Reflections on My Career as an Anthropologist
by Alan Howard

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Robert Levy, psychiatrist-turned-brilliant-anthropologist and author of *Tahitians*, said it best. After several years of friendship and occasional collaborations, he said to me: "Alan, you don't have ideas—you entertain them." He was referring to my habit of employing a variety of different theoretical perspectives in my publications. The truth is I have always loved playing with ideas to see where they lead, to see what the world looks like through their lenses.

The Early Years

I was born into a middle-class family that was secular in orientation. My father was an outspoken atheist and my mother was simply not religious in any sense, and I was not exposed to any kind of religious indoctrination. As a result, I was able think about all aspects of the human experience without feeling sinful. Correspondingly, sometime later on, I determined that only one's actions, not one's thoughts, had moral implications. I could fantasize all modes of human experience, including the most extreme, without feeling guilty. Fortunately, contrary to stereotypes regarding Jewish mothers, mine never weaponized guilt; she was a rather shy, compassionate person. In fact, I don't remember ever feeling guilty as a child or an adult, although I have retrospectively regretted my actions in some instances.

As an adolescent, I used to go on long walks, quite oblivious to my surroundings, thinking about all kinds of topics, hypothetically testing out their implications. I did quite well in school despite the fact that I read very little, aside from segments of the encyclopedia we had at home. I remember taking an almost sadistic pleasure in catching my teachers in factual errors and correcting them in front of the class. It didn't endear me to them, to say the least, but it kept me engaged.

I was eager to get to university, so decided to go to a private school in San Francisco where I was able to cram my junior and senior years of high school into a single year, and was accepted to Stanford University at the age of 17. For some reason I still don't understand, I enrolled in Stanford as an aspiring engineering student, but after a few experiences with engineering courses I decided that sitting at a drafting table most of my working life was not for me (had computers been around then, I might have felt differently). So I took an aptitude test, which ironically concluded that my best fit was as a minister!

After exposure to social science courses I decided to major in sociology, which I quite enjoyed. Following my sophomore year, I went with a friend (Freddy Paulson) to Europe on vacation. I left Stanford with $250. After driving across the country in three days, we caught a student-oriented ship in Montreal bound for Southampton, England. Once on board we managed to get jobs in the kitchen washing dishes, which paid for a bit more than half of our passage. My experience in Europe turned out to be pivotal in my ultimate choice of a profession. Being exposed to different cultures as we hitchhiked on a budget of $2 a day through England, The Netherlands, Belgium, France, Germany, Switzerland, and Italy fired my imagination. Freddy and I got quite adept at getting people who gave us a ride to offer to take us to their homes for overnight stays, by telling them about the many kindnesses that people showed us in the previous

countries we had visited. So we got to stay with families in most of the countries in which we travelled. We also stayed in youth hostels, where we met young people from many countries, so all-in-all it proved to be an intensive period of learning about culture and cultural differences. When unable to find cheap accommodations (youth hostels generally cost about 25 cents a night at that time), we looked for suitable places by the roadside to put down our sleeping bags. On the way home we took the same student ship and again got jobs in the galley to pay a significant portion of our fare. I got back to the West Coast with only a few cents in my pocket.

University Experiences

During my first couple of years at Stanford, I was a mediocre student with a B- average and a low of D in calculus, mainly because I mistakenly thought that homework assignments didn't count in the final grade and thus I had failed to turn them in regularly. However, sometime during my junior year self-reflection led me to an interest in psychoanalysis, about which I read voraciously. The pleasure I got from exploring profound ideas triggered a passion for learning and an ambition for a professional career that would prioritize thinking innovatively. From that point on I got almost straight As and graduated 10th in my senior class with a near 4.0 grade point average.

Despite the foray into psychology, my interest in the social sciences remained strong, and although I contemplated a career in psychiatry, I decided to explore my psychological interests within sociology (i.e., social psychology) and anthropology. I had never taken an anthropology class before, but in my senior year I took an introductory course in cultural anthropology from Felix Keesing and a graduate seminar in psychological anthropology from George Spindler. Although I received my B.A. degree in sociology, by the time I graduated I was more attracted to anthropology as a possible career, but the lure of psychiatry was still strong enough to keep me from making any career commitments. So I decided to spend a year getting psychoanalyzed, which I paid for by getting a job in an insurance agency as an underwriter trainee. My purpose was to explore my motivations in depth and to experience psychoanalysis first hand. But psychiatry would require going to medical school, which I had no interest in, and the thought of spending most of my days listening to patients' complaints, anxieties, etc. was decisive. Instead I enrolled as a graduate student at Stanford where I received, thanks to my primary sociology professor, Stephen Boggs, a change-of-field scholarship from sociology to anthropology.

I remember my three years of coursework in graduate school with considerable fondness. Actually, at Stanford anthropology as an academic discipline had only recently split off from a joint department with sociology, and the faculty was rather thin. Keesing was the chair, and the rest of the faculty consisted of Bernard Siegel, George Spindler, and Bert Gerow, who taught archaeology, linguistics, physical anthropology, and world cultures, all of which I was obliged to take from him without excessive enthusiasm. But I especially enjoyed a course labeled social anthropology, which was essentially a course in the history of anthropological theory taught by Keesing. In years to come I taught a similar course and graduate seminar at the University of Hawai'i with comparable content. A special bonus was a seminar led by Gregory Bateson as a visiting scholar. Bateson's perspectives on the relevance of relationships over the inherent qualities of subjects permeated my thinking to a profound degree and provided a foundation for all my future work.

As important as the coursework for my development were lengthy discussions and debates with my fellow graduate students concerning issues in anthropology, psychology, and the social sciences in general. Even more valuable was the opportunity to serve as a research assistant to a remarkable series of established scholars. During my first year as a graduate student I was hired by sociologists William and Joan McCord to help with the analysis of a large data set that they acquired from the Cambridge-Somerville Youth Study, which had originally been conducted in the mid-1930s among 650 lower-class boys from schools in Cambridge and Somerville, Massachusetts. In 1948 and again in 1956, various aspects of their adult behavior, including crime rates, alcoholism, and mental illness, were analyzed. A vast array of data was also collected on family background and attitudes, including prejudice. My participation in analyzing the data resulted in junior authorship of three publications (see vitae). In my second year of graduate school I was fortunate enough to be hired at the recently founded Center for the Advanced Study in the Behavioral Sciences, which is adjacent to the Stanford campus, as a research assistant to James Coleman, who went on to become one of the foremost sociologists of his day. The following year I was research assistant to Israeli sociologist Joseph Ben David, who is best known for his work on the sociology of knowledge. Just being at the Center around some of the best minds in the social sciences, attending seminars, having lunch and coffee breaks and playing volleyball with them, was inspirational.

Equally if not more important was my association with Robert Scott, who was also a research assistant at the Center. He went on to be Professor of Sociology at Princeton University and spent the latter part of his career as Associate Director of the Center. Bob became one of my closest friends and our friendship has been life-long. We co-published four papers together following my return from doctoral field work, including a paper on stress that turned out to be quite influential in certain circles.

Also influential in shaping my future career was a summer I spent in Hawai‘i after my first year in graduate school. My intent was to find a suitable population for a master’s thesis. When in Honolulu I decided to focus on third-generation (Sansei) women, to whom I administered Thematic Apperception and Rorschach Tests. The result was a thesis entitled *The Hawaiian Sansei: A Problem in the Study of Psychological Acculturation,* which I completed in 1957. More importantly for my future career, my experience in Hawai‘i fostered a fascination with the Pacific Islands, which led to my choice of a Pacific field site for my doctoral research and eventually to the island of Rotuma.

Before I went to the field for my doctoral research, the anthropology department at Stanford hired Kimball Romney, who was one of the founders of cognitive anthropology, with which I became quite enthralled. I began exploring the componential analysis of kinship terms and other domains of culture that lent themselves to such analysis. I passed my comprehensive and oral exams without incident and prepared a research proposal, which focused on a cognitive (ethnoscience) analysis of ceremonies. For financial support I submitted a rather different proposal to the National Institutes of Mental Health (NIMH). Knowing that grief was a hot topic at the time, I proposed to study the ways in which grief was managed in a Pacific Island society. The proposal was successful and I was awarded some $7,000 for the study.

Getting to Rotuma

When I wrote the proposals it was my intention to go to the island of Ponape in Micronesia for my field work. I read everything I could find in the Stanford library concerning Ponape and felt quite well prepared for the study. However, my request to do field work on Ponape was denied by the U.S. administration in Micronesia. They informed me that there was already a resident anthropologist on the island (Jack Fisher) and that I would not be welcome there. Instead they proposed that I do my field work on Truk or Yap, neither of which appealed to me since Ward Goodenough had recently done a comprehensive study in Truk and David Schneider had recently done field work on Yap. So it was back to the drawing board.

My advisor at Stanford, Felix Keesing, suggested Samoa (where he had worked) and Fiji as possibilities, so I immediately wrote a letter of inquiry to Fiji, which was my preference. The Fiji colonial government promptly replied, informing me that enough anthropological research had already been done among the Fijians, and they suggested that I go to New Guinea. If I insisted on doing research in Fiji, however, they offered me the possibilities of working with Fiji Indians or going to the island of Rotuma.

[The rest of this section and the following section largely consist of extracts from a paper published in the journal *Pacific Studies* in 2004 titled "Contextualizing Histories: Our Rotuman Experience" that I co-authored with my wife, Jan Rensel.]

In truth, I had never heard of Rotuma, so I rushed to the Stanford library to look it up. In a 1930s edition of the *Pacific Island Handbook*, a one-page article began with a statement to the effect that "Rotuma is one of the most beautiful and romantic islands in the South Seas." I didn't have to read further to make up my mind. The article provided some basic information about the island's size and population, which suited my research plans. It also mentioned a nine-hole golf course and reported that several of the chiefs were adept at the game! Fortunately I did not bring my golf clubs along, for as I later discovered, the golf course, which had been laid out by a British resident commissioner in the 1930s, had long since been replaced by copra sheds.

I made all the necessary arrangements and left California in early October 1959. My first stop was Honolulu, where I spent several days at Bishop Museum trying to find out as much as I could about Rotuma. I also met with Alex Spoehr, the museum's director, who warmly encouraged my research. I arrived in Fiji on October 12 and on Spoehr's advice booked into the Korolevu Beach Hotel, on the road between Nadi and Suva. When I arrived in Suva the next day I got a rude shock. After my departure from California the colonial secretary of Fiji had sent a telegram to Stanford rescinding permission for me to do research on Rotuma because of recent problems on the island. A land commission had been charged with surveying and registering Rotuman lands, but because of a provision in the regulation establishing the commission—a provision that effectively converted Rotuman land tenure from one based on bilineal inheritance to one of patrilineality—the Rotuman people had rebelled and forced commission personnel to leave the island. A great deal of anger and hostility had been aroused, and the governor of Fiji, Sir Kenneth Maddocks, had decided that an anthropologist arriving in the aftermath of this fiasco might be identified as a government agent, further aggravating an already tense situation.

The governor was away attending a meeting of the South Pacific Commission in Noumea, New Caledonia, but the assistant colonial secretary, a Mr. Hill, was encouraging. He told me that he thought an anthropologist could play a constructive role in resolving Rotuma's land problems, so I did not lose hope. Mr. Hill suggested that I make an appointment with the governor following his return to present my case. I agreed, and feeling buoyed by the rapport I had already established with Rotumans I had met, decided to persevere. I knew that Alex Spoehr was one of the U.S. representatives to the South Pacific Commission and sent him an express letter in Noumea, explaining my situation and requesting that he speak to the governor on my behalf. I felt reassured by his return note, offering encouragement and informing me that he had talked with Sir Kenneth.

My interview with Governor Maddocks went well. He asked about my research plans and listened patiently. His only concern, he explained, was that nothing happen to further unsettle the people there. He said that he would cable the district officer on Rotuma, Fred Ieli, and have him ask the Rotuma Council to decide whether to let me come. This was a wise, practical solution and suited me very well. Rather than being "sent" to the island by the colonial administration, I would be "invited" by representatives of the Rotuman people—if they agreed. I was grateful and delighted when permission was finally granted, on November 13, a month after my arrival in Fiji. The governor's only stipulation was that I avoid inquiring into land matters or politics while on the island.

Excited about the prospect of going to Rotuma, I tried to make a reservation on the next boat, the *Kurimarau,* but was told that the few cabins aboard had already been booked. I insisted that didn't matter and that I would happily travel aboard deck, like most other passengers. "No," I was told emphatically, "Europeans are not allowed to travel on deck. It would set a bad example." Since I had already become aware of ethnic segregation in its many colonial forms, I was not surprised, but I found the situation extremely frustrating nonetheless. The next boat on which I was able to book a cabin was the *Yanawai,* a copra boat, which was scheduled to leave early in December. I decided to be philosophical about the delay and ended up using the time to advantage by immersing myself in government archives, learning what I could of the Rotuman language, and taking part in various activities with my newfound Rotuman friends.

As it turned out, the *Yanawai's* departure from Suva on December 11th was anything but routine. The day before the boat was scheduled to leave, Fiji's workers staged a strike that grew violent, causing much damage to downtown Suva and shutting down transportation. The city was tense, and getting to the ship meant navigating through dangerous territory. It was a great relief to watch the troubled city fade into the horizon as we sailed away. The trip to Rotuma took five days, with stops at Levuka, Savu Savu, Taveuni, and Rabi. We reached Rotuma at midnight, December 16, and anchored outside the reef off Motusa, but we did not go ashore by launch until shortly after dawn. I then met for the first time the family who would host me for the following year.

There were no public accommodations on Rotuma. Visitors were required to make arrangements in advance to stay with a host family. While on Viti Levu I had been given the option of having the district officer arrange something for me, which I declined in favor of an offer by Lisi, one of my Rotuman friends. She suggested that I stay with her uncle and aunt, Sakimi and Seferosa, in the district of Itu'muta, at the western end of the island. They had a large house, she said, and only one daughter, Akeneta. Lisi said she would talk to her uncle, who was visiting Suva at the time, and told me that since I planned to go to Rotuma on the same boat, I would meet him before my arrival on the island. As it turned out, Sakimi went on the *Kurimarau* and assumed, since I was not aboard, that I was not going to Rotuma after all. He had therefore made no arrangements to accommodate me and was not present when I eventually arrived and disembarked. I did not know this until many months later, however. What I experienced was an initial sense of confusion in which people asked with whom I would be staying, followed by a period of waiting around while being assured that a lorry was coming to take me and my luggage "home." Akeneta showed up after a little while and without hesitation welcomed me warmly. I cannot say enough about how wonderful this family was to me during my time on Rotuma, and how much they made me feel at home.

As I imagine to be the case with most field work in isolated communities, I experienced intense highs accompanied by periods of great productivity, and depressing lows when all I wanted to do was get away from everyone. When I watched the *Yanawai* depart, realizing the next boat would not likely come for three or four months, I was apprehensive and full of doubt. But it did not take me long to feel at home, which in retrospect is quite a testament to the tolerance and patience of my host family and of Rotumans in general. My adjustment was facilitated by the fact that most Rotumans spoke English, which, on the one hand, made it easier to begin substantive research, and on the other, served as a disincentive to learning the rather difficult Rotuman language. On balance my experience during that year was overwhelmingly positive, almost magical at times, as I gained new insights into cultural values and human relationships.

