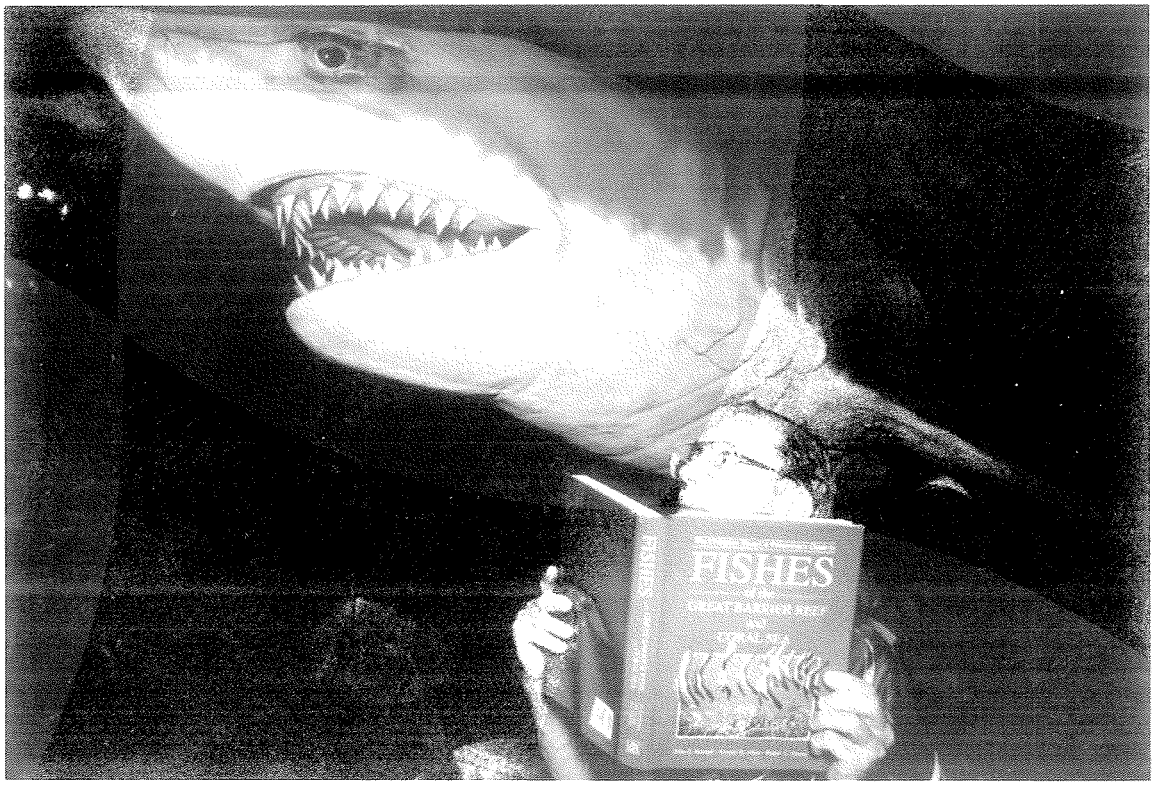

John E. Randall promotes a new book. 1991 (Photo Steven Siewert)

REMINISCING....

BY

JOHN E. RANDALL

My father, a building contractor, wanted me to become an architect. I took the maximum of mechanical drawing while attending Beverly Hills High School and during the last semester, the teacher made me a special student for architectural drafting. I also took the two courses offered in biology. One field trip of the advanced course was to Palos Verdes at low tide to examine the marine life in tidepools. I was captivated by what I saw. I had already been keeping tropical freshwater fishes in aquaria — even to the level of successfully breeding some of the egg layers such as zebra danios. So I bought a 25-gallon aquarium and laboriously filled it with seawater transported in 5-gallon water bottles. Then I collected an array of animals and plants from the Palos Verdes tidepools, installed an aerator, and expected this to be a successful start of my new hobby as a marine aquarist. The next morning everything in the tank was dead or dying. By trial and error (mostly the latter), I finally managed to keep a few sea anemones and fishes such as opal eye, señorita (caught with hook and line), and small cottids. That is, until the aquarium leaked and nearly 25 gallons of seawater went onto the floor of my father's den.

My interest in fishes had also been kindled by my mother who took me fishing from barges anchored off Santa Monica or Malibu. When I was older, I went with a school chum, Roland Boreham, on live-bait boats during summers when the catch was more exciting than the mackerel and an occasional California halibut one could get from barges, chiefly the Pacific barracuda, but always with the hope of a yellowtail or white sea bass.

I graduated from high school in February, 1942. As I had been advised to delay my entry to a university until February (my choice being UCLA, only 3 miles from my home and then $21 a semester for residents of California), I took a job as an architectural draftsman. I disliked this, and after a few months I opted to work as a carpenter's helper for my father. He had already trained me well in the use of the tools of the trade, and I had taken high-school courses in wood shop. When summer came, I decided I would like to sign on as crew on a tuna clipper. My parents opposed this, thinking of the threat of Japanese submarines, and informed me that I was to enter UCLA's summer session. My first course was General Zoology, and there was no question from then on that I wanted to be a marine biologist, not an architect.

I wanted to enlist in the Navy but my vision (strong myopia) was not good enough, so I joined the Army's Enlisted Reserve Corps while at UCLA. I had learned to ski in a rudimentary way and decided it would be better to be in the ski troops than

Ichthyology, Bishop Museum, Honolulu, Hawai'i, 96817-2704

the infantry. I arranged for a testimonial letter that I was a skilled skier and was told that I could be in the ski troops if I was called to active duty before April, 1943. The orders for active duty did not come until July. Had I been a "ski trooper," I would have joined the Tenth Mountain Infantry Division that sustained heavy casualties fighting the Germans in Italy (and never got on skis).

After a summer of U.S. Army basic training in Texas and some tests, I qualified for the Army Specialized Training Program in engineering and was sent back to school at the Los Angeles City College. With the invasion of Europe, that program was cancelled after a semester for everyone except those who might obtain a provisional acceptance to medical or dental school. My father went with me to see the Dean of the Medical School at USC . He and the dean shared some stories from being in France in WW I, and as my grades were good, I was accepted. I was to be assigned to Stanford to complete premed. However, the war was going well, so the Army's medical training program was terminated, and I became a dental assistant in the 37th Infantry Regiment just back from the Aleutians. Six months later I made it to Officer Candidate School at the age of 19 and served after that as a second lieutenant in the Medical Administrative Corps.

Upon my discharge from the Army in 1946, I saw an opportunity to work with my father to make a great deal of money building houses. The demand for housing after the war in southern California was enormous. I proposed that we become developers, first on a small scale, then larger. His response was negative. He would not engage in building what he termed "cracker boxes." I explained that we could switch the plans around, change the façades, etc., and make the homes appear different, even though they would all be basically the same. Again he refused, and I am glad he did.

While attending UCLA, I bought a 7.3-m sloop in bad condition, repaired it, and learned to sail. I was also skin diving and spearfishing, especially in summers. In contrast to the warm southern California weather, the sea is cold. Being slender, I soon had to come out of the sea to warm up. I had been given my long-john underwear when I was discharged from the army, so I dipped it in a wash basin of latex rubber, hung it up to dry, and may have had the first wet suit. The first swim fins in those early days of skin diving were shaped like frog feet, and the face masks were perfectly round with narrow hard rubber edges that one had to fit to one's face by careful cutting and sanding.

One day when I went to an Army-Navy surplus store to get some anchor line for the sloop, I spotted a steel tank wrapped in wire with an odd contraption at one end, terminating in a mouthpiece. I asked what it was and was told one could put compressed air in it and go underwater. Upon hearing that it cost only $25, I said "Never mind the anchor line, I'll buy the tank." The regulator was not attached directly to the tank but was halfway in the hose to the mouthpiece. I made a backpack of sorts and mounted the regulator on the right-hand strap at my shoulder. I decided it would be better to put oxygen in the tank rather than waste four-fifths on worthless nitrogen. There was no Aqua Lung on the market then, so I had no knowledge that pure oxygen could be lethal at a depth greater than 10 m. While diving with my new gear, I had an excess of oxygen when my right shoulder was lower than my mouth, but none if it was higher. This made me cautious, and I confined my diving to shallow water. Later while

attending Boyd Walker's first course in ichthyology at UCLA in 1948 (classmates included Ken Norris, Connie Limbaugh, and George Barlow), I used the Aqua Lung on field trips. Boyd was the first on the Pacific coast to use this early diving gear for fish collecting and research.

I discovered a new hull of a 11-m sailboat that was on sale for $3,000. I purchased it with my friend, Howard Boreham, who had a one-third interest, and we began the long task of finishing the construction. The plans called for a centerboard gaff-headed yawl, but we turned it into a keeled marconi ketch. We could not afford a lead or even a cast-iron keel, so we made a long box of quarter-inch steel plate, welded 10 keel bolts inside that stuck up well above the box, and filled it with scrap iron and cement. Then I drilled 10 holes through the keel timber for the bolts. Luckily all the holes were perfectly vertical, and the keel was raised with house jacks into place. I built the binnacle from a cast-off oak piece of banister post from the house my father built for the singer-actor Gene Autry (I had worked as a carpenter's helper on that job). We named the ketch *Nani*, Hawaiian for "beautiful" (Fig. 1).

