroot). And perhaps, just as we reach the store, the curfew-angelus will stop us, a wooden bell will clang mellowly through the village: The children must all scurry to cover, if we're near the store, it's the store steps, and sit tight until the bell sounds again. Prayer is over. Sometimes we are all back safely in my room when the bell sounds, and then the Lord's Prayer must be said in English, while flowers are all taken out of their hair and the siva song stopped in the middle. But once the bell sounds again, solemnity, never of a very reliable depth, is sloughed off, the flowers replaced in the girls' hair, the siva song replaces the hymn, and they begin to dance, by no means in a puritan fashion. Their supper comes about eight and sometimes I have a breathing spell, but usually the supper hours don't jibe well enough for that. They dance for me a great deal; they love it and it is an excellent index to temperament, as the dance is so individualistic, and the audience thinks it is its business to keep up incessant comment. Between dances they look at my pictures. I am going to have to put Dr. Boas much higher on the wall, his picture fascinates them. . . .

The times I remember with most pleasure are the expeditions we made to other villages, to the other Manu'an islands, and to the other village on Tau, Fitiuta, where I lived like a visiting young village princess. I could summon informants to teach me anything I wanted to know; as a return courtesy, I danced every night. These expeditions came at the end of my field

work, after I felt I had completed my project and so could "waste time" on ethnology by bringing up to date all the detail on ways in which Manu'a differed from the other islands.

On all my later field trips when I was working on cultures about which nothing was known, I had the more satisfactory task of learning the culture first and only afterward working on a special problem. In Samoa this was not necessary, and this is one reason why I was able to carry out my work on the life of the adolescent girl in nine months.

By studying the pre-adolescent girls I invented a cross-sectional method that can be used when one cannot stay many years in the field but wants to give a dynamic picture of how human beings develop. In Samoa I went back only one step. Later I went back to small children and then to infancy, realizing that I needed to include all the stages of growth. But in Samoa I was still under the influence of the psychology I had been taught, and I used case histories and tests that I invented, such as a picture-naming test, using pictures someone had sent me from a magazine story about Flaherty's *Moana of the South Seas*, and a color-naming test, for which I painted the hundred little squares.

When I wrote *Coming of Age in Samoa* I carefully disguised all the names, sometimes using double disguises so that the actual individuals could never be identified. In the introductions I wrote to new editions I did not include the girls I had studied among the readers for whom I was writing; it seemed extremely unlikely that any of them would ever learn to read English. Today, however, the children and grandchildren of girls like the ones I knew in Tau are attending American colleges—for nowadays half the Samoan population lives in the United States—and as their classmates read about the Samoans of fifty years ago, they wonder how what I have said applies to them.

12 *Return from the Field*

In June, 1926, I returned to Tutuila and two weeks later boarded a small ship in Pago Pago. These last weeks in Samoa were deeply nostalgic. I went back to Vaitogi, the village where I had first learned to sleep on a pile of mats and where Ufuti, the gentle chief who enjoyed entertaining visiting Americans, had presided over my learning how to pass kava correctly and how to pronounce the all-important courtesy speeches. My adopted family welcomed me as if I had been away for many years, and I had the sensation of returning home from a long voyage. Coming back to Vaitogi, I realized how homesick I had been and how starved for affection, a need that had been met only in part when I held the Holt children in my arms or played with the Samoan babies. It is the babies who keep me alive in contexts in which otherwise my sense of touch is seldom exercised. As Gregory Bateson phrased it later, it is not frustrated sex, it is frustrated gentleness that is so hard to bear when one is working for long months alone in the field. Some fieldworkers adopt a dog or a kitten; I much prefer babies. I realized now how lonely I had been, how much I wanted to be where someone else wanted me to be just because I was myself.

My adopted family comforted me, and I realized that they would gladly have cared for me for the rest of my life. Fa'amotu, my "sister," was planning to be married, and because I had remarked in some earlier, flowery speech that whereas Samoa excelled in courtesy, France excelled in making beautiful clothes, Fa'amotu wanted a