I tried to abide by the restriction placed on my research by the colonial administration—that I not inquire about land matters—but because the land commission's recent visit was uppermost in people's minds, they kept initiating the topic in their conversations with me. To my knowledge, few people thought I was a government agent, perhaps because more exotic rumors circulated—that I was a communist spy, or that I was a descendant of Charles Howard, a renegade English sailor who had married and made his home on Rotuma in the mid-nineteenth century. One of Howard's grandsons reputedly had left for America in the 1920s and had not been heard from since. "Is it true that Charlie Howard's grandson is your father?" I was asked several times. "Have you come to Rotuma to find your roots?" It was tempting to leave the answer ambiguous—Charles Howard had sired many children and being his descendant would have made me kin to more than half of the population— but I did my best to squelch both rumors.

People wanted to express their views about the land commission fiasco and wanted me to know why they were incensed—and in the process taught me a good deal about the nature of land tenure. I was therefore gratified, several months later, when Commissioner Eastern Christopher Legge (under whose jurisdiction Rotuma fell) asked me to include land tenure in my research

and to suggest ways to deal with the problems confronted by the commission. I was happy to oblige, and at the conclusion of my research produced a report pointing out the practical benefits of the current tenure system, which was remarkably flexible. My recommendation was that they avoid tampering with the rules of succession and encourage Rotumans to work out disputes informally as much as possible. Subsequently, I ended up doing my dissertation on land tenure (Howard 1962; also see Howard 1963, 1964a).

For my NIMH-funded study of bereavement behavior, I had hypothesized that a variety of cultural variables, and especially the centrality of elaborate funeral rituals, would mitigate the severity of bereavement problems. During that first year on Rotuma I had the opportunity to go to several funerals and was able to view grieving behavior first hand. I came to an understanding that I later presented in a paper, co-authored by Robert Scott, entitled "Cultural Values and Attitudes Towards Death" (Howard and Scott 1965). I was less successful in my attempt to apply an ethnoscientific approach to ceremonies, the research topic of my dissertation proposal. Although I went through the motions of doing such an analysis, the results were uninteresting and devoid of any significant insights into Rotuman culture.

My research on Rotuma was aided tremendously by two hardworking research assistants, Amai Sakami and Rejieli Mejieli. They conducted a comprehensive census of the island for me, recorded migration and marital histories, wrote down life histories, and collected data for a number of special surveys. (An indicator of Amai's abilities and his hardworking commitment to duty was his being awarded an MBE by Queen Elizabeth II in 1998 for his long service aboard cable ships.) In addition, my brother Irwin, who was an undergraduate at Reed College at the time, joined me on Rotuma during his summer vacation one year; he enjoyed the experience so much that he decided to take the following semester off to assist me.

I ended up collecting data on just about everything I could imagine that might help me to gain a comprehensive understanding of Rotuman culture. In addition to conducting a census of the entire island (with the assistance of Amai and Rejieli), I recorded genealogies; analyzed court cases concerning land disputes; administered thematic apperception tests and interpersonal relations questionnaires; and spent lots of time attending ceremonies of all kinds. In the final analysis, I think I learned more from just talking with and hanging out with people I came to feel comfortable with. However, my relationship with Rotumans at the time seems rather superficial compared to now. They came to recognize that I was a student interested in their customs, and for the most part they were appreciative and prepared to help. Still, I was cast in a rather restricted role, not only by them but also by my own obsession with collecting data.

Because I was categorized as an educated "European," people treated me with far more respect than I felt entitled to, which meant their dealings with me were mostly formal and restrained. At ceremonies and feasts I was seated with the chiefs and required to sit cross-legged for hours while being served enormous quantities of food and entertained by well-rehearsed dancers. Being cast into such a formalized role led me to understand the underlying rules of social relations in terms of restraint (a product of respect) and license (a product of intimacy) (see Howard 1964b for a formalization of this approach to social relationships).

As my preoccupation with abstract theory gave way to trying to understand topics such as land tenure and other customary behaviors, I began to think more and more in terms of discovering the cultural logic that informed Rotuman activities. This led me to see things as characterized by what I termed "activity systems" that were ordered by sets of "decision-making" principles. The neat thing about this approach is that it lent itself to numerical testing, for example with regard to the principles involved in the adoption of children (see Howard 1970), as well as to land tenure (Howard 1962, 1963). I later wrote an article, co-authored by Sutti Ortiz, an economic anthropologist at Case Western Reserve University, titled "Decision Making and the Study of Social Process," in which we discussed the advantages and limitations of decision analysis for social anthropology, examined the theoretical and methodological implications of "rationality," and presented a sequence of hypothetical steps that we regarded as requisites for an adequate decision-making analysis (Howard and Ortiz 1971).

During the year I was on Rotuma, I went through a series of phases concerning my understanding of the culture. At first everything seemed strange and undecipherable, particularly that part of the culture embedded in the Rotuman language, which I found difficult to learn. Then, after a couple of months, I found everything falling neatly into place—the result of a sweeping theory of the culture, devoid of any significant complexity. It didn't take long for that first set of insights to collapse, to be followed by periods of confusion, revisions involving somewhat more complex understandings, more confusion, more revisions, and so on throughout the year. By the time I left, I felt I understood the culture pretty well, but I realize now that the illusion of understanding is a difficult thing for an ethnographer to shed. After all, the quest for understanding is a major part of our motivation for doing field work, and to admit to ignorance or confusion is to risk being seen as a failure, by ourselves as well as by our colleagues.

Superficial as my understandings may have been, by the time I left the field I had collected an enormous quantity of data, most of it extremely well organized. I had made a point of not letting more than two or three days go by without taking time to type and organize my notes, using a modified version of G. P. Murdock et al.'s *Outline of Cultural Materials* (1945) as a guide. I also spent time writing down whatever interpretive insights I had. It took a lot of discipline to stick to this routine because it sometimes meant not attending events of considerable interest, but it turned out to be an excellent investment in the long run. My early organizing efforts not only made writing my dissertation a breeze (I finished it over a nine-month stretch while teaching fifteen hours per week of first-time courses in anthropology, sociology, the psychology of adjustment, and marriage and the family at Cabrillo Community College in northern California), but they served me well for years afterwards when opportunities arose to publish papers on a variety of topics that I had not anticipated writing about.

Returning from the Field: A Stressful Experience

My field trip on Rotuma lasted almost exactly one year, but with Irwin's help, I spent another six months collecting survey data, including household censuses, among Rotumans in Fiji. From the standpoint of theory, I would have to say that when I left the field I was committed to none in particular. In fact I felt that committing to a single theoretical perspective would limit my understandings, so I intellectually opened the door to any perspective that might shed light on a

given segment of Rotuman culture or behavior, and I was determined to entertain any viewpoint that would provide at least the illusion of understanding. However, theory was hardly ever on my mind; I was much more engaged in assembling and giving order to the voluminous data I had collected during my nearly two years of field work.

One of the experiences that fascinated me upon leaving leaving Rotuma for Suva, the capital and largest city in Fiji, was the increase in tension I felt. In the first few days I spent in Suva, the simple act of crossing a street caused me to tremble uncontrollably. It was as if my body, including my brain, had to process too much information to act comfortably. In Rotuma, time had slowed down dramatically and the amount of stimuli was substantially reduced. In urban Suva, time speeded up and the sheer degree of stimuli felt like a bombardment. After a while my body adjusted, but upon returning to the San Francisco Bay Area I experienced a similar response. These episodes led me to think deeply about the notion of stress. I related my experiences to my close friend from graduate school, Robert Scott, who was then a sociologist with the Russell Sage Foundation. He was currently engaged in studying issues associated with medical practices and found my initial thoughts concerning stress as the result of tensions produced by an overload of "problems" confronting an organism from multiple sources intriguing enough to suggest a collaborative project.

Bob was much more familiar with the current stress literature and took it on himself to bring some conceptual clarity to the sprawling body of research and writing that medical and social and behavioral scientists had conducted about stress, in which there seemed to be no common understanding of what it was or how to conceptualize it. We had worked on developing a model of stress while we were both in graduate school and during the time that Bob was a post-doc at Stanford. Our paper was far enough along in draft form for us to submit it to *Behavioral Science* in 1961, but for the longest time we heard nothing from the editors about it, because as it turned out, the noted psychiatrist, Franz Alexander, to whom it had been sent for peer review, had died while it languished on his desk. Finally, a few years later, we heard from someone on the editorial board apprising us that it had been accepted for publication and asking for revisions. To do so Bob needed to supplement his summer income. He was encouraged by his colleague, Dr. Lawrence Hinkle, a cardiologist at Cornell University Medical College, to submit a proposal to the Human Ecology Fund, a foundation headed by Dr. Harold Wolff, a renowned neurologist, whose expertise was migraine headaches and pain. The proposal was approved and we were provided with a modest sum of money to complete the project. The result was an article published in *Behavioral Science* in 1965 titled "A Proposed Framework for the Analysis of Stress in the Human Organism." Bob and I subsequently published a chapter on "Models of Stress" in a book edited by Sol Levine and Norman Scotch on *Social Stress*.

Unfortunately, we were unaware of the backstory of the Human Ecology Fund and its involvement with the CIA. It turns out that Drs. Hinkle and Wolff had been recruited by Allen Dulles to investigate and report on Communist brainwashing techniques in the 1950s, after which Wolff proposed a partnership with the CIA aimed at mastering the techniques for gaining control over human beings' thought patterns and behavior patterns. Wolff and Hinkle obtained permission from Cornell's president and high University officials to conduct experiments at Cornell on behalf of the CIA. They went on to test "secret drugs and various brain damaging

procedures" on unwitting patients (from *Alliance for Human Research Protection Newsletter* [https://ahrp.org/1953-dr-wolff-and-dr-hinkle-investigate-communist-brainwashing/].

Wolff went on to set up the Society for the Investigation of Human Ecology, a front group for the CIA, with himself as president, through which funds were funneled. Neither Bob nor I had any inclination that this was the case. We knew of Wolff as a distinguished scholar who had been president of the New York Neurological Association and president of the American Neurological Association, as well as editor-in-chief of the American Medical Association's *Archives of Neurology and Psychiatry*.

The whole issue came to light in a pair of papers by anthropologist David Price published in *Anthropology Today* in 2007. Price suggested that the CIA was shifting away from drugs and equipment during interrogations and toward coercive techniques based on intense stress or isolation to induce confessions, and for that reason they had been interested in our paper. We were horrified by the thought that our paper might have been used to develop torture techniques when our purpose was exactly the opposite, to suggest ways of alleviating stress. In truth, we did not suggest anything specifically new regarding stress-inducing conditions; rather, we merely summarized the existing literature to show how our framework provided some sort of order to the variety of stress studies.

To his credit, Price informed us of his exposé in advance of publication and gave us an opportunity to confirm that we had been unaware of Wolff and Hinkles' involvement with the CIA. That gave me an opportunity to both condemn the use of torture under any conditions and for any purpose, and to reflect on the usage of scientific knowledge. In a response to Price I wrote:

> I could liken our situation to the discovery of the potential of splitting atoms for the release of massive amounts of energy. That knowledge can be used to create energy sources to support the finest human endeavors or to make atomic bombs. Unfortunately, such is the potential of most forms of human knowledge; it can be used for good or evil. While there is no simple solution to this dilemma, it is imperative that scientists of every ilk demand transparency in the funding of research and open access to information. The bad guys will, of course, opt for deception whenever it suits their purposes, and we cannot control that, but exposing such deceptions, as you have so ably done, is vitally important.

Bob and I also co-authored an article regarding cultural values and attitudes toward death based on my Rotuman research regarding grieving, which was published in the *Journal of Existentialism* in 1965.

Getting to Know Academia

I returned to the United States (California) in August 1961 and within weeks applied for and was accepted for the faculty of Cabrillo Community College, which had opened in 1959 and was still in temporary facilities located in Watsonville, California. I was one of the few social scientists

on the faculty and was assigned to teach classes in sociology, anthropology, psychology, and marriage and the family. In addition to day classes, I taught sociology at night to adult members of the community. I didn't find that schedule particularly stressful, in large measure because it gave me the opportunity to reflect on how many of the issues I had to teach about were relevant to the Rotuman context. To put this in other terms, it provided a context for considering my Rotuman data within a comparative framework. I still marvel, however, at the fact that mostly over the Christmas vacation I was able to complete my dissertation and hand it in for my Stanford degree by the end of the year.

As satisfying as my experience at Cabrillo was, I could not pass up an opportunity to take a temporary position at Auckland University in New Zealand the following year. I had met Murray Groves, who was on the faculty at Auckland University, when he visited Fiji during my time there. Murray was the editor of the *Journal of the Polynesian Society (JPS)* at the time, and we hit it off pretty well. I had been reflecting on Rotuma's association with Fiji and had recently come across an article concerning hinterland communities. I found it an appealing way of conceptualizing Rotuma's relationship with Fiji that included flows of people, goods, and information in both directions. Using this as a framework I submitted an article to *JPS* and received an immediate acceptance from Murray. It also turned out that he was about to go on sabbatical leave and recommended me as his temporary replacement at Auckland. The opportunity to be at a university focused on the Pacific Islands was too compelling to turn down.

I really enjoyed my year in Auckland and getting to know the faculty there. Ralph Piddington was head of the Department at the time, but he was away on sabbatical and Bruce Biggs was acting head. Also on the faculty were Ralph Bulmer, Andy Pawley, Jeremy Beckett, and Roger Green, all of whom became quick friends. My teaching duties were quite limited, giving me plenty of time to work up my Rotuman research materials for publication, and by the end of the year I had five articles submitted for publication, including two on land tenure (one using a decision-making framework, the other a historical analysis of change); a paper on "Conservatism and Non-Traditional Leadership in Rotuma"; an article co-authored by my brother, Irwin, on "Pre-marital Sex and Social Control among the Rotumans"; and an article spelling out my "activity systems approach" in "On the Structural Analysis of Interpersonal Relations."

Before the year ran out, I needed to get a job back in the United States, but I decided beforehand that Hawai'i was the only place in the U.S. where I wanted to live, now that I had been thoroughly "Pacificalized." So I wrote to both the Department of Anthropology at the University of Hawai'i and the Bishop Museum to the effect that I was coming to Hawai'i no matter what, but that they should hire me because I was "a damn good anthropologist." I was awarded with a joint appointment, with my primary base at the Museum.

Research in an Hawaiian-American Community

Bishop Museum was a comfortable place for me. I had a large office and amiable companions, including archaeologists Kenneth Emory and Yoshi Sinoto, Adrienne Kaeppler, and several visiting scholars, including ethnobotanist Douglas Yen and archaeologist Roger Green. Pat Kirch, who was a high school student at the time, was a volunteer in the Department. The

Director of the Museum was Roland Force, who thankfully had a broad vision of what kinds of projects were appropriate for a museum to undertake. This included a willingness to take on a project requested by the Liliuokalani Trust, a private organization dedicated to the welfare of Native Hawaiian children. The Trust, under the direction of Myron Thompson, had conducted a survey of Native Hawaiians and found them overrepresented in virtually all categories of "social problems" and underrepresented in socioeconomic indicators of success. It was clear from the report that, statistically speaking at least, contemporary Hawaiian-Americans had become another "deprived" American minority group.

Thompson proposed that the Museum undertake a research project aimed at revealing the underlying causes of such problems with the hope that it would suggest possible resolutions. Since I was the lone social anthropologist employed by the Museum at the time, the Director asked me to take on the project. I was happy to do so insofar as such a study was the natural culmination of an interest in Polynesians, in the effects of modernization and urbanization on indigenous peoples, and in human responses to environmental stress. Hawaiian-Americans appeared to present an ideal population to indulge these three interests at once. Initial contacts with Hawaiian-Americans provided further inducement, for it was immediately apparent that these are a people whose reputation for generosity and aloha is justly deserved. It was indeed a rewarding experience to work with them.