Figure 1. The 11-m ketch *Nani* in which Randall sailed to Hawaii in 1950. (Photo C. Noegel)

In the summer of 1949, I decided to sail *Nani* to Hawaii and finish my senior year at the University of Hawaii. I borrowed money from my father to pay the one-

third interest to Howard. My mother was very upset with this plan, so my father announced that I was not going to sail to Hawaii as long as he owned one-third of the boat. I then knew in the ensuing year that I had to raise the money to repay my father.

I graduated from UCLA in February, 1950, entered graduate school, and became a teaching assistant in zoology. Other sources of revenue included working as a life guard at the swimming pool, reading examinations, tutoring football players, collecting animals for Ted Bullock's course in comparative physiology, baby sitting (among my steady customers, the actor Richard Widmark), and chartering the vessel to fraternity groups for weekend cruises to Catalina Island.

By summer I was able to pay my father and prepare for the sail to Hawaii. I obtained three other college kids as crew, Barbara and Mary from the Tiller and Sail Club at UCLA, and Jake from a community college. The day we left, the small craft warnings were up, but we set sail anyway, especially after our friends came down to Wilmington to see us off.

I had taken a Naval R.O.T.C course in navigation at UCLA, but it involved only the theory, so I had never actually navigated. At dusk off the east end of Catalina I could see the stars I needed for the sextant shots, but it proved very difficult in the rough sea. I had to go below to get out the tables from H.O. 214 and compute the fix, but I was very seasick by then. The only lines that intersected on the chart were somewhere near Palm Springs. Mary asked that I take her home. I explained that it was just my mal de mer and suggested we sail south to the shelter of Pyramid Cove on San Clemente Island, and there I would demonstrate that I could navigate. She agreed and after a day at San Clemente we sailed westward. We had two periods of calm en route, 1 of 5 days only 50 miles off the coast of California, and did not reach Hawaii until the 28th day.

In Honolulu, Jake and I were away from *Nani* when a reporter came to the vessel and interviewed the two girls. I saw the newspaper article the next day in which I was only mentioned in the last brief paragraph by name as the owner and skipper who had come to Hawaii to accept a post as a Professor of Zoology at the University of Hawaii (UH). You can imagine the reception I received when I went to the university for my position as a Graduate Assistant in Zoology.

Robert W. Hiatt, the Chairman of Zoology, asked what my plans were for graduate school. I said I intended to get a Master's Degree in a year, and sail my boat back to California the following summer, and enter the Scripps Institution of Oceanography to study under Carl Hubbs. I had already planned my research on the biology of the totoaba (*Cynosion macdonaldi*) in the Gulf of California, soon to be listed as an endangered species. Hiatt reminded me that no student in Zoology at the University of Hawaii had ever earned a masters degree in one year.

One of the labs that I was to teach was Embryology; the first was scheduled to demonstrate the reproductive organs of the chicken. I found a live rooster and hen running around the lab. Helen Au, just back to Hawaii after getting her masters degree at Boston University, was the other new lab instructor. Seeing that I did not know how to cope with the live birds, she prepared them for the dissection. After our respective labs, sharing the same chickens, I suggested that she come down to my sailboat and I

Figure 2. Hawaiian-born Helen Au, of Chinese descent, became Mrs. John E. Randall in November 1951. (Photo G. Ewart)

would prepare a dinner from the dissected birds. That was our first date; a year later she became Mrs. Randall (Fig. 2).

Hiatt called me in one day to ask if I would like to participate in an expedition the following summer, sponsored by the Office of Naval Research, to the atoll of Onotoa in the Gilbert Islands (now Kiribati). The plan was to take a sailing vessel, and I qualified because I could be both crew and naturalist. My job would be to assist the marine biologist, A. H. Banner. This was too tempting to refuse, so I gave up my plan to return to California that summer. Having met my future wife was another major factor for staying.

Hank Banner, an invertebrate biologist whose specialty was alpheid shrimp, announced that I would be the ichthyologist on the expedition. Instead of going on a sailing vessel, a U.S. Coast Guard buoy tender took us to Onotoa. My fellow grad student, Don Strasburg, the first to get a Ph.D. in Marine Zoology at UH, was also on the expedition, but his role was to assist the expedition leader, the geologist Preston Cloud (Fig. 3). Each of us was later the best man at the

Figure 3. The scientific team at Onotoa Atoll, Gilbert Islands, summer 1951. Standing, left to right, Pres Cloud, Ward Goodenough, Don Strasburg, Ed Moul, Jack Randall, and Hank Banner. In front, our assistants Teoki, Jim, and Bill.

other's wedding.

We had no compressor at Onotoa to fill scuba tanks. Instead we took large cylinders with compressed air. In my role as an assistant, I used only one scuba tank the entire two months of our stay in the Gilberts. I collected many fishes with the ichthyocide rotenone and by spearing. I also tried to obtain specimens from the Gilbertese fishermen but after they saw me putting the fishes in a drum of formalin, I could get no more from them. In their minds, there is only one thing to do with a fish and that is to eat it. However, using candy and chewing gum, I did well getting specimens from the children.

The British had advised the Gilbertese that the Americans might not be used to the women going topless, as was the Gilbertese tradition, so they wore blouses. One attractive 15-year old covered her chest only with a bib, so the view from the side was very good. Ed Moul was the land ecologist and collected insects, plants, etc. He was able to get her to assist him collecting insects. He noted that she took off her bib and used it to swat down butterflies. I think Ed collected more butterflies than he really needed.

Pres Cloud ran the expedition like a military operation. I was appointed mess officer and was told to obtain what I could in the way of fresh food from the Gilbertese. I remember buying too many spiny lobsters from women, some of whom had walked from distant parts of the atoll. After a dinner of lobsters, I arranged to have lobster for breakfast the next day, but that was not well received.

Hank Banner bruised his shin the day of arrival at the atoll resulting in a serious infection. He became progressively more ill and red streaks appeared on his leg. Fortunately, I had brought a vial of penicillin with me, which had been available only recently, and injected him with it. This cleared up the overall infection, but the huge crater-like wound took long to heal and kept him bedridden. He used the time wisely by interviewing Gilbertese about the uses they made of marine products, their methods of collecting, and their names for marine animals. He published in *Atoll Research Bulletin* (1952), and I added Part II on Gilbertese fishing methods.

Upon my return to the University of Hawaii, I asked Hiatt if I could work on my large Gilbert Islands fish collection for a Ph.D. thesis. My professor, William A. Gosline, had obtained support via the Office of Naval Research for the research on the fish collection that was to lead to two lengthy visits to the National Museum of Natural History where Leonard P. Schultz was working on a three-volume report on the fishes of the Marshall and Mariana Islands. Hiatt said no, explaining that a purely systematic thesis was not acceptable. I remonstrated by saying that I had data on habitat of the different species and some food-habit data. He still refused, suggesting that I select a family of fishes for systematic study and pick one of the species for an in-depth study of its biology. I chose the surgeonfish family and a study of the convict surgeonfish, *Acanthurus triostegus*.

Because surgeonfishes are herbivorous, it was important that I identify the algae on which they feed. I was privileged to be a student of Max Doty for his course in phycology at UH. I remember one field trip we made to Hanauma Bay where he was walking in the intertidal zone (very narrow in Hawaii because the tidal change is low)

and I was snorkeling nearby. There was a luxuriant growth of algae in the intertidal, but I was having trouble finding any in the shallows. Max asked if I could explain why there was so little algae where I was swimming and so much where he was standing. I speculated that the surgeonfishes and other herbivorous fishes could feed on algae where I was but could not get to it in the intertidal zone. He asked how could I demonstrate this. I said by putting cages down that would permit the algae to grow and not let fishes graze on it. The result was my brief paper, "Overgrazing of Algae by Herbivorous Marine Fishes"(Randall, 1961a).

I had always been fascinated with parasitology and had taken a course on it at UCLA. I started looking for parasites in the convict surgeonfish and found 17 of them. One is a large nematode, *Spirocamallanus monotaxis*, that occurs in the intestine — often in such numbers that I wondered how the fish could pass any algae through its gut. It had been described in a study of the lethrinid fish *Monotaxis grandoculis* known in Hawaii as the mu. Like *Trichinella*, camallanid nematodes are live-bearing. After determining that the surgeonfish could not be directly infected by the larvae, I read that all camallanids have an arthropod intermediate host. I then collected various small crustaceans and mites from areas where the fish were heavily infected. I was in the process of seeing if they would interact with the larval nematodes when Bill Gosline came in and asked what I was doing. When I explained, he remarked, "If you start working on life histories of parasites of this surgeonfish, you will be here 20 years." I was to spend no more time on this subject.

It took four more years for me to get the degree, but I was well trained. I had finished the classification of four genera of surgeonfishes, published a report on 396 species of fishes of the Gilbert Islands, along with a list of Gilbertese fish names in *Atoll Research Bulletin* (1955), and finished the study of the convict surgeonfish (Randall, 1961b). I gave a presentation on surgeonfish biology at the annual meeting of the American Society of Ichthyologists and Herpetologists at the University of Florida in 1954 and won the Stoye Award for the best student paper.

Yale University and Bishop Museum offered two fellowships each year for biological research in the Pacific Islands, one in zoology and one in botany. I had heard that Vernon Brock, head of the Division of Fish and Game of what was then the Territory of Hawaii, was considering introducing snappers and groupers from French Polynesia to the Hawaiian Islands. Hawaii has no native snappers of the large genus *Lutjanus* and only two groupers, the giant *Epinephelus lanceolatus* (rare) and the deep-water *E. quernus*. I told Vernon that he should investigate the biology, and in particular the food habits, of the fishes he was considering for introduction. I asked him if it would be wise to bring in a grouper that feeds mainly on small lobsters. He agreed that such studies should be undertaken, so I wrote a proposal to sail my ketch to Tahiti for this research, and I was awarded a fellowship. It consisted of $4,000 to be paid in two installments, the first of $2,000 at the onset and the last six months later. I spent the initial $2,000 on new rigging and sails, etc. for *Nani*, so I had to borrow money from my father until the next $2,000 came in.

E.C. Jones of the National Marine Fisheries Service gave me a plankton net and some jars with a request that I tow the net in harbors and bays in French Polynesia to

get samples of the copepod genus *Labidocera* for his current research. This small crustacean alternates between a benthic and pelagic mode of life. I asked how I would know if I had *Labidocera*. He said they nearly always have a little nematode coiled under the carapace. I knew the prime suspect would be the camallanid nematode that infests the convict surgeonfish and other Hawaiian fishes.

Norman Baker, a former naval officer, signed on as crew and we set sail with our almost three-year old daughter Lori in November, 1955 (Fig. 4). As in the departure from California, the red flag for small-craft warnings was up. Again, friends came down, and we took off for Hilo, which was to be our port of departure for Tahiti. The channels between the islands in Hawaii are notoriously rough on days of strong trade winds, especially the Alenuihaha Channel between Hawaii with 13,800-foot (4205 m) Maunakea on one side and Maui with 10,000-foot (3055 m) Haleakala on the other. The tradewinds funnel through the channels by venturi effect, and you can experience gusts of 40 knots or more on a day of 25-knot trades. We were all seasick in that channel.

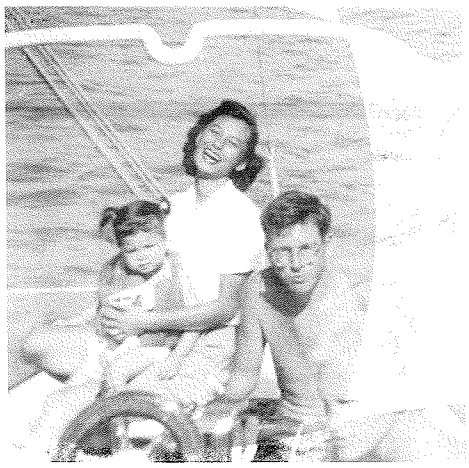

Figure 4. Jack, Helen, daughter Lori (2 1/2), and crew member Norman Baker shortly before sailing *Nani* to Tahiti, November, 1955. (Photo E. Nygren)

After arriving in Hilo and taking on our last supplies, Helen noticed the rudder moved one way when the ship rocked the other. We then saw that it was hanging on one bolt. I still shudder to think what it would be like halfway to Tahiti without a rudder.

We wanted to sail to the Marquesas before going on through the Tuamotus to Tahiti which meant making a lot of easting. The day we finished the rudder repair, the trades shifted from northeast to southeast. We decided to put off our departure until the winds shifted back to the normal northeast; this took several days. Soon after our departure from Hilo, the wind shifted back to southeast. For six days we could sail only due south to the doldrums where the only wind was from occasional squalls. Not only did we abandon the plan for the Marquesas, but we were worried about getting far enough east to make it to Tahiti. I hit upon the idea of finding the Counterequatorial Current which flows to the east. I knew it would be warmer than the North Equatorial. We took temperature readings, found the current, stayed in it for two days, and then headed south for Tahiti.

I noticed on the chart that an atoll, Caroline Island, now lay in our course. It was recently renamed Millenium Island since it was the first locality to usher in the New Year 2000. It is now part of Kiribati and the time zone was extended east to include it. The old *Sailing Directions* reported it as unihabited, but as we approached, we saw that a large fire had been built as if to attract our attention. There were six Tahitians on the atoll harvesting copra. The pass to the lagoon can be entered only by small boats, so we anchored on the lee side as best we could in about 60 m on the steep

reef front, knowing if a wind should come from the west we would soon be on the reef. Two of the Tahitians paddled out in a makeshift canoe of galvanized iron roofing. They had not seen a supply vessel for six months, and their first word was "Cigarettes?" None of us smoked, but we had a carton of cigarettes on board and passed it on to them, receiving in exchange a live rooster. We had to maintain anchor watch all night in order to be alert for any wind change. We stayed three days at the atoll, snorkeled, collected a few fish specimens, and I took underwater movies of the profusion of reef fishes. I discovered a new cleaner wrasse of the genus *Labroides* which I later collected in Tahiti. I had a paper in press in *Pacific Science* at that time naming the Hawaiian species of *Labroides*, so I cabled the editor from Papeete asking him to hold off until I could add one more new species.

Because we could not find many groupers and snappers in Tahiti, we sailed to Moorea 12 miles away and anchored in Opunohu Bay (Fig. 5) not far from where there is now a small French marine lab. It was an idyllic year. Forty years later I went back, visited the marine lab, and dived to 20 m where I had put down a mooring.

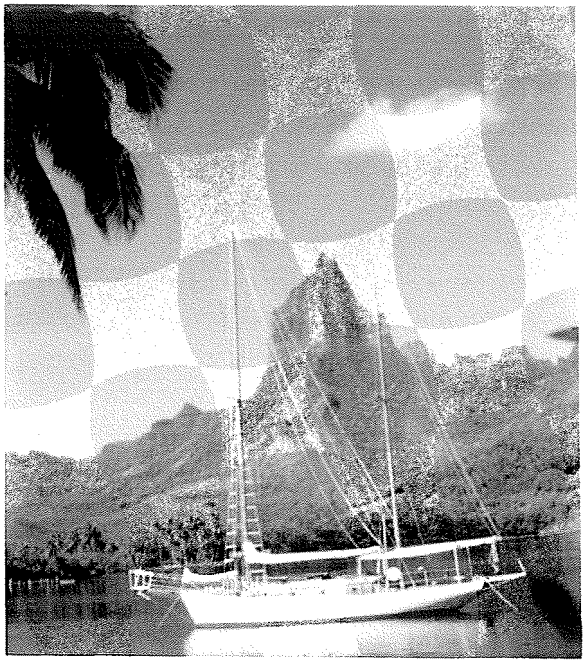

Figure 5. *Nani* at anchor in Opunohu Bay, Moorea, 1956. (Photo J. Randall)

In addition to my research on food habits, I had been asked by Hank Banner to find out what I could about ciguatera fish poisoning. This is not a common illness in Hawaii but is a major health problem at many islands in French Polynesia. I speared a large grouper (*Plectropomus laevis*) with the Tahitian name tonu, notorious for causing ciguatera. I was preparing to bury it after taking data and checking stomach contents when the Tahitian caretaker of the property where *Nani* was moored said he would like the fish. I warned him that I had heard that the tonu had a bad reputation of being ciguatoxic and the area where I collected it was known to harbor poisonous fishes. He said his wife knew how to test for the poison, so I gave it to him. He did not come to work for two weeks. Then he explained his absence by telling me that the fish had put two families in the hospital. I asked what his wife's method was to test for a poisonous fish, and he said she fed it to their cat and it had shown no distress. Later I found out from Dr. Raymond Bagnis, who was working on ciguatera in Papeete, that a cat will regurgitate strongly ciguatoxic fish. If you are using a cat for a test animal, you need to keep it confined.

I assembled what information I could on ciguatera in Moorea. I soon noticed that the species that could be toxic were mostly the large carnivorous ones such as

groupers, snappers, barracuda, and jacks, though herbivores such as surgeonfishes and parrotfishes could be poisonous in very toxic sectors. Also, it was clear that the larger the fish within a species, the more apt it was to be toxic. One poisonous area in Moorea was in the lagoon at the entrance to a stream bed. Only during torrential rains was there an outfall of freshwater at the site. I thought that the outflow of freshwater with land nutrients might enrich the area enough to cause a bloom of a benthic blue-green alga. Herbivores would graze on the alga, carnivores would prey upon them and more quickly accumulate the toxin (like DDT concentrating as it goes up the food chain).

Fanning Island (now Tabuaeran) in the Line Islands experienced a severe outbreak of ciguatera shortly after World War II. Knowing that U.S. troops had been stationed there during the war, I wrote to the manager of the copra plantation on the island, Mr. P.D.F. Palmer, and asked if he could confirm my hypothesis that sewage from the troops went into the lagoon, enriched the sea there, and caused a benthic algal bloom, and ultimately the outbreak. He replied that I was wrong. No sewage went into lagoon, and the fishes were not poisonous there. They were toxic in English Harbor where war materials had been dumped and where you could still see the scars on the reef from the ships' anchors.

I had already read of a hypothesis that linked copper to ciguatera. Wrecks of old ships with copper sheathing on the bottoms had been reported as sites for poisonous fishes, and the sea off Copper Mine Point at Virgin Gorda in the British Virgin Islands was known to be a toxic locality. Tailings from the mine were dumped into the sea off the point. Washington Island (now Teraina) in the Line Islands also had troops during the war, but the island has no lagoon and no harbor, so it was supplied by air. In 1964 the British ship *Southbank* was wrecked on the island, and the first outbreak of ciguatera occurred near the wreck. Hao Atoll in the Tuamotus was the staging area for the French atomic test at the Mururoa. Lacking a long-enough airstrip, a channel between two islets was filled so the two islets became one. Hao never had any ciguatoxic fishes until shortly thereafter; then they appeared where the sea had been dredged to provide material for linking the islets.