Several research strategies were considered, but it was finally decided that the best results could be achieved by selecting one community and studying it intensively over a period of time. This study would then serve as a basis for topically oriented research over a wider sample of the Hawaiian-American population. Nanakuli,[1] a Hawaiian homestead community located on the leeward side of O'ahu, approximately thirty miles from Honolulu, was chosen as an ideal site for the study. The community had been described as "socially disorganized" by several agencies, and statistics showed an unusual incidence of police action, health and educational problems, and social dependency. There were also reports of political factionalism and social fragmentation, such that community cooperation was lacking and social control weak The major task of the research, as initially conceived, was to analyze the cultural, social, and psychological contexts within which the behaviors of interest to social action agencies took place. My vision of the project required multiple personnel with diverse skills, so I submitted an ambitious three-year proposal to the National Institutes of Mental Health to conduct the research.

Fortunately, NIMH fully funded the project and we began working on it in earnest during the summer of 1965. The grant allowed us to fund a number of individuals who became key contributors to the research. Our first task was to collect and organize published and unpublished materials related to Hawaiian culture and the behavior of modern Hawaiian-Americans. The sources were reproduced, coded, and filed so as to provide ready retrieval. Information was also obtained from the Hawaiian Homes Commission on families living in the community, and welfare records were examined in order to ascertain the frequency and intensity of various social problems.

[1] In publications we used the pseudonym 'Aina Pumehana (Warm-hearted Land).

The grant provided funds not only for hiring graduate students as research assistants, but also two senior collaborators: Ron Gallimore, a behavioral psychologist, and Steve Boggs, my esteemed teacher in graduate school at Stanford. At the time, Steve held the position of executive secretary of the American Anthropological Association, based Washington D.C. He resigned that position to join our research team and subsequently was hired as a professor in the Department of Anthropology at the University of Hawai'i. He and his wife, JoAn, were major contributors to the project, both as interviewers and interpreters of the ethnographic material we accumulated.

In July 1965, my Thai wife, Kajorn Lekhakul, and I moved to Waianae, a community a few miles from the homestead in order to become participant observers. Since neither of us was of Hawaiian ancestry, we were not eligible to reside on the homestead itself, but we wanted to be close enough to participate maximally in their community affairs. Our move took place at about the time efforts were being made to form a Community Action Program Council under the provisions of the Economic Opportunity Act that had recently been funded by the federal government. Attending these organizational meetings provided an opportunity to become acquainted with many of the community's leading personalities as well as a chance to observe decision-making behavior on political issues.

It did not take long for us to make friends, and within weeks we were regularly attending the meetings of a number of formal organizations. We became full-fledged members of several of them in due course. Invitations to parties and lu'aus (ceremonial feasts) poured in, and within a few months our major problem shifted from trying to establish rapport to trying to regulate heavy social demands. We remained involved at high intensity for three years, terminating our residence in the area in August 1968.

During this time we had opportunities to see a core group of people in a large number of social situations. We went "slumming" with them to Honolulu nightclubs, shared rooms at hotels during club conventions, and participated in work parties organized to raise funds for one cause or another. My wife Kajorn was especially successful in working with the women and was elected to office in two organizations. We bowled regularly in a league, and I played golf for one of the local clubs. We found the entire experience pleasurable and came to cherish our friendships with many of the individuals we "worked" with. Some we came to consider among our closest friends and we took every opportunity to get together with them.

But we had our problems too. At a time when political issues connected with the Economic Opportunity Act, and later with the Model Cities Program, brought all the latent community factionalism to the surface, it was inevitable that we would get caught up in it to some extent. Even though we avoided taking sides in factional disputes, we were involved and cared greatly about the fate of the community. There were actions that we were able to take, such as aiding in the improvement of health and educational facilities, and we took them. But these, too, were political issues and elicited criticism from some members of the community. Also, there were some people in the community whom we simply did not like, and who did not like us. We made no effort to be emotion-free scientific instruments; we were human beings living in a community with which we identified quite strongly. Our object was to understand the way of life in which we had become immersed.

Initially, we conceived the research as taking place in two phases. The first, which we originally spoke of as "ethnographic" or "clinical," centered on participant observation and informal interviewing as primary data gathering techniques. The idea was to understand the behavior of community members from their own point of view, rather than from that of middle-class agency officials. We anticipated that during this phase hypotheses would be generated that could be systematically tested in the second phase. Accordingly, for three months in the summer of 1966 five graduate students from the University of Hawai'i Anthropology Department were engaged to do an "ethnography" of two families each. Their instructions were simply to record and analyze the social, cultural, and psychological worlds within which their informants lived. The topics covered included kinship and family relations, employment and economics, religion, health practices and beliefs, ethnic attitudes, dietary habits, and child-rearing techniques. It was expected that from this intensive research hypotheses would be suggested to account for observed similarities and differences in behavioral styles, and that these could be tested more rigorously at a later stage in the study.

The second phase was to involve formal interviews, pencil-and-paper personality measures, and social psychological experiments; data so gathered would be used to test hypotheses generated during the first phase. In order to avoid confounding the two phases of research, we divided the 394 households in the homestead into four random samples of approximately one hundred each. One of these was set aside as an "ethnographic sample," and it was from this group that households were selected for interviewing during the first phase. A second "uncontaminated" sample was to be used for testing the hypotheses generated by interviewing the ethnographic sample. The purpose of this strategy was to avoid testing hypotheses with the same population that suggested them, a frequent shortcoming in social anthropology.

Neat as this plan looked on paper, it did not really work to our satisfaction. Aside from practical considerations (for example, it was not possible to completely avoid "contamination," since persons from each sample were represented in the organizations with which we had continuous contact; some in each sample became close friends, and so forth), we were overly optimistic about the kinds of hypotheses we could generate from participant observation and open-ended interviews. Rather than propose specific hypotheses in order to remain true to the research design, or delay the second phase indefinitely, we adopted two basic "salvage" strategies. The first was to take a rather simple hypothesis concerning young children's behavior and to test it experimentally, with the hope of either verifying, clarifying, or reformulating it. From there we could move on to new experiments involving sounder and more complex hypotheses. The plan was to run a series of experiments with each design dependent on the results of previous ones. This strategy also called for using progressively older subjects so that clarification would be rendered not only to our theoretical and conceptual formulations, but to the development of behavioral forms from childhood through adolescence as well. We were quite gratified with the results of this stratagem. It led to a close and extremely rich collaboration between anthropologist (myself) and psychologist (Ronald Gallimore), and forced each of us to question basic assumptions in his discipline.

The second stratagem was to proceed with structured interviewing of members of households in the least contaminated sample, from a somewhat different viewpoint than originally intended. Instead of selecting and designing instruments to verify well-formulated hypotheses (which we

did not really have), we opted for instruments that would produce reasonable quantities of data in areas that had become foci of attention. The purpose was to saturate these areas with enough data to provide sufficient materials to test hypotheses as they emerged from further field work, from the social psychological experiments, more intensive analysis of unstructured interview material, and so forth. It was with this in mind that we unleashed a team of interviewers on ʻAina Pumehana during the summer of 1967. Of the 98 households in the sample chosen, interviews were obtained from members of 88 of them. Interviewers were instructed to obtain information and administer pencil-and-paper tests to every available adult in each sample household.

Included in the interview package were instruments pertaining to a number of areas, including: (1) genealogies, (2) demographic history of the household, (3) social relations with friends, relatives, and organizations outside the household, (4) role relations within the household, (5) child-rearing attitudes and behavior, (6) diet and health, (7) employment and economic management, (8) exposure to and identification with particular cultural traditions, (9) attitudes, and (10) strategy orientations (that is, the inclination to favor certain behavioral strategies over others). Data were also obtained on men's drinking behavior and conflict within the household.

The interview took well over an hour and we were concerned at first that it might be too lengthy and that many people in the homestead would balk at the time it took and lose interest, but we found the reverse to be true. We generally found that interviewees were more than happy to express their views on the topics presented and were exceptionally candid about matters that would be questionable subjects in other ethnic populations.

Our original plan was to treat these data statistically, using correlations, tests of significance, and the like, in order to determine the credibility of our hypotheses. However, since we found ourselves without well-formulated hypotheses we faced something of a dilemma. Our options appeared to be threefold: (1) we could hold the data in abeyance and work with other information until we were able to formulate more specific hypotheses, thus relegating the interview material to reservoir status; (2) we could subject the data to computerized analysis in the hopes of discovering statistically significant findings about which we could theorize; or (3) we could drop the attempt to derive statistically significant findings in favor of using the materials to clarify and sharpen our ethnographic observations. The third option amounted to using the quantitative material as an extension of our ethnographic operation rather than as a separate operation aimed at validation. We chose the third option on the grounds that our foremost goal was an ethnographic one—to generate a theory of Hawaiian-American behavior patterns. With this as the prime goal, we were less concerned with tests of credibility provided by statistical procedures than with the credibility of findings reflected against our three years of participant observation. Rationale for such a procedure was provided by Glaser and Strauss in their provocative 1967 book, *The Discovery of Grounded Theory*. The book attacks over-concentration in sociology on rigorous verification procedures and advocates a less rigid, more creative, approach oriented toward generating theory. It was an approach well-suited to my ethnographic sensibilities.

To conceptualize this approach more anthropologically, I conceived of the purpose of ethnographic research as to arrive at an understanding of the "cultural logic" that serves as a template underlying the behavior, beliefs, choices, etc. of a study population. It is an approach that has guided virtually all of my subsequent research.

The NIMH grant was renewed for an additional two years (1968–70) for the purpose of analyzing our data and writing up the results for publication. We managed to produce quite a number of publications, including my book *Ain't No Big Thing: Coping Strategies in a Hawaiian-American Community* (1974). In addition, I co-authored multiple articles in collaboration with Gallimore and others, several of which were in a 1968 volume we co-edited that also contains multiple articles by other project participants.

Back to the Rotuman Data

Even during my deep involvement in the Hawaiian project I continued to work organizing and analyzing my Rotuman data. One major body of information I collected on Rotuma, with the help of my brother Irwin, was all the birth, death, and marriage records between 1903 and 1960[2], the year of my residence on the island. I had generously been given a box of 10,000 blank 3" x 5" sheets of paper by Dr. Lindsay Verrier, a British physician who had lived in Fiji for some 30 years and had collected vast amounts of such demographic data in the role of medical demographer. Each slip of paper was used to record a birth, death, or marriage from relevant registries.

The task I undertook was to organize the births by mothers on to 4" by 6" sheets of paper so that each record consisted of a sibling set. I added the names of fathers to each sub-set of siblings if the mother had more than one mate. I then added marriage dates and death dates to each person where available. This generated 1,315 records and took hundreds if not thousands of hours of tedious searching, but was enormously satisfying, given my obsessive-compulsive tendencies. I furthermore added pertinent information to the cards I had made up for households from my census of the island in 1960. Having done all the collating, the next step was to codify the information so that it could be subjected to various forms of analysis without relying on tedious counts by hand. This was still pre-computer days, so my best option was to assign numerical codes to the data and have it punched on to IBM cards, which could be sorted using machines built for that purpose. This was a really tedious process and turned out not to be that much help, so I ended up setting the project aside until magnetic tapes were devised that could analyze the data on institutional-sized computers, but I still didn't do much work with it. However, in 1970 I was awarded a three-year grant from the National Science Foundation for the "Study of Demographic Approaches to Social Behavior on Rotuma."

This foray into demography had some far-reaching consequences. For one, because I was more interested in working with my Rotuman data than cataloguing collections of artifacts, I decided to leave my position at Bishop Museum and applied for a position in the Anthropology Department at the University of Hawai'i at Manoa. I also expressed a desire to affiliate with the Population Institute at the East-West Center to get assistance analyzing my Rotuma demographic data. As a result, in 1970, I was hired by the University half-time as a full professor and half-time by the Population Institute as a Research Associate.

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[2] The registers actually went back to 1881, the year of cession, but from then through 1902, only one name was listed for each entry, making linking records a near impossibility.

Much of the work of codifying my demographic data and subjecting it to preliminary analysis was done at the Institute. However I also got involved with several other projects there. For example, I was able to analyze some of the data from the Nanakuli project and in 1971 published a monograph titled *Households, Families and Friends in a Hawaiian-American Community* under sponsorship of the Institute. The director of the Institute, Paul Demeny, also authorized the publication of my book on the Hawaiian research, *Ain't No Big Thing,* under the Institute's auspices. In addition I worked with my Institute colleagues on a volume concerning *Population Research in Thailand: A Review and Bibliography* and published a few papers concerning populations topics, including, by title, "Population Education Report" (1974); "Demographic Socialization: Direct and Indirect" (1976); and "Anthropological Perspectives on Population Growth" (1976). My ephemeral reputation as a population anthropologist also led to an appointment on a National Institutes of Health (NIH) panel in Washington D.C. evaluating population grant proposals, and two summer appointments as the leader of population workshops for anthropologists at the University of North Carolina.

I left the Population Institute in 1975 to go full-time in the Anthropology Department at UH, but I continued to work with my Rotuman demographic data, which thankfully I was able to transfer from the tapes into a database application on my first Macintosh and subsequent computers. In this form I was able to make ready use of it in a number of ways. For one thing, although I never advertised to Rotumans my ability to construct genealogies from the data, several Rotuman friends asked me if I had any information regarding their family tree, and I was usually able to go back 3 or 4 generations—even more if I was able to hook it up with the genealogies taken by A.M. Hocart during his visit to Rotuma in 1913. The demographic data also informed a number of articles, most notably in two papers regarding the measles epidemic that struck Rotuma in 1911 that I co-authored in 2011 and 2014 with G. Dennis Shanks and other epidemiologists.

Becoming a Polynesianist

Although the uniqueness of the Rotuman language precluded Rotuman culture from being considered Polynesian in earlier classifications, it was entirely clear to me that Rotuma is Polynesian in all its basic cultural components, and for this reason I would classify myself as a Polynesianist. In pursuit of this identity, I have authored and edited many items concerned with Polynesia as a culture area, including its prehistory. For example, while at Bishop Museum I wrote an a lengthy article (with the assistance of graduate students Richard Bodman, who translated the relevant literature in the French language, and Cedric Kam, who translated items written in German) reviewing theories of Polynesian origins and migrations; I also co-edited the book *Polynesian Culture History* (1967) in which that article appeared. Also regarding traditional Polynesia, I published a review article concerning the prehistory of Polynesia (1979), and an article critiquing the approaches to Polynesian social stratification of Marshall Sahlins and Irving Goldman (1972).

Aside from these forays into Polynesian prehistory, in 1971 I edited a volume of articles entitled *Polynesia: Readings on a Culture Area,* and in 1989 I co-edited a volume with Rob Borofsky on *Developments in Polynesian Ethnology* that included a chapter, which I co-authored with John

Kirkpatrick, on Polynesian social organization. Borofsky and I coauthored the introduction to the volume as well as a chapter on the Early Contact Period and a concluding chapter speculating on the future of Polynesian ethnology. Regarding more contemporary aspects of Polynesian societies, I published an article regarding "Recent Trends in Polynesian Social Anthropology" (1969) and another titled "Polynesia and Micronesia in Psychiatric Perspective" (Bob Levy was originally asked to write the article by the editor of *Transcultural Psychiatric Research Review,* but he was otherwise occupied and suggested me as a substitute). And although my main ethnographic concerns have continued to be with the Rotuman population, both on the home island and abroad, I also contributed to research concerning the health and adaptations of modern Samoans in collaboration with Michael Hanna, Paul and Thelma Baker, and Maureen Fitzgerald, including two chapters in the book, *The Changing Samoans: Behavior and Health in Transition* (1986), edited by Paul Baker and Michael Hanna, and an article co-authored with Fitzgerald regarding "Aspects of Social Organization in Three Samoan Communities "(1990).