With Palmer's response, I knew that the common feature in all the above examples was the repetitive creation of a new surface in the sea. As wrecks broke up, as anchors dragged, and as sudden outflows of freshwater to a normally marine environment occurred, new surfaces were created. I published my hypothesis on the cause of ciguatera in 1958. My suspect for producing the toxin continued to be a blue-green alga, but I added that it might be an organism growing in association with a blue-green. Takeshi Yasumoto discovered the culprit, *Gambierdiscus toxicus*, a new genus and species of dinoflagellate, at Mangareva in the Gambier Group of the Tuamotu Archipelago (Yasumoto et al., 1979). He confirmed my hypothesis by pointing out that the dinoflagellate is associated with early-settling algae.

Another project in Moorea was finding what ectoparasites the bright blue, black-striped cleaner wrasse *Labroides dimidiatus* was removing from reef fishes. I used a small multiprong Hawaiian sling spear that I had devised to collect the wrasses. I was surprised when one turned out to be the sabertooth blenny *Aspidontus taeniatus*, hence an amazing mimic of the wrasse, and it had completely fooled me. I noticed that there

was often a pair of these blennies near a pair of the cleaner wrasses. I could tell the two apart by the overhanging snout and the long continuous dorsal fin, but only when I was near them. I watched adult fishes come to the *Labroides* cleaning station and carefully keep their distance from the blennies. Then a subadult of the sailfin tang (*Zebrasoma veliferum*) mistakenly posed with fins erect for one of the blennies; the blenny darted in and tore off a piece of the duped tang's fin. I then speared the blenny and recovered the piece of fin from its stomach. Therefore, the blenny not only enjoys protection from predation in its guise as a cleaner, but it is able to get closer to its prey. I also observed the blenny quickly nip off the tentacles of sabellid plume worms.

There is a second color form of the cleaner wrasse in the Society Islands and Tuamotus that has a rust-colored blotch below the lateral black stripe. When a cleaner wrasse has this blotch, then the mimicking blenny lurking nearby does also. In American Samoa the same cleaner wrasse has a color form in which part of the black stripe posteriorly on the body is replaced with bright yellow and the blenny has the same color pattern. I always wanted to perform an experiment by catching the blenny (easier to do than the wrasse, because it backs into a worm tube when chased) and switching it with the blenny of the other color pattern at well isolated locations, such as patch reefs of an atoll lagoon, but I never did. I fully expect the translocated blenny to take on, in time, the color of the resident cleaner wrasse.

Another intriguing question on color in fishes that I have always wanted to investigate is why shore fishes in deeper water take on more red pigment. Because the red end of the spectrum is filtered out first with depth, I have guessed that adding red pigment would make a fish less visible. I planned to place a species of fish that I know gets red in deeper water in an aquarium with red filtered from the light source. I expect in time that it will add red to its coloration. Also I wanted to test the visibility of fresh specimens of the same species of fish with and without red color in the deeper diving depths. I wonder if I will ever get to this.

Helen was watching drifting yellow hau leaves in Moorea along the shore in the bay when she noticed that one apparent leaf was moving lateral to the current, and she determined that it was the young of the spadefish *Platax orbicularis* (often mistakenly called batfish). She also saw a black fragment of what appeared to be a leaf moving back and forth in the surge at the shore that turned out to be a juvenile razorfish (*Xyrichtys*). In addition, we discovered the young of *Acanthurus pyroferus* that mimics the beautifully colored pygmy angelfish known as the lemon peel (*Centropyge flavissima*). These observations culminated in a review of mimicry and protective resemblance in fishes (Randall and Randall, 1960).

When I had been a graduate student during my second visit to the Fish Division of the National Museum of Natural History in 1954, Ted Bayer invited me to join him for a trip to Miami to do some preliminary work for an exhibit on coral reefs. While there I met F.G. Walton Smith, the Director of the Marine Laboratory of the University of Miami, and mentioned my availability in a year for a position as an ichthyologist at the laboratory should such a post become available. Just as I was leaving Hawaii for Tahiti, I received an offer from him, but I had to write that I had already accepted the fellowship.

Shortly after my arrival in Tahiti, I prepared a proposal for a Fullbright Scholarship to Australia where I planned to study the sensory perception of prey by sharks. At that time, fishermen and spearfishermen knew that sharks were quickly attracted to a struggling fish on the end of a fishing line or spear by the low frequency vibration, but the scientific world had not investigated this. From my preliminary correspondence, I knew that the chances were very good that I would get the fellowship. As I was mailing the proposal at the Papeete postoffice, I was told that I had a cablegram. It was from Walton Smith offering me a job at the University of Miami in a year. Here was a major fork in the road. If I took the job, I would have to give up the cruise to Australia and my plan of taking *Nani* around the world in stages. I wisely accepted the job.

Helen was nearly 8 months pregnant at the time we were ready to sail back to Hawaii. We tried to get space on the seaplane that flew to Honolulu, but it was booked well in advance, and the first attempt to get on a Matson liner was not successful. A doctor friend warned that it was not wise for Helen to give birth in Tahiti. The alternative of sailing to Hawaii with her aboard was not good either. Hank Banner wrote us that should we consider that, we might be qualified to write such books as "Delivery in the Doldrums" or "Midwifery in the Mid-Pacific." Fortunately Helen and Lori got passage on the next Matson ship for Hawaii. I obtained as crew Jeremy Hewett, a recent Oxford graduate in law who had planned to go to New Zealand, and a young American, Eugene Marsh. We set sail in July, 1957 via the Tuamotus and Marquesas. I collected a few new species of fishes in the Marquesas and vowed that I would get back there someday for intensive collecting. We made good time to Hawaii, 17 days from the Marquesas to Hilo, but I arrived in Honolulu one day late for the birth of our son, Rodney. Jeremy later sailed *Nani* to southern California where it was sold.

I conferred with Vernon Brock about his plan for introducing snappers and groupers to Hawaii. I said there was not a single species in French Polynesia that should be brought into the Hawaiian Islands. The larger snappers and groupers cause ciguatera, and the smaller ones would have little value as food or game fishes. I recommended that he consider the Nassau grouper for introduction from Florida or the Bahamas. He said that would be too expensive. Instead, snappers and groupers were brought in fishery-vessel live wells to Hawaii from the Society Islands and Marquesas and three species have been established (Randall, 1987a). *Lutjanus kasmira* has undergone a population explosion and spread throughout the Hawaiian chain. Because of its small size, it has little value as a game fish and it is not popular as a food fish. It is strongly suspected of causing the reduction of the populations of some species of fishes of greater value. The grouper *Cephalopholis argus* is increasing in numbers. In adult size it has caused ciguatera, so no fishery has developed for it. The third species, *Lutjanus fulvus*, is not common and seems to be causing no problem.

I had taken motion-picture films of our cruise to Tahiti and of my research on fishes in Moorea. This resulted in two TV presentations on "Bold Journey", one of which I narrated, the other narrated by Helen. We received $4,000 in payment, and with this I was able to repay my father for his loan of money when I was in Tahiti.

Soon after my arrival in Miami, I was invited on an expedition on a schooner to

the Exuma Cays in the Bahamas to survey an area under consideration for a land-and-sea national park, which has since been established (Randall and Ray, 1958). Oris Russell, then Minister of Agriculture and Marine Products of the Bahamas and also a member of the Exumas survey party, offered me a position as the Fisheries Officer in the Bahamas. He was especially anxious to see a study made of the biology of the queen conch (*Strombus gigas*). He was afraid it would meet the same fate as the overexploited conch of the Florida Keys. I was interested, as I liked the Bahamas, but he could not match my salary as an Assistant Research Professor at the University of Miami, then $6,000 a year.

One of my first dives after getting established in Miami was off Alligator Reef in the Florida Keys. I spotted a drab gray-brown fish in 28-m depth that I did not recognize, and speared it. Not wanting to swim up to the boat with one specimen, I put it inside the front of my swim trunks. That was a mistake. The fish was the soapfish *Rypticus saponaceus* which I soon discovered had a skin toxin that proved to be a powerful urethral irritant. One of the graduate students made up a limerick about this episode which added to the infamy. Later I wrote a paper with five Japanese colleagues (Randall et al., 1971) about this skin toxin in the soapfishes which we named grammistin. It is very effective in deterring predation.

In late 1957, I obtained a National Science Foundation grant to study the ecology of coral-reef fishes in the Florida Keys. I had barely started the study when I heard that the National Park Service wanted a three-year marine biological survey of the Virgin Islands National Park on St. John. I was selected to head this project and succeeded in transferring my NSF grant to the Virgin Islands. The National Park Service renovated an old Danish estate house at Lameshur Bay for our base of operations and provided a jeep (four-wheel drive was needed to get there from Cruz Bay, owing to the poor roads; see Figure 6) and a skiff with outboard motor. A different graduate student was to be selected as diving assistant for each of the three years, and Herb Kumpf was the choice for the first year. He and I made our first trip to St. John and found that the renovation meant only that a roof had been provided, outhouse built, and a generator and kerosene refrigerator installed. We worked for two weeks installing a sink, building cabinets, etc. The only source of freshwater was from rain that drained from the roof into a small cistern. We put a 55-gallon drum above the washroom, painted it black, and pumped water from the cistern to it where it warmed during the day. We drained our shower water to the ground below two lime trees, resulting in bountiful limes all year.

Figure 6. The Lameshur Turnpike from Centerline Road on St. John to Lameshur Bay. (Photo H. Randall)

Our first project was to map the marine environments all around St. John to the 10-fathom curve. The Park Service provided a 10-m vessel with an operator for this survey. We did not realize what an enormous task it was until we started it. St. John is 8 miles long west to east and nearly 4 miles wide at the widest point; it is deeply dissected with bays, so the coast line and that of nearby cays is more than 58 miles long. We finally devised what we called a dive sled whereby a snorkeler could be towed behind the vessel, and by depressing the inclined plane of the sled, could quickly descend and even more rapidly come up by reversing the process. We obtained aerial photos of St. John from the U.S. Coast and Geodetic Survey. While Helen consulted the aerial photos, Herb and I alternated in being towed as we zigzaged in transects around the coast reporting the bottom type to Helen. One feature of the photos made our survey much easier. Along most of the coast there is a band of bare sand between the fringing reef and the seagrass beds that showed well on the aerial shots (Fig. 7). Herb and Helen published the survey (Kumpf and Randall, 1961).

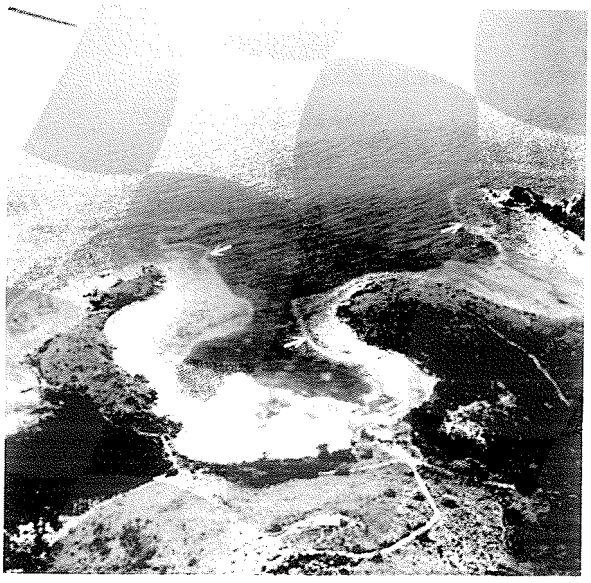

Figure 7. Aerial photograph of Lameshur Bay, St. John, U.S. Virgin Islands. Note the light band of sand (arrows) separating the fringing reef from the dark-colored seagrass beds seaward. (Photo J. Randall)

I had wondered why the seagrass did not grow adjacent to the reef. At first I thought that the sand near the reef was coarser and more shifting, but samples of sand across the band did not show any difference. Then I saw a parrotfish swim up from the bottom to eat a piece of seagrass that was drifting down. When detached from the bottom, pieces of seagrass float to the surface. In time, small encrusting organisms grow on them and they eventually sink. I then knew the answer. Parrotfishes and other herbivorous reef fishes will not venture far from the shelter of the reef because of fear of predation, so their grazing on the seagrasses is concentrated near the reef. How to show this? I decided to transplant a corrider of seagrass across the bare sand. Before I finished, parrotfishes were already starting to feed on the seagrass. So I built a corridor of concrete blocks across the sand into the seagrass bed, ending in a small artificial reef of blocks in the seagrass. In time the fishes created a sand band around the little artificial reef (Randall, 1965).

I decided that an artificial reef built in the seagrass well away from the fringing reef could be more productive than the fringing reef because it would provide more food for reef fishes by the broad expanse of seagrass around it than the fringing reef rimmed on one side with bare sand. This is especially true for grunts and snappers that use reefs for shelter during the day but range out to sand flats and seagrass beds to feed at night.

I made an artificial reef of 800 concrete blocks in Lesser Lameshur Bay at a depth of 9 m in April 1960 and monitored the recruitment of fishes to the reef (Fig. 8). Two years and four months later, the reef was ringed with net, and all the fishes killed with rotenone. Two comparable rotenone stations were carried out to collect fishes from the fringing reef. The artificial reef produced 11 times the biomass of fish as either station on the fringing reef produced (Randall, 1963).

Figure 8. Tony Chess (standing) and Gladston Matthias with one of our fish traps used to capture and tag reef fishes at St. John. (Photo J. Randall)

Remembering Oris Russell's request for a study of the biology of the Queen Conch, I suggested this as a thesis subject to Herb. However, Fritz Koczy, the oceanographer from the Marine Laboratory of the University of Miami, convinced Herb that he should be an oceanographer, so I decided to undertake the study. I started by tagging 104 of the smallest conchs we could find (the smallest was 83 mm and the average was 110 mm) by drilling a hole through a spire and affixing an orange or yellow spaghetti tag clamped with a monel tab bearing a number. Over a 15-month period they grew an average of 52 mm a month. We wondered why we had not seen live conchs smaller than 83 mm during the day but could find smaller empty shells on the beach. Night diving revealed that the small conchs bury in the sand during the day.

As the larger tagged conchs were difficult to recover because they moved more than the juveniles, we put 16 in a large area that we enclosed with a 12-inch high chicken-wire fence. Three weeks later the third-year diving assistant, Robert Schroeder, reported that the conch did not like being captive; they were trying to get over the wire fence. I checked and found they were feeding on algae growing on the wire.

Now and then we found our tagged conchs as a crushed pile of fragments. We suspected that the spotted eagle ray (*Aetobatis narinari*) was the most likely predator. We finally speared one weighing 55.5 kg that contained the remains of 41 queen conchs of the size we were tagging but with no shell fragments or opercula. Other predators on small conchs included various mollusk-feeding fishes, the spiny lobster *Panulirus argus*, the tulip shell *Fasciolaria tulipa*, and *Octopus vulgaris* which can also prey upon adults. Adult queen conch complete with intact shell have been reported from the stomachs of tiger sharks.

One of the large tagged conchs in our enclosure disappeared, but a very old empty shell was found that had not been there before. The mystery was solved when we observed a large hermit crab (*Petrochirus diogenes*) living in a conch shell and feeding on a live adult queen conch. We surmised that a hermit crab had crawled over the wire fence, eaten one of our conchs, discarded the old shell, adopted the tagged one as a new home, and crawled out.

Oris Russell invited Helen and me to the Bahamas to see if I could demonstrate that the queen conch lays its eggs deeper than 9 m, the limit in depth that conch fishermen were then able to fish by hooking them with a long wooden pole. On my first dive I found the egg mass in 14.5-m depth. But I also found the crushed remains of an adult conch in a pile (I still have some of the fragments up to 15-mm thick). I asked Oris what animal could have crushed such a massive shell. He did not know, but the Bahamian operating the boat, who had been a conch fishermen, said it was the loggerhead turtle (*Caretta caretta*). We confirmed this by talking with men at the Nassau market who killed and cleaned the turtles.

While checking the sex ratio of the queen conch off the Berry Islands in the Bahamas, I noticed that verges (the long slender copulatory organ) of some of the males were bitten off. I cannot imagine a worse example of coitis interruptus. The good news is that verges were seen in various stages of regeneration. The identity of this sneaky verge-eating predator remains unknown. My paper on the biology of the queen conch was published in 1964 in *Bulletin of Marine Science of the Gulf and Caribbean* (now the *Bulletin of Marine Science*). Also published in the same journal in 1964 was Helen's paper on the biology of the West Indian topshell, *Livona pica*.

I soon discovered when in the Virgin Islands that much systematic research was needed on the reef- and shore-fishes in the Caribbean area. During a visit to the Nassau fish market in the Bahamas, I saw three different species of the porgy genus *Calamus* with yellow on the nape. That had been the recognition feature for tagging of what I was treating as one species for my research on food habits and growth. On a trip to the Muséum National d'Histoire Naturelle in Paris, I asked to examine the syntypes of *Calamus calamus* and found three different species in the jar. I selected one as a lectotype that conserved the name most authors have used for the species. A revision of the genus was published (Randall and Caldwell, 1966). I wrote 18 other systematic papers on West Indian fishes, some describing new species found while deep diving with Tony Chess, the second year's assistant on St. John, and other young colleagues.

Our residence and marine lab on St. John was visited in February, 1960 by the author, John Steinbeck. He wanted to try scuba diving, so we put a tank on him in shallow water, but he did not do well because his moustache caused his face mask to leak. I also dived on several occasions with Clare Boothe Luce who was very good underwater, and I introduced Laurence Rockefeller to snorkelling after getting a prescription face mask for him. He donated the land on St. John to the U.S. for our 29th national park.

During the third and final year in the Virgin Islands, John Lewis, the Superintendent of the Virgin Islands National Park, asked if I would survey Buck Island off St. Croix because he had heard glowing reports of the beauty of its reef and marine life. I was skeptical that it would be any better than the reefs of St. John, but I was wrong. I made a film of the island with Bob Schroeder's help that included underwater scenes, a green turtle layings its eggs, nesting sea birds, and aerial shots from a helicopter. The film went to the Department of the Interior, and not long thereafter the island was proclaimed Buck Island Reef National Monument by President Kennedy (Randall and Schroeder, 1962; Randall, 1971).