Critiquing Ethnocentrism

My interest in psychological anthropology continued throughout my career, although it morphed away from its psychoanalytic origins toward a more eclectic approach. A tell-tale incident in this regard was a paper I wrote exploring the relevance of Interactional Psychology—which focuses on persons-in-situations rather than on personality characteristics per se—for anthropology. In it, I argued that the interactionist framework fit well with then-current trends in anthropology that emphasized the contextualization of behavior and intracultural diversity. That paper, by the way, illustrates precisely what Levy was talking about when he said that I entertained ideas rather than "had" them. In that piece I was playing with an idea to see how cultural phenomena might look through its lens. I submitted it to the journal *Ethos*, which is associated with the Society for Psychological Anthropology, where it was rejected, primarily on the basis of an extremely negative review by someone who was clearly biased in favor of psychodynamic approaches. Undeterred, I submitted the article to the *American Anthropologist*, arguably a more prestigious journal, where it was published without revisions in 1982. (Ironically, *Ethos* currently advertises itself as "an interdisciplinary and international quarterly journal devoted to scholarly articles dealing with the interrelationships between the individual and the sociocultural milieu, between the psychological disciplines and the social disciplines. The journal publishes work from a wide spectrum of research perspectives.")

My problem with using psychiatric approaches, including psychoanalysis, to understanding human behavior, was that I found them quite ethnocentric—based on a presumed "normality" associated with proprietary Western middle-class cultural norms. The results, all too frequently in my opinion, led to the characterization of cultural differences in terms of the deficiencies of various ethnic groups—of their deviations from those norms. But the problem of ethnocentrism was not confined to psychiatric approaches; indeed, I found it to be inherent in much of the social science literature at the time.

The first opportunity I had to express my dissatisfaction with such approaches was in an article published in *Comparative Studies in Society and History* reviewing three books published in the 1970s: Ronald P. Rohner's *They Love Me, They Love Me Not: A Worldwide Study of the Effects*

of Parental Acceptance and Rejection; Hyman Rodman's *Lower-Class Families: The Culture of Poverty in Negro Trinidad*; and Charles Hampden-Turner's *From Poverty to Dignity: A Strategy for Poor Americans*. The title of my article, "An Arsenal of Words: Social Science and Its Victims" (1978), set the tone. After reviewing the contents of each book, I concluded by drawing attention to three issues that were raised by the authors' analyses: universalism versus particularism, objectivity versus subjectivity, and pathology. Regarding the issue of universalism versus particularism, my point was that when applying generalizations about human behavior to particular cases, distortions are likely to be introduced to the extent that context is ignored, which can deprive people of their humanity, reduce them to objects, and provide the rationale for their manipulation. This can result in a social science that can be used against the people we portray, regardless of our sympathies. Regarding the issue of objectivity versus subjectivity, I argued that what is important is that we do not remove from consideration those subjective components of a people's lifeways that are essential to personhood. This is not to deny the value of an objective approach, I argued, since the very essence of objectivity is the capacity to alter perspectives as the particular cases under observation require, to admit data for review that had not been anticipated, and to accept the tentative nature of one's conclusions. And regarding pathology I argued against the prevailing notion that patterns of undesirable behavior can be explained by character defects located within individuals. I argued, following Gregory Bateson, that pathology must be located not within individual actors but in the relationships that generate and support the behavior patterns involved.

Along the same lines, Bob Scott and I contributed a chapter to the *Handbook of Cross-Cultural Human Development* (1981), edited by R. H. Munroe, R. L. Munroe and B. B. Whiting, in which we argued that a strong middle-class bias in social science has resulted in a body of research findings that focuses on alleged deficiencies in minority groups, and that this has led to faulty understandings and weak theory. We proposed that the purposes of social science would be much better served by "naturalistic" studies focusing on the prevailing characteristics within minority groups than on presumed deficits. We identified two main types of deficiency formulations. One such formulation focuses on attributes socially valued by mainstream groups that are observed to be absent or weakly represented within particular minorities, resulting in their being described as "disorganized," "normless," and "unstable," while individuals are described as "ego deficient," "immature," or "lacking in motivation," "self-control," "the ability to delay gratification," and so on. The second type focuses on attributes socially devalued by mainstream culture resulting in such descriptions of families as "matricentric" and individuals as "prone to violence," "hedonistic," or "present oriented." We ended the chapter by advocating a "naturalistic" approach to the study of particular populations, by which we were referring to a methodology that aimed to generate concepts with a high density of information about the people being described so as to reconstruct social reality from their perspective—which generally involves employing concepts used by the people themselves.

Since this was a pet concern of mine, I was delighted when I was invited to write an epilogue to a volume resulting from a series of sessions at the Association for Social Anthropology (ASAO) annual meetings focusing on "ethnopsychology." (Shelly Rosaldo had been a discussant in those sessions and had been expected to write the epilogue, but unfortunately she died in an accident while doing field work in the Philippines prior to the concluding session.) In the epilogue I noted (with pleasure) that the common ground uniting the contributions to the volume was

> a shared dissatisfaction with Western psychology as pretender to a universal analytical framework for personal experience. To begin with, several of the participants explicitly questioned the conceptualization of personhood in Western psychology, with its strong emphasis on individualism, that is, on isolating the individual as the basic unit of analysis. Dramatizing the deficiencies of this approach stood as one of the more important contributions of the volume. By describing "folk theories" of human conduct in a variety of settings, even though limited to one geographical region, the authors documented the wide array of alternatives available for categorizing human experience and for making sense of it. In particular, they demonstrated the necessity for framing such efforts in their appropriate cultural contexts.

I took the opportunity to discuss a number of issues inherent in the notion of ethnopsychology, but the chapter gave me a fine opportunity to express my dissatisfaction with Western psychologizing and to champion anthropological efforts to introduce culturally sensitive approaches to psychological theorizing. The book, *Ethnopsychology and the Prospects for a Cultural Psychology,* was published in 1985.

Forays into Hypermedia and Website Construction

Another area of interest that captured my imagination was the development of hypermedia and the Internet in latter part of the twentieth century. I bought the sixth Macintosh computer sold in Honolulu in 1983 and immediately started to experiment with Hypercard as an application for storing and linking ethnographic information. In 1988 I published an article in the third volume of *Cultural Anthropology* entitled "Hypermedia and the Future of Ethnography" in which I explored the possibilities for using hypermedia to construct much more complex, interactive ethnographies. (The editor of the journal, George Marcus, didn't bother to send the article out for peer review and exercised his right as editor to accept it without modification.)

In 1996 I created the Rotuma Website (www.rotuma.net), which was intended as a vehicle for the increasingly diasporic Rotuman community to keep informed about any news of concern to Rotumans (including from news media), to make announcements regarding events, and to share contact information with one another. On the site I also provided background information regarding the culture, history, language and other aspects of Rotuma, including access to many publications on various topics. Prior to the development of Facebook and other social media, it provided the main link for members of the Rotuman diaspora to communicate with one another. I continue to manage the site, which now consists of more than 16,000 pages, and keep it up to date. The website has also served as a means of conducting surveys among diasporic Rotumans.

One of the most rewarding aspects of managing the Rotuma Website was my association with Major-General Jioje Konrote. While he was commanding officer of the United Nations peacekeeping forces in the Middle East, he regularly sent me reports of his activities to post on the site. I remember on one occasion he sent me an apologetic email for not sending a report because he had a meeting scheduled with Boutros Boutros-Ghali, then secretary-general of the

United Nations. That such a meeting should have priority was quite easy for me to accept. Konrote, whose father I had befriended during my initial fieldwork, went on to become Fiji's high commissioner to Australia from 2001 to 2006 and to serve as President of Fiji from 2015 to 2021. His and his wife Sarote's friendship has been very precious to Jan and me.

Creating and managing websites became something of a hobby for me. In addition to the Rotuma Website, I created or redesigned websites for the UH Anthropology Department, and then for the departments of Political Science and Civil and Environmental Engineering as well as for other organizations and individuals. In total I have created or redesigned sixteen different websites over the years.

Publishing Results of My Doctoral Research

However, over the course of my career my primary concern has been with the ethnography of Rotumans on the island and in diaspora. Following my 1959–61 field work on Rotuma and among Rotumans in Fiji, I published a book, *Learning to Be Rotuman (*1970) and papers and book chapters on a wide range of topics. The book was the result of a meeting I had with Sol Kimball at an AAA conference. He was looking for ethnographic accounts of education in non-Western societies, and since I had acquired a good deal of information from the schools in Rotuma, I volunteered to produce a volume. The book was quite successful, selling more than 5,000 copies. It got mixed reviews, the most damning by Richard Shweder, then a graduate student at Harvard, who accused me of "Bongo-bongoism and intellectual arrogance." It so happened that a few years later I read a couple of articles by Shweder that I thought were particularly insightful and wrote him something of a fan letter, not realizing he had authored that negative review. Not long after, Rick was up for promotion at his university and named me as one of his referees, at which point I remembered who he was. I wrote him a strongly supportive letter that included a caveat that I was clearly not biased in his favor since he had accused me in print of bongo-bongoism and intellectual arrogance, so it might be intellectually arrogant of me to write such a strongly supportive letter for him. The committee evidently relayed that part of my letter to him, because when we met in person sometime later we had a good laugh about it, and he apologized for his graduate-student overzealousness to write a critical review.

In addition to *Learning to Be Rotuman*, in the years between 1961 and 1990 I wrote 14 journal articles and 7 book chapters regarding Rotuman topics, including politics (4), land tenure (2), relations with Fiji (2), kinship (1), pre-marital sex (1), death and grieving (1) adoption (1), education (1), traditional medicine (1), culture change (1), adaptation abroad (1), history (1), myth (1), field work (1), dispute management (1), and custom (1).

Major Influences

Four main influences on my work during this period are worthy of mention. One was my participation in the Association for Social Anthropology in Oceania (ASAO), of which I was a founding member in the late 1960s; the others were my friendship with Bob Levy, exposure to the writings of Gregory Bateson and personal interactions with him, and my association with Marshall Sahlins during the times he spent in Hawai'i over the course of several summers and a sabbatical year.

The great thing about ASAO is that the format of its meetings initiated topics for comparative analysis that in several cases stimulated me to write articles on topics I had not previously considered, but about which I had a rich body of data at my disposal. The first ASAO symposium, in the late 1960s, resulted in a volume edited by Vern Carroll on *Adoption in Eastern Oceania* (1970), to which I contributed two chapters, one about (informal) adoption in Rotuma, the other about the practice among Hawaiian-Americans. A subsequent symposium resulted in a book regarding *Exiles and Migrants in Oceania* (1977), edited by Mike Leiber, to which I contributed a chapter regarding the genesis of a distinctive Rotuman identity as a result of the migration of Rotumans to Fiji where they encountered a social milieu based on ethnic differentiation. A third instance resulted from some ASAO sessions regarding field work experiences of note, which led to a volume entitled *The Humbled Anthropologist: Tales from the Pacific* (1989), edited by Phil DeVita. My contribution to that volume concerned the way I had been taken aback when the first Rotumans I met turned out to be far more worldly and sophisticated than I anticipated. To this day, ASAO continues to be a major source of inspiration for me to write and publish. Those early instances were just the beginning, but the post-1990 phase included a change in my life circumstances that warrants separate consideration and that I will discuss later (see the section "Back to the Field").

In addition to my participation over the years as an organizer or co-organizer of ASAO sessions, as a contributor or co-contributor of papers, and as a discussant for several symposia, I served as program organizer of the second ASAO meeting in 1973, and on the Board of Directors for two different terms.

I was fortunate enough to become friends with Bob Levy early in my career, when I started work at Bishop Museum and the University of Hawai'i. Bob was a Fellow at the East-West Center, in the process of analyzing his Tahitian material; I was grappling with my Rotuman field notes. I found great joy discussing theoretical issues with Bob. His insights into Polynesian culture in general, and Tahitian culture in particular, were so keen, so penetrating, that they forced me to rethink every assumption I had ever made. These sessions, which took place in a variety of settings—on beaches, in lounges, during long walks, over chessboards, on visits to France and Switzerland—meant much more to me than the intellectual stimulation they provided. Bob made doing anthropology fun. A major reason I was attracted to the discipline in the first place was because of the myriad puzzles it presented; working them through with Bob made the pursuit of answers a positively delightful activity. I honestly believe that it was the power of his stimulation that sustained me through the early part of my career. Quite frankly, my enthusiasm for anthropology waned at times, but after each opportunity I had to spend time with Bob, I came away reinvigorated.

We only collaborated on one project, but it was an important one for me. Jeannette Mageo, an anthropologist who had done her field work in Samoa, suggested to me that we co-organize an ASAO session on spirits in Polynesian settings. I agreed and suggested Bob as a discussant. We went through what came to be the usual ASAO three-year cycle of annual sessions developing ever-more polished papers, with Bob providing a continual stream of insights that shaped the direction of our final output, a book entitled *Spirits in Culture, History, and Mind* (1996). The three of us co-authored a chapter in the book theorizing about the relationships between "Gods,

Spirits, and History," which was largely Bob's brainchild. I also contributed a chapter regarding "Discourse and Belief in Spirits on Rotuma" that included an extended discussion of the nature of "belief."

My exposure to Gregory Bateson began with a seminar at Stanford that he offered while I was a graduate student. I remember giving a presentation analyzing a professional wrestling match I had seen on TV from a psychoanalytic point of view (I was still in my psychoanalytic phase at the time). As well one can imagine if you are familiar with Bateson's perspective, my presentation didn't go down well with him, and he subjected it to a devastating critique that stimulated me to carefully read his writings, which I found compelling, and which contributed decisively to my abandonment of psychodynamic analysis. I had further exposure to Bateson in the late 1960s in Hawai'i, where Vern Carroll, who was in the process of organizing a volume of Bateson's writings, held a weekly meeting with a small group of us, including Bateson. It provided a great venue for exploring ideas, and again I found his critiques compelling, even when they were aimed at undermining perspectives I had been exploring. The most important thing I learned from him was that the "atomic" view of human beings—seeing them as isolates whose behaviors could be understood in terms of their traits, beliefs, attitudes, or personalities—was faulty, and that a proper perspective required an understanding of the relationships in which individuals were embedded. That perspective informed the critiques of social science that I referred to previously, and it remains at the heart of all my subsequent research, even though I may not have done it justice in every instance.

My association with Marshall Sahlins was result of his multiple visits to Hawai'i during the 1970s and 80s in his pursuit of an ethno-history of the Hawaiian people. We got to spend a good deal of time together, on campus, in restaurants, and on the Pali Golf Course. I think it's fair to say that Marshall did more to set the modern theoretical agenda for social anthropology in Oceania than anyone else. He was largely responsible for bringing history back into the theoretical spotlight after it had been left to languish by social and cultural anthropologists in the region. This I found quite compelling and almost everything I wrote about Rotuma subsequently includes historical background or is outright history. It also set me off on an obsessive-compulsive quest to scour libraries and archives in the Pacific region (including Fiji, Australia, and New Zealand) and Europe (mostly in England and Italy) for historical information regarding Rotuma. I also was privileged to witness firsthand Marshall's transformation from an evolutionary materialist to a symbolic culturalist. In early 1976 he asked me if I would read a manuscript he had recently completed and was planning to send in for publication. I warned him that I wouldn't be hesitant to be critical if I felt it was flawed (I had been quite critical of his *Social Stratification in Polynesia* in my 1972 review article, referred to previously), but he gave me license to do so. In fact I found the manuscript thoroughly compelling and told him so. It was subsequently published as *Culture and Practical Reason* (1976). A review by Robert McKinley stated the significance of the book eloquently:

> The main thrust of this book is to deliver a major critique of materialist and rationalist explanations of social and cultural forms, but the in the process Sahlins has given us a much stronger statement of the centrality of symbols in human affairs than have many of our "practicing" symbolic anthropologists. He demonstrates that symbols enter all phases of social life: those which we tend to

> regard as strictly pragmatic, or based on concerns with material need or advantage, as well as those which we tend to view as purely symbolic, such as ideology, ritual, myth, moral codes, and the like.