Helen and I collaborated on a study of the spawning and development of the parrotfish *Sparisoma rubripinne* (Randall and Randall, 1963). I had chanced upon a large spawning aggregation of this species in 21 m off Reef Bay, St. John, that was predictably there every day of the year after about 1 pm . One day I saw another species, the green *Sparisoma axillare*, swimming wildly in the spawning aggregation and chasing the reddish *S. rubripinne*. That seemed very strange. Why would one species try to interrupt the spawning of another? Later I saw a fish half way in color between the reddish *S. rubripinne* and the green *S. axillare*, and we eventually proved that *S. axillare* is the terminal male of *S. rubripinne*, the result of a change in sex. Whereas the initial phase of *S. rubripinne* may be either male or female, the terminal green phase is always male, that maintains a harem, fights with other males at the periphery of his territory, and spawns with one female at a time.

This study needed additional observation after I left the Virgin Islands in 1961 for a position as Professor of Zoology at the University of Puerto Rico in Mayaguez, so I returned to St. John on a flight from San Juan to the Virgin Islands with four full scuba tanks (the airlines in those days did not realize the hazard of flying with full tanks). A park ranger took me in a small boat 2 miles from Lameshur Bay to Reef Bay. I used three tanks for a period of 3.5 hours at the 21-m depth and came up with a request for the fourth tank so I could decompress. There was no fourth tank! It had been left at the dock back at Lameshur Bay. I decided to skin dive down to pick up my spear gun on the bottom. As I pushed off the bottom, one swim fin came off (the pair had been borrowed from a friend and they were a little too large). I dropped the spear gun, kicked off the other fin, and frog kicked up, barely making it to the surface. The pain from the bends came soon after starting for Lameshur Bay. We could not go very fast because the wind came directly at us, creating a short chop. By the time we arrived, I had severe pain in all my joints, including my neck. We picked up the full fourth tank and went to the deepest place nearby that I knew was 18 m deep. I was surprised at how quickly the joint pain abated after I reached the bottom, but I knew it could come back if I did not maximize that one tank of air. No scuba compressor was available on the island, and there was no decompression chamber in the Virgin Islands or Puerto Rico. I lay on the bottom and conserved the air as best I could, then slowly came up the anchor line in stages. That evening I had a little pain in one shoulder; the next morning it was gone. Our parrotfish paper was finally published in *Zoologica*, 1963.

In Puerto Rico, I taught Ichthyology and Fishery Biology (Figs. 9 and Fig. 10). Dr. Juan A. Rivero, then the Director of the Institute of Marine Biology, sat in on my ichthyology course, the first that had been given at the university. A year later he offered me the position of Director of Research of the Institute. Not long thereafter, he came into my office with a smile and said he was looking at the new Director of the Institute. He had just been made Dean of Arts and Sciences. I protested by saying I never wanted an administrative post, but he prevailed. He also wanted me to take over as Director of the zoo that he had established at Magueyez Island in La Parguera, the site of the marine laboratory (only offices were maintained at the university at Mayaguez). I also objected to that, but he said I should agree because there was a plan to transfer the zoo to Mayaguez, and I would be able to retain some of the zoo funds for

Figure 9. Randall in Puerto Rico, July, 1963. (Photo H. Randall)

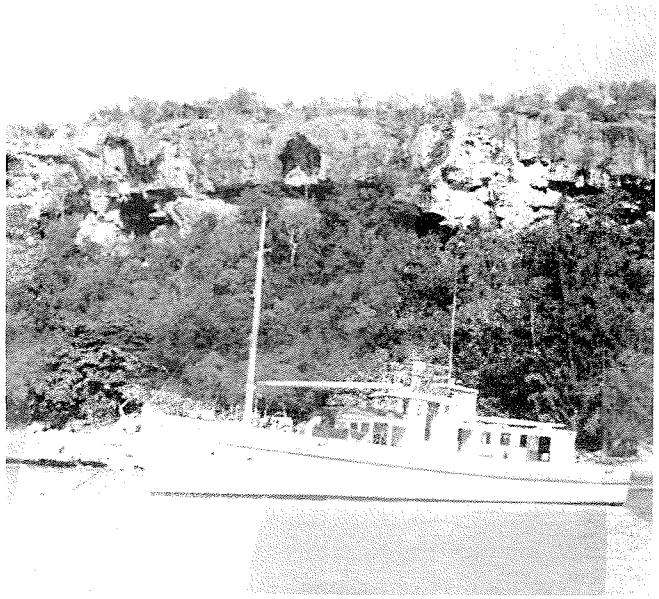

Figure 10. The 20-m *Carite*, research vessel of the Institute of Marine Biology, University of Puerto Rico, at anchor, Mona Island, 1964. (Photo J. Randall)

marine biology. I succeeded in transferring the zoo as quickly as I could.

Being a Director of a marine laboratory was fun for about two weeks but, with that and teaching, there was not much research time. Nevertheless, I continued my study of food habits of West Indian reef fishes that I had started on St. John and eventually published (Randall, 1967, deemed a *Citation Classic* in 1985).

I also started a book on West Indian fishes from photographs I took of fishes after removal from the sea by a method I published in *Copeia* (Randall, 1961c). Knowing how costly color plates would be, I took color photos only of the most colorful species and settled for black and white for the rest. With about half the text finished, I tried to find a publisher, but could find none. The World Publishing Company in Cleveland agreed to publish if I eliminated all the color figures. Although advised against it, I finally submitted the text and photos in abbreviated form as *Caribbean Reef Fishes* (1968) to T.F.H. Publications, but I was displeased with the result.

In 1965 the opportunity came to return to Hawaii as Director of the Oceanic Institute on Oahu adjacent to Sea Life Park. I wrote a proposal to study the life history of the camallanid nematode that I had been told by my professor to discontinue, knowing that the copepod *Labidocera* would be my prime suspect for the intermediate host. The proposal was rejected. The study was later published by Thomas Deardorff, and *Labidocera* was the intermediate host.

Anxious to cease being an administrator, I was able to move after a year at the Oceanic Institute, working half time at the Hawaii Institute of Marine Biology of the University of Hawaii (Fig. 11) on the ciguatera project with Hank Banner and half time

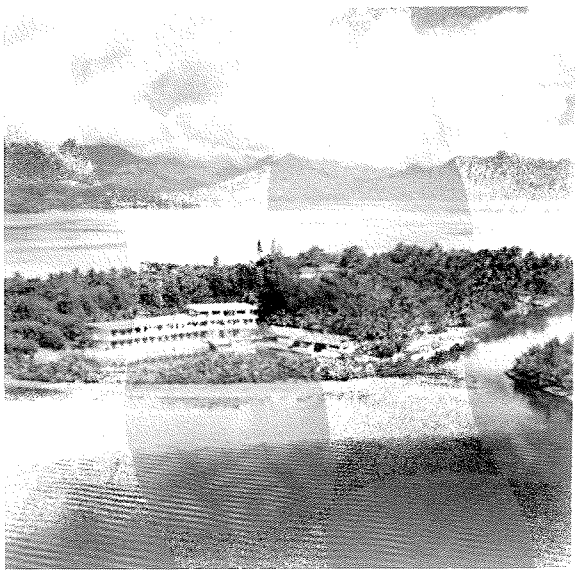

Figure 11. The Hawaii Institute of Marine Biology of the University of Hawaii at Coconut Island in Kaneohe Bay, Oahu, 1967.

at the Bishop Museum. Helen also obtained a half-time post on the ciguatera project. In 1970, I shifted to full time at the Bishop Museum in Honolulu, but continued as a member of the Graduate Faculty in Zoology at the University of Hawaii.

When I came to the Museum, there had never been a Curator of Fishes. I found the fish collection of about 5,000 lots with the specimens wrapped in cloth and jammed into jars of ethanol packed in cardboard boxes in the carpenter's shed. It was a big job unwrapping them all and putting each lot in a separate jar. Eventually I was awarded a grant from the National Science Foundation to move the collection to a new building, and I took over the fish collections from the University of Hawaii, Hawaii Institute of Marine Biology, Honolulu Laboratory of the National Marine Fisheries Institute, and the Oceanic Institute. Over the years I have made extensive fish collections throughout the Indo-Pacific region and the Bishop Museum now has over 38,000 catalogued lots of fishes, of which 2,409 are type specimens. Also files of my fish photographs, both tank photos on 120-mm film and 35-mm underwater photos, are maintained in four refrigerators at the Museum; nearly 10,000 have been scanned by the International Center for Living Aquatic Resources Management (ICLARM) in the Philippines for FishBase.

One of my tasks as a biologist on the ciguatera project was to collect large moray eels of the species *Gymnothorax javanicus*, notorious for causing ciguatera. Fifty-seven workers on a military base in Saipan ate one eel of this species (misidentified as *G. flavimarginatus*), said to have been one-foot thick. Within 20 minutes they knew they had been poisoned. In spite of immediate gastric lavage, 14 became comatose and two died (Khlentzos, 1950). This moray is rare in Hawaii, but common at Johnston Island, so we collected them by traps and by spearing. We sent them frozen to the Hawaii Institute of Marine Biology where the tissues were extracted with hot ethanol and fat solvent to obtain the toxin for biochemical and pharmacological study. Occasional small whitetip reef sharks (*Triaenodon obesus*) entered the eel traps at Johnston Island. We opportunistically tagged and released them to monitor growth and movements. Other aspects of the biology of this common reef shark were also investigated (Randall, 1977).

Much of my early field work after starting the split position at the Hawaii Institute of Marine Biology and the Bishop Museum was carried out at the Mid-Pacific Research Laboratory at Enewetak Atoll in the Marshall Islands (Fig. 12), beginning in 1967. One of my assignments on the ciguatera project was to collect large specimens of

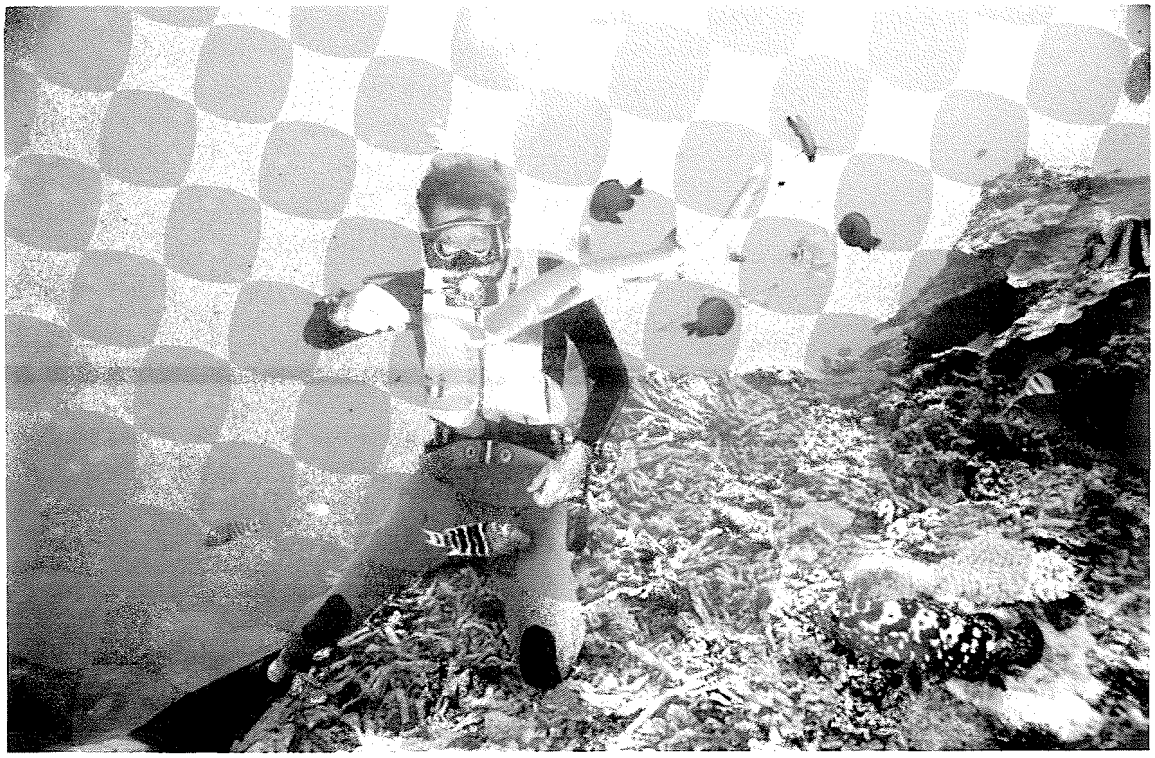

Figure 12. Randall feeding a small whitetip reef shark at Kwajalein Atoll, Marshall Islands, 1976. (Photo N. Bartlett)

fishes that are the worst offenders for this toxemia, determine their toxicity, and study their food habits, etc. (Randall, 1980a). The main objective was to be able to advise the Enewetakese, who were planning to return to their atoll, of the level of ciguatera. They had been evicted from the island when we conducted our atom-bomb tests.

Many of the fish specimens were obtained by spearing, meaning a greater risk of interaction with sharks. Already three divers had been bitten by gray reef sharks at the atoll, and I had a close call when one went into its threat posturing (Johnson and Nelson, 1973). I shot my Hawaiian sling spear at the shark, not knowing then that this was the worst thing I could do. It made a violent twisting maneuver, and the spear flipped out. At that moment the shark was facing the anchor line instead of me, so it bit and shook on the line and then went on its way.

In July 1975, while diving with Russell E. "Shot" Miller in the same pass at 18-20 m, he banged on his tank with his powerhead handle to alert me that a gray reef shark was posturing just behind me. I turned to see the shark heading for him, apparently attracted by the sound. I took a moment to arm my powerhead and then looked back to see the shark biting Miller on the head. Blood was pouring out, green at that depth, and his face mask was cut off. He rocketed to the surface with the shark following. I did not want to fire my powerhead because of fear of hitting the diver. Fortunately, the shark moved off and I helped Miller to the boat. He had seven gashes in his head (above the hairline) that required 25 stitches. He told me he had hit the

shark with his powerhead, but the shell did not detonate. These small sharks apparently only give a quick slashing bite if their threat posturing is not heeded. It is a mistake to move toward them or do anything provocative at that time. One diver at Enewetak merely fired a flash picture at a posturing shark and was then bitten on the elbow.

While at the British Museum (Natural History) in London, I asked Wyn Wheeler of the Fish Division to see the jaws of the white shark from Australia that was reported by Günther (1870) to have been 36.5 feet (11.1 m) in length. The jaws, however, seemed much too small, so I said they must be from some other shark. No, the museum number on the jaws was correct, and one tooth was missing-the same one missing in Günther's photograph. So I measured the height of the enamel of the largest tooth and the perimeter of the upper jaw (using a string and laying it straight for the measurement). Then I measured teeth and jaws at several museums for which the lengths of the sharks had been reliably recorded, prepared two graphs against total length, and plotted the British Museum specimen's data into graphs. Its length was about 17 feet (Randall, 1973a). Three other publications on the white shark claimed lengths of 11.3, 9.0, and 6.4 m which I was able to refute (Randall, 1987b). Nevertheless, from anecdotal reports, I believe a length of 6 to 7 m is possible for *Carcharodon carcharias*.

I was always more concerned with the tiger shark than the white shark because I generally dive in tropical seas where the latter is rare. On the way to the Marquesas in 1957, I stopped at the atoll of Takaroa. While snorkeling in the pass at dusk, I saw something entering the pass below me so huge that, at first glance, I believed it was a submarine. My next thought was a cetacean until I saw the tail moving horizontally. I knew what the whale shark looked like, with its characteristic white markings, so I concluded that I had seen an enormous tiger shark.

In 1978, at the drop-off at Leroy, Enewetak, I was in a cave at 46-m depth while my dive partner, Rhett McNair, was guarding the entrance with the powerhead he had invented because of the prevalence of silvertip sharks. After the dive, he told me that a whale shark had swum close to him and then went up to the surface. I asked how large it was, and he said over 20 feet because it lay alongside our 19.5-foot boat and he saw the tail break the surface. When I quizzed him about the characteristic features of the shark, it was soon apparent that he had seen a tiger shark. His response, "I thought it was a tiger shark too, but I did not know it got that big."

I knew from my review of the biology of the tiger shark (Randall, 1992a) that the largest specimen listed in the scientific literature was one from Cuba that measured 18 feet (5.5 m). But one other personal experience convinced me that this species may reach 20 feet (6.1 m) or more. I was diving off Sodwana Bay in northern Natal in April, 1979 with Margaret Smith, Director of the J.L.B. Smith Institute of Ichthyology, and four other divers. We set rotenone in 12 m and picked up many fishes. There was one full tank left, so I asked for that to continue collecting more specimens. As I approached the surface with a full net, I saw a jack that I did not recognize and speared it, resulting in a big struggle by the fish. I handed it into the boat, along with the net full of fish and asked for an empty net so I could go back down. Instead I was told to get in the boat because the surf was building up. While taking off my gear, I heard the

exclamations, "Look at the size of that tiger shark! It's longer than the boat and going right where Jack speared the fish." All four divers saw the shark and guessed it was about 20-feet long. On shore we heard from fishermen that they had seen an enormous tiger shark in the bay. About a year later there was an article in the Honolulu newspaper about a diver in Sodwana Bay who was killed by a large shark; both of his legs had been taken off. The article stated that the shark was probably the white shark. But in subtropical Sodwana Bay it was much more likely a tiger shark.

For all my diving there was only one occasion when I believed I was about to be attacked by a shark. I was setting rotenone at the reef edge at Ras Muhammed at the southern tip of the Sinai Peninsula in the Red Sea with two Israel graduate students in October, 1975. They were working the reef flat and I used scuba gear. I had a cold and could not get deeper than about 1-2 m because my ears would not clear. The sea at the same site the previous day had been clear, but this day it was murky. I had three different species of sharks, the blacktip reef, whitetip reef, and gray reef, feeding on the dead and dying fishes as I was trying to collect specimens, and I was very nervous. Once as I looked around I saw a stocky brown-colored shark about 2-m long just turning away from one of my swim fins. I decided it might be coming back, so I put a shell in my powerhead and waited just below the surface. The shark came straight for me out of the murk, and I barely had time to fire the powerhead. As the sea cleared, I could see the carcass sinking, along with a large remora, half of which had been blasted away. I wanted to collect the shark, but was unable to get down because of my ear problem. I think the species was *Carcharhinus obscurus*, as we later collected one from nearby in the Gulf of Aqaba.