In 1981, when Marshall was in Honolulu on sabbatical, he and I co-organized a seminar on Polynesian chieftainship that was attended by a distinguished group of scholars including Greg Denning, Doug Oliver, and Adrienne Kaeppler, among others. At the time Marshall was deeply involved in analyzing myths, a product of the influence of structuralist Levi-Strauss when Marshall had spent a couple of years in France as part of his Vietnam war protest in the 1960s. I had not paid much attention to Rotuman myths up to this point, but the challenge of analyzing them afforded by the seminar led me to publish two papers inspired by French structuralism. One, written during the seminar, was titled "History, Myth and Polynesian Chieftainship: The Case of Rotuman Kings" (1985). The other was "Cannibal Chiefs and the Charter for Rebellion in Rotuman Myth" (1986). I subsequently published additional articles that were based on the analysis of symbolism and metaphors, including "Symbols of Power and the Politics of Impotence: The Mölmahao Rebellion on Rotuma" (1992), and three others co-authored by my wife, Jan Rensel: "Animals as Metaphors in Rotuman Sayings" (1991), "Rotuma: Interpreting a Wedding" (1994), and "Rotuman Culture as Reflected in Its Proverbs" (1998).

The French Connection

The cannibal chiefs article turned out to be most auspicious. After distinguished French anthropologist Maurice Godelier read it, he invited me (along with Jan) in 1992 to participate in a session he was conducting regarding bodily symbolism. We spent a glorious week in Paris and presented a co-authored paper, in English, that was titled "Only Skin Deep: Social Order and the Body on Rotuma." The paper was translated into French and published as "Une Profondeur qui S'arrête à la Surface de la Peau: Ordre Social et Corps à Rotuma" in the book *La Production du Corps* (1998). Thus began a most rewarding relationship with French anthropologists. We were invited back to France in 1999 for a month and split our time between Paris and Marseille, where we were engaged in activities at L'ecole des hautes etudes en sciences sociales (EHESS). It was a wonderful opportunity for us to get to know some French colleagues who have done research in Polynesia, and to familiarize ourselves with their publications. Our French was rather rudimentary, but just about all of them were fluent in English. The following year we were invited to participate in a conference in Paris regarding the Christianization of Oceanic societies. Jan and I presented a paper on "The Recent Rotuman Experience with Christianity," which included an account of the recent introduction of several new Christian denominations to Rotuma (e.g., Assembly of God, Jehovah's Witnesses, Seventh Day Adventists) and its implications for social life on the island. The paper wasn't published but is available on my personal website <alanhowardanthro.net>.

Our involvement with European colleagues was greatly enhanced by our participation in the European Society for Oceanists (ESfO). I was asked to be one of four keynote speakers at the initial meeting of the organization at Nijmegen, The Netherlands, in 1992. My speech was published in *the Journal of the Royal Institute of Linguistics and Anthropology* in 1994 as "History in Polynesia: Changing Perspectives and Current Issues." Jan and I participated in a

number of subsequent ESfO meetings in Leiden (1999), Vienna (2002), Marseille (2005), Verona (2008), St. Andrews (2010), and Brussels (2015), during which we engaged with many impressive scholars, several of whom became close friends.

Back to the Field

To backtrack: The decade of the 1980s was a time of great changes in my personal life that spilled over into my professional career. In January 1983 my wife Kajorn was charged and convicted of embezzling substantial sums of money from the East-West Center, where she was employed as an event coordinator. Upon investigation it became clear that she had engaged in a prolonged period of deception and lying to me about her activities, which led me to file for divorce. In June that year my mother died suddenly, compounding a depressive mood, and leaving me with little enthusiasm for anthropological projects. However, at the time I was serving as chair of our anthropology department's graduate program and was chair of our admissions committee. I was particularly impressed with an applicant by the name of Janet Rensel, a mature woman in her early thirties with a history of working in environmental education. She also had the highest GRE scores we had ever gotten from an applicant. Jan came to see me about our M.A. program in May and entered it in August. I hadn't thought much about a new relationship after my divorce from Kajorn, but I really enjoyed talking to Jan and found her increasingly attractive. We began a clandestine romantic relationship (clandestine due to the general taboo on professor–student romances) that lasted for two years before we married in 1985. Jan had completed her M.A. degree in cultural anthropology and originally had no plans for continuing on for a PhD. However, in 1987 I had a six-month sabbatical leave and we decided to spend it touring the South Pacific: Tahiti, the Cook Islands, New Zealand, Australia, and Fiji, including a two-week stay in Rotuma. It was the first time I had been back to the island in twenty-seven years. (It had been impractical to go while I was teaching because transportation by boat was too erratic so I couldn't count on being back on time—it wasn't until 1981 that an airstrip was built and weekly flights were scheduled— but even then flights were frequently cancelled for one reason or another.)

The trip revived the feelings of love for the island and its people that I had experienced on my first visit. To reintroduce myself to the various communities we visited, I brought along a bundle of 8" x 10" black-and-white photos I had taken of people in 1960 and distributed them to their descendants, kinsmen, village mates, and in some cases, to the people in the photo. It was a wonderful way re-establish rapport and make new friends. Most of the people I had been close to in 1960 had either emigrated to Fiji or had died in the interim, but I was able to reconnect with a few who remained.

I was especially pleased that Jan seemed to be as enchanted with Rotuma as I was, so I posed to her the possibility of her doing doctoral research there, and was delighted that she responded with unbounded enthusiasm. Thus began a research partnership that has been spectacularly rewarding for both of us. It turned out that we have complementary research skills; I tend to see broad patterns and she is incredibly skilled at seeing details and how they fit, or do not fit, those patterns. It also helps that she is a trained editor, which has made writing articles and books a genuine pleasure.

We returned to Rotuma in 1988 during my summer break from UH, which gave Jan a chance to gain permission for her research and to test some of the research instruments (mostly questionnaires) that she had developed. We settled on the northeast side of the island in the district of Oinafa, where she would eventually conduct here doctoral research, and were hosted and assisted by Tarterani Rigamoto, grandson of the revered district chief, Tokaniua, who had passed away in the interim between my visits. While Jan focused on her intended project, I engaged in participant observation with particular attention to the changes in customs and activities that had occurred in the interim since my initial visit.

Researching Rotuma as a Duo

The following year, in 1989, Jan applied for and received a Fulbright Grant to spend the better part of six months in Rotuma doing her research. This gave us an opportunity to get more deeply involved in activities, especially in the district of Oinafa where we paid Tarterani to refit an abandoned house on his property for us to live in. We furnished it with a double bed, a modern toilet and shower, a gas stove and kerosene refrigerator, and a work table and shelves, so we were very comfortable. The house was only a few yards from the beach, so we were lulled to sleep each night by listening to the waves.

We had brought with us two computers—an early desktop Macintosh and a Toshiba laptop—as well as a small Honda 110 volt generator, a flexible solar panel and deep cycle marine battery, and a Tripp Lite voltage regulator. Everything worked to perfection. The solar setup kept the battery nearly fully charged regardless of the weather (although most days were quite sunny), and kept the lights and a fan always in working order, and we used the generator to run the Mac. I published a paper in the *American Anthropology Newsletter*, "The Solar-Powered Anthropologist" (1991), reporting about our experience.

The core of Jan's research was a survey of the daily interactions among 17 households in Oinafa over a 13-week period. It involved responses to a Household Daily Activities Questionnaire (which had been translated into Rotuman). She personally collected the forms and spent a few hours every day reviewing and comparing the forms with each other and with records she collected from the local co-op, the bank, and the church, as well as her own observations of people's activities. Where inconsistencies arose, she interviewed household members to clear up anomalies. Households were given a weekly small gift of $5 Fijian for their participation but it was probably unnecessary. Not only did the participants frequently thank Jan for the work she was doing for Rotuma, but they also started giving her gifts of food. So Jan began to bake breads and cakes, often using the pumpkins that were growing prolifically on the land adjacent to our house, and brought them to the participating families in return.

As part of Jan's study, we engaged fourteen teachers during a school break to conduct a household survey of households on the island. The information collected overlapped to a considerable degree with the census data from 1960, which allowed us to detail some important changes that had taken place in the interim. But in addition, the 1989 survey included information about absentee household members, remittances, household inventories, and

household histories. We used the comparative data in a number of subsequent publications, especially those dealing with culture change on the island. Jan's dissertation, "For Love or Money: Interhousehold Exchange and the Economy of Rotuma," earned her a PhD in 1994. As for me, I took advantage of the time we were on the island during 1989 to participate in as many activities as possible, to take detailed notes on what I observed, and to interview willing individuals about their experiences, beliefs, and a range of topics of interest.

We returned to Rotuma during the University's summer break in 1990, for one week in 1991, for two weeks in 1994, for a month in 1996, for two weeks in 1998, for two weeks in 2001, for one week in 2004, for two weeks in 2012, and for two weeks in 2019. These multiple return visits, on top of my year on the island in 1960, have given us a long-term view of how things have changed, and continue to change, on the island. What we have observed from year-to-year has convinced me that a single visit, even for a year or two, is inadequate for understanding cultural dynamics in an ever-changing world.

To sum up my notion of change on Rotuma over the past 60 years that I have been observing it, I have used the concept of complexification, in reference to fact that there has been a continual process of adding more options, more contingencies, more coping strategies, and a wider range of cultural models available to Rotumans than in the past, rather than one cultural model simply replacing a previous one.

The Changing Nature of Field Work

Our repeated visits to Rotuma has led us to contemplate the nature of field work, particularly the implications of long-term field work for ethnography. One manifestation of this concern was a special issues of *Pacific Studies* (2004), co-edited by John Barker and me. In addition to co-authoring the Introduction, Jan and I contributed an article detailing our experiences over the years and the ways in which our positions as researchers were transformed.

In later visits to Rotuma we didn't do any formal research projects but simply participated in various events, visited with people who had become our friends, gave invited talks to students at Rotuma High School, and just went about the business of daily living. There were several implications of our acceptance into the community that had profound implications for our role as anthropologists. For one thing, as we formed friendships with certain individuals and families, we were drawn into their networks of friends and adversaries. This had the advantage of including us in the rumor-mill as well as less public events that occupy the concerns of most Rotumans on a daily basis. The disadvantage of this massive flow of information is that it made it difficult for us to formulate generalizations about Rotuman culture or behavior patterns, because for any generalization that one might care to make, there were too many exceptions that we knew about. This made us pay more attention to intracultural variability, and to count cases whenever possible while searching for patterns.

Another drawback to our increased personal involvement was that it was impossible to remain neutral when conflicts developed involving the families of our friends. Our loyalty was expected and failure to show it risked a breach in the relationship. One quite dramatic incident, which

turned out to be transformative, occurred toward the end of our 1990 visit. At the time we were still being hosted by the Rigamoto family in Oinafa and had established strong ties to the family. However, we had also developed reciprocal friendships with a number of other households, which posed a problem for our host family. From their point of view we were members of their household and were expected to order our relationships to mirror theirs. Unfortunately they were involved in disputes with a number of families and were unsettled by our friendly relations with them. What made the situation more dire was the fact that Tarterani's father-in-law, Kausiriaf, who was Oinafa's district chief, had become increasingly unpopular in the years prior to our visit. The conflicts he had with families in the village affected our host family's relationships as well. Though little was said, we could sense the tensions and noticed a distinct cooling of our relationship. The situation came to a head when members of Kausiriaf's kin group, who were responsible for his selection as chief, became disillusioned with his leadership and voted to depose him and select a new chief. Their decision was approved of by 80% of the households in our village. Although Kausiriaf refused to accept their decision, the group leaders proceeded with their plans to install a new chief. They recruited from Fiji the brother of one of the key disputants and persuaded him to return to Rotuma to take on the responsibilities of chieftainship.

The last straw for Tarterani was our attendance at the installation ceremony of Poar, the man selected to replace Kausiriaf. We had never before witnessed a chiefly installation and were delighted at the opportunity to document it, but to Tarterani this was an act of betrayal. Realizing on our way home from the ceremony that Tarterani was going to be incensed, I went to visit a Methodist catechist neighbor who had managed to stay neutral in the dispute to discuss our intention to make a formal apology (requiring a roasted pig, ceremonially presented) to Tarterani for any distress we had caused his family, but while I was there, Tarterani burst into our cottage and told Jan that he wanted us out of the house immediately. She pleaded with him, explaining that we were scheduled to leave the island in ten days, but he was adamant. I tried talking to him, but to no avail. Although it was already late afternoon, he demanded that we be gone by nightfall.

What happened next astonished us. In a matter of minutes, as word of our plight spread around the village, people began to show up to console us, to propose alternate accommodations, and to offer assistance in moving. Before long, two trucks pulled up at our front door with almost all the young men from the village. They moved our furniture, appliances, and personal possessions onto the trucks while Tarterani looked on from his verandah. The men then proceeded to strip the solar power wiring from the house, and offered to pull out the louvered windows and iron roofing since we had paid for them too, but we requested that they not do so. The men expressed considerable anger at the way we were being treated, feeling it reflected badly on the whole village. One of the subchiefs later apologized for not coming to help: he told us he was so angry that if he had seen Tarterani he would not have been able to control himself.

Several people offered to buy things from us, knowing we now had no place of our own to put them. We offered the appliances, including our solar setup and a generator, to some of our friends at very nominal prices (to formalize the transfers), and we gave away the furniture (most of which Tarterani had built out of materials we had purchased). We spent the first night at the home of an outspoken critic of Kausiriaf, a man who was instrumental in trying to depose him. The following day, at our request, we were taken to the district of Itu'muta, where we had been

invited (prior to these events) to spend time with the family of Harieta and John Bennett (a Rotuman woman and her American husband from Rhode Island). It was a comfortable arrangement since John had built a small apartment with modern conveniences and a delightful verandah with a spectacular view. Many of the Oinafa villagers accompanied us on the truck. For me, it was like returning home, since I had lived in Itu'muta during my initial 1960 field trip.

The event was transformative insofar as it all but removed the last barriers to complete acceptance into the community. Until then we had been treated with a respectfulness that constituted something of a barrier to any kind of intimacy. After the incident, a number of people explicitly said to us, "Now you know what it's really like to be a Rotuman," implying that the facade of polite respect had been penetrated and they could no longer present themselves in a purely positive light. People began to tell us their own stories of fractured relationships within their families, spontaneously revealing intimate details they would never have exposed before. In addition, our stance of neutrality in the chiefly dispute had been shattered. We were now identified with one of the factions, in a strangely heroic-martyred way—and thus became insiders to that faction (fortunately a much larger one) and outcasts to the other. It was as if the frame for our relationships had changed from one of ethnicity and nationality to one of political allegiances within Rotuma.