After relocating in Hawaii in 1965, it was soon apparent that the greatest need in research on fishes in the incredibly rich Indo-Pacific region was in systematics, and most of my fish papers while based in Hawaii have been taxonomic. I did not realize that I had published so much in fish systematics until Bill Eschmeyer produced his monumental three-volume *Catalog of Fishes* (1998) and tallied the number of new species that various authors have described. He said I have the highest number of any living ichthyologist, with 448. By the end of this year this will top 500, which is the number of species of fishes that Linnaeus described in his *Systema Naturae* (1758). However, it cannot compare with the 1,925 fish species described by Bleeker; 1,859 by Valenciennes; and 1,734 by Günther.

In 1963, while examining monacanthid fishes at the National Museum of Natural History, I spotted a specimen from Easter Island that was a new species but had been misidentified as a filefish known only from Japan. On checking the literature on fishes of this remote South Pacific island, I learned that only 31 species of fishes had been reported (Rendahl, 1921). I inquired how one could get to Easter Island and was informed that a supply vessel came from Chile once a year. The choice was a stay of one week or one year. One week was not enough, and a year was too long.

After the U.S. built an airstrip on Easter Island for a missle-tracking station, I obtained a grant from the National Geographic Society to collect fishes at the island and flew in by Lan Chile via Tahiti in 1969 with Gerald R. Allen, then one of my graduate students, along with a scuba compressor and tanks. Randall (1970, 1976) published two

popular accounts of our research at Easter Island. Further fish collecting on two later trips,with Louis H. DiSalvo and Alfredo Cea Egãna, increased the inshore fish fauna of the island to a mere 126 species (Randall and Cea Egãna, 1984; DiSalvo et al., 1988), but the level of endemism is 22.2%, hence second only to 23.1% for the Hawaiian Islands in the Indo-Pacific region (Randall, 1998).

I wondered what the fish fauna might be at other remote South Pacific islands such as Pitcairn and Rapa, and I was anxious to return to collect fishes in the Marquesas. I was fortunate to obtain another grant from the National Geographic Society in 1970 to take the 30-m schooner *Westward*, then attached to the Oceanic Institute, on a 7-month cruise to islands of southeastern Oceania (Fig. 13). Three other colleagues were included in the scientific party: Harald A. Rehder of the National Museum of Natural History for mollusks; Dennis M. Devaney of the Bishop Museum for other marine invertebrates; and Yosihiko Sinoto, senior archaeologist of the Bishop Museum. Crew members were selected who were both sailors and divers. I joined the vessel in Tahiti and we visited Mangareva, the Pitcairn Islands (an article on Ducie Atoll by Rehder and myself appeared in 1975 in *Atoll Research Bulletin*), Rapa, Austral Islands, Cook Islands, Society Islands, Tuamotu Archipelago, and the Marquesas (popular articles by Randall, 1973b, 1974, 1978a, and 1980b). The fish fauna of Rapa was reported by Randall, Smith, and Feinberg (1990), of the Pitcairn Islands by Randall (1999), and of the Marquesas by Randall and Earle (2000). Much museum work was needed after our return to Hawaii to curate the huge collections of fishes made during the cruise and to label and file the many color photographs taken of fishes.

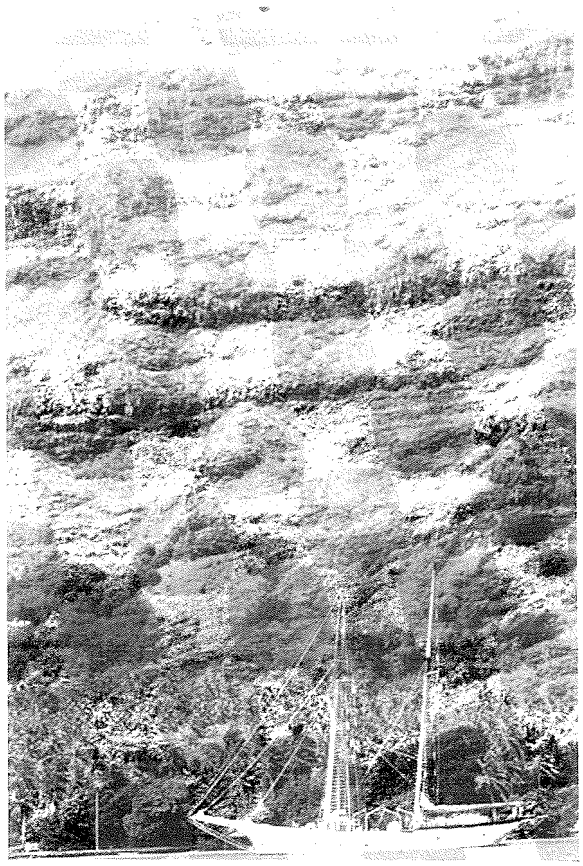

Figure 13. The 30-m *Westward* at anchor, Mangareva, 1970. (Photo J. Randall)

The majority of coral-reef fishes are so colorful that it seems criminal to illustrate them in black and white, yet most journals require authors to pay for color plates. With support of grants from the National Science Foundation and the Engelhard Foundation, Helen and I launched a new series entitled *Indo-Pacific Fishes* in 1982 for systematic revisions of genera or higher categories of fishes in the Indo-Pacific region with funds provided for color reproduction. Thirty-one of these monographs have been published to date, and four more are in press.

The most gratifying of my publications have been guidebooks on fishes written to serve the need of both the scientist and the layman. I have published ones for the Indo-Pacific on the Red Sea (1983), Hawaiian Islands (1985, 1996), Great Barrier Reef (with Allen and Steene, 1990), (Fig. 14), Maldive Islands (1992b), and Oman (1995); in addition, *Sharks of Arabia* (1986). I am currently working on a volume on South Pacific fishes to be followed by a more definitive treatment of the reef and shore fishes of the Hawaiian Islands. I am also coeditor with Phillip C. Heemstra of South Africa of a large, multiple-author volume in progress on the fishes of the western Indian Ocean, including the Red Sea and Persian Gulf. Eventually, I plan to complete a comprehensive book on the fishes of the West Indies (Fig. 15) that I started 37 years ago. Publishers of guidebooks today want illustrations in color, so I have been taking underwater photographs of West Indian fishes of which I had only black and white

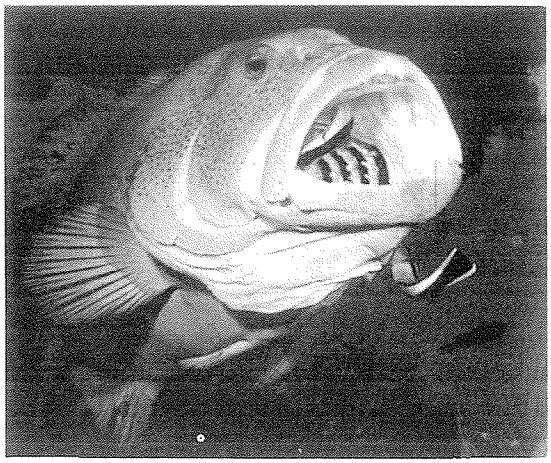

Figure 14. The grouper *Plectropomus leopardus* and a pair of cleaner wrasses (*Labroides dimidiatus*), Heron Island, Great Barrier Reef, 1991. (Photo J. Randall)

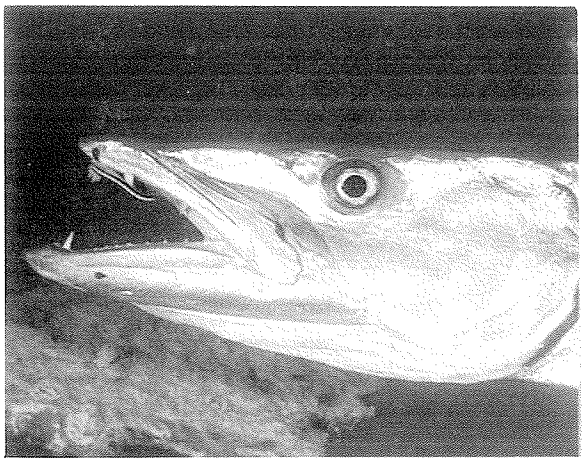

Figure 15. *Sphyraena barracuda* with the cleaning goby *Gobiosoma randalli* in its mouth, Bonaire, 1985. (Photo J. Randall)

photos before.

In my 52 years of diving (Fig. 16) I have become progressively more alarmed at the degradation of reefs and other marine environments from such destructive processes as chemical pollution, siltation from dredging, runoff following deforestation, unwise introductions of exotic marine oganisms, fishing with explosives and cyanide, and especially overfishing, in general. All these deleterious effects on reefs induced directly by man may prove to be minor compared with the impact of global warming. We have seen the extensive coral death in many tropical areas from the warming of the seas during the most recent El Niño. I am pessimistic about the future because of the failure of the industrial nations of the world to greatly curtail the release of CO_2 into the atmosphere that is principally the result of the burning of fossil fuels. I fear that we will soon be facing far more extensive coral mortality and the resulting deterioration of reefs.

As biologists, there seems to be little we can do with respect to global warming other than increasing our warnings of the consequences from the enormous emissions of

Figure 16. Randall off Satonda Island, Indonesia, 2000. (Photo L. Pozzoli)

CO_2 the world is producing, but we can do more than we have to promote conservation in the sea. I have long been a strong advocate of the need for more marine reserves to protect our dwindling marine resources (Randall, 1969, 1978b, 1982). These reserves really work and this becomes obvious over time even to fishermen who first opposed them.

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