We left Rotuma several days later with mixed emotions. Despite the hurt feelings and a sense of having failed in our roles as neutral anthropologists, we relished the new levels of intimacy that resulted. The somewhat idyllic perspective we had of Rotuma had been shaken, but at the same time we felt more emotionally involved. Our experiences had awakened intensified emotions that were previously muted, perhaps because we had been trying to get along with everyone equally well. Following the episode with Tarterani, and the growing intimacy that developed with others during our return visits, we were experiencing emotions much closer to those we experience with our own relatives—complex mixtures of love, joy, irritation, frustration, and so on. And as our relationships grew more complex, we felt our intellectualized understandings of Rotumans (as a category) give way to an affective interplay with specific individuals that depended on circumstances, events, gossip, and other contingencies.

This ultimately led us to abandon our analytical pursuit of the kind of systemic understanding that lies at the heart of scientific analysis. Instead, we began a highly contextualized, historically informed quest to comprehend events. The episode also motivated us to become closer to individuals whom we have come to like or love, without regard for what we might learn about Rotuman culture. The highlight of this increased intimacy was our eventual (informal) adoption of two of the offspring of Katerina and Aisea Aitu. Aisea was a brilliant Rotuman teacher who became a senator in Fiji's Parliament and a dear friend. When their daughter Yvonne enrolled in Brigham Young University in Hawai'i, he asked us to look after her, so we took on the role of her surrogate parents. A few years later, her brother Walter also came to O'ahu to attend BYUH, and we took on a similar role with him. After graduation, Yvonne married Peter Sunia-Mafileo, a Tongan from Southern California, and eventually they had a daughter, Elisapeti, who is for all practical purposes our grand-daughter. Walter married an American woman, Rachael, and they have three children who are our de facto grandchildren. The delight we get from these relationships is unbounded.

In 1991 we returned to Rotuma for a short visit, which had the effect of demonstrating to the people on the island that our previous misadventure had not deterred us from our commitment to the community. However, we did not return to Oinafa. Kausiriaf had won the legal battle; the high court in Fiji decided that under Fiji law only the prime minister could depose a Rotuman chief. Most of the people nonetheless supported Poar and refused to participate in activities presided over by Kausiriaf. Tensions remained high, with people from one faction shunning those on the other side.

Working with Elsapeti Inia: A True Cultural Savant

Finding the social atmosphere in Oinafa oppressive, we accepted the invitation of Elisapeti Inia to host us at her home in the village of Savlei, in Itu'ti'u district, on the southwest side of the island. We were also invited to stay with other friends and accepted some of their invitations, including an overnight stay with Poar in Oinafa. This further cemented our identification with his faction, and resulted in an exceptionally warm reception by his supporters. We repeated a similar pattern on subsequent visits to Rotuma.

I had first gotten to know Elsapeti in 1960 when she was a teacher at Rotuma High School where I spent quite a bit of time sitting in on classes. She was a brilliant teacher with high expectations from her students. I remember the words she had posted, in English, above the blackboard in front of the class: "None But the Hard Workers Deserve Success." We became quick friends and joked about being "blood brother and sister" in reference to our experience at a "picnic" involving a visit to the sacred islet of Hatana shortly after my arrival on Rotuma. The islet is surrounded by a fringing reef that keeps boats from landing, so we had to cross the reef by foot while waves were crashing all around us, thankfully assisted by some young men from the village of Losa. Unfortunately, I had no calluses on the bottoms of my feet and ended up spilling a fair amount of blood. Elsapeti also ended up bloodied; hence her reference to our "blood relationship."

Staying with Elisapeti was especially rewarding for us. She was clearly the most knowledgeable Rotuman alive regarding all manner of information regarding Rotuman culture. Whereas other Rotuman elders were knowledgeable about certain aspects of the culture, she had taken a scholarly approach to accumulating information about its every aspect, from language, to ceremonies, folklore, arts and crafts, etc. She also had memorized genealogies of a large segment of the population (in one visit to stay with us in Hawai'i, she was able to establish kinship relations among all the Rotumans living here).

She took on the role of "big sister" to us and became our teacher and mentor. Staying in her home also turned out to be a boon for my pet project at the time. I had begun working on a biography of her late husband, Wilson (Howard 1994), and had the opportunity to peruse Wilson's papers and to interview Elisapeti at length. Doing a biography seemed a natural outgrowth of the process that had engulfed us. It was an opportunity to focus on the life of a man I greatly admired, quite apart from the fact that he was a Rotuman. In a sense the project mirrored the shift in our orientation toward field work: culture was shunted to the background with individuals foregrounded. We were now more interested in coming to grips with how people managed their lives than with the culture that may have patterned their behavior.

We ended up working on several projects with Elsapeti through the 1990s. During this period she came to stay with us in Honolulu for some extended visits, and we stayed with her when we were in Rotuma. It was our mission to help her to write up and publish good portions of the cultural knowledge at her disposal. This resulted in a book of Rotuman sayings, *Fäeag 'es Fūaga (Rotuman Proverbs)* (1998) and a book detailing rituals, *Kato'aga: Rotuman Ceremonies* (2001). I might add that although we spent hundreds of hours working with Elsapeti on these productions, we did not feel entitled to claim co-authorship as we considered the information contained within to be *her* knowledge, not ours. Jan and I did, however, contribute an essay to her book on sayings titled "Rotuman Culture as Reflected in Its Proverbs."

Elsapeti was also a key contributor to *A New Rotuman Dictionary* (1998), along with German linguist Hans Schmidt, and another Rotuman friend Sofie Arnsten, who was married to a Norwegian. C. M. Churchward, a missionary-linguist, had published *Rotuman Grammar and Dictionary* in 1940, but the dictionary segment only had translations of Rotuman words into English without the reverse. So Jan and I took it upon ourselves to create a finder list of English words that, using Churchward's definitions, we translated into Rotuman. The fact that neither Jan nor I are schooled in linguistics led us to be rather timid regarding the project. We also knew that the language had changed in the half century since Churchward's publication, that he had failed to identify numerous flora and fauna, and that he had made some mistakes in translation. Fortunately, Hans had spent 1993 doing doctoral research on historical changes in the Rotuman language, so we offered to turn over our project to him and he graciously accepted the challenge.

Elsapeti's command of the nuances in the Rotuman language was exceptional; she had tutored many newcomers to the island, including Catholic priests, anthropologists (Jan and I), and linguists (Hans) in the intricacies of the language. (We were mediocre students and never learned to be fluent speakers, although we learned lots of words and can read Rotuman moderately well.) Sofie had an extraordinary gift for languages, not only being fluent in modern Norwegian, but having mastered Old Norse as well. Fortunately, Hans had the opportunity to consult extensively with both of them about problematic translations. Their many contributions warranted their being listed as primary authors of the wordlist, for it is primarily their knowledge that is reflected in it. Hans was the scholar who tapped and organized their knowledge; Jan and I were only the drones who made the information accessible by digitizing it.

Publications about Rotuma since 1989

Our publications regarding cultural phenomena on the island, and the ways it has been changing since my 1960 research, have continued till the present day. Since my return to Rotuma in 1987, I have written singly, or co-authored with Jan, articles or book chapters on politics (1989, 1992, 1994, 1996, 1997), dispute management (1990), culture change (1991, 1997), wedding symbolism (1994), Rotuman seafaring (1995), spiritual beliefs (1996), disabilities (1997), bodily symbolism (in French 1998), youth (1998), righting wrongs (2004), land issues (2011), photography (2913), the culture of graves (2016), and an article concerning a history of ethnographic research on Rotuma (2021). We also have written an article concerning the changing nature of old age on the island that has been accepted for publication as part of a special issue of the journal *Pacific Studies* in 2023.

All of these articles and book chapters have been subjected to peer review and published in professional journals and books. However, we have also written six conference papers, two reports, and nine other unpublished papers that are available on my personal website <alanhowardanthro.net> As for books I have written or edited, all but the last two were published by academic presses. *Island Legacy: A History of the Rotuman People*, which I wrote with Jan, was first published in 2007 by Trafford Press, a self-publishing organization. We had originally submitted it to the University of Hawai'i Press, but they would have required us to cut the text by one-third and severely limit the number of photos and illustrations, which we were unwilling to accept. *Island Legacy* has since been reprinted in 2022 by Pacific Studies Press in Suva, Fiji. Our most recent book, which Jan and I collated and edited, is a e-publication regarding *Rotuman Life Experiences 1890—1960*. It consists of two volumes and includes life histories of men and women on Rotuma in 1960. The life histories were mostly collected by my assistants at the time, Amai Sakimi and Rejieli Mejieli.

Researching the Rotuman Diaspora

As I became increasingly aware of the Rotuman diaspora, including the emigration to countries like New Zealand, Australia, the United Kingdom, and the United States, in addition to considerably numbers who moved to Fiji, the lure of studying the adaptation of Rotumans abroad became compelling. I had, of course, spent several months in 1959 and 1961 in Fiji, mostly in Suva, participating in activities within the Rotuman community there, and from 1987 onward Jan and I have spent a good deal of time in Fiji on our way to and from Rotuma. We therefore have gotten to know a lot about the Rotuman communities there. One thing that became clear from the research that my brother Irwin and I conducted in 1961 was that Rotumans were adapting extraordinarily well to the challenges of urban life. They had also developed a reputation for being hard and reliable workers and were disproportionately represented in managerial and professional occupations. I wrote about that in an article published in the journal *Human Organization* titled "Plasticity, Achievement and Adaptation in Developing Economies," (1966), in which I suggested some factors that might account for this phenomenon.

Following the completion of Jan's doctoral research in Rotuma, we decided to shift the focus of our research to Rotuman communities in diaspora. Thus, in 1994 when I was again on sabbatical, after spending some time in Lautoka and Suva in Fiji and in Rotuma, we traveled to Australia, where we spent a month, mostly in Sydney, and then to New Zealand where we spent an additional month in Auckland and Christchurch. In 1998, after a two-week stay in Rotuma, we again went to Australia, spending three weeks with the Rotuman communities in Sydney and Melbourne. Subsequently, in 2002, we spent a week in New Zealand, dividing our time between Auckland and Wellington. We returned to New Zealand (Wellington) and Australia (Brisbane) in 2008, and again in 2011, when we spent most of our time in Australia visiting Rotuman communities in Sydney, Melbourne, Canberra, Brisbane, and Perth; in 2019, we spent some time with Rotumans in Auckland and Wellington in New Zealand.

In addition to these research trips abroad, we have taken advantage of the opportunity to participate in "Rotuma Day" celebrations in Hawai'i, the U.S. mainland, and Canada. Rotuma

Day celebrates Rotuma's cession to Great Britain in 1881, and has become a cultural holiday everywhere there are Rotuman communities of sufficient size. The celebrations generally consist of Rotuman-style feasts, singing and dancing. In the larger communities such as the San Francisco Bay Area and Vancouver B.C. in Canada, a hundred or more people may attend, including many non-Rotumans who have befriended or married Rotumans.

During our trip to Rotuma, Fiji, Australia and New Zealand in 1994, several Rotumans informed us of relatives who lived in Hawai'i and provided us with their contact information. Soon after our return home, we decided to invite them all to our place for a get-together. We were surprised to hear that several of them didn't know each other before that. The event was the first step in forming a Rotuman community here on O'ahu, and on September 9th, 1995, the Rotuma Association of Hawai'i, O'ahu, was formed at the home of Paul and Maria Dumas in Kailua. Munue Tavo was elected President of the Association, and Pasepa Breckterfield was elected Secretary-Treasurer. The Association was formed with the purpose of promoting the continuation of the Rotuman language and customs among families here in Hawai'i, especially among the younger generation. Activities included celebrating Rotuma Day (May 13th), performing Rotuman dances on appropriate occasions, teaching Rotuman language and customs to the children of Association members, and sharing information, eating Rotuman dishes, and just having fun being together. We met monthly for potluck picnics and held occasional camping outings. I circulated a monthly newsletter that contained news items from the media and reports from various Rotuman communities around the world. The organization lasted though 2007, when it dissolved as a result of several families moving to the mainland and squabbles within the group regarding the use of funds (members had contributed regularly to a group fund on a regular basis). Since then, Jan and I have continued to meet with certain families now and then.

In addition to these face-to-face encounters, we have made use of Internet surveys to acquire information regarding the adaptation of Rotumans abroad.

In 2012, Matt Bray, a part-Rotuman in Canberra, Australia, initiated a survey regarding cultural and linguistic preservation in diasporic Rotuman communities. Announcements of the survey were posted on Facebook groups "Rotumans on Facebook," "Rotumans Overseas," "Rotuman Pride," and on the Bulletin Board page of the Rotuma Website. The purpose of the survey was described as "to take a global snapshot at the status of Rotuman language and cultural maintenance for those who've moved away from the island. Understanding the attitudes and behaviours of individuals, families and communities will give us a sense of how people feel about their Rotuman culture and what is being done to preserve it." The survey included 35 questions and was responded to with various degrees of completion by 236 participants, all of whom lived abroad (at the time, there was no Internet access on the home island). The survey included questions regarding sense of identity, use of the Rotuman language, engagement with Rotuman activities, and visits to Rotuma.

In July 2016, I initiated a survey regarding the significance of Facebook for Rotumans in diaspora. The survey yielded 186 responses. Questions were included concerning digital interactions with friends and relatives; sharing of news, photos and videos; use of the Rotuman language; and assessments of the impact of Facebook on people's life circumstances. The survey led me to the conclusion that Facebook has provided Rotumans scattered around the world the

wherewithal to communicate on a regular basis with one another, to share news and visuals, to express opinions on issues of common concern, and to enhance their sense of Rotumanness, which is the hallmark of grounded communities. Furthermore, enhanced communication has increased visits, reunions (often on the island of Rotuma), and the sharing and exchanging of physical resources transnationally. The results of the survey formed the foundation of my article in the journal *Diaspora* titled "Diaspora No More? The Role of Facebook in the Development of a Global Rotuman Community" (2019).

Another source of information regarding Rotumans abroad is a section of my Rotuma Website that I established as a forum where people could express their views on matters of concern. To date, fifty-two topical forums have been generated. They can be roughly grouped into four major categories: (1) political issues; (2) aspects of Rotuman identity; (3) the development of Rotuma; and (4) problems confronting Rotumans on the island.

Prior to the advent of Facebook, I had included on the website an interactive database that allowed users to fill out a form providing information about themselves so that friends and relatives who have lost track of one another could get back in contact. Users were able to provide information about their home district and village on Rotuma, their gender and age, and their parent's names, in addition to current location, and mailing and e-mail addresses. The option of providing additional information about oneself or one's family was also available. The format allowed individuals to modify and update data, and to search for others by using several different criteria. I deleted the register from the site because Facebook came to fulfill the same function more extensively.

Caroline Clark conducted an online survey of Rotumans who visited the Rotuma Website in 2005 for her master's thesis at the University of British Columbia. She reported that 90% of her 151 respondents "believe that the website works to preserve Rotuman culture," and that 100% "believe that the website creates and maintains a sense of community among the global Rotuman diaspora."

One other survey worth mentioning was conducted by Rotuma High School students in 2012 concerning the migration experiences of 90 return migrants. In August of that year we were honored guests at a Founder's Day celebration at the high school, based on my biography of Wilson Inia, the founder of the school. We were hosted by the principal, Perry Gabriel, and his wife, Siteri, with whom we stayed for two weeks. Principal Gabriel asked us to hold workshops for both teachers and students regarding how to conduct research, and we were more than glad to oblige. The main reason for the request was that doing a research project had been made a graduation requirement, but neither the faculty nor the students had much experience in this regard. Perry suggested that we work with the advanced students in Forms 3-6. We had already constructed a questionnaire for interviewing returnees, and we adapted it for survey purposes. We held a pre-interview workshop with the students, going over the questionnaire in detail to explain its purpose and to answer any questions they might have. They were instructed to select an interviewee who had been away from the island for several years before returning. Several teachers volunteered to be interviewed if a student could not find someone suitable. At the time we were participating in an ongoing ASAO session on return migration organized by John Taylor and Helen Lee, so we had a real stake in the results of the survey. Our contribution to the

resultant volume, *Mobilities of Return: Pacific Perspectives,* was a chapter on "The Rotuman Experience with Reverse Migration" (2017). Our acknowledgements in that paper include the names of all 90 students whose interviews we used.

Our writings about Rotumans abroad began in 1999 with my article "Pacific-Based Virtual Communities: Rotuma on the World Wide Web," in *The Contemporary Pacific*. We followed this in 2001 with "Where Has Rotuman Culture Gone? And What Is It Doing There?" in which we presented data in historical perspective on Rotuman communities in five different locations: Rotuma, Fiji, Australia, New Zealand, and Hawai'i. We examined the patterning effects in each location of four key variables, including (1) macrosocietal attitudes; (2) the nature of the migration flow; (3) the size of communities; and (4) differential socialization experiences. We concluded with reflections on the conceptual modifications needed to understand the contemporary Rotuman experience. In the introduction of the article, we discussed the concept of culture as it applies to contemporary communities.

One of the key issues we sought to illuminate was the question of identity among Rotumans and Part-Rotumans in diaspora. This was the central theme of "Rotuman Identity in the Electronic Age" (2004); "Issues of Concern to Rotumans Abroad: A View from the Rotuma Website" (2012); "Being Rotuman on the Internet" (2017); and the aforementioned "Diaspora No More? The Role of Facebook in the Development of a Global Rotuman Community" (2019). Our interest in the nature of Rotuman communities abroad is the central theme of "Rotumans in Australia and New Zealand: The Problem of Community Formation" (2014).

Reflections on My Research Experience

I very much enjoyed doing research. I think that I was fortunate to have chosen Polynesia as a culture area, for I have found both Rotumans and Hawaiians remarkably forthcoming in interviews and incredibly willing to accept a stranger into their midst, including an inquisitive anthropologist. My first research in Rotuma was positively transformative of my character. I landed on the island with a thoroughly American attitude toward life, being personally ambitious for achieving distinction in my chosen field, and was generally highly competitive in every endeavor I engaged in. Exposure to Rotuman culture, with its emphasis on humility, group activities, and the primacy of establishing and maintaining good relationships set me to questioning my previous values. After leaving the field I found myself increasingly gravitating toward a Rotuman outlook on life until, at the age of 40-something, I self-consciously decided to abandon my earlier ambitions and focus instead on the work itself. The audience I was writing for shifted from my anthropological colleagues to Rotumans. That meant avoiding jargon or engaging in esoteric debates in the field and instead trying to write as clearly as possible for a general audience, primarily Rotumans. The shift was easy because I valued clarity and came to love the process of crafting ethnographic papers, chapters, and books. I was no longer concerned about my colleagues' opinions of my work, which was quite freeing. My orientation turned more and more to Rotuman history and less and less to theoretical issues in anthropology. Ironically, I suppose, just about all the items I've submitted for publication have been accepted for publication.

Perhaps another consequence of my lack of a fixed theoretical viewpoint is that I have always enjoyed working with colleagues, regardless of their discipline. The result has been a large number of co-authored articles, books, and special issues of journals. I have co-authored 33 articles with fellow anthropologists, 8 with sociologists, 3 with a psychologist, and 2 with epidemiologists. Two of the books I have published were co-authored, and 8 of the edited volumes were co-edited. I really enjoy the intellectual exchanges involved in crafting a point of view acceptable to both me and my collaborators, as opposed to championing any particular point of view.

My Teaching Career at University of Hawai‘i

As I mentioned earlier, my first appointment at the University of Hawai‘i was part-time while I was based at Bishop Museum. When I left the Museum, in 1971, I took a split appointment between the UH Department of Anthropology and the East-West Center's Population Institute, and in 1976 I went full-time in anthropology as a UH professor.

I was not what I would call a charismatic teacher. From my observations, charismatic teachers generally champion a cause or popular point of view, such as ecological activism, and/or lend a theatrical flair to their teaching. I met neither criterion. I aspired to be neutral regarding controversial topics, and my lectures were free of dramatic or droll performances. But I do think I was a good teacher and did my best to make my lectures engaging and interesting enough to keep the attention of my students.

Like most of the department faculty, I taught a range of courses from lower-division undergraduate courses to graduate seminars. In the undergraduate courses I enjoyed lecturing and always diligently prepared my material. My general routine was to prepare an outline of my lecture to distribute to the class in order to help them follow what I would be talking about. During the latter years of my career, the university encouraged more direct engagement with students so I altered my classroom procedures accordingly. One of the courses I was regularly assigned to teach was the history of anthropology, an upper-division course that was essentially, as I taught it, a history of theory in anthropology. At first I taught it as a lecture course, but as time went on, both to keep myself from getting bored going over the same material year after year and to meet the prodding to actively engage the students, I began to role-play the theorists I was covering. I divided the class into groups who were to play the role of journalists, and for each lecture, one of the groups would be responsible for interviewing me in the roles of everyone from Edward Tylor to Marvin Harris, to Clifford Geertz, and even Margaret Mead. I did my best to respond to questions from the perspective of the theorists whose role I was in. It was challenging and required good preparation, but it was fun.

I pushed role playing to the limit in one of my introduction to cultural anthropology classes, which met twice weekly. It was a small class of twenty-some students, which made it manageable with the help of a teaching assistant (Denby Fawcett) and my wife Jan. I created a background scenario for the students in which they had been living on a Pacific island that was destroyed by a volcanic explosion. They had escaped by canoe and eventually landed on another island, which was quite large and contained two ethnographically documented tribal groups: the

Samai, a people known for being non-violent, and the Dani of New Guinea, who were chronically involved in tribal warfare. The students were required to read published accounts of each group since they would have to accommodate to each of them in their new environment. I created a map of the island showing the landscape, terrain, flora and fauna, and the locations of each tribe. The students' first task was to choose a place to settle. One class each week was dedicated to a group meeting at which they had to make decisions that confronted the group. The other class meeting focused on discussing assigned readings, which gave the students background on related topics from published ethnographic accounts.

To facilitate students' involvement in their roles, I asked them to create names for themselves, and invited them to select from a set of sounds, including clicks, that could be incorporated in their names. I also provided a set of related ancestors from whom they had to choose their descent. Once individuals had made their choices, we dedicated a class session for them to find out how they were related to each other based on these ancestral ties. We also had a session in which they were to choose which social values they wanted to emphasize among a range of possibilities. Once these basics were decided, I produced a weekly newsletter relating events that occurred affecting the community and required a course of action. For instance, in one newsletter I reported that some chickens had been stolen and they had to decide what to do about. If they required more information I would provide it.

It was apparent to me that the students became very much engaged in their roles, which came through profoundly in our last regular class. I selected a few students to play the role of sailors off a U.S. naval vessel who landed on the island and offered to transport the islanders to the United States where they could settle in a modern community with all its benefits. I was astonished to see how ardently the "islanders" defended their lifestyle and rejected the so-called benefits they were being offered. ("Social Security? But we already have social security--in our relationships with each other here!") In the end they unanimously rejected the offer. Our final gathering was an outdoor feast in which, at the students' request, I played the role of a high chief and conducted a ritual celebration. Exams were based on an Introduction to Anthropology textbook that emphasized environmental conditions in the shaping of culture. I think the course was very successful, but it was a lot more work than a normal lecture course; I couldn't have managed it without my two super-diligent assistants.

On the whole, I enjoyed teaching undergraduate courses quite a bit, although I noticed a trend over the years that played a significant role in my decision to retire at the age of sixty-five, in 1999. When I had begun teaching in the 1960s, the majority of undergraduates were full-time students who took anthropology as an elective out of genuine interest. However, over the years, an increasing proportion of students were working full or part time and only seemed to be interested in getting their degree in the easiest way possible. I found their attitudes discouraging and felt like only a small percentage of the students were actively engaged.

Graduate seminars were another story. I very much enjoyed discussing topics of concern with graduate students, and on occasion, faculty members who attended my seminars. My main seminar assignment was titled "Ethnology," although it actually was focused on the history of theory in anthropology. It was a required course for graduate students in cultural anthropology and I taught it annually for most of my tenure. The seminar covered the same ground as my

undergraduate course on the history of anthropology, but the students were required to read and report to the whole group on selected theorists whose contributions would then be discussed. I am gratified to know that a number of students from those classes have enjoyed distinguished careers in anthropology or related fields.

Three of my seminars stand out in my memory as especially exhilarating. One I've already mentioned—the one I co-organized with Marshall Sahlins. The other two also were attended by more faculty members than students. One on the ethnography of Samoa was attended by Paul and Thelma Baker and Mike Hanna, who were conducting research on the physical adaptation of Samoans at the time; John Mayer, who was teaching Samoan language classes; and a few Samoans from the community. The other seminar of note was on anthropology and history and was attended by historians Brij V. Lal and David Hanlon, archaeology graduate student Thegn Ladefoged, and a couple of other faculty members.

I also taught for a semester as visiting lecturer at the University of Copenhagen in 1969. It was a time when Marxism was popular among the students there, so I decided to offer them a critique, in which my main point was that Marxism was only one of several 19th century theoretical positions and that the world had changed significantly since then. So why buy so whole-heartedly into a passé theory when they should be spending their time formulating their own? It resulted in some lively discussions.

Also worthy of note was spending a semester-at-sea in 1982 during which we had stops for a few days as a time in Japan, Taiwan, Hong Kong, Thailand, India, Sri Lanka, Egypt, Israel, Greece, Morocco, Spain, ending up in Fort Lauderdale, Florida. I can't say that I enjoyed the time we were at sea when I was teaching (two courses: one on contemporary cultures, using examples and readings from the places we were visiting, and one on doing ethnography, in which the students were assigned small projects in each country). I didn't mind the teaching—the students were great—but on top of the fact that I'm subject to motion sickness, the deans assigned to the ship at that time made the social atmosphere as unpleasant as possible in the interest of combatting the reputation of the ship as a party at sea with little concern for academics. It only resulted in unnecessarily repressive measures on their part and defiance on the part of most of the students and some of the faculty, including me. I did enjoy our time in the ports, however, and found my encounters with the inhabitants of each country delightful for the most part.

In 1992 I was privileged to teach a Study Abroad semester in London. Aside from losing a good portion of our wardrobes when the host school's premises flooded, it was a marvelous experience for both Jan and me. There were only a handful of students from Hawai'i in my classes so the teaching burden was minimal and there were lots of extra-curricular activities, including group trips to interesting places like Stratford-on-Avon, Bath, and Stonehenge, as well as free tickets to plays and concerts. In contrast to their mainland counterparts in the study-abroad program that year, I found that the students from Hawai'i were remarkable in their easy grasp of cultural nuances and behavioral expectations.

I served on quite a number of master's and doctoral committees and generally enjoyed engaging students while they were writing theses or dissertations. But I had a reputation for being tough, mainly because I had high standards and was not reluctant to be critical, so a number of students

avoided putting me on their committees, even though from the standpoint of their topics I would have been an obvious choice.

As far as administrative positions were concerned, I successfully avoided ever being department chair during my tenure at the university. I wanted as much time as possible for research and writing and did not want to get involved in university politics. I did, however, agree to serve as chair of the graduate program in anthropology from 1976 to 1985, which was not overly demanding of my time and allowed me to help our graduate students resolve issues with the administration and to facilitate their progress toward their degrees.

Homage

As I think back on my career I realize how many people contributed substantially to whatever successes I have had. I can't possibly name all of them, nor can I do them sufficient justice, but I'll do my best to pay homage to those who stand out in my memory.

At Stanford three of my teachers played key roles in the trajectory of my career: One was **Steve Boggs,** who not only introduced me to the social sciences via sociology but also supported me for a scholarship to pursue graduate studies. As I reported above, I later recruited Steve and his wife JoAn to work on my Nanakuli study and we became the best of friends till their deaths, respectively, in 2022 and 2023. Another teacher at Stanford who affected my subsequent career was **George Spindler**, whose graduate seminar in psychological anthropology that I took as a senior convinced me that anthropology was the discipline I wanted to pursue. Finally, I took a course taught by **Felix Keesing** that was called "social anthropology" but was actually a history of socio-cultural anthropological theory. I really enjoyed the course and modeled my subsequent courses in the history of anthropology and my graduate seminar in ethnology on it.

But it was not only my teachers at Stanford who had a major impact on my development. My friendship with **Bob Scott**, who was a graduate student in sociology at the time and a fellow research assistant at the Center for Advanced Study in the Behavioral Sciences, had a profound affect on my development as a writer. At the time my knowledge of the social science literature was quite limited in comparison with Bob's. I had some promising ideas, but to get them published I needed to relate them to the relevant literature, and Bob was able to do that with a high degree of professionalism. We ended up publishing four very well received papers together. Bob taught at Princeton for several years before taking the post of assistant director at the Center for Advanced Study. He also became a life-long friend and we have spent a lot of time together away from academia.

As I reported above, getting to Rotuma in the first place was something of an ordeal, owing to the reluctance of the British colonial administration's to permit an anthropologist to go there in the wake of a land commission fiasco. I owe a great debt to **Alex Spoehr** for supporting me by persuading **Sir Kenneth Maddocks** to give me the opportunity. Sir Kenneth granted me permission provided it was okay with the Rotuma Council of Chiefs. The Council gave their permission following a communication from **Faga Hoeflich**, whose grandfather (FaFa) was Maraf Mamatuki, the fabled paramount chief of Rotuma at the time of cession. She was therefore highly respected by the current Rotuman chiefs. Her assurance that I was a respectful and

responsible person no doubt played a crucial role in gaining the chiefs' acceptance. I got to know Faga as a result of meeting **Alex Rae**, her brother, on my first night in Fiji at the Korolevu Beach Resort, where Alex was the personnel manager. He was an incredibly impressive man in his sixties—a paragon of a Polynesian chief with a remarkable, sophisticated intellect. He was well read in several fields and knew more about the history of boxing than one could imagine (he had been a boxer in his youth). He was gracious and supportive of my intentions (see Howard 1990b for a more detailed account of my meeting with Mr. Rae and his sister). In Suva, Faga invited me to stay with her and her lovely daughter, Liebling, who had won the first Miss Hibiscus competition in Fiji three years earlier. They introduced me to the Rotuman community in Suva from which I got my first taste of Rotuman culture. I wrote about my first encounter with them in "The First Rotumans" (1989).

I stayed in Fiji from October 12 until December 11, 1959, when I shipped out for Rotuma on the *Yanawai*. As it turned out, the two months I spent in Suva had been well spent and extremely productive. Not only did I get to know a good deal about Rotuman culture as it was practiced in and around Suva, but I had an opportunity to consult, and collect, a wide range of archival and other forms of documentation regarding Rotuman history. Several individuals were particularly generous in helping me. **Reverend Alan Tippett**, a Methodist missionary with an M.A. in history (he later got a PhD in anthropology at the University of Oregon), who was at the Davuilevu Mission at the time, offered to provide me with the mission documents regarding Rotuma that he had in the mission library. He even offered to have his secretary type up lengthy passages so that I could have copies. Equally obliging was **Ian Diamond,** who was in charge of the Fiji Archives, which had an extensive collection of documents concerning Rotuma. I spent considerable time in the archives sifting through documents and hand copying extensive sections that I regarded as particularly important. Ian willingly provided me with any duplicates he had on hand to take with me.

Two other individuals, both medical doctors, made significant contributions to my Rotuma research: **Dr. Lindsay Verrier** and **Dr. Humphrey Evans**. As I have already written, Dr. Verrier provided me with the equipment to record a vast amount of demographic data. Dr. Verrier also put me in touch with Dr. Evans, who had served as District Officer on Rotuma for about four years during two terms in the 1940s and 50s. He was an amateur ethnographer and photographer and had collected a wide range of data, focused mainly on the material culture and chieftainship, supplemented by photographs, charts, and diagrams. He kindly had his Rotuma photographs reproduced for me and provided me with a wealth of documents to examine and copy.

One significant outcome of the information I was exposed to in Fiji was a developing passion for learning about Rotuman history, which became a prominent theme in my future work. It led to later visits to a number of libraries and archives in search of historical records regarding Rotuma, including the Mitchell Library in Sydney, the Turnbull Library in Wellington, the British Museum and Colonial Archives in London, the Pitt-Rivers Museum in Oxford, the Marist Archives in Rome, and various sources on the Internet. Not only did I co-author with Jan a book on Rotuman history, *Island Legacy: A History of the Rotuman People* (2007), but many of my articles have a historical perspective, regardless of topic.

During my year in Rotuma, from December 17, 1959, to December 14, 1960, many people facilitated my research and befriended me, but a few stand out as especially important. First of all there is the family of **Sakimi Farpapau**, his wife **Seforosa**, and daughter **Akeneta**. They hosted me in the most gracious way possible for the entire year. Sakimi was the epitome of the ideal Rotuman man, and although I wasn't able to communicate with him very well because he spoke no English and I never became fluent in Rotuman, his demeanor and interactions with others taught me a lesson in what Rotuman culture is all about. Akeneta was fluent in English and acted as interpreter within the household and, on occasion, during various events. She was exceptionally well versed in Rotuman custom and later taught it at Rotuma High School, so she was a valuable resource when I needed clarification of some aspect of Rotuman culture. Akeneta had adopted a two-year old namesake of her father, Sakimi mea mea (little Sakimi), whom we met again as an adult on my return to the island in 1987. Sakimi and his wife Sarote have unfailingly hosted us for full-day visits on each of our subsequent visits.

At this point I find it necessary to offer a caveat regarding my religious orientation as a secular humanist. Although I myself am not religious, I have always respected the religious beliefs and practices of others, and have enjoyed the incredibly welcoming friendships of a number of priests, nuns, and ministers. In Rotuma that first year, the family I stayed with was Catholic, one of a few in a predominantly Methodist district. I went to mass with them on occasion, and sometimes went to a Methodist service, but for the most part spent Sundays working or engaged in other activities. However, my friendship with **Father Beattie** at Upu Mission, and **Fathers Maguire** and **Kitoleilei** at Sumi Mission were unbelievably rewarding. Upu Mission was within comfortable walking distance from my home at Lopo, in Itu'muta. Father Beattie's residence was on the isthmus between Motusa and Itu'muta and enjoyed cool breezes most of the time. I was overjoyed when he offered to provide me with a desk in his house to use as an office. The desk was in part of the house that was screened in on both sides, letting the breeze pass through and keeping out the pesky flies and mosquitos. I made it a practice to take at least a couple of days a week to write up my notes and speculations about how to interpret the information I was collecting. As a bonus, I joined Father Beattie for regular morning and afternoon tea breaks served by resident nuns. These tea breaks provided a great opportunity to discuss his experiences in Rotuma and a wide range of worldly issues. I really enjoyed his company. To sweeten the deal, so to speak, his mother regularly sent him tins of cookies from his home in New Zealand, and since he was somewhat of an ascetic, he was reluctant to eat any but asked the nuns to serve them to me.

Often on Sundays, Father Maguire invited me to Sumi mission, on the south side of the island, for rather sumptuous lunches and a relaxing afternoon talking about, among other things, his Rotuman experiences. Father Kitolelei, Rotuma's first ordained priest, was usually there to share his experiences. At Upu, one Rotuman nun in particular, **Sister Madeleine**, became a good friend who alerted me to some subtle aspects of Rotuman behavior patterns, including the tendency of Rotumans to touch everything they are looking at. On the Methodist side, **Reverend Taito** invited me to his home in Noa'tau and shared his views with me regarding Rotuman culture. We also developed a long-term relationship with **Sister Leotina**, a Catholic nun, after she invited me to teach the children in her class at Upu to play softball. We subsequently got together with her in the United States, where she had been assigned, and in suva after she had retired.

Without a doubt, as far as my 1960 research was concerned, I owe the greatest debt to **Amai Sakimi** and **Rejieli Mejieli**, whom I hired as research assistants. They were both extremely conscientious and between them collected an enormous amount of data, including a complete household survey of the island and the life histories of seventy adults from all parts of the island and a range of age groups. I prescribed a set of topics for them to follow so there is a lot of comparability between them. As noted earlier, Jan and I compiled and edited the life histories and created a two-volume e-book entitled *Rotuman Life Experiences 1890–1960* (2020).

When I returned to Rotuma in 1987 on sabbatical with Jan, after a 27-year absence, it initiated a entirely new phase in my career. I had not contemplated doing any further research before that visit, but Jan's enthusiasm for doing doctoral research on the island inspired me to engage once again, with my main focus on the cultural, social, and political changes that had taken place since my original research. Among the major changes: Rotuma was now a part of the Republic of Fiji and no longer a colony of the British Empire, and although there had been a significant population increase in the Rotuman population there was also a considerable outmigration to Fiji and abroad, resulting in a population decline on the island, a great deal more impact of the money economy, and a host of political and social changes. Our Rotuman research in the following years in Rotuma, Fiji, New Zealand, Australia, Hawai'i, England, Canada, and the U.S. Mainland was facilitated by a great many people, mostly Rotumans, but including others who also earned our gratitude.

In 1987, while in Suva while waiting for our flight to Rotuma, we were kindly invited to stay at the home of **Ron Crocombe**. While there, **Vilsoni Hereniko**, who was teaching at the University of the South Pacific (USP), came to visit with us. Thus began a lifelong relationship as friends, and later as colleagues. In a couple of subsequent stays in Suva on our way to or from Rotuma, Vili and his wife at the time, Pat, hosted us in their home on the USP campus. Vili translated Jan's household exchange questionnaire into Rotuman, which was a big help to her. We facilitated Vili's coming to the University of Hawai'i on a scholarship to work on his doctoral dissertation at USP. We read his dissertation manuscript multiple times and provided extensive feedback. When his supervisor at USP ignored his submission for over a year, I took the initiative to intervene and move things along until his supervisor agreed to send it to external referees, who overwhelming approved it. Vili was later hired by the UH Center for Pacific Islands Studies and after a few years served a term as the center's director. Jan and I materially supported Vili and his now-wife Jeannette's film project that resulted in *The Land Has Eyes*, which won multiple awards at international film festivals. We were on the island when the filming began and later when Vili and Jeannette took the finished film back to Rotuma to show it in several locales around the island. I later published a paper regarding "The Making of the Land Has Eyes" in *Visual Anthropology Review* (2006).

Jan and I had arrived on Rotuma in 1987 expecting to stay with Elsapeti Inia, but she didn't get the message sent by her daughter informing her of our arrival, so she did not meet us at the airport. Seeing our plight, **Tarterani Rigamoto**, grandson of Gagaj Tokaniua, who was chief of Oinafa at the time of my initial field work, invited us to stay at his home in Oinafa. We accepted, and he and his wife **Fakrau** hosted us for three consecutive years, from 1987 till the end of our 1990 visit, when we had a falling out (discussed above). During this three-year period he was

helpful to Jan in facilitating her research in Oinafa village, but his attempts to exercise control over our relationships with others and his expectations regarding our financial commitments resulted in an uneasy relationship at best. The teachers in both the government schools in Malhaha and the Catholic schools at Sumi and Upu were very supportive of our research and helped us gather data on several topics. **Vicente Makrava**, who was manager of the National Bank of Fiji at the time, helped us achieve our research goals by providing valuable economic data regarding Rotuma's economy. We also owe a considerable debt to **Torika Mareta**, who helped us with our housekeeping and became a dear friend.

Of all the people to whom we owe a debt, **Elsapeti Inia** has to be placed at the top of the list. Not only was she our main teacher and consultant regarding Rotuman culture and language, but she also hosted us at her home in Savlei for most of our visits to Rotuma after 1990. In 2013, we were hosted for two weeks by **Perry Gabriel,** the Principal of Rotuma High School at the time, and his wife, **Siteri**, at their home on the grounds of the high school in Malhaha. As noted earlier, Perry had invited us to be special guests at the second occurrence of Wilson Inia Day. Inia, husband of Elsapeti, had founded the high school in 1958 and was its first headmaster. Perry had read my biography of Inia *(Haf Ran Ta: The Morning Star)* and decided he should be honored as the founder of the school. It was a great occasion, marked by feasts, sporting and musical events, skits put on by the students, a competitive pageant featuring floats from each district, and a Miss Rotuma competition. The event was emceed by **Gagaj Tamanau Taukave**, a consummate musician and dance choreographer. He introduced us with the utmost warmth.

We have also occasionally been hosted for brief but very enjoyable visits by **John and Harieta Bennett** at their comfortable home in Maftoa, Itu'muta, and have treasured opportunities to see them during their visits to Hawai'i.

In 2019 we were hosted for a two-week stay on the island by **Dr. John Fatiaki and his wife Sineva**, along with several members of their extended family. Their home in Motusa is equipped with all the modern conveniences, including an air-conditioned bedroom and hot showers, luxuries we had never before experienced in Rotuma. John is an avid fisherman, and we went out on the boat with him a couple of times and really enjoyed the experience.

Dr. Fatiaki and Sineva also hosted us in 2017 on one of our many stays in Suva. (We generally stayed in Suva while waiting for transport to Rotuma, often for extended periods as a result of plane delays or cancellations. Inclement weather, algae on the grass runway, mechanical problems with the small 14-seater airplane, or the unavailability of pilots were all causes of delays.) On that occasion, flights to Rotuma were cancelled for a six-week period following torrential rains that damaged the runway, obviating any chance we had of going to Rotuma that year. Staying with and getting better acquainted with the whole Fatiaki family was a joy.

Our main sanctuary in Suva for many years has been the home of **John Tevita** and his wife, **Susana**, daughter of Wilson and Elsapeti Inia. They hosted us many times and treated us as members of their family. As a bonus, John is an avid golfer and provided me with clubs when he took me along to play at Fiji Golf Club. On a few occasions we also stayed with Susana's sister **Betty** and her husband, **Isimeli Cokanasiga**, and we have treasured getting to know the children and grandchildren in both families.

We were also hosted by Rotumans in each of the countries we visited during our research regarding Rotumans abroad. In New Zealand, which we visited several times, we were hosted by **Savika and Roger Oakley** twice at their home in Bombay, south of Auckland, and by **Reverend Jione Langi** and his family in Auckland for a six-week period in 1994, during which Rev. Langi helped us make a census of Rotuman families in New Zealand and provided us with a history of Rotuman organizations in the country. Subsequently his daughter **Rowena** and her husband **Fereti Semesi** hosted us in our visits to England while Fereti was in the British army. They have always treated us like family members, and to their son, Opetaiah, we are in the position of grandparents *(mapiga)*.

In 1994, we were hosted in Christchurch by **Sanimeli Gibson**, who later married **Gagaj Maraf**, chief of Noa'tau, where they also hosted us briefly on occasion. **Ofa Dewes**, a distinguished researcher into Pacific Islander health issues, also hosted us for one of our visits to Auckland, and she has stayed with us in Honolulu when here attending conferences.

We have also been hosted multiple times by **Arthur Shaw** and **Ravai Titifanue** in Wellington; their adopted daughter, **Hatamara**, is our *sigoa* (godchild). Our other *sigoa* is **Harieta Vilsoni**, daughter of **Maniue and Vika Vilsoni**, who became dear friends while they were stationed in Rotuma, Maniue as a teacher, Vika as a nurse at the hospital. We were present on the island when Harieta was born and attended her baptism.

On our initial visit to Australia in 1994, where we were feted by the Rotuman community in Sydney, we were hosted by **Marie Dickinson** for about three weeks. Marie (pronounce Maré) was wonderful to us in every way, from insisting that we take her incredibly comfortable bedroom while she slept on a cot downstairs, to shepherding us around to Rotuman church services and arranging for us to meet prominent Rotumans in the Sydney area.

In subsequent visits to Sydney we were hosted by **Semesi David and his wife, Ursula**, at their Bondi apartment, and by **Steve and Meseini Walker** in Ruse, a suburb of Sydney. Semesi also generously offered to let us use his car in Rotuma, an offer that we gladly accepted. (Incidentally, the first time we ever heard a mobile phone call in a car was while Semesi was driving us around Sydney while delivering Meals on Wheels.) We stayed with Steve and Meseini on several visits to Australia and also traveled around England with them one year when our trip there overlapped with theirs. We also became good friends with **Rejieli Flexman**, who worked with the Methodist Mission in Sydney. Rejieli became a regular email correspondent and on one occasion arranged for us to stay with her son **Hifa** and his wife **Melissa** while we were in Sydney.

In 1994 we also visited Melbourne, where we were hosted by **Torike Sanireve** and **Kapieni Patresio,** who took us to a meeting of the local Rotuman Association, where, following the business part of the meeting, they engaged in a fund-raising card game. It gave us a good opportunity to become familiar with the dynamics of Rotuman organizations abroad, including the great sensitivity regarding the management of finances. We also stayed with Torike on a subsequent visit to Melbourne, after Kapi had died.

During our 1994 trip to Australia we also drove to Adelaide, where we hosted for a couple of days by **Oni Hanfakaga and his wife Betty**, and in 2008 and 2011 we were hosted in Brisbane by **Marie Howard**.

We attended Rotuma Day celebration in British Columbia in 2003, 2004, and 2010 and on each occasion were hosted by **Konousi Aisake and his wife, Sandra.**

On our several visits to England we have been hosted repeatedly by the family of **Loraine and Richard Tizard-Varcoe**, as well as by **Fereti Semesi and Rowena Langi** (mentioned above), and by **Harieta Muaror and Sheridan Rosser**. Following Harieta's untimely death, Sheridan and his second wife, **Ann,** also hosted us in Sandhurst.

On the European continent, we have stayed with anthropological colleagues **Paul van der Grijp and Françoise Marsadon** in France, where we were also hosted by **L'Ecole des Hautes Études en Sciences Sociales** (EHESS) in Paris and Marseille. We were also hosted by **Michael Houseman and Marika Moisseeff** in Paris on a couple of occasions. In Germany we've been hosted by **Wolfgang Kempf & Elfriede Hermann** at their home in Göttingen, and by **Hans and Heike Schmidt** in Hamburg. Our visits with these European colleagues have wonderful in every way and we are enormously grateful for the hospitality and friendship shown to us on each occasion.

Finally I would like to mention two colleagues who have provided a great deal of stimulation over the course of my career. **Jack Bilmes** joined the UH Anthropology Department in 1973 and quickly became one of my best friends. He was an original thinker and I always benefited intellectually from our endless discussions. Jack was also a talented athlete and we spent a good deal of time together playing ping pong, tennis, and softball. I first encountered **Alex Mawyer** when he was an undergraduate student in a graduate seminar I taught on ethnographic film. He went on to get a PhD in anthropology at the University of Chicago, and after teaching at Lake Forest College in Illinois for a few years was hired by the UH Center for Pacific Islands Studies in 2014. Alex has one of the most brilliant minds I've ever encountered. He reads voluminously and is incredibly well informed on a wide variety of academic (and literary) subjects. It has been my pleasure to collaborate with both Jack and Alex on projects resulting in publication.